

How to Analyze Your Data without Lying about God

49 Sermons and a Reflection on the Systematic Analysis of Ethnographic Data

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Despite often heroic commitments to immersion in a given field context, and despite frequent theoretical fluency in analysis, anthropologists rarely show the steps by which they analyze and interpret their data. Because of this, the validity of anthropological analyses is generally difficult to assess, the reinterpretation of existing studies is often a dubious proposition, and the ethical implications of research sampling strategies are often impossible to know. This essay makes use of a corpus of sermon data collected over a summer at a Catholic monastery to illustrate the limitations of sociocultural, ethnographic anthropology as currently practiced and offers several strategies for the interpretation, analysis, and presentation of anthropological data meant to fix these limitations.

Online enhancement: appendix.

What Does God (or Your Advisor) Want You to Do, Anyway?

Saint or sinner, damned or saved, when one lives in a monastery, one spends a lot of time in church. The Rule of Benedict, a 1,500-year-old exposition of monastic living, which guides many Catholic monastic orders to this day, proscribes eight routine “hours,” or occasions, throughout the day, occurring roughly every three hours, when brothers should stop what they are doing and join for group prayer (the full schedule is also called the *Opus Dei*). This sort of prayer tends to be fairly repetitive, consisting as it does of poetic biblical readings, hymns, and passages from the New or Old Testament of the Christian Bible. Imagine standing, sitting, calling, responding, singing, chanting—all in a regular round, to get a sense of how the hours tend to go (Irvine 2010:3, “Marking the Hours”). All told, there is not much room for spontaneity, commentary, or innovation. Given that stable default, when someone says something unscripted, it is interesting.

In the summer of 2011, I lived in a cell at a Catholic hermit monastery (the hermitage), situated on a mountain in the western United States, researching how these monks knew what God wanted them to do. This was the summer after my first year of graduate school in anthropology and my first time attempting ethnographic fieldwork. Truth be told, I did not really know what I was doing or how I was supposed to document, analyze, and interpret a group of monks’ relationship to their God. My methods training had been both inadequate and, as I have learned, fairly typical of sociocultural graduate training in anthropology in Europe and the US. What I got before going to the field were (1) some different ways to take

notes, (2) a rundown of the most recent edition of Russ Bernard’s (2005) ubiquitous methods textbook, and (3) an immersion in typical anthropological mystification—a sequence of exhortations to seek the human, embrace the intersubjective and unexpected, and *just* connect. This last bit—the propaganda—was meant to reassure me and my colleagues that we would figure out field work when we got there.¹

So, precocious and terrified of failure, I set about getting in the rhythm of everyday life and taking lots of aimless notes as I did so. I lived in a hermit cell and joined the work schedule by cleaning (lots of guesthouse toilets, actually) and baking (fruitcakes and granola, mostly). I also attended nearly all prayer

1. When I was in graduate school, beyond the hazy admonitions from most professors I encountered, this sort of account of the mystique of fieldwork was manifest in works like Cerwonka and Malkki’s (2007) account of the improvisational nature of fieldwork and ethnographic understanding. A more updated version of this treatment of anthropological work would be Pandian’s (2019) *A Possible Anthropology: Methods for Uneasy Times* in which anthropological work is described as “a practice of critical observation and imagination, an endeavor to trace the outlines of a possible world within the seams of this one” (4). Elsewhere, we hear about “the transformative force of encounter” (11), about rooting “in attentive engagements” (14), about “philosophizing out of doors” (16), about “an openness to mythic reality” (36), about “a method of experience” (46), that “methods cannot be taught, only undergone” (72), that “episodes of misery, loneliness, tedium, and radical doubt are almost inevitable” (72), about “a fundamental mode of imagination, its approach to humanity as an open horizon of displacement” (80), about “the method [of] . . . seeking out mirrors in which to see ourselves” (85), about “[the promise of] the joy of unexpected communion” (110), and that “[anthropologists are] metaphysicians of description” (119).

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hours. In addition to keeping regular field notes based on my participant observation (*my* daily round), I also conducted life and religious history interviews with most monks (all but one) and all lay residents of the monastery, reviewed the hermitage's archive of monastery-specific religious texts (charters, constitutions, and hagiographies), and surveyed their records of all the people who had come and gone at the monastery. In short, I did "ethnography." In most sociocultural anthropology writing, too, the last few sentences would be adequate (and, in some cases, would be treated as overkill) to establish my authority as a credible ethnographic witness and persuade most anthropological readers to trust the generalizations that I will make about monks and how they know what God wants them to do.

What tends to follow fieldwork explained in this sort of rote enumeration of time-in-place and terse allusions to methods-employed (as well as obligatory excursions into the positionality of the researcher vis-à-vis the people they are living with [which I will rehearse below]) is a sequence of exemplary vignettes or interview excerpts that anthropologists weave together with theoretical and analytic commentary that amounts to an academic account and argument. Generally, anthropologists tacitly assert, via their ethnographic authority, that these examples and vignettes are both appropriate and representative of the vast corpus of data that their time-in-field assures the reader that they have. Generally speaking, too, this approach to writing ethnographies seems to work pretty well for the discipline: anthropologists produce a steady stream of theoretically innovative, occasionally moving accounts of people here, there, and everywhere (or as Laura Nader might have it, of people up, down, and sideways).

That said, all is not well with this approach to presenting evidence and arguments in anthropology. In what follows, I will suggest that the practice of tacitly asserting the importance of vignettes or quotes, in the absence of a broader, more systematic treatment and explanation of the anthropologist's data, leads to several problems with anthropological scholarship and larger disciplinary practice. First, in relation to our scholarship, without an explanation of our data analysis, and the distribution of the evidence from which our examples come, the authority of our work rests *solely* on authorial assertion and scholastic analogy. Should a reader distrust the persona the anthropologist writes and the experience the anthropologist asserts, then there is precious little to fall back on to defend an article or book. Second, insofar as we have an ethical imperative to explain how we generate our work, to our research participants, to our colleagues who may wish to reinterpret our work, or to the larger public who may need more persuading than claims to authority, lacking a treatment of where examples come from again leaves our writing with precious little to defend itself. Finally, this manner of writing serves to mystify the anthropological project and poorly prepares graduate students to do fieldwork. Nerves and anxiety are probably normal at the start of an apprenticeship; enthusiastic and charismatic neglect of trainees is not. It is not too much to ask to know what exactly a trainee is supposed to do to learn their craft and vocation.

To illustrate the pitfalls of this assertive form of writing and arguing, I will spend a bit more time talking about the monks I spent a summer with. Specifically, I will talk about their sermons, given daily during Mass, as one way to start understanding how they saw their relationship with God.

Over the last decade or so, I have spent much of my research time studying how the financiers who manage financial capitalism in the United States exercise power (e.g., Souleles 2019a, 2019b, 2020). This research has generally been among people who are difficult to access and on processes that span geographically dispersed locations with little opportunity for sustained participant observation work (Souleles 2018a). As such, I have had to develop unusually explicit presentations of data and thematic analyses to justify the interpretations I make (e.g., 2019a:517–520). Key to this analytic approach has been an explanation of the nature of the ethnographic data I am sampling, an explanation of variation within that data, and then thematic analyses that rely on that sort of presentation. Often, given the scattered nature of my field sites and research participants, I am able to make persuasive arguments only by being this explicit. For example, I cannot lean on the old trope of time spent in a research site or on a project to justify my authority. What I will suggest here is that the sort of approach to ethnographic data that I am advocating allows anthropologists to make better arguments, ones that do not rely overly on authority and assertion. A bit more immediately, too, this approach will allow me to make sense of ethnographic data I collected some time ago at the hermitage but could not quite handle then.

At the hermitage, the sermons, regularly occurring as they were, were written individually by monk-priests (six of the 14 brothers were priests²), who led Mass and whose sermons function as a sort of opaque running commentary on monastic life, in light of the day's scriptural readings. I will walk through a typical way to analyze this corpus of data, one that asserts a representative example, and then show a few more systematic approaches that make for a sharper, more defensible argument. The beauty of the approach, I will suggest, too, is that most anthropologists likely have the data to do it and probably already do some version of this behind the scenes of their own writing. Embracing the approach I will suggest to analyzing data should also go some distance toward demystifying the process of fieldwork. But before we get to that analysis, we ought to hear a bit more about the monks.

A Vignette for Clarity: Peter and Paul; Shepherds and Founders

Unlike most Catholic monks who live in close quarters and in dormitory-style rooms (e.g., Asad 1993; Irvine 2010; Lester 2005), the brothers I lived with were hermits, living in stand-alone cells, clustered around the hermitage's church, refectory, and library. They felt that their purpose in monastic life, their

2. Additionally, one who was not a priest occasionally filled in and gave the homily.

monastic “calling,” was to be alone with God and devote themselves to prayer, simultaneously serving as a living witness to devoted, surrendering piety and using prayer and missionary work to make positive change in the world. In this context, prayer is instrumentally effective in changing aspects of our shared world. This order of monks had modified the Benedictine constitution of monastic life as well as Benedictine devotional hours to suit their desire to spend time with God and away from the other people they lived with. These modifications in fact characterized their specific order and were represented in their founding documents. Practically, this meant some thoroughgoing division of their time. First, they divided the light from the dark, suggesting that the monastery should be silent at night from 6:00 p.m. to 5:30 a.m. (the start of their first prayer hours). Then they divided their day in three; first they spent a third of their time doing conventional monastic work: everything from maintaining their guest house, to baking things to sell, to bulldozing roads on their property; second, they spent a third of their time alone praying and meditating; and, third, they spent the remaining balance of their time in communal group activities—sharing lunch as well as four occasions for group prayer in church.

This sort of divided time, in turn, is an illustration of the basic value commitment and tensions that animate the monastic life in which I was living. Very basically, the hermits I got to know were committed to a solitary life of prayer but also felt that they could best lead a solitary life of prayer in a community of other hermits. So they had to spend a good amount of time, social time, time with other people, keeping their community up and running—hence their split schedule. This split commitment was further complicated by a commitment to martyrdom (dying for one’s faith), which in the present-day hermitage was interpreted as some sort of a call to evangelize and reach out to people beyond their cloister walls. I suggest that understanding and living with the seemingly incommensurable facts that in the hermits’ spiritual universe, solitude comes only from community and that martyrdom might lead one to breach or leave both community and solitude is key to understanding a lot of what gets said and done at the monastery.

