

NOTES ON ANTHROPOLOGICAL METHOD, mainly in the key of e

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*There is only one method in social anthropology,
the comparative method – and that is impossible.*
Sir Edward Evans-Pritchard¹

PROLEGOMENON

It is exceptionally difficult to address questions of method for anthropology at large unless one reduces the field to a caricature of itself. To wit, the discipline – if anything so obviously polythetic, anything so unruly in its expansiveness, can be called a discipline at all – extends from the “hard” biological sciences and archaeological forensics through “comparative sociology” in the British tradition, technical linguistics of the various American schools, and ethno-everything in the formalist mode, to the “soft” hermeneutics of interpretive and historical ethnography. And this barely begins to exhaust the field. Even if we take Clifford Geertz’s (1973) fashionable saw for the seventies, that anthropologists “do ethnography,” it is difficult to pin down what this might mean in general, replicable terms; after all, Geertz’s often imitated, widely-cited methodological approach – which was fleshed out only illustratively, never rigorously – was itself directed toward ideographic particularity and away from anything that might present itself as nomothetic, general, theoreticist. Since then, moreover, the discipline has become much more diverse, much less coherent; having departed the village, the reservation, the island, both its ends and its means are more contested than ever before.² Indeed, if, for David Lodge (1999:52), writing a novel is “like playing chess in three dimensions,” doing anthropology nowadays is like playing it in four; the fourth being the terrain of the virtual, the electronic commons that ties even fairly remote social worlds into an expansive, if often exclusionary, global ecumene. In short, what follows is a *reductio*, if not *ad absurdum*, then certainly to the point of being a very provisional, very partial, very primitive sort of statement about contemporary ethnographic practice. It omits, for example, the question of historical method, critical though it clearly is to what we do much of the time; also the methodological challenges faced by anthropology as the scope and scale of its objects metamorphose.³ It also refuses the idea that the qualitative social sciences can or should measure their methodological worth, or be evaluated, against the quantitative social sciences, whose techniques, their authority notwithstanding, are no less open to radical doubt (Comaroff and Comaroff 1992). The challenge for both lies in commen-

¹ This much cited aphorism was never actually published; see Needham (1975:365).

² Not only has its hallowed concept of “the field” been subjected to deconstruction (see e.g. Gupta and Ferguson 1997), but the dilemmas attendant on doing ethnography “in the modern world system” have been the object of considerable debate since the 1980s (see e.g. Marcus 1986; Bamford and Robbins 1997; fn. 3 below).

³ Jean Comaroff and I have addressed the question of method in historical anthropology elsewhere (1992); our take on methodical approaches appropriate to the ever more expansive anthropologies of the postcolonial, global age are discussed in another recent essay (2003).

uration: how do we arrive *at once* at the necessary conceptual terms *and* at techniques of producing knowledge commensurate with the problems that we seek to address? This much is self-evident. It is here that the difficulties begin.

THREE OR SO QUESTIONS

On the matter of rigor, and its mortifications

Socio-cultural anthropology is often accused, to its mortification, of lacking all methodological rigor. Or worse yet, of having no discernable method at all – beyond its reliance on the self-assertively authoritative eye (or, more capaciously, the senses) of the ethnographer. Recall, in this respect, Levi-Strauss (1976:35): ethnography is a means of producing knowledge in which “[t]he observer apprehends himself as his own instrument of observation” (cf. also Foucault 1975). As Jean Comaroff (n.d.) has pointed out, its frank faith in the role of subjective experience in empirical investigation, and in the testimony of the lone investigator, has *always* been controversial: the anthropological mode of induction has repeatedly been accused of ineluctable ethnocentrism, of fetishizing difference, of celebrating imaginative idiosyncrasy, and of a cavalier disregard for replicability, refutability, or reliable accountability. To be sure, its most invoked technique, “participant observation,” is often dismissed, at worst, as an oxymoron, at best, as a disingenuous synonym for “hanging out” with “the natives,” whomever they may happen to be. Or, in slightly more dignified terms, engaging them in a “long conversation” (Bloch 1977)

Bracketing for now all the epistemic and ethical problems said to inhere in the methodological practices of anthropology – they are little different from those attendant upon the techniques of less reflexive disciplines and a lot less severe than those intrinsic to, say, modern journalism (see Steinberg 2002) – ethnography, done well, can and often does evince a great deal of rigor. And a high measure of refutability. For an ethnographic account to pass methodological muster, insisted Max Gluckman – founder of the Manchester School, which shaped post-WWII anthropological practice and developed some of its most innovative techniques – it ought to present primary data of sufficient quantity and depth to allow them to be reinterpreted from a theoretical perspective different from the one that produced them. This was the tacit standard to which much of British anthropology, famously empiricist in its orientations, held its practitioners until very recently.

