SOCIOLOGICAL METHODOLOGY

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HOW NOT TO LIE WITH ETHNOGRAPHY

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This paper describes a simple strategy for doing more reliable ethnography: after fieldwork has commenced, investigators can use thought experiments to recognize inconvenient phenomena. Two examples are discussed: "the ethnographic trial" and the "inconvenience sample." The paper uses Clifford Geertz's classic "Notes on the Balinese Cockfight" as a case of how work could be made more reliable with such strategies. It highlights the value of systematically identifying aspects of the situation under study that have been excluded from the analysis.

One of the most popular ways to gain access in ethnographic research is known as convenience sampling: phenomena are included in a study on the basis of their availability, rather than through random sampling. Because such data cannot be representative in a statistical sense, and cannot necessarily even tell us anything about the larger population from which they come, there is no small amount of hand wringing and distress about the horrors of such procedures. In response, some very insightful things are sometimes written to teach field researchers the logic of better scientific inference (See, for example, Small 2009).

Though many ethnographers would wish to proceed in accordance with the logic of "best" scientific practices, they bracket that

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knowledge and still keep to the old ways of doing things, choosing their subjects on the basis of availability. In that context, we should supplement our lectures about scientific sampling with discussions of strategies that might help mitigate the impact of the procedures ethnographers are in fact using.

In "Science as a Vocation," Max Weber wrote that "the primary task of a useful teacher is to teach his students to recognize 'inconvenient' facts," by which he meant "facts that are inconvenient for their party opinions." Following Weber, I would argue that for every ethnographic project there are phenomena that are extremely inconvenient from the standpoint of the line of thinking or theory that has emerged from the fieldwork. The method of ethnography should accustom itself to explicitly identifying such phenomena.

1. THE ETHNOGRAPHIC TRIAL

One of the ways that I can accustom myself to inconvenient phenomena is to imagine that I will stand trial for ethnographic malpractice. An attorney has brought a claim against me on behalf of my study's readers. The trial will be held at a courtroom near the site of study, and witnesses who know about my subject will be called. The important thing about these witnesses is that they will be the ones I most fear hearing from because what they know is least convenient for the impressions I have given the reader. They may also have been the least convenient for me to get to know.[1]

In such a trial, we are not interested in the rights of the community under study or even the rights of any of the people being called to the witness stand, but the reader's right to a reasonably reliable rendering of the social world. In such an imaginary case, the jury will be

[1] The inspiration for this approach comes from those ethnographers who follow the strategy known as "analytic induction," believing that the best way to improve their theory is to "maximize the chance of an odd case turning up" (Becker 1998:86; see also Lindesmith 1947; Katz 2001), "in order to force revisions to the theory that will make the analysis valid when applied to an increasingly diverse range of cases" (Katz 2001). The ethnographic trial is essentially meant as a single instance of looking for cases that force revision, but not in the service of causal inference or airtight theory as it would be in analytic induction.

told to distinguish between two kinds of errors: those which, when corrected, would lead to a reversal or significant alteration of the reader's impression of how the phenomenon under study works; and harmless errors, which do not require such a revision. According to the precedent that governs rulings in my imaginary court of ethnography, "Before we hold that an error has affected a reader's substantial right in a reliable account, thus requiring reversal, we must conclude that, based on the entire record, a reasonable possibility exists that, in the absence of the error, the impression might have been substantially different." An error is harmless when the remaining evidence would have led to the same overall impression in the reader's mind or forced no revision to the theory.

Fieldworkers' entrée points are usually very consequential for who else they get to know. They rely on the social networks of initial subjects who "refer" future contacts. Once researchers select an entry point, the chances of getting to know all the people or phenomena equally well are limited due to cleavages within groups. Also, becoming close to some people often precludes getting close to others. Thus, the method of entry often leads to bias by reducing the likelihood of achieving a good cross section of the population. This means that the definition of the situation that researchers will come to understand, the kind of routine events and practices observed, tends to be limited. Meeting some people instead of others, or occupying one social role over another, can be consequential for what sociologists can explain. They will tell about society from certain perspectives at the expense of others. When ethnographers don't have to worry about hearing from the witnesses they have never met or talked to, they more easily sidestep alternative perspectives or deceive themselves into thinking that these alternative perspectives either don't exist or don't have implications for their developing line of thinking.

