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Ethnography as theory*

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Ethnography is never mere description, rather it is a theory of describing that has always been controversial as to the what and how thus inspiring a dynamic intellectual process. The process has been methodologically eclectic and innovative, governed by both consensual and outdated rules. Throughout more than hundred years of Anglo-American ethnography, observation has been combined with a wide variety of theoretical outlooks from structured-functionalist to critical writings.

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Ethnography has commonly been summarized as description, albeit description in context, but not exactly theory. Yet, theory is defined as the analysis of a set of facts in their relation to one another, or the general or abstract principles of any body of facts, which to my mind makes ethnography most definitely a theoretical endeavor, one that has had and still has worldly significance, as description and explanation. Thus, the ethnography itself as well as its explanatory use is a theoretical endeavor.

Historically, doing ethnography involved living and talking with people, "being there" and "participant observing," an attempt to understand how the people studied see and account for their world, which includes the anthropologist. Ethnography has also been commonly connected to the idea of holism; cultures are interconnected, not fragmented; they are whole systems, and therefore any description of them, to be complete, must tackle the whole. The reality of doing and writing ethnography has always been more complicated than simply assuming and even arguing the interrelatedness of cultural elements. Are we recording what people say they do, how we see them living, or how they want the anthropologist observer to know them? Ethnography, whatever it is, has never been *mere* description. It is also theoretical in its mode of description. Indeed, *ethnography is a theory of description.* The whole of a culture cannot be assumed, and there has

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never been a total consensus on how whole is whole enough, especially when dealing with questions of boundaries. Nor has there been agreement on what makes ethnographic reporting "factual," a problem in mainstream scientific work as well. The absence of agreement or total consensus has been the strength of anthropology's ethnography, inspiring a dynamic process of "doing ethnography" that resonates with changing worlds in and out of academia.

Thus, from the beginnings of anthropology there was controversy, a simultaneous romanticizing of "being there" among isolated, exotic people, and doubts concerning limitations of a methodology that at times has sought to answer all the essential questions regarding the human condition. At the same time discussions of the many possibilities of ethnography have been cause for discomfort, or at least uneasiness about the stability of our field endeavors and the continuous need for revitalization. With James Mooney (1896), we had the nineteenth-century beginnings of a critically engaged ethnography and ethnography as critique of Western thought. With W. H. R. Rivers (1906) and to a lesser extent Bronislaw Malinowski ([1922] 1984), the ethnographer proceeded as if conducting a laboratory-bounded natural-science experiment. With Gregory Bateson ([1936] 1958), and to some extent Sir Edmund Leach ([1954] 1965), the ethnographer proceeded much more like an ecologist. The ecological model of ethnography, whatever that is, is *not* the laboratory model, nor a linear-cause-to-effect-hypothesis-proving model sometimes associated with the theoretical work of A. R. Radcliffe-Brown.

These forerunners were not governed by any one doctrine and did not adhere to a single model, yet they were all doing ethnography by most people's standards: they went, they observed, they stayed, they returned home and wrote ethnography. They were methodologically eclectic and included quantitative techniques. They were not afraid to innovate on and create techniques that they found to be necessary for pushing forward their work—which was often described as urgent anthropology, salvaging the cultures of non-Western peoples before they were erased by the Euro-American colonizing adventures.

Although ethnographic standards above the radar *were* debated, there were unstated rules or consensuses about how to do ethnography that were not debated openly, although there were some brief hints of debate in the 1960s, especially with the publication of *Reinventing anthropology* (Hymes 1969). As a graduate student at Harvard University in the 1950s, I understood that an unstated consensus had already been long established concerning what ethnographic work should be. Although Bateson's highly original *Naven* (1936), reissued in 1958 (to the distress of the more scientistic Harvard faculty) with an additional methodological epilogue, and Edmund Leach's solid *Political systems of Highland Burma* (1954), which had a deliberate focus on power and its uses, the unstated rules were clear: we were to work in non-Western societies, write about them as if they were bounded entities, ignore power politics which included colonial and imperial presence, ignore similarities between "us and them," deplore 19th century unilineal evolutionism and exceptionalism but still practice it. Perhaps the only sign of the philosophically inspired questioning, to develop further in subsequent decades, was the fact that Wittgenstein, Cassirer, and Langer (not Marxian philosophers) were being read widely by graduate students, whether or not they were interested in linguistics.