As a brief aside, it is worth keeping in mind that due to the monks’ constitution as a hermit monastery and commitment to solitary living with God, their basic understanding of Catholicism will likely look different and contain different emphases as compared to more common and social orders of monastics (e.g., Irvine 2010; Lester 2005) or even as compared to everyday lay Catholics (e.g., Orsi 2010). For those familiar with Catholicism, some unusual theology and terminology, or understanding of ritual, or emphasis in doctrinal beliefs, all attending the tension between solitude, community, and martyrdom, should be taken as a reflection of what Catholicism looks like for this particular order of monks.

So in a typical ethnographic account, this would be the point at which I would introduce some sort of indicative evidence—a vignette or series of quotes that would illustrate the tension

between solitude, communal life, and metaphorical martyrdom that I am talking about, which could then lead to a reflection on how monks knew how God wanted them to navigate these tensions. The evidence I would present would both show that the tension exists and give some indication of the ways that monks live that tension. Further, the evidence would serve as a perfect encapsulation of the themes I am writing about and would tacitly stand for a larger body of data.

I realize that this is quite an assertion. I am saying that this mode of illustration and analysis is the predominant way that sociocultural, ethnographic anthropologists write. While I have a personal sense of this from a number of years of reading widely and teaching methods in anthropology, a bit more precision (and a bit less assertion) might better persuade the reader. In that vein, in preparing this article, I reviewed recent ethnographies from three different academic publishers: the 2020 general anthropology list from Duke University Press (11 books), the last six ethnographies in the University of Nebraska’s Anthropology of Contemporary North America Series, and the last six ethnographies in the LSE Monographs on Social Anthropology series, currently published by Routledge. The idea of this sample was to capture a fairly prestigious general list (Duke), a topically specialized list (Nebraska), and a non-US anthropological tradition (Routledge). My hope is that this sample would cover a broad enough cross section of recent anthropology to be representative. It helps that each of these lists publishes on a variety of topics and theoretical orientations.

The point of my review was to (1) catalog the methods enumerated in each book across a wide variety of ethnographic writing and (2) to check whether there was any systematic treatment of data, anything beyond the presentation of appropriate illustrative data (the silver bullet vignette or quote) to make an argument. Specifically, by “systematic treatment of data,” I mean (1) an explanation and/or enumeration of the sampled data from which interpretive analysis is being generated and (2) an explanation of the distribution and relative frequency of patterns noted in data that can then justify interpretive claims. The practical way I set about conducting this review was to read through each ethnography and take detailed notes when each author wrote about their methods, their data, and their analysis and then to see how their methods, data, and analysis supported the larger arguments they made in their book. In addition to distinguishing whether a given ethnography contains systematic presentation and analysis of data, in some cases where an author has not done this sort of analysis, I have both identified and noted cases in which elements of their methods and analysis, though partially presented, are strong and could go some way toward developing a systematic analysis of data should they so choose, as well as cases in which the author’s project would make systematic analysis of data inappropriate.³ The specifics of my review work are cataloged in the appendix (available online).

3. It is worth noting, too, that authors are not exclusively responsible for the absence or presence of data in a given book. Publishers also

This review confirmed my supposition: anthropologists, by and large, do not write about the analysis of their data or present how they come to their assertions. I found only two systematic presentations and comparisons of ethnographic data: Zwissler's (2018:51ff.) comparative account of three different spring rituals in her account of feminist, religious activists in Toronto, Canada, and Yang's (2020:C3, C4, C5, C8) comparative illustration of devotional and religious practice in Wenzhou, China. Occasionally, when anthropologists stray from observational or interview data, as in the case of field experiments (Regnier 2021: 132ff.) or enumerations of physical objects (Komarnisky 2018: 140–141) or survey work (Schwenkel 2020:16–17, 194), they will systematically present portions of their data and explain how those data lead to an interpretation or analysis. There is also a lot of variation in how specific people are in explaining their methods and sampling, with some relying on the briefest claim to time spent in a place (e.g., Hull 2019:41, 54) and others giving a thoroughgoing logic to site selection and an explanation of what was sought in participant observation (e.g., Blanchette 2020:10–11; Diaz 2020:15, 21–22, 170ff.; Westermeyer 2019:13–14). Generally, though, anthropologists give a summary of their fieldwork and then argue by asserted example. As I have suggested, sermons at the hermit's Mass are a good source of data to illustrate this sort of presentation and its limitations.

Group prayer at this monastery was a slightly idiosyncratic variation on the typical Benedictine hours I spoke about above. The monks observed just three of the Benedictine prayer hours as well as one group Mass, making for four occasions throughout the day in which the bells rang and folks came to church. Mass, in turn, is the central Catholic church service and is celebrated, at the hermitage, in the middle of the day. Mass, though a globally stable Catholic ritual, oddly enough, too, was the place in the monk's day in which the most public moment of unscripted commentary on group life could occur.

Mass is distinct from Benedictine hours for a number of reasons. First is the "celebration" of the Eucharist. This happens in the second half of the half-hour to hour-long Mass service and is essentially a stylized, compact reenactment of the biblical story of Jesus Christ's last supper before his martyrdom, crucifixion, and resurrection. At a peak moment, among prayers and chants and raised hands and lowered brows, bread (the eucharistic wafer) and wine become Jesus's flesh and blood, a sacrifice for all Catholics in good graces with the church to commune, take the sacrament, and eat some of Jesus's body and thereby become an instantiation of a larger "body of Christ" and the Church here on earth (see also in Silverstein 2004:626–627). By contrast, the other occasions for group religious observance, the three Benedictine prayer hours, do not celebrate the Eucharist.

The other main difference, at least from a ritual point of view, between Mass and the Benedictine prayer hours, is that at the close of the first half of the Mass, after some scripture reading, a

enforce norms around how much space should be devoted to data and analysis.

priest gives a homily, directly addressing everyone attending the Mass. Given how monastic life is suffused in silence and routine, reliant on prayerful and close readings of sacred texts, and attended by saintly inspiration, this struck me as an ethnographically interesting moment—one monk formally and directly addressing the congregation in the context of the most ritually special time of their day, more or less at their own discretion. And, as we might expect, the inherent tensions in this hermitage's religious calling came up in sermons. One missive from Brother Wendell, captured in a field note, will show this:

6/29/2011 (12 Monks and 22 Lay People Present)

Brother Wendell gave the Homily for Father Tiresias who was Presiding

Wendell said that "Peter and Paul" is an ancient feast, first written about in 349, which Wendell said clearly shows that it was celebrated earlier. For a time, they were separate [feasts and] then brought together. He talked about Peter as a shepherd and the founder of the church and Paul as an audacious missionary. He noted they were both empowered by the spirit. He talked about how Peter was given the keys to the kingdom in order to found the church. He said it was like borrowing someone's car or using someone else's house—you're using it, sure, but at the end of the day it's someone else's and you'd better not get a scratch on it.

We're tending the church for God, Wendell said; and we have to ask how the institution will lead people to God. [He then] transitioned to the common life of Benedictine monasteries and how one stays the course [therein].

In this sermon, I would suggest that Wendell was doing two things. First, he was pointing out that it takes different abilities to succeed as the sort of Christian these hermits aspire to be. On the one hand, one needs to be a bit of an institutionalist and build a church like Peter. On the other hand, something wilder, something untamed, some manner of the missionary Paul's audacity is also necessary. Together these impulses make a church and provide a scripturally rooted example of overcoming seemingly incommensurable religious imperatives and synthesizing them into a vital whole. God wants both.

The other thing that Wendell was doing is pointing out that there is a long lineage of people dealing with seemingly incommensurable spiritual commitments. Peter and Paul founded the church 2,000 years ago, and the monks Wendell is talking to are members and stewards of that same church. This historical continuity transfers what might seem a novel crisis of devotion into a long-standing one. Moreover, Wendell takes off some of the edge from dealing with religious contradiction in suggesting that the trials of daily life might be subsumed under the virtue of the steady stewardship of an institution, community, and group that has both preceded present monks and will, presumably, outlive them. God takes the long view of religious life; we all might do well to reflect on that.

In the normal course of ethnographic writing, once I had laid down some context and a specific example, I could then treat that as a trustworthy baseline for extending my analysis

of following God through conflicting values at the monastery and speak to larger interpretive frames and topical agendas ambient in the social sciences: say, the role of discipline in institutional, monastic life (Asad 1993; Foucault 1995), the rise of charismatic Christianity in America since the 1960s (Luhrmann 2012), or the construction of gender in the context of a religious calling (Lester 2005). Once I have offered background and a bulletproof vignette, once I have proven something exists due to my expertise and my selective presentation of data, I can then bring my particular research into larger academic conversations and historic contextualization. For my part, I would likely want to write more about the way the monks understand their place in an immanent, religiously alive past and use this to inform what they expect for their future (as in Abercrombie 1998; D. Souleles, unpublished manuscript), thereby saying something about cosmology, history, and memory in the mediation of a relationship with God and saintly ancestors.

But there is something mystifying about this whole process: first, even though I reported in capsule format the way I conducted fieldwork in an attempt to bolster my ethnographic authority (interviews, participant observation, archival and textual material), and even though I have offered a really good little bit of empirical data, I have still cherry-picked a sermon. I have intentionally though tacitly led the reader to think that this sermon can somehow speak for the rest of the sermons that I saw over the summer that I was at the monastery. Also I have done this without presenting any sort of systematic treatment or analysis of the corpus of sermons I have collected. After all, my authority as a field-worker tacitly argues for my ability to make that judgment.

At this point, too, it is worth noting how the sort of critique I am making of ethnographic practice differs a bit from those one commonly encounters in anthropology and among those who do ethnography. Sociocultural, ethnographic anthropology looks and reads the way it does because of a series of critiques of how anthropology understood its subjects of study in colonial and imperial contexts. Starting around the early 1970s, anthropologists and others made the simple observation that most of the people anthropology had studied to that point had not in fact been pristine examples of simple societies and cultural totalities (Asad 1973; Fabian 1983). Rather, anthropology created the literary illusion of timeless, often-stereotyped others via their ethnographic writing (Clifford and Marcus 1986; Said 1978; Trouillot 2003).