At this point, the reader might be asking the question that a reviewer asked me in response to an earlier version of this paper: "I'd think it is only a 'bad ethnographer' that is 'satisfied with' treating the first encounters and people as data. I'd like to see examples of research that could be made better by employing this strategy." To show that this is even true of our exemplars, let us look at the work of someone who few would accuse of being a "bad ethnographer."

2. BEYOND THE COCKFIGHT

Clifford Geertz is widely acclaimed as the leading interpretive anthropologist of the past half century. His paper "Deep Play: Notes on the Balinese Cockfight" is the most widely cited work of "thick description," which he defines as "setting down the meaning particular social actions have for actors whose actions they are, and stating, as explicitly as we can manage, what the knowledge thus attained demonstrates about the society in which it is found and, beyond that, about social life as such" (Greetz 1973:27). In depicting the social significance of the cockfight, Geertz admirably strives for a kind of "ethnographic completeness" (p. 427). This is not a naïve realism that promises "the whole story" or life "as it is," but rather a pragmatic approach to doing the best one can given the limits of ethnographic method.

Geertz's essay on the Balinese cockfight is an attempt to describe the meaning of the cockfight for the people in a village and to state what this tells us about Balinese society and social life more generally. He begins with a now legendary rapport tale emphasizing that people in the village ignored him until he and his wife joined them in running from the police during a raid:

> We ran down the main village street. . . .About half way down another fugitive ducked suddenly into a compound—his own, it turned out—and we . . . followed him. As the three of us came tumbling into the courtyard, his wife . . . whipped out a table, a table cloth, three chairs, and three cups of tea, and we all, without any explicit communication whatsoever, sat down, commenced to sip tea, and sought to compose ourselves. (P. 415)

Soon after, a police officer arrived looking for the village chief and asked the Geertzes "what in the devil did [they] think [they] were doing there." Their host of five minutes produced

> an impassioned description of who and what we were, so detailed and so accurate that it was my turn, having

> barely communicated with a living human being save my landlord and the village chief for more than a week, to be astonished. We had a perfect right to be there, he said, looking the Javanese upstart in the eye. We were American professors; the government had cleared us; we were there to study culture; we were going to write a book to tell Americans about Bali. And we had all been there drinking tea and talking about cultural matters all afternoon and did not know anything about the cockfight. (P. 415)

There is, however, much more to Geertz's entrée into the village than this. Though he makes very little of it, he and his wife got in through a contact in the colonial regime, who hooked them up with the village chief, who put them in the house of a man who was both the chief's cousin *and* brother-in-law. Geertz does not say anything else about this man in the popular cockfight paper, but in *Kinship in Bali,* an earlier book he coauthored with Hildred Geertz, he identifies him as a blacksmith who made "delicately tuned gamelan orchestra gongs and xylophones" (p. 38) and was a member of the village's "economic elite" (p. 38). (I rely on that earlier work for all facts about the village that don't come from the more famous cockfight essay.) This chief and his brother-in-law belong to one of the four major factions of village life. Though we are never told in the cockfight essay what these factions are, according to the earlier work they are kin groups, though "just as strong an argument could be made that they are religious groups, or microcastes" (Geertz and Geertz 1978:5).

By clarifying who would be the most inconvenient witnesses, Geertz certainly could have been more transparent about how he got his information about cockfighting and what perspectives are privileged in his account. Yet, as we will see, this study of cockfighting essentially began early in his stay in the village, so it is unlikely that he could have immediately known enough about the categories of village life to do a sample that drew on all the factions. Furthermore, because he and his co-author were there to do an analysis of "modern urban economic life" (Geertz and Geertz 1978:33), he backed into his study of cockfighting and may not have known it would be a subject for writing until he had seen numerous matches. In this sense, Geertz is like many ethnographers who believe they could not possibly do an adequate sample because they do not know the categories of daily experience in advance of being there. In addition, many who go into the field—particularly those who use the method of "grounded theory"—are unable to define the target population in advance because the *object of explanation* is not sufficiently clear or emerges in an ongoing way throughout the research process. By the time the researcher knows what he or she is studying, it would seem to be too late to undertake any kind of systematic survey.

