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Authorship, research assistants and the ethnographic field

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This rich special issue opens up a large set of questions about authorship that goes well beyond the question of research assistants in the field. I will first explore this issue, and then attempt to position the question of research assistants in a wider ‘field’. I will argue that Middleton and Cons (and their contributors) are quite right to focus on research assistants in the field, because the neglect of this topic leaves a large gap in anthropology’s self-reflexivity. More importantly, and this will be my final point, a focus on field assistants pushes the boundaries of ‘the field’ itself. This point, along with other emergent reconceptualizations of ‘the field’, has the potential to radically rethink the temporal and spatial configuration of fieldwork.

The problem of authorship and authority in ethnographic work

The focus on field assistants in this special issue highlights the problem of authorship and authority in ethnographic work. Perhaps more than many other academic fields, successful ethnographic work depends upon a wide range of collaborations. Unlike the sciences, where the contributions of multiple people to a research project are acknowledged in long lists of authors, ethnographic work is almost always ‘single-authored’, *claimed* to have been the work of a single individual.

While we all know that this claim to sole authorship is always fictional, and that the pressures of sole authorship are connected both to the rise of ‘the auteur’ and notions of private property, the claim to ‘sole authorship’ is particularly misplaced in a discipline like anthropology, and in a craft like ethnography. It is true that anthropologists, unlike scientists, do not work in large teams in a laboratory. But the spirit of cooperation and collaboration that makes for a successful laboratory is present in ethnographic work. What, then, makes it disappear in the discipline’s own self-presentation and self-representation?

One obvious difference between anthropology and modern sciences is that, for the most part, anthropologists continue to function, like most other scholars in the

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humanities, in a craft tradition rather than an industrial model of production. Instead of Taylorized processes broken down into sequential steps, to be done by different grades of laborers (laboratory assistants, doctoral students, postdoctoral fellows, faculty, principal investigator), the craft of ethnography requires that the anthropologist be an integral part of each step of the research process, from the formulation of the problem to data collection and write-up. The correlate of this model of academic production is that, as in other craft traditions, teaching future generations is done largely through processes of apprenticeship. In the case of anthropology, teaching happens in small graduate seminars and individual face-to-face discussion.

However, this does not mean that ethnographic work is done by individuals sitting alone in their offices facing a blank computer screen. Ethnographic work is the result of many different collaborations, some of which begin even before the stage of 'professionalization' in a formal doctoral training program. Of course, there are differences in how anthropologists are trained throughout the world, and my comments below are limited to the example of the US. Students often come to doctoral programs in the United States with a dissertation project already formulated, the result of previous interests shaped by teachers in an undergraduate program, or life experiences, or work experiences during or after an undergraduate degree. Thus, in some ways, the field of research has already been defined. Then, through coursework in the doctoral program, proposal-writing seminars, discussions with their advisors, and with other graduate students, and lectures and presentations that they hear, students' interests are further refined or shaped. A doctoral proposal for funding has already been shaped by multiple collaborators and interlocutors, perhaps most intensively by advisors and other graduate students. Once a proposal is submitted, other people contribute to it as well: reviewers who read proposals may give 'feedback', and the funding agency itself might end up shaping a proposal in various ways by deeming that it is not feasible, or too ambitious. Finally, faculty and students at host institutions in the nation-state where the fieldwork is conducted may guide the research by proposing alternative research projects or modifications to the existing one, based on significant recent events or trends.

In their respective essays, Cons, Middleton, and Hoffman report on how their own research trajectories were altered or shaped by research assistants. Gold's and Holmberg's essays demonstrate how the research agenda is shaped by the involvement of the researcher in the lives of the families who provide them with housing and support, in this case the families of their respective research assistants. I will come back to the particular role played by research assistants in the data gathered in ethnographic research, because, as this collection emphasizes, their role as authors of the research is perhaps even more central than some of the other collaborators mentioned so far.

Let me for the moment complete the list of collaborators. After fieldwork, the data often has to be transcribed or collated by a variety of methods, and research assistants or companies may be hired who put the data together into a form that is

usable. As the researcher translates the findings into chapters or papers, other interlocutors shape the ideas or interpretation of the data, perhaps even pushing the researcher to do new fieldwork on areas that she has overlooked, or where the evidence is incomplete. The work is also shaped by feedback from writing groups, comments from advisors and other graduate students, and from audiences at professional presentations. What emerges in print and in presentations, therefore, is a highly mediated product, the result of the labor of many different people. And, yet, the conventions of academic writing privilege only the labor of one individual – ‘the author’.

Shouldn't we really be asking: ‘Who *is* the author of the ethnography?’ Once we abandon the pretense that it is the result of the Herculean labor of an individual, a completely new set of questions opens up for examination. For example, we could ask: How are different authors to be acknowledged? Those who helped shape the research, those who helped do the fieldwork, those who helped write the ethnography – all played different roles in the production of the final product, the ethnography. How is their contribution best acknowledged? Is it sufficient to relegate it to the short ‘Acknowledgements’ section of the book? Given the conventions of ethnography, the people in the field who helped often cannot be named even there, their presence being relegated to pseudonyms.

