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# Data: On Learning How to Ask, See and Feel

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## Abstract

Ethnographic research involves coming to know a society's culture and religion in several different ways both quantitative and qualitative. Interviewing, field recordings, systematic observation and the researcher's own (inter-)subjective experiences are some of the most common methods of ethnographic data collection, but integrating these multiple methods is no mean task. Every method presents unique possibilities and problems for answering our research questions. A method that at first seems to be revealing can end up having limited applicability; and conversely, others that may at first blush seem shallow can end up leading to significant insights. This article argues for the necessity of critically assessing how data is produced and suggests that data emerges when the researcher learns how to view their experiences and observations in new ways.

**Keywords:** data; ethnography; interviews; video recording; embodiment; anthropology.

## Introduction

Of the critical terms included in this collection, "data" perhaps most directly frames ethnography as a social scientific endeavor. The term's connotations of positivism and quantifiability may even produce a kind of apprehension in

readers who prefer to think of the ethnography of religion as a more humanistic endeavor. As several of my co-contributors here emphasize, ethnography (both in the sense of a research methodology and texts that report the findings of those methods) is the product of intersubjective experiences, and any good ethnographer knows that in order to do our work we must carefully cultivate and mindfully manage the social relationships we build with our research consultants and collaborators. But ethnography, as a scholarly enterprise, is also the product of rigorous data collection and analysis, by which I simply mean that it is a process of learning to recognize what is (potentially) meaningful to our ethnographic subjects' religious lives and finding ways to make that meaningfulness legible to ourselves and our readers. Doing so, I would argue, demands that scholars be attentive not just to what happens around us when we are in the field, but also to how we categorize, systematize and interpret our experiences in the field.

Realizing the lofty, if also somewhat clichéd, goals of seeing the world through another's eyes or rendering the strange familiar and the familiar strange invariably means taking note of all manner of qualitative and quantitative information. Yet, information does not in and of itself constitute data. Data is what information becomes when we assess, scrutinize, evaluate and organize it into useful categories to meet our research goals and objectives. It may be true that everything we encounter in the course of our research is potentially important information; but it is also true that not everything is equally important, and that even things that may seem to be significant can end up not being a major part of our research and vice versa. Thus, an important part of doing ethnographic research is learning ways to sort through our observations and experiences to create the data upon which our analyses can be built.

In this article I want to share three short stories about my own process of finding data in my work on Q'eqchi'-Maya people's engagement with Catholic Christianity in Cobán, Guatemala that I think illustrate the importance of thinking both prospectively and retrospectively about what it is we do as ethnographers. Before that, though, I think it is useful to say a little bit about the context of my research and its aims. My research began its life as a doctoral dissertation project. My graduate training was in linguistic anthropology, and I originally conceived of the project as one that would examine the politics and ideologies of language and ethnicity among Guatemalan Mayas. I chose to set this work in the context of a Roman Catholic parish because I saw a potential overlap in Maya cultural rights activists' goals for language revitalization and the Catholic Church's commitment to using vernacular languages in the liturgy. This was thus not initially a project about religion per se, but suffice it to say, over time my interests shifted, and I became more focused on various aspects of my interlocutors' religious lives.

In particular, I became interested in understanding a conflict that had emerged between relatively more traditionalist, "Mainstream" Catholics and new "converts" to the Catholic Charismatic Renewal. That conflict involved the complex ties that people made between their use of indigenous language and sense of ethnic identity, but of course it also encompassed other ideas about what it meant to be authentically Q'eqchi' and authentically Catholic. Each of these short stories about how I came to reconceptualize my data highlights some mistake or error I made along the way. I do so not to be self-deprecating or out of a sense of modesty, but rather to illustrate that ethnographic research is processual and in many ways as dependent on the researcher's ability to respond to accidents and happenstance as it is on planning and preparation.

## Learning How to Ask

At the time I was enrolled in it, my graduate program did not offer a course on ethnographic methods. This was a program that prided itself on covering the traditional "four fields" of anthropology (i.e., sociocultural, linguistic, physical and archaeological) as well as a specialization in psychological anthropology. The feeling was that since student projects across those fields would be quite different from each other, rather than trying to fit research methods into a single course or even series of courses, students would be better served by working more closely with their advisors to develop their methods as they prepared their research proposals. I thus entered my PhD candidacy with a much stronger sense of the theoretical frames that would guide my research questions than of the methods that would help me answer them. I knew what I wanted to find out about how language was mobilized to shape Q'eqchi'-Mayas' ethnic and religious identities, but only a vague sense of how I would spend my time in Guatemala trying to figure that out. Ethnographic research, I had come to think, was something akin to "deep hanging out", and I imagined that once I was living in Cobán I would naturally intuit what I needed to pay attention to.