If Geertz was to be placed on trial in an ethnographic court, he would have reason to be concerned that the witnesses would highlight that nowhere in the cockfight essay has he pursued the interpretations of people in all the family factions; he appears, in fact, to have only a convenience sample of one of the four. There is also no sense that he seeks to discover the perspective of the village's poor—its unskilled laborers in brick, tile, and cigarette factories (see Geertz and Geertz 1978:38). Geertz's problematic, explaining the significance of the cockfight in a village characterized by extreme factionalism among four different family groups and sharply divided into rich and poor (Geertz and Geertz 1978:37), would seem to lend itself to some strategy for understanding various perspectives. The Geertzes made the following observations in the earlier book on this village:

> Two fully cooperative and intelligent Balinese from the same village may give completely variant accounts on matters that the ethnologist believes to be crucial to his formulations. They may give strikingly different descriptions of the organization of the same concrete group of kinsmen, or they may even use completely different terms to identify that group. On a more abstract level, the same two informants may give entirely different lists of the various kinds of kinship groupings that they know about. (P. 1)

While their entrée comes through giving money to the chief's relatives (some of which might have ended up in the chief's pocket for making the arrangement), it also comes with immediate access to the elites of the village. In the trial, Geertz might be asked how his method of entrée influenced what he came to know about the cockfight. After the entrée tale, we do not get to know his articulate host in any more detail, except to learn that he becomes one of Geertz's "best informants" and the only one that we learn anything about. It is no wonder therefore that the meaning of the cockfight that Geertz conveys seems to reflect the point of view of those who drink tea over tablecloths in their back yard and talk easily about "cultural matters" with an American professor. Indeed, much of Geertz's argument in the essay would seem to be influenced by this basic fact. Witnesses from the poorer classes would likely suggest that Geertz has taken the view of the economic elite who are hosting him. It is the elite definitions that he appears to have most access to. After all, if only one informant is ever mentioned and we don't know what family faction he comes from, this would almost surely lead to an extreme bias.

Geertz's observations culminate in an argument that the Balinese cockfight under discussion is a dramatization of in-group/out-group distinctions and status concerns. Avoiding extended ethnographic description, he "pronounces" a series of "facts" and asks the reader to accept his assurance that "concrete evidence, examples, statements and numbers that could be brought to bear in support of them, is both extensive and unmistakable" (p. 437). Among these claims are (1) a man virtually never bets against a cock owned by a member of his own kin group; (2) if your kin group is not involved, you will support an allied kin group; (3) if an outsider cock is fighting any cock from your village, you will tend to support the local one, and so on. While all of these claims make sense, we believe them because they conform to common sense about in-group/out-group relations. However, once we realize that Geertz never observed members of the different families *in situ* or asked them how they actually felt, and that he saw class resentment following distinctions between the elite and the poor as largely irrelevant to understanding the scene of the cockfight, questions arise about how the account might differ if we were to hear directly from the witnesses, especially those from subordinate classes. Given the extreme levels of inequality in the village described in Geertz and Geertz (1978)—but not referenced in the cockfight paper—the commoners chosen to serve as witnesses might remind the jury that he didn't keep records of their actual bets and talk and never hung out on the edges of the cockfight where they gather before pronouncing that they always root for the cocks of their own family leaders.

It will be important for the court properly to conceptualize and identify empirically the relevant units of analysis that, in ethnographic work, can range from individual people, to social roles, situations, kinds of interactions, cultural forms, events, groups, organizations, blocks, neighborhoods, and communities. It may be that one or more of them overlap: which ones, how? It is also important to realize that some units of analysis within any given study may even be large enough to sample probabilistically. Not sampling probabilistically, the court may assume, affects the ability of the ethnographer to make *general* claims about or interpretations of the kinds of interactions, events, cultural forms and meanings that he or she encounters. In small samples where probabilistic sampling is impossible, the court should be skeptical of claims that attribute great weight to what "sometimes", "often", or "frequently" happens.[2]

3. TOWARD "INCONVENIENCE SAMPLING"

A primary task of ethnographers is to help their readers recognize phenomena that are inconvenient for the line or theory that has emerged from their fieldwork. Ethnographers well into their studies could, as a matter of course, ask a few simple questions: Are there people or perspectives or observations outside the sample whose existence is likely to have implications for the argument I am making? Are there people or perspectives or phenomena within the sample that, when brought before the jury, would feel they were caricatured in the service of the ethnographer's theory or line of argument? Answers to questions of this kind can help the investigator to create an "inconvenience sample."