In response to this critical assessment of sociocultural ethnographic practice, sociocultural anthropologists suggested a number of fixes to both how anthropologists work and how anthropologists think about the people that they study. These fixes ranged from a castigation of the post-World War II generation of anthropology who professionalized and bureaucratized the discipline, coupled with a hope for the revitalization of earlier, more charismatic disciplinary antecedents (Hoebel, Currier, and Kaiser 1982), to suggestions that anthropology drop things like the culture concept and seek to navigate a world of juxtaposition and flux (Fox 1991), to suggestions

that anthropologists seek relevance by studying, among other things, power in contemporary societies (Hymes 1972). Taken together, these critiques and fixes pushed anthropology to consider the context and history of the people whom anthropologists describe in their analyses and see how any group of people is connected to larger world system (e.g., Mintz 1985; Wolf 1982). Moreover, these critiques opened space in the discipline for historically neglected and ignored topics like gender (Rosaldo and Lamphere 1974) and race (Baker 2010).

I accept this inheritance and maintain that the creation of homogenous, ahistorical, cultural others is both bad science and harmful politics. I also accept that there is an irreducible artifice in ethnographic writing, and really any nonfiction writing, that anthropologists actively temper via their professional standards and ethical commitments. What I am suggesting here, though, does not really have much to do with these larger issues of topical delimitation, on the one hand, that we might understand as setting the stage for active anthropological work and analytic priorities, on the other hand, that we might understand as shaping the sort of interpretive treatment that anthropologists give to whatever data they have gathered. Rather, I am trying to speak to a space in between, in which, regardless of topic and theoretical agenda, anthropologists need to make sense of their empirical data and assert some sort of general coherence to what they have seen in their fieldwork. That is to say, I am speaking to the relatively stable foundation of anthropological work: that across these topical and conceptual shifts in anthropology, anthropologists still do ethnographic fieldwork and use the resulting field notes, interview transcripts, and archival ephemera to make their arguments about patterns in social life that *then* make the basis of whatever larger argument the anthropologists pursue. Ethnographic work comes after topical selection and before analysis. It is the in-between empirical motor of anthropology.

More recently, largely emerging from sociology, there has been a sustained critical dialogue about ethnographic fieldwork and how it can or should change given the rise of novel data recording technologies and digital data formats; scrutiny from other epistemological traditions (such as law or journalism); the fact that many people's lives are now, at least partially, lived online; and general pushes in other sciences to create open data repositories under the banner of "open" science (all summarized in Murphy, Jerolmack, and Smith 2021). Again, what I am suggesting in this article largely skirts these conversations. My observations about the systematic treatment of ethnographic data work whether fieldwork and data collection/generation occur online or off and work under a basic affirmation of ethnographic inquiry (empirical inquiry that is context specific, long term, and relationship driven) as a valid approach to generating knowledge despite its dissimilarity to the forms of data other more quantitative disciplines gather (Tsai et al. 2016). To borrow the language of the philosophy of science, we might see ethnographic, sociocultural anthropology as an inductive mode of field-based inquiry that, though different from many other empirical traditions, generates valid

knowledge about the world. Anthropological research requires seeing phenomena (say, cultural meaning) in that phenomenon's specific context (a laboratory or a control generally will not do), and it requires meaning to emerge over the course of immersion and participation in human relationships (deductive theorizing is generally a misleading dead letter). In addition, meaning and culture data do not seem to require statistical sampling, as they are fundamentally relational data, specific and often fairly common to a given group of people—for example, it does not really matter where you start in a specific language community; you will get a general sense of what people mean by “dog” or “family” or whatever other common concept fairly quickly, as that sort of cultural knowledge works by common understanding (Handwerker and Wozniak 1997). Moreover, and a bit more narrowly, my approach to explaining thematic variation and distribution in ethnographic data bears some affinity to recent attempts to visually map coding patterns in ethnographic data stored and sorted with computer-based data management software (e.g., Abramson and Dohan 2015).

All told, what I am suggesting about sleights of hand in ethnographic analysis due to the treatment of data seems to be a bit of a blind spot in larger conversations about what sociocultural, ethnographic anthropologists argue about and the topics that sociologists and other nonanthropological ethnographers are worried about. Given that, and to illustrate the nature of my empirical misdirection, as well as to illustrate the benefit of more systematic analysis in ethnography, I will now turn to the other 48 sermons I collected and show how Wendell's does and does not fit.

The Other 48 Sermons, or Why Does the Wine Keep Flowing?

Across the 49 sermons that I had, three themes and two residual categories came up repeatedly. The way I generated these themes was to literally print out my field notes, cut out each sermon individually, and sort them according to what they seemed to talk about (Emerson, Fretz, and Shaw 2011:143; Ryan and Bernard 2003:94). Nestled in this approach were a number of assumptions that I should explain before I reveal the themes I am claiming.

First, I assumed that patterns and repetitions would occur in the thematic content of the sermons I was analyzing. This is no small assumption. That said, whether we say it or not, and however we label and theorize these patterns (culture, ontology, discourse, semiotic ideologies, porous social orders, etc.), anthropologists assume that there are learned patterns in human social life. We assume that repeated instances of the same phenomenon will be similar and different in traceable ways that allow us to describe patterns of variation and then make generalizations about how people's lives work (e.g., Dolgin, Kemnitzer, and Schneider 1977; Geertz 1973).

Second, once one accepts that there are learned patterns in social life of whatever sort, one is left with the question of adequacy, or of saturation. How do I know that I have seen

enough sermons to make generalizations about them? What scope and scale can my generalizations take? This is a vexed question in ethnographic research that we have only partially answered. Metaphorically, there is an idea that when one pays attention to a specific type and form of meaning data about social life, one will at some point exhaust novel variation and reach a point of saturation (e.g., Guest, Bunce, and Johnson 2006). “Saturation” as a metaphor imagines water dripping into something absorbent like a sponge—there is a point at which the sponge can absorb no more water and the sponge is said to be saturated. No further quantity of water will meaningfully add to that already soaking wet sponge. Quotidian examples in social life might help illustrate this way of thinking—if one takes something banal like all the ways that some group of people tend to greet each other, fairly quickly, as long as the group or its conditions do not change too much, some small amount of greetings would accumulate alongside a steady trickle of idiosyncratic one-offs. In the context of participant observation, we might imagine a researcher's steady comfort or at least competence in a given field setting or around a certain group of people as an accumulation of many instances of this sort of saturation, personally registering in some way, and then adding up to some sort of unexamined habitus or sense of competence and maybe even ease.

Cognitive anthropologists have actually gone some distance toward quantifying saturation with respect to themes in interviews (Guest, Bunce, and Johnson 2006), cognitive list items (Weller and Romney 1988; Weller et al. 2018), and metathematic analysis of interviews across multiple field sites (Hagaman and Wutich 2017). Moreover, cognitive anthropologists have used these sorts of methodological approaches to talk about everything from the local typologies of color and race in Puerto Rico (Gravlee 2005) to the way health, social class, and local ideas about attainment of the good life in parts of Brazil relate to one another (Dressler 2015).

Anthropologists generally would do well to pay more attention to this body of scholarship, as it provides a methodological metaliterature in which we might more firmly ground our claims that our data accurately represent what we say they do. For our purposes here, the 49 sermons that I gathered one summer might reasonably count as representative if we accept that, as speech events, we can analogize them to interviews in which themes occur. Guest, Bunce, and Johnson (2006) found in a corpus of interviews on sex work conducted in Ghana and Nigeria that 92% of all themes they could code emerged after 12 interviews, and increasingly few new codes emerged across their 60-interview sample. Given this, again if we accept the analogy, 49 sermons would seem to safely get us to thematic saturation. Moreover, this is what we will see: a few major themes appear repeatedly across the sermons, and a number of one-offs that are somewhat difficult to categorize round out the rest (see also Souleles 2018*b*). Given the focused, didactic nature of a sermon, we might also imagine a sermon as basically the equivalent of one interview question put to a priest and answered in public: “Given whatever is going on in the monastery,

your own life, and the larger world, what should the rest of the church community think about the readings in today's Mass?"

And finally, even after we have accepted that there are knowable, repetitive patterns that characterize human existence, and even after we have accepted that there is a methods literature that can guide us to operationalize a search for saturation with our own data and the phenomena that we record, the assumption remains that the anthropologist is competent to assess themes. This is a tricky problem that in part can be solved by methodological craftiness (e.g., member checks). But as in any genre of nonfiction writing in which we create narrative, following an author will always rely on an appeal to some manner of authority and require a leap of faith of some size on the part of the reader. Presumably, the more data researchers show, and the clearer they are about how they collected them and the standards by which they analogize and analyze them, the more straightforward it is to make a claim for whatever sort of analysis they have made. Here I have suggested that the number of sermons I gathered is adequate for analysis by some analogic reasoning to other methods study and a claim that I hit saturation. Ethnographic authority and fluency with local categories of thought are a bit trickier, though.

It is unclear how long someone needs to be in a place taking notes to learn enough background to contextualize any given ethnographic findings. Classically, anthropologists were supposed to spend a year or two in a field site. But that colonial-era thumbnail presumed a level of alterity that did not apply to my monastic work. In my case, I was on-site for only a summer. But like all the monks at the hermitage, I am a white male English-speaker from the United States. To show some of what this meant, I was so socially legible that a number of the monks thought, and seemed to hope, that I could and maybe would become one of them. This sort of identification with the field-worker is not the case in many field sites. Moreover, beyond these sociological categories are the biographical ones. My maternal grandmother had me baptized as Catholic, and in partial, though incomplete, furtherance of this destiny, I attended a couple of years of catechism and Mass as a young teenager (though I no longer attend church or consider myself a Catholic). Given this, I had a lot more background knowledge than people who study in places more removed from where they grew up have. Moreover, in the eyes of some monks, I had already been "saved" due to my baptism.

So what I would suggest is that a combination of my analogic reasoning about the nature of the data I have and the authority I have established for myself as a field-worker, and in the context of my larger biography, should persuade the reader that I have adequate sermon data and contextual expertise to say something about how these little speech events tend to go and what they mean at the monastery. Now, one does not necessarily have to accept my argument, but at least I have given the reader some standards according to which they might disagree as opposed to simply asserting my case.