At the outset, in Anglo-American anthropology, participant observation in a non-Western society was justified as a practice in defamiliarization. According to this scenario, anthropologists move to a place removed from their own culture with the idea that the newness and unfamiliarity they confront will allow them to discover or figure out something about the people they visit that would be a contribution to anthropology as "the science of man." Participant observation was the key operational phrase. Places such as an island in the Pacific, a tribal village in Africa, or a pueblo in the American Southwest were common research sites. The goal, for the anthropologist, was to figure out the pieces of the social system and discover how they fit together in a *bounded* sense that was sometimes modeled after Radcliffe-Brown's organic metaphor of society. Multisided interpretations at times served to widen the scope of intellectual possibilities, but such instances were rare. More commonly, interpretations were closed. An example is Clifford Geertz's (1973) essay on the cock fight in Indonesia, which found no ethnographic space for the half-million people killed by Indonesian government forces at the same place and time. The massacre was included only as a footnote, not an unfamiliar example of elision in the history of anthropology. As I noted in my 1999 review of Geertz's later work *After the fact: Two countries, four decades, one anthropologist* (1995), Geertz rejected positivism, borrowed from the philosophers named above, and was unable to deal with political and economic power. For him the 1966 massacres in Pare were "hardly . . . a memory at all" (1995:10), indeed hardly knowledge that is humanistic, reflexive or situated—suitable for a literary ethnography.

Throughout more than a hundred years of Anglo-American ethnography, participant observation has always been combined with theory, whether functionalist, structural functionalist, interpretive, Marxist, progressivist, evolutionary, symbolic, feminist, or just plain critical. Given shifting theoretical and methodological frameworks, anthropology and ethnography, as a discipline and research practice, have remained open to innovation. In ethnography itself the theory was in the writing, and throughout the 20th century, anthropological theory has not proceeded in a linear fashion (although historians of anthropology often depict it as such)—from functional, to structural functional, to structuralism, to interpretive, reflexive, critical, and so forth. Today, all of those theories are in use to some extent or another. I do not mean to indicate that there are no paradigm shifts over time, that is, a shift in an implicit body of intertwined theoretical and methodological presuppositions, but that anthropology has always appeared to be theoretically heterodox. While there appear to be competing schools, anthropologists who take one side or another in theoretical debates, what defines anthropology and ethnography as such are not these divisions but rather what is shared, held in common, which includes the unstated.

In James Mooney's 1896 multi-reservation project, fieldwork was carried out in the eastern and southwestern parts of the United States with Cherokees, Kiowas, and Cheyenne, focusing on religion and the ceremonial use of peyote. In *The ghost-dance religion and the Sioux outbreak of 1890*, the book for which he is most remembered, Mooney was concerned with what today could be termed control and resistance, with social movements, with the political use of religion, and with the civil and human rights of Indians. He compared the nativistic movements with the deeds of white Europeans as a way of provoking a sympathetic understanding of Indian deprivations in land and livelihood and the tragic implications of wrongs done to them as reason to protect them further from the demands of White society. However, perhaps because he explicitly wrote Whites into his ethnography of Indian peoples (and perhaps because his book was well received by the reading public), he invited the ire of missionaries, the U.S. government, educators, and anthropologists who sought to turn the people they called savages toward so-called progress and civilization with some using humanitarian rationales, i.e. the only way to save Native Americans. By writing white people into his ethnography, equating them to Native Americans, Mooney, who was the son of Irish immigrants and a self-trained anthropologist, violated what was already in the late nineteenth century an unwritten rule: ethnography is about the other, *not* the other intertwined with their conquerors, not about us and them. He was dismissed as an amateur by academics like Franz Boas, or a lover of Indians by government officials and the Indian Bureau, and was subsequently barred from fieldwork on American reservations (Moses 1984: 222–35).

At the time of Mooney's death, prevailing views of professionalization narrowed the boundaries of what constituted a real as versus amateur anthropologist, a kind of "specialized competence" was coming to the fore. On many fronts Mooney was ahead of his time. He included the colonizers as well as the colonized and on equal footing and by doing so already defined an ethics of research decades ahead of the American Anthropological Association's Code of Ethics.

Some decades later, in 1922, Malinowski, in his work with the Pacific Trobriand Islanders, underscored the scientificity of ethnography by outlining three methodological tenets of research: statistical documentation, attention to the imponderabilia of actual life and observed behavior, and the recording of spoken statements indicating the mentality of native thought. Before becoming an anthropologist, Malinowski had studied philosophy, mathematics, and physics in Poland. His methodologies were meant to allow the ethnographer to "grasp the native's point of view, his relation to life, to realize his vision of the world" (Malinowski [1922] 1984: 25). Yet if we read reviews of *Coral gardens and their magic* (1935) by Malinowski's contemporaries, we glimpse what he was up against: "The use of magic, which is analogous to the delusions of grandeur and the fear constructs of the individual neurotic, may be the invariable result of man's limited ability to control his environment. But to extol it thus as the 'very foundation of culture,' as Malinowski does, is not justified on scientific grounds" (Stern 1936: 1018).