Fortunately, the granting agencies that I was relying on to fund my fieldwork did not labor under such illusions. Grant applications demanded that I make a prospective accounting of what I planned to actually do once I was in Guatemala. I remember meeting with my advisor—Kathryn Woolard—to go over the first of many, many (many) project proposal drafts, and having her look at the methods section and saying something to the effect of, "All anyone wants to do anymore are interviews. It's as if that's the only thing anyone thinks ethnography is". They're not? "Interviews are part of it", she continued, "but you need to be more focused on observing. Also, remember what Charles [Briggs] said about interviews".

In *Learning How to Ask* (1986), Briggs draws on his frustrating experiences as a novice ethnographer conducting research with Mexicano wood carvers in New Mexico to argue for a critical re-examination of the interview as a research method. As he tells it, early on in his research when we would ask his interlocutors informational questions, they would most often reply with non-committal answers, perfunctory responses or pleas of ignorance on a subject. Questions that a researcher might think of as wholly neutral, such as asking the meaning of an image, when a tradition started, or elaboration on a carving technique, were answered with a mere sentence, a "who knows", or simply a shrug. It was not that people didn't know the answers to these questions or didn't have things to say on the subject, though. As he spent more time with people and began carving alongside them, he found that his interlocutors would go into great detail about those subjects and others. The problem had never been the answers or who could know them, but rather who could initiate conversation and under what circumstances. Simply put, the ethnographer had not (yet) earned the right to ask questions, and each time he tried to force the issue by initiating what he perceived to be the neutral and innocuous genre of talk (i.e., the interview), he was in fact flouting the community's norms of how people should interact with each other.

Brigg's key point is that all cultures have certain ideas that regulate how people can and should interact with each other, how knowledge is formulated, and what can and can't be conveyed through language. The culture of (largely) Anglo-American ethnographers is no exception to this. If a person wishes to do ethnographic research that is both respectful of a community's communicative norms and that has at least a chance of understanding how they view the world, they must do two things: 1) learn how to be communicatively competent in the community they are studying; and 2) critically examine their own presuppositions about communication. The first of these tasks can arguably only be done through careful and sustained fieldwork, and the second can and should begin well before one goes into the field, but both help us develop a better understanding of cultural difference.

Armed with this insight, I went to do my fieldwork already aware that interviews could be problematic and expecting that they would only be one tool among many, but still planning to use them. Unsurprisingly, when I tried to conduct semi-structured interviews I ran into problems, albeit different ones than the ones that Briggs had experienced. First, I found that many people resisted committing to a specific time and place to be interviewed, and even when we did schedule one, they sometimes didn't show up until much later than we had agreed. This was an early lesson about how our expectations about time can vary quite significantly across cultures. I also found that some people who agreed to be interviewed

seemed to clam up and not want to say much even when at other times they had been quite loquacious. Perhaps, more vexingly, though, others would launch into very formal and formulaic monologues during interviews. What I had misjudged in framing my interactions with my interlocutors as *entrevistas* (interviews) was the referent that that genre of speech had locally. To them the word "entrevista" connoted a news radio interview, a genre in which a reporter asked questions meant to elicit brief statements of fact or opinion, which were then circulated to a dispersed media audience. This was clear when their answer began by addressing "*los amigos en Estados Unidos*" ("our friends in the United States") and then launched into what seemed like a series of pat answers ready to be clipped and used as soundbites. Their understandings of and goals for being interviewed were distinctly at cross-purposes with what I meant by the term and wanted out of the interaction. I was looking to open up personal reflections which I would later reinterpret for an academic audience; they wanted to put forth a set of statements that would effectively represent the imagined voice of a singular community to a mass media audience.

On the other hand, there were plenty of opportunities to have the sorts of conversations I wanted to have that yielded much better insight into my informants' lives and perspectives on the world. Going grocery shopping with a woman named Qana' Esperanza one day, for example, led to a long discussion about what constituted healthy food and where one could procure it, that in turn led her to teach me much about how Q'eqchi'-Mayas understood their identity in relation to Guatemala's multicultural society. On another occasion, the discovery and summary execution of a small snake we found while digging a footpath behind the parish center prompted one of the parish's senior lay leaders to ask if I knew the story about the mountain spirit that his ancestors said had inhabited that hill long before a church had been built there. Scheduling interviews and running through a set of questions was sometimes productive, but more often than not it was when my interlocutors and I were engaged in some other joint activity—even if that activity was just "resting" at someone's home over a cup of coffee and a piece of *pan dulce* (sweet bread)—that conversation would flow, and that they would reflect on their community, church and the nature of the world around us. I found that I could still ask follow-up questions and steer the conversation as I would have in an interview, but, freed from the strictures of that genre, my interlocutors were now also free to ask me questions, introduce new subjects and otherwise direct our talk to highlight things that they thought were important. It was when I learned to behave in ways similar to my interlocutors that we had productive exchanges, even if it also meant that I didn't end up with a large corpus of audio recordings of those interactions.