One might think that I did not spend enough time at the monastery to give a contextual account of anything that goes

on there. However, if one feels more time is necessary, the onus is on the critic to explain how much time I should spend there, what I should pay attention to, and what more time would give me. They should be specific. There were no multiple-year agricultural cycles to follow, nor was there a new language to learn, so some of the more common fallbacks for a reflexive claim to a year or two do not really work here. That said, Catholics follow a year-to-year liturgical calendar and cycle through various holy days on an annual cycle, which I did see only part of. Alternatively, one might think that I spent enough time and have some level of contextual and interpretive expertise but that I did not gather enough sermons. Again, this might be fair enough. But the onus, again, is on the critic to explain why they do not think I got enough sermons given the analogic methods argument I made about thematic saturation in interviews. In any event, this is the sort of pragmatic accounting for data and interpretive adequacy that would have helped me as a trainee anthropologist and that I think would make anthropological writing more persuasive. Given the general lack of presentation of data and analysis along the lines I am suggesting in most ethnographies, this sort of scrutiny I am outlining is generally impossible.

Now that I have clarified the set of assumptions that I think are present in any presumption of competence to analyze ethnographic data and modeled some of what this thinking can look like, we can actually turn to what the monks had to say and see how it blows up my use of the silver bullet Wendell sermon above. That is to say, we can see what the three predominant themes present among the sermons were.

First, and perhaps most expected, were 16 sermons that advised the monks away from a particular course of action ("admonition sermons"), often in the context of a moral parable or analogy to the day's Bible readings, or a particular saint that was being venerated in that day. Second, and more particular to their order, were 15 sermons on how a person becomes more open to God, more Godlike, or simply more aware of God ("becoming sermons"). Whereas admonition sermons might be encountered in the average diocesan, or community, Catholic church, becoming sermons often drew on strands of Christian mysticism to contemplate the disquieting manifestations of God's presence and absence. A third category were seven sermons that were theologically erudite and academically nuanced and sought to interpret continuities across the Old Testament, the New Testament, and the present world to illustrate the continuous immanence and presence of God and the way the world continues to be shaped by Christ's intervention ("exegetical sermons"). An example of each in field note form follows. After the examples, I will run through a comparative analysis of the sermons showing how their variety gets at the different ways the monks think about what God wants them to do. What I hope to show is how much we miss by simply taking that above Wendell sermon as speaking for all. (NB: The first sermon below is in fact the Wendell sermon I presented above, repeated here to illustrate how it differs from other sermons when the larger analysis of all sermons I collected is present.)

Admonition Sermons

6/29/2011 (12 Monks and 22 Lay People Present)

Brother Wendell gave the Homily for Father Tiresias who was Presiding

Wendell said that “Peter and Paul” is an ancient feast, first written about in 349, which Wendell said clearly shows that it was celebrated earlier. For a time, they were separate [feasts and] then brought together. He talked about Peter as a shepherd and the founder of the church and Paul as an audacious missionary. He noted they were both empowered by the spirit. He talked about how Peter was given the keys to the kingdom in order to found the church. He said it was like borrowing someone’s care or using someone else’s house—you’re using it, sure, but at the end of the day it’s someone else’s and you’d better not get a scratch on it.

We’re tending the church for God, Wendell said; and we have to ask how the institution will lead people to God. [He then] transitioned to the common life of Benedictine monasteries and how one stays the course [therein].

Becoming Sermons

6/28/2011 (7 Monks Present)

Father Job Presiding

Job started talking about how St. Irenaeus knew Polycarp who spoke out against Gnostic or private revelation in the early church as opposed to the apostolic revelation. In his homily he was talking about the passage from [the gospel book of] Matthew, I think when the apostles are out on a boat with Jesus and [Jesus] is sleeping. There is [a] storm and waves and all [of] that; and the apostles are worried. They wake [Jesus] up and he calms the waves saying, “ye of little faith.”

Job suggested that Jesus is taking refuge and sleeping in all of us so that we can feel the revelation of the gospel fresh—it’s not a fait accompli. He talked a bit about John of the Cross [a Spanish Catholic mystic from the 1500s] looking out his window in the darkness, and talked about how without the dark you wouldn’t be able to see fireflies—you can’t behold in wonder without the dark.

Jesus is asleep for that very reason[;] from that divine repose comes our fresh experiences. [Job] noted at the end, that even on a battlefield a soldier needs to learn how to sleep. Theresa Avilla, the mystic, towards the end of her life was sick and went to confession for falling asleep in church. The confessor said, “what is wrong with falling asleep in your parent’s arms?”

Job went on to say that the story [Jesus calming the storm and the sea] held in all four gospels so there must be some truth in it.

Exegetical Sermons

7/17/2011

Father Tiresias Presiding

Tiresias started riffing on the line [from the Gospel of Matthew and reflected elsewhere in the Bible] that, “the righteous [will] shine like [the] sun,” and said that Jesus has that same shining in the embrace of God’s love. We orient ourselves by what will come, that sort of transfiguration of [a] loving embrace. [Tiresias] also noted that in this translation we get God as, “lenient.” He noted, though, that penance is required. [Tiresias] then noted by contrast that cyclic repetitive reality is not ultimate reality.

God is profligate and prodigal with his infinity poured out and liberates us from ceaseless futility—[the book of] Ecclesiastes [is] not the last word. Tiresias then pointed out that the first creation is that futility, and Jesus brings a second creation. At the wedding at Cana,⁴ the water is that first creation. Then the wine is Jesus; and unlike real wine it will not disappoint us; somehow Jesus’s wine is music that does not stop; somehow the whole thing goes on and on and on. Humans through all this are creative and generative somehow.

Tiresias later talked about chapter six of [the Gospel of John] when Jesus walks on water. [The apostles] took him into the boat and they were immediately at their destination; that is their second creation. Now it is not from God but in God.

Tiresias [then] talked about Camus’ plague, a contagion like original sin that is somehow endlessly persistent. Jesus reverses that contagion and gives the contagion of new creation.

One first thing I notice is that Wendell’s admonition sermon is the most straightforward of the bunch. After speaking to the importance of having different approaches to being a Christian in the stories of Peter and Paul, he notes that the church is not ours; the monastery not ours—rather we are in and of each as stewards. Therefore, we need to act with consideration of those who came before and those who will come afterward. Do not wreck the church, because it is not yours. Other admonition sermons caution the audience against “grumbling” or becoming “help-rejecting complainers.” Still others advocate for “purity of heart,” “leaps of fate,” and “doing what needs to be done.” These admonition sermons then offer a straightforward moral injunction supported by any number of analogically relevant stories or parables. They acknowledge the difficulties and the contradictions of being a hermit, a monk, or a Christian, and then they give some advice about how to overcome.

By contrast to admonition sermons, becoming sermons can be unsettling. Like admonition sermons, becoming sermons acknowledge paradox but offer no straightforward moral lesson to see a believer through. Rather than being all present and ever vigilant, Job suggests that God sleep. And when God sleeps,

4. The wedding at Cana is a story from the biblical book of John (2:1–12), in which Jesus and his disciples are at a wedding that runs out of wine. Despairing, the host implores Jesus to help, and Jesus saves the party by turning several jugs of water into wine.

in darkness, at night, or even on the battlefield, we have the fresh opportunity to discover God. Darkness and absence can be fruitful grounds for faith. Rather than a simple moral suggestion, Job is showing us that there are scary circumstances in life that can become redolent of God by God's absence. This is not straightforward. Other admonition sermons share Job's sort of reference to Christian mystics and frame Catholic religious commitment in a particularly monastically inflected way: what does it mean, by taking monastic vows to become the "bridegroom of Christ"; how does the "dark night of the soul" allow us to extinguish our ego; how can unconditional surrender bring true joy? These gnostic and paradoxical frames for God seem to emerge specifically from this community of hermits and their contemplative vocation and offer a vision of Catholicism that we may imagine as different from a Catholicism that might make sense to the laity who do not have a hermit cell to go home to.

Finally, exegetical sermons are the oddest of the bunch, as they require a particularly erudite, theologically inspired apprehension of Christianity and monasticism. Here Tiresias is bouncing back and forth between the first creation of the Old Testament and the second creation of Jesus and the New Testament, identifying chains of narrative continuity between them. Jesus interrupts repetitive dreary history and offers salvation. He contrasts the fatalism of Ecclesiastes, a notably pessimistic Old Testament Bible book ("Everything is meaningless" [1:2] or "No one remembers the former generations, and even those yet to come will not be remembered by those who follow them" [1:11]), with the salvation that Jesus somehow offers. The old creation is the water; the new creation is the wine at the wedding of Cana. Jesus offers a new creation that, in its particulars, addresses and remediates the problems of the old creation such as original sin. Other exegetical sermons make arguments that numerical correspondences, such as there being 12 tribes of Israel and 12 apostles, or that there are commonalities across Old Testament wisdom figures and Jesus's life, suggest a larger biblical, Christian form of history, in which Jesus's coming is intricately foretold and his eruption into history is consequential in biblically knowable and patterned ways, though also ways that defy common human logic and understanding.

It is worth noting, too, that the exegetical sermons, unlike the other two categories, were the work of Tiresias and one other monk who both had extensive philosophical educations and had spent a long time pondering theology. This suggests to me that there may be other forms of sermons at other monasteries and churches, carried by other monks with other sorts of lives and communities that were simply not present at the hermitage because of who was on the current roster and who actually lived there. This, though, is not much of a problem for my purposes, as I am taking "sermons given" as my unit of analysis. Moreover, the sort of analysis I am advocating would lend itself readily to comparison to variations in other contexts in which sermons show up and speak to some of the values of a given Catholic community.