There are two kinds of benefits that can come from constructing such a sample in the middle or toward the end of a study. First, we can sometimes gain a better understanding of biases or lack of nuance

[2] Of course, if you can only select a small number of cases, then any method of selection, including randomness, will not let you generalize with any degree of certainty. King, Keohane, and Verba (1994) give a fascinating example with three observations where random selection gives you the wrong answer two-thirds of the time. This is a very important insight for scholars who think that they can use random selection to achieve generalizability with a small N.

that has entered into our study before it is too late to actually dig deeper. After Geertz was in the field for a few months, he might have asked: If people from any of the other four family factions or economic classes than the one I stay with were called to the witness stand, would they testify that my account had been biased by the members of the one family faction and economic strata I had come to know? If investigators commit to asking questions of this kind before their study is over, they would be more likely to get to know these other groups *before* leaving the field.

A second benefit derives from inconvenience sampling when a fieldworker simply can't get to know the inconvenience sample. Here he or she can at least gain a better understanding of the bias or particular lack of nuance that has entered into the study. If we make it regular practice to force ourselves to specify why a particular sample is so difficult to access, we can clarify the biases in our study in as systematic and transparent a way as possible (Goldthorpe 2007). In this respect, actually reaching the inconvenience sample before the end of the study may be the gold standard, but where it is not practically feasible, we can ask how the inconvenience sample would have helped readers imagine aspects of the situation that are otherwise not transparent.

4. CONCLUSION

In 1954, Darryl Huff published *How to Lie with Statistics,* the best selling statistics book in the history of the field (Steele 2005), from which the title of this paper is derived. Huff was an undergraduate sociology and journalism major at the University of Iowa and received his master's in journalism there before going on to become the editor of *Better Homes and Gardens* (Steele 2005). Upon his early retirement, he moved to California with his wife and began a second career as the freelance writer of sixteen "how to" books, including *Twenty Careers of Tomorrow* and *How to Work with Concrete and Masonry* (Steele 2005). In 1986, Gary King published "How Not to Lie with Statistics: Common Mistakes in Quantitative Political Science," a widely cited and foundational article in the development of methodological thinking in that discipline. Like Huff's book and King's paper, this article focuses on a little discussed problem with regard to how ethnographic data can be used and misused—by failing to acknoweldge inconvenient phenomena.

Huff believed that numbers could be manipulated to support any argument, and he was concerned with the kinds of manipulations that unsuspecting readers would not know to look for. In a more recent effort to sort through matters of this kind, Howard Becker has argued that misrepresentation becomes a moral wrong in the eyes of readers when they come to realize that an "effect was achieved by means that [they] . . . weren't fully aware of and therefore can't be critical about" (2008:133). We can best improve our methods by engaging in practices that reassure our readers that they can trust they know *how* they have been convinced. It is a lack of transparency that results in a sense that the wool is being pulled over a reader's eyes. Our goal should be to institutionalize methods that make it normative for us to be as up front as possible about how we have achieved our effects.

There are situations in which there is no way to avoid telling stories with ethnography that won't be lies to some people. As Becker himself pointed out in his classic essay "Whose Side Are We On?" the act of trying to get perspectives from people at various levels of a system can end up being a project of infinite regress—"there is no end to it" (Becker 2008: p. 247). And yet too many investigators are tempted to take the wrong lesson from that paper: that we should accept *from the outset* the limits of what we can know from having talked to certain people and not others; or that we need not acknowledge those people outside the sampled population whose subjectivities can be written off as part of the "infinite regress." This all becomes a rationalization to stop digging and never explain to the reader what other subjectivities or phenomena also existed in the field, and their implications for the findings that are presented. Here I am reminded of Robert Solow's comment, quoted by Geertz (1973) in another context: that this "is like saying that as a perfectly aseptic environment is impossible, one might as well conduct surgery in a sewer" (p. 30).

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