There are two solutions to this problem of authorship. The first is to expand the notion of ‘author’ so that the multiple contributions of different people can be fully acknowledged. I have always wondered at the line when ‘the author’, after acknowledging the contributions of many people to the final product, says, ‘I alone take full responsibility for all errors’. If the interpretive community to whom the person belongs takes credit for that piece of work, should they not also take responsibility for its mistakes and failures? Taking responsibility for all errors, of course, is another way of privileging the author’s own voice, or of discrediting the labor of others in the production of the work. What would ‘expansion’ of authorship mean in the production of ethnographies? A long list of authors, as in scientific articles? And would field assistants, who help in the collection of data, be included in such elongated by-lines?

There is another approach to this question of multiple ‘authors’ that is much more radical, but which strains against current conventions of accreditation and ownership. The rise of the concept of intellectual property, and the monetization of educational capital, make this strategy less likely to succeed, but it does offer a radical rethinking of the importance we give to ‘authorship’. An analogy might help make the point here. Mughal miniatures are widely recognized as masterpieces of ‘medieval’ painting. Yet these masterpieces were produced in craft factories: one artist painted the sky, another painted peacocks, a third specialized in drawing women, etc. When the painting was finished, it was unsigned. No single individual produced the painting, yet its beauty and artistic value were undiminished by the absence of a creative genius to whom it could be credited. Authorship and connoisseurship produce each other in a regime of private property. If we admired or criticized ethnographies for what they did, and not for who produced them and

under what circumstances, we might let the work stand ‘on its own’, that is, without an author.¹ Despite the postmodernist claims of ‘the death of the author’, the centrality of authorship in a capitalist economy obfuscates the *social* nature of the production of anthropological knowledge.

Research assistants in the field

In situating the particular problem to which this issue is dedicated – the role of research assistants in the field – in the context of the problem of ethnographic authorship and authority, I do not intend to reduce, under the heading of authorship, either the singularity of the contribution of research assistants or the myriad ways in which they configure ethnographic work.

Before leaving the question of authorship altogether, however, it might be helpful to make two points. First, that just because many people are responsible for creating the ethnographic text it does not mean that all contributions are of the same kind or that they are equal. Even if one were to stay within the discursive framework of authorship, we lack a sophisticated vocabulary for dealing with the range of authors that help produce ethnographic work.

Second, we need a means by which to acknowledge the particularity of different individuals in the shaping of the ethnographic work. It is not just a matter of ‘how much’ someone contributed, it is also about the particular roles taken by different people that index their distinctive contributions. Research assistants in the field do much more than complicate our ideas of how to attribute authorship; they fundamentally configure the process and results of data collection, and our notion of ‘the field’ itself (an issue that I will take up in the next section).

Unlike its sister disciplines such as sociology and economics, a central value in anthropological work lies in collecting your ‘own’ data. Not only is anthropological data often largely qualitative, but it depends in large measure on the affective and bodily practices and peculiarities of the ethnographer. Moreover, ethnographic data is inherently social: much more than survey research, it is created through encounter and experience, it is vulnerable to contingency and circumstance, and it can be influenced more easily by the negative pressures of governments and locally-powerful people. In this milieu, research assistants in the field can play an especially important role in configuring and shaping the ethnographic project.

If this is the case, why has there been so little discussion of research assistants in the field? Middleton and Cons are absolutely correct in pointing out that discussions of the role of research assistants are few and far between in the ethnographic literature. Somehow, even in ‘self-reflexive’ ethnographies, research assistants manage to disappear in the ‘self-presentation’ of the author who is asserting her authority.²

How does one explain the absence of the research assistant in the field in the published literature? There are many possible explanations for this lacuna. The anthropologist may fear that reference to a field assistant might expose her

linguistic incompetence. One dirty little secret of the discipline is that most anthropologists do not attain 'near-native' competence in the languages of the people they study, and to draw attention to the fact that one does not understand, or did not initially understand, what people were saying undermines ethnographic authority. Using an assistant may also underline the inability of the anthropologist to culturally negotiate the fieldsite. It may expose how little the anthropologist knew about her fieldsite and the people who live there. Finally, using a native research assistant may also be accompanied by postcolonial guilt: paying a native assistant a salary may put the ethnographer in a difficult ethical and moral position, as he may find himself replaying the dynamics of a racial, colonial order that he finds repugnant. (Middleton's essay is exemplary in confronting this issue, but all the essays hint at this problem.) Thus, embarrassment, concealment, and postcolonial guilt may all be important reasons why the native assistant's role is not acknowledged as fully and explicitly as it might.