Malinowski broke ground with what today would be called multi-sited fieldwork, and scientific rigor. He described the Kula ring of reciprocal trade and friendship that connected a series of island societies. The Kula exchange was fundamental to social relationships because partners are connected for life through mutual obligation and support. In Malinowski's description, Trobriand life appears to the readers as reasonable. Writing culture as reasonable was a conscious strategy for Malinowski and his editors, specifically because he was refuting European notions of primitives who only act in terms of self-interest. He was not a comparativist; he let his ethnography speak about Us, more or less *implicit* observations, whether he wrote about law and order, magic, science, and religion, or sexuality.

A decade later, New Zealander Reo Fortune published *Sorcerers of Dobu* (1932) in which he recounts the irritation of Australian colonial administrators because he as the ethnographer was relativizing sorcery as a form of social control that makes conformity strategically wise in societies without well-developed legal mechanisms. Sorcery, for these Melanesian Dobuans, had a function and Fortune gave it standing. He violated the rule, the normative frame imposed by colonial administrators, that degraded sorcery and those who would practice it to the level of barbarism. Although the normative frame of the ethnography did not fit colonial and imperialist objectives or even native preferences, Fortune used the threat of government or mission to secure information about sorcery. Colonial control enabled him to carry out his work, but he opposed the social turmoil brought on by colonial administration. Anthropologists did not always share the goal of “civilizing” and developing the natives. After 1930 independent anthropologists were banned from fieldwork in Papua, and Fortune was not appointed to government posts following World War II. But by then he was at Cambridge University.

In the same and following decades Max Gluckman, E. Epstein, and Peter Worsley were accused of being left to communist and their access to field sites was denied outright, as with James Mooney in the previous century. Gluckman’s 1936–38 fieldwork was conducted in South Africa. His 1940–42 publication “Analysis of a social situation in modern Zuzuland” was an assault on the concept of the bounded tribe. It was a cross-section study demonstrating the impossibility of conceptual segregation. For him, South Africa was a single society “composed of heterogeneous culture groups . . . overlapping, interpenetrating, and cross-cutting” (MacMillan 1995: 64). He was criticized for living like the natives, eating their food, “bringing himself down to their level,” accused of being pro-Russian and a communist, and then banned from further fieldwork in the area by the Secretary for Native Affairs. Although many anthropologists cooperated with colonial officials, anthropology as a discipline has consistently disturbed received knowledge and challenged many imposed norms of colonial administration. The same was true with E. E. Evans-Pritchard’s ethnography, *Witchcraft, oracles, and magic among the Azande* (1937), in which he describes the role that witchcraft plays in the life of the Azande, something that appeared irrational to British colonial administrators, as we saw in the Dobuan sorcery example. Many years later (1996), I edited *Naked science: Anthropological inquiry into boundaries, power, and knowledge* and encountered publishing problems over arguments I made with my associates over the same issues that Evans-Pritchard made about rationalities. Primitive mentality as a concept is alive and well, although misplaced in application.

In spite of the exceptions, anthropologists’ complicity with Euro-American colonialism can be perceived by the use of colonialist terms; anthropologists historically called those being studied “primitives”; the ethnographer conceived of his or her world as civilized. This much they shared with the colonial official. Evolutionary and comparative approaches, where so-called primitive societies were believed to be located at an earlier stage of development through which modern societies had already passed, encouraged researchers to use their own categories unselfconsciously—categories such as law, religion, politics—to describe others as if such were “natural” categories. But then there were those who continued to upset the apple cart.

Gregory Bateson, with his book *Naven* ([1936] 1958), violated many of the accepted rules of doing and writing ethnography. His work on the Iatmul of the Sepik River region of New Guinea was not holistic, at least not by any traditional definition of the term. In his ethnography he described only one ceremony, not the whole of Iatmul society. At the start he writes, “I shall first present the ceremonial behavior, torn from its context so that it appears bizarre and nonsensical; and I shall describe the various aspects of its cultural setting and indicate how the ceremonial can be related to the various aspects of the culture" (1958: 3). Each chapter of the book is an experiment in explaining the ceremony using different lenses. In the functionalist chapter, he attempted to show that such an explanation implied that change would be blocked, innovation would be stifled, and conflict would be seen as pathological. Bateson's work is a criticism of standard classic ethnography, written long before Marcus and Fischer's 1986 work on cultural critique. *Naven* was a study of the nature of explanation, an examination of "the scientists'" way of putting the jigsaw puzzle together (1958: 280). Theoretical concepts, he argues, are "really descriptions of processes of knowing adopted by scientists . . . and no more than that" (ibid: 281).

In my reading of the history of anthropology, the two figures who are central to critique of ethnocentrism were Gregory Bateson and Edmund Leach. Of the two, Leach was probably the less abstract and the most radically explicit.