Taken together, we might understand these three categories of sermons as occupying a larger "genre" (the "sermon" or "homily") within which their specific variation occurs.⁵ We may understand a genre as "one order of speech style, a constellation of systematically related, co-occurrent formal features and structures that serve as a conventionalized orienting framework for the production and reception of discourse" (Bauman 2000:84). A sermon is produced at a specific point in the Mass, after the day's readings and singing and before the Eucharist. A sermon is also marked as taking place by being a nonproscribed, nonbiblical, nonhymnal speech occurring in the Mass. It is one of two times in a Mass at the hermitage that people speak relatively freely (the other being the offering of prayer intentions). Catholics and Catholic hermits know when to expect a sermon, based on the repetitive ritual structure of sitting, standing, singing, and chanting that occur in a Mass. They also know a sermon by its open though still didactic structure. And that structure takes the form of some sort of a relatively linear exposition of stories, fables, and analogies that all lead to a concrete lesson, moral, or takeaway. The sermon is a form of instruction, and it is received as such. If it fails to clearly instruct, then the sermon has failed for that person. This in turn leads to my residual categories: seven sermons that do not land ("don't land sermons") and four sermons I failed to categorize ("miscellaneous sermons").

For seven don't-land sermons, I could not discern any sort of cumulative point or argument—they often felt like a pile of idiosyncratic stories and anecdotes that held meaning for the presiding priest and not for me. Perhaps sometimes the monks were stressed, were busy with other things and could not prepare, or just might have been having an off day. Then for four miscellaneous sermons, I understood where they were going, but I could not integrate them into my generic typology of sermons. I note that the miscellaneous sermons all happened in my first week of fieldwork and perhaps simply reflect my

5. One does not have to go terribly far in American Christianity to find other genres of preaching. Reporting on Appalachian Pentecostalism, specifically as mediated over the radio, Blanton (2015) describes a form of commenting on the Bible, Jesus, God, and the world that involves short, staccato sentences punctuated by "a guttural percussive gasp for breath at the end of the chanted sermonic line" (9) that is meant to imitate the inspiration of the Holy Ghost. In turn, this style and intervention on the part of the Holy Ghost leads to an improvisational style of preaching in which biblical fragments, moral exhortations, and prophetic expectations are blended and cascade, one over the other, to engrossing, raconteurish effect (e.g., sermons reported in Blanton 2015:36–51, 105–114, 150–155). Though a number of the brothers at the hermitage had been born again in the spirit in the course of the Catholic charismatic revival that hit their portion of the church in the 1970s and 1980s, and despite some of them being gifted with tongues, I never heard this sort of Pentecostal preaching.

Elsewhere, Harding (2000) reports on the way fundamentalist, "born-again," American protestant preachers make use of "witnessing," that is, casting themselves and their audience or listener as central characters in a free-form assemblage of scriptural allegories, intended to incorporate the listener and, hopefully, lead them to salvation (40).

lack of ability at the time to recognize and record the salient details of a sermon to make it fit genre conventions. Alternatively, they really could just be other sorts of sermons. In any event, residual categories in cultural analysis are OK⁶ (in fact, should be expected), particularly given that we see meaning systems as open, changing, and subject to contestation, categorical overlap, and partial understanding.

Rather than any given sermon being capable of standing in a straightforward way for the contradictory values hermits seek to live out, close analysis of the variation in actual sermons given shows that there are three predominant forms that sermons take, each offering significantly different approaches to what one should think about and do in monastic life. It is not that the first admonition sermon from Wendell was wrong; rather, taking it as general would make for an incomplete analysis. Admonition sermons offer simple piety as a solution to the contradictions of Christian life. Becoming sermons by contrast suggest that straightforward, agentive, moral action is often inadequate to a spiritually authentic life. Rather, one often needs to surrender to exhaustion, give up action, and open oneself to the mystery of God's agency in the world. Finally, exegetical sermons are filigreed exercises in illustrating the limitation of human ideas of causality and time when confronted with the weirdness of Christ's divine intervention in the world. Each form of sermon acknowledges paradox in monastic life, yet each also offers radically different ways for addressing that paradox. Moreover, if we just took Wendell's admonition sermon as speaking for all other sermons, despite them being a real plurality of sermons given, we would miss the fact that more often than not, the monks in their sermons actually give up on directly agentive acts to deal with the contradictions of their order's spirituality. Rather, they lean into one mystery or another and trust that God will show them the way.

A Way Forward

Part of the allure and promise of ethnographic work in sociocultural anthropology is that, unlike just about any other

6. It is also worth noting that residual categories come up in every sort of cognitive anthropological approach and add another helpful way to think about the fact that data that are difficult to categorize always come up in ethnographic research. Free lists are a good, parsimonious example of this (Weller and Romney 1988). A free list is a form of structured interview in which a group of people are asked to list examples of something that is locally relevant—say, cold remedies, edible plants, or types of crimes. What generally occurs when multiple lists are collated is a sort of “scree plot,” that is to say an accumulation of terms and items that come up very commonly and are often easily categorized, then a long tail of residual “one-offs” that perhaps make sense to people but are not central in a given meaning or knowledge system and do not have a ready type or category to contain them. Again, this is normal and I suggest simply shows that cultural meaning systems are always in flux, always are at least a bit heterogeneous, and always have potential for change (e.g., Souleles 2018b). Given this, it is OK not to be able to categorize everything one encounters.

social science, anthropologists are willing to make long-term, ongoing relationships with the people they study and are open to the fact that good research takes time. In trying to understand people, the path of the researcher is often circuitous, full of dead ends and false starts, and is just about never linear. While these assumptions and practices on the part of anthropologists have led to a steady stream of topically and theoretically innovative scholarship, I have here suggested that it has also often led to a weak style of arguing from authority and cherry-picked examples, as well as a general mystification of the actual work of data collection and analysis.

In remedy to these limitations, I have suggested that anthropologists systematically collect and analyze their data and then report on that systematic analysis. A reader should be able to have some sense of why an anthropologist sampled what they did, why one should think that sample was adequate, and what variation is present in the data that the anthropologist has selected that led to their specific claims. I have also modeled what this sort of anthropology might look like. At root, I feel that anthropologists should do more work to convey how they know what they know and should resist the urge to overgeneralize from lone examples that can be passed off as representative with neither argument nor evidence. Not only will this approach to data and analysis make our arguments stronger; this more pragmatic approach should make training in anthropology a bit less confusing.

Given all that, I will allow Father Gus, with an admonition sermon, to have the last word:

Admonition Sermon

6/26/11

Father Gus Presiding (12 Monks 15 Lay)

Gus said that we are all creatures of habit—it's hard to turn off whatever we do. The centurion in the gospel is able to do so. He's used to giving orders, yet he is humble before Christ. Gus went on to say that when we get something that is not owed to us, we are unworthy. We ought to be grateful and willing to do something in return.

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Comments

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Questions and Answers

When does something become data? Only when questions are posed that direct our attention to specific observables that

count as answers. Unless we pose a question, we do not know where to look within the limitless complexity of what is merely perceivable and may be merely anywhere.

What about questioners? If Bobby's questions and Johnny's questions emerge from unrelated antecedents (as often happens in third grade, especially during recess), their answers are not commensurable with each other and do not contribute to a common enterprise of the sort that relies on many people working together. Unlike recess, anthropology is one such enterprise. Ad hoc questions do not contribute much to it because their answers do not add up to anything together.

If Bobby and Johnny grow up to become anthropologists one day, how will their professional careers differ from third-grade recess? The differences are too numerous and profound to enumerate or classify and too obvious to belabor. Yet despite this unclassifiable vastness, is anything perchance the same? One thing, perhaps: the field sites and social processes that anthropologists investigate are unclassifiably varied, and since questions emerge at least in part through fieldwork, the questions that anthropologists ask are also unclassifiably varied. Anthropologists' questions are never ad hoc in relation to their own fieldwork, of course, but can appear ad hoc when compared to each other. The topics of the 23 ethnographies listed in Souleles's appendix give us a flavor: fear, love, and technoscience, animality in factory farms, whiteness, rightful killing, breast milk sharing, Chinese migrants, abstract labor, slavery, and many others. The questions that emerge from this range of ventures will surely appear ad hoc when listed side by side. What might this imply about questioners over time? Since published ethnographies tend to be treated as guidebooks for graduate students, what might this list imply about the capacity of this field to reproduce itself over time?

Souleles makes a modest proposal: anthropologists should commit themselves to a more systematic discussion of methods of data sampling and of the social distribution of the patterns observed. I could not agree more. In fact, he makes the case so well that I have little to add to his discussion of this issue by itself.

As I read his essay, however, I was struck by another issue that Souleles does not discuss but for which his discussion has broad implications. These implications emerge only if we step away from the figure of ethnographer as an individual and focus instead on the manner in which questions and answers are linked within a field of knowledge and on what such linkages imply about the unity of a social science over intergenerational time.

Systematic ways of describing data that count as answers can generate systematic ways of posing questions. Questions can be limitlessly varied by topic (as above) but can nonetheless have a systematic relationship to each other if the manner in which they are posed is held accountable to (anticipates) a systematic way of describing data that count as answers.

Since ethnographies are meant not just to be written but also to be read by others (and *inter alia* by those who will one day be writing ethnographies themselves as future scholars), to bring

about a likeness in methods for posing and answering questions is to bring about a kind of unity within a field that (even while it remains "an inductive mode of fieldwork-based inquiry" pursued by many individuals across many field sites) can nonetheless become visible to its own members (particularly across intergenerational time, which is where it is created) as a unified enterprise whose parts speak to each other. The more questions to be asked are anchored to answers already given the more likely it is that common frameworks of collective inquiry will emerge. For those who study the diversity of human experience across the subfields, perhaps this is the only comparative framework they need. For those who study common topics, to have at least this is likely to equip them with even more, with ways of pursuing questions that might not occur to those who lack it.

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Advancing Analytical Transparency in Cultural Anthropology

Souleles provides an admonition sermon for sociocultural anthropologists: despite our deep commitments to inductive research questions and the long and intensive fieldwork needed to answer these questions, "all is not well" with the ways that anthropologists present evidence and make arguments in ethnographic research. The norm, Souleles shows, is for sociocultural anthropologists to argue for the trustworthiness of the researcher instead of demonstrating the trustworthiness of the research. In short, sociocultural anthropologists too often fail to show the receipts of their analysis. This is clearly a problem in a post-truth society where scholarship, science, and academic expertise are increasingly scrutinized for political aims (Harsin 2018). But Souleles does not take this tack with his admonition. Instead, he masterfully shows us how the process of systematizing our data analysis (and reporting on it) leads to more robust findings and more nuanced interpretations of our data. Showing our receipts, Souleles argues, not only is an ethical imperative but also makes us better researchers.