There is thus a political impetus in Middleton and Cons's focus on the (native) research assistant. Interestingly, in their Introduction, they do not entertain the possibility that the research assistant could be a foreign graduate student whom the advisor brings along for her field research (a situation that is quite common in field research in archaeology and biological anthropology). In Cons's example from Bangladesh, the 'native' research assistant is actually a non-native city dweller whose own relation to the ethnographer's subjects is that of an outsider, if not a foreigner. Focusing as they do on the paid, native, field research assistant, the contributors to this issue expose yet another of anthropology's dirty little secrets, the reliance of anthropologists on others whose labor, connections, and practical knowledge are then normally disavowed in the 'writing up' of the ethnography.³

By itself, the task of exposure and scandal has lost its ability to shock as anthropologists have increasingly subjected their own practices to self-critique in a manner that has few parallels in adjacent disciplines in the social sciences and humanities. It is this capacity for introspection that enables anthropology to be able to self-renew and regenerate as a discipline. The focus of this issue is not so much to shock and scandalize as it is to increase self-awareness about the framing of ethnographic authority. The question is not whether to acknowledge the presence and role of research assistants but how best to do it, and what kinds of authorship to attribute to them.

The particularity and peculiarity of research assistants in the field lies in the fact that they are involved in the collection of ethnographic data, and not so much in its subsequent reworking and framing in the process of 'writing up'. In qualitative data collection, however, the distinction between 'data collection' and 'interpretation' is never clear-cut: there are different rounds of interpretive work that go into the making of an ethnography. The 'raw data' is itself interpretive, relational, affective, and contextual. As the different examples in this collection illustrate all too clearly, native research assistants may shape whom the ethnographer speaks to, what is said, what is translated, how the encounter with an 'informant' is staged,

etc. The 'data' that is then obtained is multiply mediated, social, processual, and highly contingent.

Paying attention to the role of the research assistant is then not simply to acknowledge the presence of another who helps shape the process of data collection, it is to be explicit about the process by and through which data comes to be recorded, to diagram as carefully as possible its multiple mediations, and to record and chart the conflicts and difficulties of 'data collection'. It seems to me that the essays in this issue argue against the disappearance of data in polished interpretation, pointing to the value of highlighting how the *process* of data collection itself can aid the interpretive enterprise. Not only does such an enterprise make the multiple authors of ethnographic work more evident, it also makes the distinction between the stages of 'data collection' and 'writing up' less clear and less self-evident. However, one of the most important interventions such a focus might produce is a rethinking of the idea of 'the field' itself.

'The field' created by the field assistant

Another very important intervention made by the essays in this issue is to think about 'the field' that is constructed by the different workers in the field: not just the ethnographer, but the ethnographer's assistants, and her interlocutors, enablers, or opponents. In the rethinking about the field after *Anthropological Locations* (Gupta and Ferguson, 1997), the idea of the field has been developed by thinking of it as a relational and temporal entity as much as a spatial one. The question asked by this issue has to do with how the different workers who do ethnography construct a field from those interactions. The job, as Middleton and Cons state in their Introduction, is to think anew 'the relations of the *field* to the *workers* that bring it into being'.

The most important point here is that the field is not a given space that the fieldworker walks into. Rather, it is constituted by the network of connections and linkages forged in doing fieldwork. And that is where the research assistant enters the picture. As these essays repeatedly demonstrate, the research assistant is often key to which networks are created and how they come into being. He is the person who makes certain kinds of possibilities for fieldwork 'emerge' by actively forging connections and networks between the ethnographer and local actors.

One interesting twist that this line of thinking introduces is that, as the relationship between the researcher and the research assistant changes, the field 'shifts' in that different people and relationships come into focus while others recede in importance. Thus, if the field is formed by the network of relationships itself, then it cannot be a static object but a constantly moving one. Viewed in this manner, 'writing up' an ethnography using 'the data' collected during fieldwork is to open up to process and method in a manner that is still largely unaddressed in the ethnographic literature.

Notes

1. I should hasten to add that I do not mean by 'standing on its own' the usual formalist point that the work should be seen in a decontextualized manner, as a transcendental and timeless object.
2. Under the rubric of 'research assistant' here I am thinking not only of the paid assistant but of the various assistants who work to ease the way of the researcher even when she is purportedly working 'alone'.
3. Gold's essay, as has her previously published work, most explicitly acknowledges the role of her research assistant and his family.

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Author Biography

Akhil Gupta is Professor of Anthropology at the University of California, Los Angeles, and Director of the Center for India and South Asia at UCLA. Professor Gupta's research interests include development and the state, ethnography of information technology, environmental anthropology, and India and South Asia. He is the author of *Red Tape: Bureaucracy, Structural Violence and Poverty in India* (Duke University Press, 2012) and *Postcolonial Developments: Agriculture in the Making of Modern India* (Duke University Press, 1998). He is the co-editor of *Anthropological Locations: Boundaries and Grounds of a Field Science* (University of California Press, 1997), *The Anthropology of the State* (Blackwell, 2006) and *The State in India after Liberalization* (Routledge, 2010).