I shall say more, in a moment, of the rigors and replicabilities of anthropological methods. First, though, a qualification: because their objects of research are diverse and protean, because the truths they pursue tend to be less nomothetic than those sought by most other species of social science, and because they are conditioned by the vernacular social realities and contingent human preoccupations with which they are confronted, it is always hard to lay out a set of ethnographic techniques in the abstract. Not, at least, without specifying the empirical problem to which they are to be addressed and the theoretical concerns that motivate, and are motivated by, that problem; in this sense, anthropology always rests on a dialectic between the deductive and the inductive, between the concept and the concrete, between its objectives and its subjects, whose

intentions and inventions frequently set its agendas. The failure to grasp this may account in part for the autonomic dismissal of ethnography as unrigorous, unreplicable, unfalsifiable, and all the other (non-)u words with which it is regularly damned. More pragmatically, it means that its technologies have to be taken as a repertoire, an imaginative tool kit, whose various elements may be deployed, combined, and refashioned in an almost infinite variety of ways. It goes without saying that it is the ethnographer's obligation to explicate how and why s/he has deployed those elements in the way s/he has; *vide*, for example, our effort to specify how we went about designing a theoretically-principled methodology with which to arrive at an account of, and to account for, the occult economies of postcolonial South Africa – economies made manifest in rampant witch-killing, zombie conjuring, AIDS related rape, financial frauds, and other complicatedly interconnected phenomena (Comaroff and Comaroff 2003).

The methodological toolkit available to the ethnographer – in addition, of course, to more conventional techniques, such as interviews, surveys, and focus groups of sundry sorts – begins with a number of “traditional” instruments whose uses are well-established: the extended case method and the social drama, for instance, both of which have well-elaborated, replicable procedures, and suggest ways of extrapolating general processes from singular events (see e.g. Epstein 1967; and, as applied in legal anthropology, Comaroff and Roberts 1981; Gulliver 1979); the recording of “natural discourse” (in court cases, public meetings, informal conversations, ritual cycles, etc), and of “native” exegeses on that discourse – all of which may be digitized for deposit and review – for purposes of both formal and content analyses; symbolic analyses of the kind pioneered by Victor Turner (e.g. 1967), which laid out a carefully ordered series of observational and interpretive procedures; the documentation at periodic intervals of “routine” activities, parsed by salient social categories (gender, generation, class, status, etc.); ethno-mapping and the elicitation of other vernacular modes of representation, which, by virtue of being available for transcription and dissemination, may be subjected to scrutiny and re-analysis; the collection and aggregation of life-histories and the developmental cycles of collectivities, ranging from families to large-scale associations (see e.g. Goody 1958); network and transactional analysis (e.g. Mitchell 1969); and so on and on.

For anthropologists of my own generation, these remain some of the essential components of a solid spectrum of rigorous techniques that produce the kind of data out of which compelling accounts may be written: accounts that may be invoked in support or rejection of broad theoretical positions, of more specific explanations for social and cultural phenomena, of claims about the human predicament, past and present. Of late, they have been augmented by more literary and hermeneutic modes of descriptive-analysis, and by rather more impressionistic, aesthetic, reflexive approaches to the act of observation itself; also by attention to domains of human existence – from mass media and finance capital to the workings of the electronic commons and the dispersed practices of governmentality, to mention a few at random – hitherto not subjected to ethnographic methods. These can and have yielded exceptionally detailed, rigorous accounts of an extraordinary range of phenomena; but their rigor is not easily specified in programmatic terms. Indeed, they rely, for their persuasiveness, on their plausibility – itself

often judged by virtue of their density, their imaginative scope, their capacity to bring a wide range of recognizable “facts” into a single descriptive-analytic purview – and their aesthetic composition.