In making his arguments, Souleles shows us *his* receipts. His evaluation of methods reporting in sociocultural anthropology comes from a review of 23 ethnographic books across three academic presses—Duke, University of Nebraska, and Routledge—capturing a prestigious list of ethnographic books, a topical specialized list, and a non-US list (respectively). He justifies his selection as aiming to cover a broad enough cross section of recent sociocultural work in anthropology to be representative.

While Souleles's sample is adequate to demonstrate a clear norm within US and European sociocultural anthropology, it

is an overstatement to characterize this sample as representative. Sociocultural anthropology has a long tradition of documenting and innovating systematic methods in ethnographic research: Lewis (1953), Spencer (1954), Pelto and Pelto (1978), Spradley (2016 [1979]), Quinn (1987), Bernard (2017), Weller and Romney (1988), D'Andrade (1995), Mattingly and Garro (2000), Ryan and Bernard (2003), Hruschka et al. (2004), Dressler et al. (2005), Gravlee (2005), Guest, Bunce, and Johnson (2006), Ember (2009), Negrón (2012), Hagaman and Wutich (2017), Hennink, Kaiser, and Marconi (2017), Wutich et al. (2021), Beresford et al. (2022), Mitchell et al. (2022), Roque et al. (2024)—to name just a few. Sociocultural anthropologists who make their analytical methods explicit are widely published across anthropological and interdisciplinary journals and academic presses. Nevertheless, Souleles shows that methods reporting is almost absent across three high-profile outlets for ethnographic research, explains how this trend is illustrative of broader norms in sociocultural anthropology, and demonstrates why this is a considerable problem for our discipline.

So what is the way forward? Souleles argues that sociocultural anthropologists *should* use systematic data analysis methods and *should* report them, but changing disciplinary norms is difficult. How do we implement the lessons in Souleles's admonition? In the spirit of continuing the conversation, I offer two points for consideration.

Expand research methods training in cultural anthropology programs. Souleles begins his article recounting his lack of systematic methods training as a cultural anthropology graduate student. This experience is not uncommon. In a recent survey of over 1,300 professional members of the American Anthropological Association, Negrón et al. (2024) found that surveyed anthropologists overwhelmingly reported that their training was deficient in data analysis methods. Additionally, Ruth et al.'s (2022a, 2022b) analysis of 107 syllabi of methods courses taught across US anthropology departments found that only 17% syllabi in the sample contained lessons, readings, or activities on systematic methods for managing and analyzing qualitative ethnographic data.

To state the obvious, sociocultural anthropologists need more training in qualitative data analysis. A start would be for departments to require courses devoted this. Such courses might include the basic building blocks of qualitative data analysis (e.g., theme identification [Beresford and Bernard 2023; Ryan and Bernard 2003], coding text [du Bray 2023; MacQueen et al. 1998], and structured comparisons [Bernard et al. 2017; Pacheco-Vega 2023]) and introductions to established qualitative data analysis (QDA) traditions like grounded theory (Glaser and Strauss 2017 [1967]), schema analysis (Quinn 2005), and classical content analysis (Krippendorff 2013). Outside of degree programs, organizations like MethodsNet (2024) and the National Science Foundation Cultural Anthropology Methods Program (Wutich et al. 2024) offer QDA workshops, courses, and teaching resources. Expanding QDA training opportunities will provide sociocultural anthropologists with the skills needed to execute and ensure analytical transparency.

Establish new disciplinary norms and standards for reporting research methods. Souleles demonstrates why reporting analytical procedures and decisions is vital to the peer review process. Without it, reviewers cannot properly evaluate a piece of research. Yet of the 22 journals published by the American Anthropological Association, only five journals explicitly require methods reporting in their author guidelines (*Annals of Anthropological Practice*, *Anthropology of Consciousness*, *Feminist Anthropology*, *Ethos*, and *Museum Anthropology*). And no AAA journal outlines specific standards for reporting qualitative research. Establishing reporting standards within anthropological journals will ensure that authors provide the necessary information for peers to evaluate their research and encourage new norms for methodological transparency in our discipline.

Some may argue that establishing reporting standards is impossible (or detrimental) in sociocultural anthropology because of our diversity of epistemological approaches. But over the past two decades, disciplines from social work, to nursing, to health sciences (all of which also encompass a range of epistemological traditions) have advanced explicit standards for reporting qualitative research in ways that preserve epistemological diversity: the US National Science Foundation's Interdisciplinary Standards for Systematic Qualitative Research (Lamont and White 2005), O'Brien et al.'s (2014) Standards for Reporting Qualitative Research (SRQR), and Wu, Wyant, and Fraser's (2016) author guidelines for manuscripts reporting on qualitative research provide a few examples. These standards require that authors make epistemological approaches clear and explain how they analyzed (or plan to analyze) their data based on such approaches. Anthropology journals can follow suit and borrow from established standards and/or develop their own, to ensure analytical transparency.

Moving forward, sociocultural anthropologists must take practical steps to implement the points raised by Souleles. I thank him for continuing important methodological conversations in our discipline and expertly showing us how and why transparent research methods are essential for the credibility and trustworthiness of our research.

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This article by Souleles is an engaging and valuable argument that anthropologists should, but generally do not, give an accounting of our data as part of supporting our analytical and theoretical claims. As such, ethnographic writing still often relies on establishing the experience and credibility of the author. Souleles highlights this through a review of the systematic methods discussions—or general lack thereof—in recent ethnographic books across three publishers. While Souleles points

out that some claims to authorial authority are still needed, he argues that providing a clear overview of one's data and how they inform an argument, alongside insight into one's relevant experience (both personal background and fieldwork experience), is much more persuasive. It also provides the grounds for discussing precisely what more could be needed in terms of methods and data collection to be sufficient for analysis.

On the one hand, the suggestion that anthropologists provide a systematic accounting to readers of their data and the extent to which it supports their claims seems fairly straightforward. On the other, it is a suggestion that pokes at some significant sore spots in anthropology as a discipline. This includes how cultural anthropology has often positioned itself as the antithesis to quantitative approaches, seen recently in responses to "big data" as highlighted by Douglas-Jones, Walford, and Seaver (2021:11–12), for example, but also debates about the extent to which anthropology is a social *science* versus humanities-based discipline and lingering questions around the power of anthropologists in the making of ethnography. Given the brief scope of this commentary, I will focus on some specific questions in relation to the first issue, which intertwines with collaborative work I have done exploring the possible role of computational methods in anthropology (Breslin and Albris, forthcoming; Breslin et al. 2022).

The systematic approach to explaining data collection and analysis as "a way forward" that Souleles suggests points to a kind of quantitative assessment of one's research material, even given the relational nature of ethnographic research. As the example indicates, in brief, 16 sermons were "admonitions," 15 were on "becoming," seven were "exegetical," seven were "don't land," and four were "miscellaneous." One could even think about translating this into percentages such that the admonition sermons, like that by Brother Wendell, account for just over 30% of the cases (or approximately one-third). The value here is, as Souleles suggests, to give a holistic overview of the work that was done and the extent to which a given example is representative and generalizable. Doing so provides the reader with an understanding of the overall place a vignette or ethnographic description fits in a body of material, which can then support the analytical and theoretical claims we make. This can also lead to a more holistic analysis that takes into account different cases. Yet such an accounting can (rightfully?) also undermine our claims if such examples are indeed cherry-picked and not representative.

I largely agree with the spirit of what Souleles argues, especially as it pertains to providing means to better account for and teach ethnographic fieldwork to new anthropologists. It builds on some limited discussion on validity in anthropology (e.g., DeWalt and DeWalt 2011; Fife 2020; Sanjek 1990). This includes the common discussion of "saturation" mentioned by Souleles but also triangulation between methods and using appropriate sampling methods (see, e.g., DeWalt and DeWalt 2011). Roger Sanjek (1990) further suggests "theoretical candor," "the ethnographer's path" through their analysis, and "fieldnote evidence" as constituting "ethnographic validity."

Quantitative methods also have a current and historical role in anthropology, and so, to the extent the accounting proposed by Souleles aligns with such methods, it also has precedence (see Breslin and Albris, forthcoming).

Yet numbers (and methods) have politics (Martin and Lynch 2009). Some of these politics are already embedded in our analyses, such as decisions about classification and what counts as meaningful for a focus of analysis, as Souleles points out. What such accounting could challenge, however, is the value of the particular or singular case and what counts as evidence. Of course, Souleles is not suggesting to ignore cases that are only one-third (or less) of our data, to see them as insignificant, or to ignore diverse forms of ethnographic evidence. But some aspects of counting may nonetheless shift epistemological perspectives of the value of data, including seeing our diverse research material as "data" (Douglas-Jones, Walford, and Seaver 2021; Grinberg 2016). Does a passing comment in a conversation count the same as a drawing, as a feeling, as an interview response? There are new decisions about what counts in making ethnographic material accountable. And while counting does lend support in claims for generalizability, generalization is not always the goal of anthropology. Similarly, there is sometimes value in obfuscation—for *not* counting. In some cases in my research, for example, accounting for the identities of participants (e.g., in terms of gender or race) would have made them easily identifiable. The poetics of ethnographic writing allows me to include their perspectives without revealing their identities.

I would argue that what Souleles proposes is valuable to make space for and normalize in anthropological practice. The proposal can be seen as an extension of reflexive ethnographic practice and of discussions of validity, asking us to account for and show the reader the shape and extent of the patterns we as ethnographers deem meaningful and significant. It can easily be a facet of showing the ethnographers' path to their conclusions and intertwined with fieldnotes as what makes up anthropological evidence, following Sanjek. At the same time, we still need to navigate the politics and poetics of ethnography to incorporate such practices on anthropological epistemological terms. This also includes navigating the requirements of publishers (a challenge Souleles acknowledges), where it is increasingly common for journals to require various forms of accounting, even as the first advice for turning a PhD dissertation into a book is to cut the methods chapter.