To be clear, the lesson to draw from this is not that ethnographers shouldn't do interviews. There can be distinct advantages to invoking this genre, especially if one's research is with members of a culture who consider the interview as an optimal means for conveying information. The interview frame also potentially allows one's interlocutors to have a greater sense of what is and isn't "on record", and thus to make determinations about what they wish to share with a wider public or not. In that regard, the interview can help build trust and confidence between the researcher and their collaborators. My point here is that good ethnographers need to critically examine their own cultural biases, including those that inform how we think that knowledge is formulated and transmitted. Ethnographic data is always produced through interpersonal interactions, and so methodological rigor demands that we be aware of how we construe and manage those interactions.

## Learning How to See

My partner Nicole Peterson, who had finished her field research just as I was due to start mine, advised that I should take a handheld video camera to the field. She had found that having a camcorder device was a good way to get invited to important community events, since people liked having their major life events on video and an ethnographer could start building relationships with people in the community by being a part-time, pro-bono videographer. I thought this was good advice, so I purchased a basic video camera to take to Guatemala.

I arrived in Cobán with a camcorder, but no real practice or skill in using it. Invitations to events as a videographer were not abundant, but I did eventually start video recording Masses and other church events. I immediately ran into logistical and technical problems. The lighting conditions in the church and village chapels where these events occurred were typically poor and it could be difficult to find a place to set up the camera to frame what I took to be the major actions I wanted to capture. Moreover, I had underestimated the local cost and availability of the MiniDV cassettes that my camera used and had to record on the lowest video settings to make the most of these media. Nonetheless, I started to put together a small, but not particularly well-produced corpus of videos of religious life. I reasoned that the poor video quality would not be too important, because as a linguistic anthropologist what ultimately mattered was the verbal data that I was collecting by simultaneously recording the events on high quality digital audio. Images from the videos would be useful for conference presentations, but they didn't in and of themselves constitute data.

Back in California after the end of my fieldwork, I began to revisit those videos and discovered an important piece of Q'eqchi'-Mayas' religious practices that

I had not initially been focused on—bodily movement, and more specifically, gesture. Watching the videos, I became acutely aware of how very different the bodily movements of Mainstream and Charismatic Catholic preachers were when they spoke in church. It was not the case, of course, that I had been entirely unaware of these differences in the field; as a participant observer I had experienced the distinctive "feel'" of each group's services and had learned to behave in each of them according to the expectations and tacit rules that governed them. However, seeing the events play out on a computer screen rather than all around me created a context for me to see those differences more clearly. Removed from the immediate context of a religious service, I could train my eyes on bodily movement; and with a simple click of the mouse, I could even make the sounds go away so that what was left was just a visual record of what I had observed. With the help of another mentor—John Haviland—I found anthropological literature on gesture and bodily movement that helped me to unpack that basic insight, and to build a model in which I could see verbal and bodily differences as part of the same process of religious differentiations I was documenting for these groups.

Noticing differences in gesture and bodily movement also led me to read some of my fieldnotes in a different light. Thinking about the body's role in language and communication made the ways that people talked about intra-congregational differences stand out. It dawned on me that Mainstream Catholics' complaints about Charismatic Catholics' noisiness were often accompanied by descriptions of them jumping around, too, or with pantomimes of the latter's characteristic "hands up, palms out" prayer posture. Likewise, Charismatics' critiques of their Mainstream Catholic counterparts discursively linked the latter's bodily stillness to their lack of faith. Taken together, these insights led me to understand that not only was bodily comportment potentially something worth further exploring, but that it was in fact discursively related to the major social differences that I had been examining between congregations.

Walter Benjamin once argued that the invention of photography revealed humanity's "optical unconscious" (2015: 68). He contended that just as the invention of psychoanalysis had uncovered heretofore hidden dimensions of people's psychological lives that could be scrutinized, so too the invention of photography had provided us a means to more closely and carefully examine aspects of our visual lives that had until then passed unnoticed. With a camera we can zoom in to bring miniscule or distant things into view, and we can slow down time to see how movement unfolds. A camera, in short, can help us render (some of) our visual field into data. The technology does not however do that on its own; rather, what a camera can do for us is produce a record of (some of) the things we have observed, and which we can further examine.