For Leach, as was true with Mooney earlier, anthropology was just as much about *us* as about *them* (Leach [1987] 1989: 13).[1]

> There is no modern as opposed to primitive society, no static versus dynamic. We are all rational. Others have to solve problems of existence, build boats, cure their sicknesses, just as we do. Social systems are open, not bounded and never to be found in equilibrium. The other is interesting because what we see in them is directly relevant to understanding ourselves.

He spoke of a long tradition of attributing ignorance to native people, attributing to them a kind of childishness, superstition, an incapacity for rational thought. He believed, as Malinowski wrote in his *Magic, science and religion* (1948), that Western scholars should assume that people are as rational and credible in so-called primitive societies as they are in their own, and in that way discover the rational explanations for what might seem to be strange behavior. For Leach, double standards were not defensible. You should not apply one standard to primitive man and quite a different one to a so-called civilized man. Nor is it tenable, he argued, to use racist ideologies that ascribe intellectual inferiority to natives. He disapproved of the preservationist ethic of romantics, but neither did he approve of applied or development anthropology, which he thought to be neo-colonialist.

Leach understood the anthropologist and the people they studied to be co-temporal (Tambiah 2002: 259–63, 429–55). One is no more contemporary than the other. He developed the idea of positioned or situated knowledge. The anthropologist and their informants are differentially situated and thereby cannot be, in the strict sense, objective. He also had little sympathy for the narcissism of contemporary critics and their style of writing critiques—all aspects that in my view highly recommend Leach's thought. But the way out of the unstated consensuses is not as easy as Leach believed or makes it seem. Anthropologists continue to be blind to assumptions that he challenged. This is because, as Eric Wolf stated in 1969, anthropology is not autonomous; it is a reflection of the society of which it is a part. Ethnographers are caught in their culture much as the people they study. Thus, we are all complicit, although to different degrees. Wolf's observation relates

[1] See also Tambiah (2002), chapters 10 and 17.

directly to the matter of how we treat our ethnographic innovators, both past and present. Anglo-American culture is not culturally motivated to analyze the power elite or to educate ourselves in the realities of power—an observation that motivated me to write "Up the Anthropologist" (Nader 1969) in which I analyzed the obstacles and objections to studying up, down, and sideways.

If the classic ethnographies were all rooted in place and ethnic communities—the Todas, the Andaman Islanders, the Trobrianders, the Azande, the Dobu, etc.—and if it is true that what ethnographers do is in part dependent on their culture of origin, then what kind of ethnographies have begun to appear in a context of rapid globalization, the rise of new imperialisms, and a desire to reinvent anthropology? After all, changing industrial and technological means have transformed the world and us with it. June Nash ([1979] 1993) studied Bolivian Indians working in mines that are embedded in a world system of which her country of origin is also a part. Hugh Gusterson (1996) studied nuclear weapons workers in a U.S. National Laboratory, a place that produced weapons of mass destruction that potentially could affect all the peoples of the planet. As a critical medical anthropologist, Margaret Lock (1993) studied the differential construction of menopause in Japan and North America indicating the role of pharmaceuticals in defining "change of life." And Ted Swedenburg (1995) studied *Memories of revolt* in Palestine, a place that today reverberates politically around the world, though not necessarily in ethnography. Ethnographic localities are now embedded in larger political and economic circuits, connected increasingly by global exchange. Even community-based ethnographies, such as my own, *Harmony ideology: law and justice in a mountain Zapotec village* (1990), have worldwide significance because dispute resolution tools established through Spanish colonial control that I identified at the community level are also to be found as techniques of pacification operating in the arenas of international law, trade agreements, and the like.

Although the above-mentioned ethnographers have their adherents, opposition to innovative, eclectic, and open-ended loose and strict works have been dismissed by judgments that use words like "journalistic," "political," "non-analytic," or "unscientific," outrage that the ethnographers, by placing themselves, their societies, and those that they study all on equal footing, have crossed a line, violated an unspoken consensus. It is still easy to denigrate or to mark the boundaries of acceptable ethnography, even though it has been clear for a good long time now that science is not and cannot be politically neutral. In past times, and in the present, people still argue for scientific objectivity, a concept that often does little else than conceal the scientist's highly subjective position.

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L'ethnographie en tant que théorie

Résumé : L'ethnographie n'est pas uniquement description, mais bien plutôt une théorie de la description qui a toujours été l'objet de controverses sur le quoi et le comment, inspirant ainsi un processus intellectuel dynamique. Ce processus fut méthodologiquement éclectique et innovant, de même que guidé par des règles à la fois consensuelles et dépassées. Pendant plus d'un siècle d'ethnographie anglo-américaine, l'observation a été associée à une grande variété de perspectives théoriques, allant du structuralo-fonctionnalisme jusqu'aux écrits critiques.

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