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A Novitiate in a Period of Change Revisited

As someone who has focused on research design and methods in anthropology for quite a long time, this piece by Souleles

was both welcome and, at the same time, troubling. While reading this article, I was reminded of Sampson's classic study of a monastery. Sampson (1968) spent approximately 12 months conducting his research at St. Anthony's Monastery (pseudonym) in a northeastern state in the US in the 1960s. The research sought to examine the role of social relationships, in the form of social networks, in dissensus maintenance and resolution, from a sociological and social psychological perspective. For some background, his research took place in the mid-sixties, a time of change in the Catholic Church given Pope John XXIII's attempts to move away from many Catholic Church traditions. Sampson stated that his first objective was to gain access when initially entering the field, stating: "Not knowing anything of this setting, the researcher adopted the anthropological 'take-me-to-your-leader'" strategy, and was introduced to Father Paul" (Sampson 1968:247). His methodological approach combined qualitative and quantitative methods (e.g., participant observation, surveys, semistructured interviews, and experiments), which we might refer to today as "mixed methods." Following the conducting of experiments, he moved into a phase of research he referred to as "quasi-participant observation" in which he lived at the monastery participating in some community activities, a kind of "active participant observer" (Johnson, Avenarius, and Weatherford 2006). As is inevitable when engaging in active participation in any group, some identity needed resolution, and at first he took on the role of "Catholic on retreat." I highlight this research since it serves as an interesting comparison to Souleles's study of the hermitage and stands in contrast to the underlying reasons for his reflections and his struggles in the field. In addition, Sampson writes about some of the same methodological concerns as Souleles but from a very different perspective.

The Sampson study had explicitly outlined multiple phases of the methodological approach in a step-by-step manner, something that Souleles points out is rarely done in anthropology. Sampson's study, because of its explicit methodological details, would allow for replication of research on another monastery or even another social setting with similar characteristics of a "total institution," as coined by Irving Goffman. One of Sampson's committee members at Cornell University was John M. Roberts, an anthropologist who was the first to write about the theoretical concept of distributive cognition. He was also an anthropologist who was quite comfortable with both quantitative and qualitative methods. Indeed, methods in anthropology are rarely taught at the graduate level, and if methods are taught, they generally involve methods about how to write an ethnography and not how to collect and analyze data. This tradition of methods-free training runs deep in anthropology. Two stories stand out concerning the historical roots of this rather cavalier approach to field methods. Bernard (1988) relates the story of Charles Wagley approaching his elder colleague Alfred Kroeber for advice on how to teach a field methods course. Kroeber's terse response: "Some can and some can't (Wagley

1983:1)." Wagley went on to teach the course but did not remember much about it. Michael Agar in his book *The Professional Stranger* wrote about a first-year graduate student at Berkeley who nervously approached Kroeber's office for advice about her upcoming fieldwork. "Well," said Kroeber, returning to his typing, "I suggest you buy a notebook and a pencil" (Agar 1980:2). As Souleles points out, this opaqueness surrounding the ethnographic training process has fostered an ethnographic mystique and a trial-by-fire aura. I could imagine one of his professors advising him before entering the field to, as he states, "just connect."

The National Science Foundation has funded efforts to help lessen this problem of a lack of methodological training concerning both faculty and graduate students in cultural anthropology. I was involved in one of these efforts along with several others including Russ Bernard, Susan Weller, and Amber Wutich. Well over 300 PhD students attended the three-week-long Summer Institute for Research Design in Cultural Anthropology that ran for 20 years. Student feedback suggested that one of the single most important benefits concerned the lessening of anxiety about conducting fieldwork following their training. Souleles writes about the "nerves and anxiety" of the ethnographic apprenticeship, but an understanding of explicit and systematic methods lessened the anxiety of student ethnographers who attended the institute since they had an explicit plan and clear methods, thus reducing the potential for confusion and uncertainty in the field. They would know what to do and how to do it, and that was comforting.

Souleles brings up several methodological points including the systematic coding of interviews, saturation, cognitive anthropological methods, and what constitutes ethnographic data, among other issues. These are all important, but I was most interested in what we can learn from this social context. He points out that sermons are a good source of data. Like Sampson's work, it might be informative to see how social relationships, in the form of social networks, relate to the variation in the thematic content of sermons and other interview materials and how variations in thematic content reflect the social dynamics of the monastery. But this reflects the potential for a myriad of theoretical and systematic methodological approaches that could come to bear on the study of this monastery. I conclude where I started by expressing both how I welcome this discussion and at the same time am troubled. Methods training of graduate students in cultural anthropology in the United States has been limited despite a variety of efforts to advance methods training for both faculty and graduate students. It is heartening that Souleles advocates the need for evidence and systematic methods in pursuit of producing more convincing, valid, and reliable ethnographic work. However, I am cautiously optimistic in the hopes that Souleles, and anthropologists like him, will advance the training of anthropologists extolling the virtues of ethnographic approaches and methods that, to quote Souleles, generate "valid knowledge about the world."

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Ethnographic Authority and Cross-Disciplinary Collaboration

As a cognitive anthropologist, my reflections on ethnographic authority largely stem from my interactions with psychologists. Anthropologists clearly generate knowledge and understanding that is of real value to scholars in other disciplines, so the exercise of objectifying and explaining the process whereby we know what we know has been a useful one. Over the years of collaborating with many wonderful, smart psychologists, some of the questions that have forced me to clarify for myself—as much as for others—the process of ethnographic knowledge production have included: “Do you follow a child around? How many children and for how many days?”; “What can we do beyond stating that something or the other is complicated?”; and “Why does the ethnographic description need to be in the main body of the article; can we have it in the supplementary materials?” Answering these questions, and many others, has brought to light for me both the unique value of ethnographic methodology and some of its blind spots. What it has also revealed, however, is that we will not resolve our anxieties by borrowing solutions from other disciplines. Some of them we just have to live with, because it is worth it.

In brief, I want to make three points. Increasing transparency and accountability of data analysis would not only improve training in anthropology but also make our work more accessible and increase its bearing on debates across disciplinary boundaries. Second, incorporating systematic elements into open-ended ethnographic inquiry does much the same thing and, in addition, produces valuable insights. Third, neither of these will resolve the conundrums inherent to ethnographic authority (“Did I get this right?”). And nor should they, because despite its imperfections, our ethnographic approach is producing knowledge that is unique and valuable, and our slow, open-ended modes of research and analysis need to be protected. But we should seek checks that help reinforce these valuable features, such as the involvement of an intellectual community in generating ethnographic authority.

Do you follow a child around? Souleles is making an argument for a shift toward a more systematic treatment of data and analysis in anthropology. I am all for it and agree that we could be doing more to prepare doctoral students for their first fieldwork. For example, taking a simple leaf from the book of psychology and many other disciplines, we could make our data and analysis more visible through supplementary materials of journals. When revising a dissertation into a monograph, these details often get erased to make it seem less like a dissertation. The explorative and open-ended aims of ethnography deem that rather than predetermined protocols, researchers need to be equipped with the right methodological and ethical *dispositions*, to be able to make choices in the

course of their research. Therefore, many of us hold the view that advanced anthropological training takes place through being exposed to ways anthropologists approach various questions and topics. If this is the case, then why not provide this exposure through more accountability (albeit much of it retrospective) about our data and analysis? Providing detailed information about it in supplementary materials of journals and monographs would go some way toward demystifying and making ethnographic practices more transparent and more easily replicable or adaptable.

What’s the next step beyond stating something is complex? Souleles is contrasting his systematic treatment of a corpus of sermons with making claims on the basis of the classic ethnographic authority of “being there.” What he seems to downplay is the influence of his ethnographic data on producing and analyzing the corpus. It appears that Souleles has done what cognitive anthropologists do with ethnography and experiments, that is, bringing together an in-depth understanding of specific cultural-historical context and a systematic exploration of a detailed aspect of it. In this approach, the systematic elements, be it field experiments, quantitative observations, or something else, do not replace ethnography. Rather, their design, hypothesis, ecological validity, and interpretation are very much grounded on it. These methods do not therefore create ethnographic authority; they rest on it. These two modes of inquiry sometimes challenge each other and oftentimes together make a strong argument.

A productive use of vignettes can be, for example, to illustrate the richness of the context and the salience of a pattern. That is to say, even with the idiosyncracies of a given interaction or an event, and with things that do not fit, we have observed a pattern so salient that it repeats itself with some consistency across our data. So while Souleles is arguing against overgeneralization, I am arguing against undergeneralization. Can we push our analysis from complexity to then what? Is there something we can still say about China, about human cooperation, or about humanity? Putting ethnographic inquiry into conversation with more structured approaches has pushed me both toward generalization and to making critical anthropological contributions to multidisciplinary debates about human behavior.

Can we just put the ethnographic description in the supplementary materials? My answer has been that ethnography is not a description but always an analysis, informed by theory and prior scholarship. The line of analysis and argumentation needs to be visible for readers to be able to follow and potentially challenge it. It is indeed possible to reduce 10 pages of ethnographic analysis into a table, especially if you have had some orientation toward quantifying your data already while collecting it. But this hides aspects of ethnographic knowledge production that are as important as the data.

One aspect that usually remains largely hidden in writing is the involvement of intellectual community. Claims to authority through solitary thinking process are surely not how ethnographic analysis really takes place. We design, analyze, and write

in conversation with our peers and interlocutors. This is not to argue against Souleles's point about making this process more visible and making ourselves more accountable. But I have to argue against the trend of systematizing everything. We do not want to downplay the valuable training that comes through exposure in the field to a way of life or the understanding that builds through partaking in broader intellectual communities.

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What a treat to be invited to discuss Daniel Souleles's insightful and provocative deconstruction (should I say takedown?) of ethnographic methodology! As somebody who just launched a laboratory to explore and experiment with the future of ethnography (the Critical Ethnography Lab), I want to share why I think Souleles's piece raises questions that are worth thinking with, even if one might disagree with some of the answers.

Souleles is right on the money when he suggests that anthropologists have somewhat exchanged methodological sharpness for theoretical (self-indulgent?) reflexivity. This is replicated in most (contemporary) anthropological writings, which, at best, discuss the researcher's positionality. Missing here is an overview of how ethnographic evidence fits in the researchers' overall corpus of data and what kind of design and methods were used to generate them. In Souleles's experience, as in mine, this cavalier approach to methodology stems from the (lack of) training in this area, which tends to offer very little in terms of data collection techniques; strategies to assemble and analyze archival, economic, or even discursive datasets, or even how to "recruit" and engage with potential participants. Instead of methodologies, we tend to hear variations of the lone anthropologist's parable: a story of how a naive researcher negotiated access to (impervious) fieldwork and, thanks to their perseverance, was able to excavate nuggets of ethnographic understanding that changed their (and our) understanding of the world.