By the time I discovered the possibilities for data in my videos, I did not have the time, money, or frankly the desire, to go back to Cobán for another extended period of fieldwork to get better video samples. I had already spent nearly two years in Guatemala and wanted to move on, so I decided to work with what I had. Doing so required adjusting expectations of what I could reasonably observe in the videos, making difficult decisions about what parts of which tapes were of good enough video and audio quality to merit inclusion in the study, what of their content could reasonably be submitted to quantitative methods of analysis, and what was suitable for qualitative interpretation. In short, I had to figure out how to interpret the corpus of videos as a dataset that would hold up to the kind of analyses I wanted to do. I could only do that because I had produced the videos as part of an extended period of ethnographic fieldwork that had given me intimate knowledge of the larger cultural and religious context that simply wasn't present in the images themselves. So even if the conditions under which I had produced the videos made them less than ideal research tools, I was able to develop a methodology for analyzing them and reach adequate interpretations about how bodily action factored into the emerging differences between Mainstream and Charismatic Catholics because the videos were part of my larger ethnographic project.

The lesson here isn't that ethnographers need to make videos (much less so naively), but rather that the records we keep of our fieldwork all have the potential to become useful sources of information well after we have left the field. Until I started writing up my research, I had no idea that bodily movement and gesture would be a crucial part of my work, and while my videos didn't start off as data, I was able to turn parts of them into data once I had an interpretive framework to work with.

## Learning How to Feel

The most distinctive feature of Guatemala's Holy Week celebration is a series of processions in which lay Catholic associations carry massive *andas* or platforms bearing images of Jesus through the city to recapitulate the Passion narrative. Doing research with Catholics in Guatemala meant that the extended season of holidays that began with Advent in late November, and ran through Christmas, New Year, the parish's patron saint's feast, Lent, and into Holy Week in late March was an exceptionally busy time as I struggled to be part of, and to plan for, the various rituals associated with them in addition to keeping up with the usual schedule of weekly Masses and semiweekly lay-led religious services. I was looking forward to having some time to relax and not having to take notes or try to follow up on conversations after "my" parish performed its procession on Maundy Thursday. A few days before that procession, a letter arrived inviting members of the sodality to participate in

another sodality's Good Friday procession. My interlocutors suggested that I should join them for that event, and when I accepted, it was not because I thought I would be doing research but rather out of a mixture of wanting to spend more time with people who were becoming my friends and to satiate the curiosity I had had as a child about what it must be like to participate in these spectacular rituals. So, I joined them on Good Friday with the mentality that this was my own time and not research as such. After we helped carry the bier, we chatted a bit and everyone left to go home to relax. The next day I was physically sore, but generally happy that I had had that experience of participating in the procession. As with my experiments in video documentation, it wasn't until much later, in this case several years later, that I revisited my memories of the procession as potential data.

At a conference panel on intersubjectivity I saw two anthropologists lift a table together to illustrate how people make miniscule sensorimotor adjustments to their movements in relation to each other. Jointly accomplishing tasks like that is a routine and banal part of everyday life, but it also illustrates something of the irreducible complexity of how people relate to each other on a bodily level. Seeing that demonstration brought back the memory of struggling to lift the bier during Holy Week many years earlier. How had we been able to jointly accomplish the task of carrying the heavy wooden structure without dropping it? How had we each managed to take our place under it without explicit direction? What meanings had those physical efforts had in the context of the procession? Pondering these questions in light of my memories of that day set me on a path to look for other scholarship that could help me make sense of those experiences and led me to consider more thoroughly the role of bodily movement in shaping my interlocutors' sense of religious identity. It allowed me to build on the insights I had gained in looking at individuals' gestures to see how the coordination of bodily movements lay the groundwork for religious experience. Doing so required revisiting field notes, videos and photographs I had taken of others carrying the processional biers, but I could also draw on my own memories to craft my interpretations.

There are two lessons to take from this. The first is that being open to new experiences during fieldwork is part of what makes the methodology of ethnography particularly useful. There are a number of ways that one could study those Holy Week processions, but it was only because I had been there for a long time and gained a place in the social world of Catholics in Cobán that I was invited to be part of them and have that first-hand, bodily experience. Second, beyond being sources of data, the various records we keep during our fieldwork (notes, journals, audio recordings, photographs, videos, newspaper clippings, etc.) are mnemonic devices that help us re-experience (to be sure, in limited ways) sharing another person's lifeworld. The combination of those records and our memories can help

us re-evaluate our participant observations and see things about them that we might initially have missed.

## Conclusion

My experiences learning how to do ethnographic research were idiosyncratic and shaped by my particular circumstances and interests, and I would not by any means recommend that anyone seek to emulate exactly what I did. In any case, the data produced intersubjectively by ethnographers is essentially irreproducible, which is part of what makes ethnographic texts appealing, and what leads some people to mistakenly think that ethnography happens by chance. Nonetheless, I hope that my reflections here will prompt both novice and experienced ethnographers to think critically about the data that make up their research. Doing so might mean taking a critical stance towards one's planned research methods, finding new ways to reinterpret one's notes or other records as usable sources of data, or thinking about how to incorporate other kinds of insights as ethnographic data.

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