

ADDRESSING THE ISSUE OF GENERALIZABILITY OF ETHNOGRAPHIC FIELDWORK

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ABSTRACT. This essay offers a concise review of ethnographers' longstanding concern with the geographically and temporally bounded character of classical participant-observation fieldwork. It begins by outlining the archetypal construction of fieldwork before examining two contrasting positions on the generalizability of ethnographic data, positions that illustrate the wide spectrum of attitudes held within the discipline. Building on a Geertzian perspective, the essay reconceptualizes generalizability as a mode of theory production aimed at rendering intelligible the conceptual structures that organize social action. I argue that this form of theoretical generalizability is best advanced by engaging three influential critiques of ethnographic practice focusing on the deconstruction of fieldwork's taken-for-granted premises, the challenge to single-site generalization, and the move beyond reductive inductive–deductive dichotomies.

Keywords: Ethnography; Fieldwork; Generalizability; Geertz; Theory Production; Methodology

The ethnographic method was born as a way of focusing the attention of the researcher on the experiences, actions, and understandings of the members of the social world he or she attempts to study (Atkinson 2001). In order to record these elements, and attempt to give them meaning, ethnographers are committed to the method of participant observation, regarded the royal road to achieving their scholarly objectives. Of course, I do not mean to imply here that the ethnographic method is limited to, or should be reduced to, participant observation, but the concept provides the Aristotelian *differentia specifica* that allows me to define and position it alongside other ways of studying the social world.

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Going “into the field” for a couple of months had become more or less synonymous with doing ethnography (Emerson 2001; Emerson et al. 1995) . This led the first practitioners of the ethnographic method to conceive their approach to the object of their study as inherently bounded in space. The need for in-depth analysis seemed to require a focus on a bordered location, as well as a moderately long period of immersion in the lifeworlds of those construed as targets of scholarly attention.

A clear example of this tendency to actively cut out a bounded “piece of land” and attribute to it a sense of wholeness and completeness usually associated with autonomous and distinct social phenomena is offered by Evans-Pritchard’s classical study *The Nuer* (Evans-Pritchard and Alexander Street Press. 1969). To support my claim that the beginnings of ethnography as a method of research involved a vision of fieldwork that is spatially bounded, it is revealing to note that Evans-Pritchard defined the object of his study as an “ethnographic area.” The classic (in both senses of the term) ethnographer, focusing on uncovering, classifying, and making intelligible the internal relations structuring his ‘field,’ tended to neglect, or worse, actively disregard as irrelevant and biased, the relationships between the group under study and anything that existed outside the narrowly defined ‘ethnographic area.’

I do not make far-reaching claims. I believe that I have understood the chief values of the Nuer and am able to present a true outline of their social structure, but I regard, and have designed, this volume as a contribution to the ethnology of a particular area rather than as a detailed sociological study, and I shall be content if it is accepted as such.” (Evans-Pritchard and Alexander Street Press. 1969, 15).

This is highly significant for the limited perspective the author had on the social phenomena that constituted his object of study. It reveals a propensity on the part of the ethnographer to avoid, exclude, or diminish the influence of any social phenomena not located within the perimeter he had imagined to be his ‘field’ —in a word, anything that was not local.

If the fieldwork of the ethnographer was bounded in space, it was not at all so in terms of time. Quite on the contrary, the ethnographer’s work seemed to have taken place in the framework of a timeless time. Keeping with quoting with the classics I will appeal to the introduction to Edmund Leach’s *Political Systems in Highland Burma*, (1970) where he meditates on the time-reference of classical anthropological literature.

This prejudice in favour of ‘equilibrium’ interpretations arises from the nature of the anthropologist’s materials and from the conditions under which he does his work. The social anthropologist normally studies the population of a

particular place at a particular point in time and does not concern himself greatly with whether or not the same locality is likely to be studied again by other anthropologist at a later date. In the result we get studies of Trobriand society, Tikopia society, Nuer society, not 'Trobriand society in 1914', 'Tikopia society in 1929', 'Nuer society in 1935'. When anthropological societies are lifted out of time and space in this way in the interpretation that is given to the material is necessarily an equilibrium analysis, for if it were not so, it would certainly appear to the reader that the analysis was incomplete. (Leach 1970, 7).