Here I strongly agree with Souleles: the mythology of the lone anthropologist is mystifying of the actual realities of fieldwork. We need better, more realistic training and much more grounded discussions of our datasets. We could learn much more from design anthropologists—including those who had to move into industry—who had to become much more explicit about their methods. But is this an issue of saturation and/or of having a representative sample? Should ethnographic data aspire to be representative?

This is not a rhetorical question but one I keep asking myself (and being asked) throughout my research on artificial intelligence in aviation. Can ethnographic insights be robust enough to build safety-critical technologies? "Human factors" researchers, generally trained in psychology, welcome ethnographic vignettes.

But before taking the data seriously, they want numbers, metrics, standardized surveys, and large datasets that can prove the relevance of any insights—or at least their limitations.

The problem, as Souleles notes, is that anthropological praxis is often iterative, circular, and, well, odd. Because we work with a small number of people over time, it is often impossible to choose our "sample." Usually, it is the other way around, and our participants choose when and whether to help us. This serendipity makes ethnographic data uneven in quality as much as quantity. Some interlocutors give us better insights, open more, or allow us to see unique moments of their own lives. Others might not or might straight up lie. Sometimes we are lucky to be at the right place at the right time, observing one-off situations—conflicts, discrepancies, celebrations—that offer alternative or summative ways of understanding longer-term material or cultural processes. Ethnographic processes make the idea of having representative samples tricky at best: simply counting the number of interviews with a given code will not suffice.

Anthropological data offer a second challenge: ethnographers tend to learn, evolve, and change with(in) the field. My own fieldwork experience suggests that the first few months can be very slow. By comparison, I tend to collect a great deal of meaningful data at the end of a period of immersion or even in subsequent visits. This proportion will change depending on individual researchers' styles (I tend to listen more than question). It is also a consequence of the evolving nature of relationships with participants and of how attuned an ethnographers' instincts have become to their environment. Early phases of fieldwork can be naïve, because ethnographers might lack context. Yet for the exact same reason, they can also be extremely insightful and crucial to question specific assumptions. "Saturation," in an ethnographic context, is not only an objective measure of data being repeated. It is also a subjective state of mind, where we (the researchers) stop finding new and odd things within myriad experiences of fieldwork.

So what can we do to be methodologically explicit and contextualize our data trends if our data remain incommensurable at a deep epistemological level? Other social sciences do not really tackle this problem when they enumerate their procedures or produce data tables to frame their specific evidence. In my experience collaborating with teams that came from more "methodologically conscious" disciplines, data trends are often used to justify a generalizable model. In other words, explicating how representative one's sample is does not serve as an entry point into people's lives, their specific circumstances, or historical contexts. Instead, rigid sampling strategies, and methodological discussions, tend to exclude people, voices, experiences that do not fit a given hypothesis or model—and would not do for our ethnographic goals.

I do not have any masterful solution to this conundrum. On the one hand, one of the superpowers of ethnographers is the ability to use vivid social situations to encapsulate a place, a moment, and an analytical approach. Here "vignettes" make sense not because they are representative data or samples but

because they offer a piece of a puzzle—a clue to interpret a social context, which requires the reader to engage in active analytical work, a leap of imagination, empathy, and analysis. On the other hand, would you want any AI systems that might pilot your next plane to work through patchy data, poetic insights, and unique occurrences? Or would you want them to have a robust (and large) corpus of information, with clear definitions to decide when to rely on ethnographic data—and when they are a glitch rather than a norm?

Perhaps the solution is a compromise, where we do not eliminate completely the partiality of anthropological insights. Instead, we offer more robust guardrails (disclaimers?) around the relevance of our data for a given empirical project and in a specific socio/material context.

Reply

In keeping with the language of the monastery, I confess that I am pleasantly surprised to have received (more or less) general agreement with a call for *a bit* more systematicity in the presentation of ethnographic description and analysis and basic support for the example I provided. Thanks, one and all, for your generous reading, as well as your sympathy, support, and suggestions. Since I do not need to reengage too much with my own argument, I will use this reply to widen out and explain some of why I think anthropology is in the situation that it is in with regard to methods and data and why I wrote the piece the way I did and then agree with some of the contributors' suggestions for ways forward. That said, it is probably best to start with the little bit of trouble that did in fact bubble up.

If I understand Johnson correctly, he is troubled by this essay because, as he rightly points out, there are specific examples in the anthropological archive of the systematic presentation of ethnographic data (even with regard to the study of monastic life!) and that despite the existence of these examples, and despite having spent much of his career offering NSF-sponsored methods training for cultural anthropologists, this presentation-of-data stuff is still a problem within sociocultural anthropology more generally. I think this echoes some of the point that Beresford makes in distinguishing between what seems to be going on in ethnographic monographs (i.e., not much methods, or data, or analysis talk) and what a set of anthropologists have been doing for some time, that is, being explicit about methods, and data, and analysis, as well as actively innovating. To that end, Beresford's bibliography is a real gift to append to this article. If anyone is looking for a place to start, one could (and often does) far worse than reading through that citation list.

What I think Beresford and Johnson get at, too, is a more general situation: namely, there is an active and ongoing subset of anthropologists who are exceptionally methodologically competent and make ongoing contributions to methods liter-

atures within anthropology and beyond (even going so far as to found and operate methods clinics and institutes and journals), *and yet* this subset does not shift general disciplinary practice or the general manner and mode of anthropological training. Both Breslin and Kajanus gesture toward why this might be the case.

Breslin notes that there is a seduction to quantification, one that privileges certain forms of precise arguments over those that are more interpretative. Similarly, Kajanus observes that this sort of interpretation and appreciation of nuance are often what anthropologists offer when working with other sorts of researchers. Moreover, this is why Kajanus is wary of "systematizing everything," which is precisely what many other research traditions push (anthropologists) to do. What these points suggest is that the uptake of a more careful and explicit analysis of ethnographic data is not *just* a matter of skill and competence and experience but also something more ideological having to do with many anthropologists' self-conception vis-à-vis other ways of knowing about the world—that is to say, it has to do with how anthropologists see themselves when they consider what other people-who-write-about-people do.

Not too long ago, at a multifold seminar on crises in anthropology, Adrianna Link made the observation that the "history" of anthropology is generally taught as a history of theory. That is to say, most anthropologists do not spend much time on (or really seem to care) about how specifically they and their institutions came to be. Rather, anthropologists see their discipline as the transmission and transmutation of ideas: how does a preoccupation with cultural forms and types turn into a default embrace of practice theory, say, and not an account of how all these departments got here, who built them, and with what collections and whose bones and whose money and why, never mind what might one owe to that legacy?

In this way, anthropologists, in their self-conception at least, are basically idealists attached to notions of theoretical and intellectual progression and often see their discipline as being committed to ever-better, evermore moral forms of interpretation, nuance, and meaning in opposition to the corroding possibilities of generalization, reductionism, or (heaven forbid) application. In this sense of things, it does not really matter how many people have been innovating in methods and for how long. Methods thinking and data systematicity are categorically beyond the pale and fall outside of the stories that sociocultural anthropologists tell about themselves and tell to their graduate students. Beresford's point about the general silence on methods in journal guidelines is just one illustration of this. Weak or nonexistent methods curricula in grad school are another.

This is actually part of the reason why I wrote this article the way I did. I reckoned that anthropologists win arguments only if they use stories and write ethnographically—polemics, logical deduction, and/or complaints just will not do. I figured that to make my relatively minor point about anthropologists being *a bit* more systematic in their reporting and interpretation of the information they gather, to make this point successfully to anthropologists who might worry about the

taint of too much systematicity or reduction or science, I would have to write with a light touch, a lot of examples, and a Geertz-y voice. I would have to ethnographically show, not tell. If this works, this article should travel.

My ultimate hope is that people learning to be anthropologists (as well as those sympathetically teaching those folks) will read the article and see a bit of themselves in my dilemma and appreciate practically how much there is to gain by allowing ourselves to push our arguments and interrogate the degree to which we have demonstrated what we claim. This does not require us to “systematize evertng” or to abandon our questions, our commitment to induction, or our fieldwork. Rather, this just allows us to do all this stuff a little bit better, with a little bit wider (potential) audience. To my mind, some more attention to our data and our arguments is really the only way to allow the considerable wisdom of the anthropological enterprise to speak to a wider audience, an audience that may not simply take ethnographic authority on faith.

Mattioli’s discussion of his ethnography lab, as well as some of the practical concerns he mentions, brings this point home. Obviously, we would want to know how people feel on planes, and what pilots think about their jobs, and the context in which they operate. However, we would not want an airplane autopilot trained on just “[our] patchy data, poetic insights, and unique occurrences.” Agha is correct that our questions generate certain sorts of data and presuppose certain standards of reliability. What Mattioli shows in his example and gestures to in his applied laboratory work is some of what our questions are good and bad for. If we think pilots are uncomfortable with a certain automated technology, surely we would want to be as precise as we can about how we know about this discomfort. Also, this is not just a matter for applied anthropologists. We owe it to whomever we study to explain how we come to our generalizations about their lives. What the larger extant methods literature in anthropology shows us is that we do not have to give up and become economists, or psychologists, or consultants, or little experimentalists to explain ourselves. We have to have a clearer sense of what we do when we do fieldwork and write about people. We have to have some perspective on the whole of what we have learned and how we make claims from that. We just have to show a bit more of our work.

Beresford’s suggestions for some clearer reporting standards in our publications and better methods training in our graduate programs are obviously good ones (and if anyone is hiring, I may be looking). More basically, though, anthropologists have to want to change. With luck this small essay on monks and their sermons can show just how misleading some of our breezier ways of writing can be and how little we would have to compromise to do better, to be more persuasive, and to reach a broader audience. We spend a lot of time in anthropology talking about crises and the discipline being in danger, catching fire, and so on. Any sort of way through whichever crisis one imagines will likely require asking better questions, finding a broader audience, and joining allies who value our work. And

this means making better arguments about what we claim to know. In any event, this all seems a small price to pay to keep the wine flowing and flowing and flowing.

—Daniel Souleles

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