Which leaves the question as posed: what are the standards of rigor in anthropology? In the abstract, there are none, none that are universally shared. Given that all method is mediated by theory and *vice versa*, our standards are, in the final analysis, determined contextually. If anything more general is to be said, it is that we tend to assess our techniques of knowledge production by the degree to which they yield data about which a cogent argument can be made in terms of prevailing conceptions of plausibility, persuasiveness, parsimony, density. Or, as Gluckman put it, the extent to which they yield accounts about which we may reasonably disagree, accounts that may reasonably be subjected to reinterpretation.

Communicating our Differences, Differentiating our Communications

The biggest challenge for anthropology is *not* to find more rigorous methods. As I have said, despite stereotypic caricatures to the contrary, ethnography practiced well – a qualification that applies to *all* methodology – is quite rigorous enough. Not only can it stand critical scrutiny as a mode of producing knowledge; it has long yielded truths of enormous insight and value, often to the discomfort of conventional Western wisdom. The challenge, in my own view, is to convince its practitioners that they owe it to themselves, and to their colleagues in other disciplines, to *explicate* their procedures fully.⁴ There is, if we are to be honest, a degree of high-handedness, even assertive contempt, among anthropologists, for speaking about our method: ethnography, to put it crudely, is what makes us different – or, rather, used to – and the rest of the social sciences can take it or leave it. Of course, “doing fieldwork, an extended spell of participant observation, remains a necessary rite of entry into our collegium (Ortner 1997:61); like all rites de passage, its mystique lies in *not* disclosing too much of its secret, even when the secret is that there is not much of a secret to it at all. Understandable though this might be as an exotic cultural practice, there is something self-defeating to it. After all, on purely epistemic grounds, making an argument of commensuration for the relationship between an ethnographic account and the mode of producing it is an essential part of making a persuasive theoretical claim, or offering a persuasive explanation, for the phenomena under anthropological scrutiny. To be self-evident about all this: If an anthropologist wants, for example, to assay the view that a new religious movement has arisen in response to changes in local material conditions, her or his claim can only ever be as strong as is his or her demonstration that the new religious movement actually exists, that the alleged material transformations have actually occurred, and that the two

⁴ I do *not* have in mind here the kind of reflexivity called for in the 1980s, which had it that the more an anthropologist revealed about her- or himself – as if such revelations are ever unmediated or unedited – the better placed the reader would be to assess her or his accounts of others. For the most part, this call became a prescription for a great deal of numbingly boring, self-serving prose and amateur autobiography; it is a chapter in the recent history of the discipline better left closed for now.

things share a set of spatial, temporal, and experiential coordinates – all of which demands that we be shown, explicitly, the means by which these things have been established. Without this act of commensuration, no theory or explanation, however exquisite, imaginative, ingenious, will stand scrutiny. For all the fact that this is obvious, it seems rarely to be honored in the discipline. If we are to communicate about our practices, and convince other disciplines of their worth and their rigor, we have to begin with three principles: explication, explication, explication.

All the rest follows. The extent to which there are or will be common criteria for designing and evaluating research between anthropology and the other social sciences depends on how much the latter incorporate ethnography into their repertoires. And are prepared, reciprocally, to make the effort to understand what it actually *is*, what the e-word actually stands for. To the degree that they do – and many of them are, increasingly, if sometimes only at their margins – engaging in common substantive discussions about technique is more than appropriate, largely because anthropology *does* have a large and well-honed toolkit to offer.

Topics of Caprice and Corn: Promising Horizons for Qualitative Research

There are very few topics which *cannot* be illuminated by qualitative research, again well done. It depends how they are defined and formulated, what questions are asked of them, what it is about them that we wish to explain. Ethnography, these days, is being applied to an extraordinary range of things, from the caprice of futures markets to the science of genetically modified corn. And a great deal in between. Indeed, one way to answer this question as posed is to elicit an inventory of doctoral dissertation titles from any major anthropology department in the country – or, better yet, several of them. One thing becomes clear: in the rapid expansion of subject matter deemed suitable for ethnographic treatment, ethnography itself is undergoing an imaginative explosion, its horizons ever widened by its modes of knowledge production.

A closing thought. Perhaps the time has come to address the conundrum at the core of this workshop, well...methodically. And methodologically. How? By doing an ethnography of ethnography. If nothing else, it might prove that Evans-Pritchard was wrong all along. Anthropology has, from the first, had a plurality of techniques. What is more, comparatively speaking, its methodological tools have grown over the years. Indeed, far from being impossible, they open up all sorts of promising possibilities for the future.

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