The 'classic' approach of ethnography toward time seems to have been grounded in the assumption that the 'natives' are not part of the contemporary, progressive, linear timeline. Instead, they are assigned to cyclical, immobile time structures, that is, before the advent of 'history' and its vicissitudes and never-ending alterations of the 'today.' Classical anthropologists do not think they analyze a culture at a specific point in time, say, this year or that decade, but rather that culture in general.

We have established by now that the method of participant observation was connected to strong assumptions about the boundedness and strict demarcation of its object of study, in comparison to other methodologies of social research. This fact raised the issue of the generalizability of findings and theories that arise out of the (allegedly limited relevance and scope of the) study of local groups conceived as distinct and isolated societies and cultures.

In the following section, I will cover three ways which I found exemplary in which ethnographers have answered concerns related to generalizability. At one extreme of the continuum constituted by the possible ways in which ethnography has reacted to these critiques lies the attempt to question the meaningfulness of the very concept of 'generalizability,' usually alongside the two other terms that accompany it: 'validity' and 'reliability.' To illustrate this stance, I chose the passionate article by Valerie J. Janesick: *"The Dance of Qualitative Research Design"* (2000). Janesick's position is exemplary in its rejection of quantitative methods of scientific inquiry. Her stance is perhaps best defined by the concept of "methodolatry," which she coined to oppose "a slavish attachment and devotion to method" (2000, 215). Over-focusing on the method of research at the expense of the actual substance of the story being told, the researcher runs the risk of losing "the human and passionate element of the research." Methodolatry is operationalized by the author in the "trinity" of validity, reliability, and generalizability, which tends to be fetishized by a number of social scientists.

The substance of Janesick's critique is directed against traditional notions of generalizability. She claims that for the researcher interested in "questions of meaning and interpretation in individual cases," the traditional way of

thinking about generalizability is inadequate. The value of a case study lies exactly in its uniqueness, and the ethnographer should celebrate that instead of attempting, in vain, to generalize it using “psychometrics that rule our research and (...) decontextualize the individual.” (Janesick 2000, 217) The alternative favored by Janesick stresses the “presentation of solid descriptive data, so that the researcher leads the reader to an understanding of the meaning of the experience under study” (2000, 215). I beg to differ. While one should cherish the specificity of one’s method of research, and indeed the work of an ethnographer does open to her the richness of local or individual settings, abandoning all theoretical pretension and indulging in “solid description” harms more than helps the status of the practitioner of the ethnographic method among other social scientists.

At the other end of the spectrum I locate the theories of Clifford Geertz, who managed to produce perhaps the most brilliant reformulation of the generalizability of the research results grounded in participant observation. In his *“Thick Description: Toward an Interpretive Theory of Culture”* (Geertz 1973, 55–77), Geertz addresses the need to generalize in the study of cultures:

“(...) the essential task of theory building here is not to codify abstract generalities but to make thick description possible, not to generalize across cases but to generalize within them.” (Geertz 1973, 72).

Thus, Geertz reformulates the epistemological objective of participant observation. Rather than inductively generalizing the trends observed in the social world in order to come up with increased accuracy of predictive statements, participant observation may be better apprehended using a challenging analogy with clinical inference:

“Rather than beginning with a set of observations and attempting to subsume them under a governing law, such inference begins with a set of (presumptive) signifiers and attempts to place them within an intelligible frame.” (Geertz 1973, 72)

Thus, the work of the ethnographer should be, from an epistemological point of view, more similar to the active scan of the clinician analyzing a set of symptoms in order to diagnose the family resemblance that connects them (the illness), rather than to that of a physicist in search of the regular patterns governing his object of analysis. As ethnographers:

(...) our double task is to uncover the conceptual structures that inform our subjects’ acts, (...) and to construct a system of analysis in whose terms what is generic to those structures (...) will stand out against other determinants of social behavior. (Geertz 1973, 73).

This new way of defining the theoretical body that is the product of the ethnographic method does not reject the idea of generalizability as useless. Instead, it re-conceptualizes its meaning. Yet, this does not mean that, after giving up the pretension of uncovering general laws, the ethnographer is left with a self-celebratory enjoyment of the richness of the field—and even less so with the Sisyphean task of starting from theoretical scratch with every new study. Theories and concepts do circulate and are refined from one study to the next, but in this case: “progress is marked less by a perfection of consensus, than by a refinement of debate.”

The post-Geertzian era brought about new insights into the relationship between the local, bounded character of the fieldwork and the general or limited usage of its findings. In a reflexive effort, scholars re-analyzed the conceptual core of their discipline, and these critical stances bore interesting fruits on the issue of generalizability. In the following section, I will address three stances: Akhil Gupta and James Ferguson’s “Discipline and Practice: The ‘Field’ as Site, Method and Location in Anthropology” (1997), George Marcus’ article “Ethnography In/Of the World System: The Emergence of Multi-Sited Ethnography.” (1995) and of Stefan Timmermans and Iddo Tavory’s “Theory Construction in Qualitative Research: From Grounded Theory to Abductive Analysis” (2012). To my mind, these are still the most relevant contributions to the topic.

“Discipline and Practice” is the opening chapter of *Anthropological Locations* (1997) and lays its conceptual and epistemological foundations. In an original fashion, the article takes a reflexive perspective on the intellectual history of the method of participant observation and questions the way in which the idea of ‘working in the field’ was constructed and modified over time. The dichotomy ‘field’/‘home’ is undermined through the analysis of their alleged spatial separation, of the rituals of ‘entry’ and ‘exit,’ and of the ‘hierarchy of purity’ of field sites. The authors question the types of knowledge provided by the Geertzian “study in villages,” not “study of villages.” Their question, “Why do we study ‘in villages?’” reveals an unacknowledged hierarchy of topics, based on the unfamiliar, the different, and the local—“read: not like at (First World) home”—as topics of ethnographic inquiry. In the same vein, the “real anthropologist” seems to be judged according to a yardstick that tends to eliminate women, minorities, or non-Western researchers writing about the societies in which they grew up. Thus, according to Gupta and Ferguson, the field of participant observation research has to be re-invented.

The two authors advocate the abandonment of the commitment to the ‘local’ and its replacement with “attentiveness to epistemological and political issues of location.” Instead of a vision of discrete, separate cultures (fields conceived geographically), they support a perspective shaped by fields of

unequal power relations. The researcher thus ceases to be the privileged and unique “expert” solely enabled to uncover a specific form of knowledge. Instead, they are assigned the task of:

(...) forging links between different knowledges that are possible from different locations and tracing lines of possible alliance and common purpose between them. (...) we view a research area less as a ‘field’ for the collection of data than as a site for strategic intervention. (Gupta and Ferguson 1997, 39)

Thus, according to Gupta and Ferguson, the debate on the generalizability of ethnographic research is grounded in a false assumption. It begins with the idea that participant observation is a method that can be applied only in geographically limited cultural areas, by a specific archetype of researcher, and that it yields expert and specialized forms of knowledge. By deconstructing these unacknowledged premises of ‘fieldwork,’ they succeed in relocating the debate onto promising new grounds.

Another perspective that shares a critical stance toward the classical way of conceiving the ‘field’ and ‘fieldwork’ was offered by Marcus in *“Ethnography In/Of the World System: The Emergence of Multi-Sited Ethnography”* (1995). As indicated in the title, the article offers a perspective on ethnographic fieldwork, exploring the methodological implications of the conceptual and practical shift from conventional single-site locations to multiple sites of observation and participation. Placing this conceptual shift within the intellectual tradition of what he calls “the intellectual capital of so-called postmodernism” (Marcus 1995, 97), Marcus begins by asserting that ethnography consists essentially of a search for cultural logics that are “always multiply produced” (Marcus 1995, 97), and therefore are ideally suited to analysis not only through the classical study of a geographically bounded locus, but through strategies that involve following connections and associations across multiple sites and locations.

Acknowledging the disintegration of grand narratives such as global capitalism or the world system, the author highlights the collapse of the distinction between the lifeworlds of ethnographic subjects and larger systems. He advocates the need to rediscover:

“new paths of connection and association by which traditional ethnographic concerns with agency, symbolism, and everyday practices can continue to be expressed on a differently configured spatial canvas.” (Marcus 1995, 98)

Marcus acknowledges the risk of attenuating the power of fieldwork by losing “some of the mystique and reality of conventional fieldwork” (Marcus 1995, 100) in the shift toward multi-sited ethnography. Yet, what is not lost—and is, in fact, enhanced by the new perspective—is the capacity of participant

observation to produce translations “from one cultural idiom to another” (Marcus 1995, 100). This is achieved by abandoning the simplistic dualism opposing the researcher to the ‘native,’ in favor of a richer analysis of multiple social locations and the need to relate them. The researcher undertakes the sensitive task of mapping themselves and renegotiating their identity as the research landscape changes. The writing of this type of ethnography emerges as a particular mode of publication in its own right.

The movement of the researcher through multiple sites—following people, things, metaphors, stories, biographies, or conflicts—enables them to bypass the simplistic debate over the generalizability of findings anchored in a single, spatially and temporally bounded location. By shifting the frame of reference and transposing fieldwork into new coordinates, the perspective suggested by Marcus allows the ethnographer to conceptually and practically move forward—not only closing the debate on generalizability, but also opening a range of new ones.

Last but not least, I address the contribution of Timmermans and Tavory in “*Theory Construction in Qualitative Research: From Grounded Theory to Abductive Analysis*” (2012). Here, generalizability is related to the explanatory power of the theory that results from ethnographically grounded research: “the theories developed in abductive analysis denote an attempt to generalize causal links and descriptions of the world out of particular empirical instances.” (Timmermans and Tavory 2012, 174).

In an effort to bypass the inductive vs. deductive dichotomies in the construction of fieldwork-grounded analyses, the two authors advocate an approach “aimed at generating creative and novel theoretical insights through a dialectic of cultivated theoretical sensitivity and methodological heuristics.” (Timmermans and Tavory 2012, 180). Stressing the importance of deductively checking new explanations against empirical data to test theoretical robustness (Timmermans and Tavory 2012, 175) the authors focus on “unanticipated and surprising observations” which allow the researcher to overcome the limits of inductive approaches such as grounded theory (Charmaz 2017; Glaser and Strauss 1967) or analytic induction (Katz 2001).

In short the novel manner of approaching ethnographies can be summarized as follows:

“abductive analysis involves a recursive process of double-fitting data and theories. An abductive inference involves making a preliminary guess based on the interplay between existing theories and data when anomalies or unexpected findings occur. If the existing theories fully account for the empirical phenomena, the researcher has simply verified an existing theory. (...) pushing the data against existing theories will likely identify changed circumstances,

additional dimensions, or misguided preconceptions. Anomalies, which are inevitably both empirical and theoretical, then require the development of tentative new theories.” (Timmermans and Tavory 2012, 179)

Therefore, the promise of a generalizable case-study is predicated upon the quality of its theoretical acumen. And constructing an insightful theory is, in the opinion of the two authors, also the outcome of methodological rigor, which can be achieved through the processes of revisiting, defamiliarization, and alternative casing.

It was my intention to offer a short review of the ethnographers’ concern with the geographically and temporally bounded character of classical participant-observation fieldwork. I began by presenting the manner in which fieldwork was archetypically construed. I then presented two opposing stances defending the generalizability of ethnographical data, stances that, in my view, exemplify the wide range of attitudes that practitioners in the field hold on the issue. I embrace a Geertz-ean perspective that reconceptualizes the concept of ‘generalizability’ as an effort of theory production focused on weaving a system of analysis that makes understandable the conceptual structures informing our subjects’ behavior. This form of theoretical generalizability can be better put into practice if we heed three seminal perspectives on ethnographic work: Gupta and Fergusson’s deconstruction of the unacknowledged premises of ‘fieldwork,’ Marcus’ argument against generalizing from a single ‘site’ and Timmerman and Tavory’s invitation to move beyond the reductionist inductive vs. deductive relationships between theory and data. I conclude this essay with the survey of these three critiques of the ethnographic theoretical and methodological corpus, critiques that reformulate and translate the debate into seminal new coordinates.

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