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THE ANTHROPOLOGIST'S FIELDWORK AS LIVED WORLD
Margaret Mead and Reo Fortune among the Mountain Arapesh*

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THE CONCEPT OF LIVED WORLD AS A METHODOLOGICAL RESOURCE FOR THE HISTORY OF ANTHROPOLOGY

In her work on the Walbiri people of Australia, the island society of Gawa in Papua New Guinea, and the transformation of New York City in the nineteenth century, Nancy Munn presents us with an exemplary model for attending to the micro-particular forms in which social action and interaction take place, and drawing from them theoretical insights of great philosophical depth and wide applicability to other times, places, and peoples. A key methodological component of this model is the process of piecing together the specifics of people's concrete activities into series and contrastive sets in order to grasp not only what they are doing, but also, more revealingly, what their world must be like in order for those actions to achieve the purposes set for them. The profound point at the heart of this method is one Munn has impressed upon her students and developed in a number of wide-ranging studies, both published and unpublished.[1] Her point is that people's activities – be they seeking the cause of an illness, mounting a mortuary exchange, or fashioning a canoe – presuppose a particular sort of world, with particular frameworks of cause and effect, relations of space and time, value, meaning, and so on, even as the activities themselves, through their form, play a role in actually producing these frameworks they take place within. So people's activities have not just

* An earlier version of this paper was presented by Ira Bashkow at a festschrift panel in honour of Nancy Munn at the Annual Meeting of the American Anthropological Association in San Jose, California, in November 2006. Françoise Dussart deserves recognition for organizing this session. The paper builds on our own field experiences in the Mountain Arapesh region, as well as on archival research in the Margaret Mead Papers at the Library of Congress. Fieldwork on Arapesh languages was conducted by Dobrin from December 1997 to March 1999; she was joined by Bashkow for nine of these months. Most of our time in the field was spent in Wautogik village on the Sepik coast in East Sepik Province, Papua New Guinea. Short trips were also made throughout the region to gather comparative data. Dobrin's fieldwork was made possible by an NSF Dissertation Improvement Grant, a Fulbright-Hays Training Grant for Doctoral Dissertation Research Abroad, a Wenner-Gren Foundation for Anthropological Research Predoctoral Grant, and the kindness and support of the people of Wautogik village. Our work in the Margaret Mead Papers was carried out with support from the Dean of the College of Arts and Sciences and the Vice President for Research and Graduate Studies at the University of Virginia. We are grateful to friends and colleagues for inspiration and comments on various facets of this project, including Ellen Contini-Morava, Fred Damon, Patricia Francis, David Golumbia, Richard Handler, Roger Lohmann, Susan McKinnon, Peter Metcalf, Andrew Moutu, Bernard Narokobi, Jim Roscoe, Rupert Stasch, George Stocking, Gerald Sullivan, and Roy Wagner.

1 Munn (1977, 1986, 1990, 1996, 2004)

their intended effects but also world-making side-effects: they wire reality itself, creating the phenomenal lived world as a particular sort of reality in-built with certain possibilities for action, interaction, meaningfulness, feeling, and so on. Generally, people take this reality for granted as previously given. But in fact the world that seems given to them is in part constructed by their own activities.

In this paper, we apply Munn's idea of the culturally-made lived world to the history of anthropology, because of course it is not only the natives of anthropological study who build the worlds they experience; it is something done by everyone, including anthropologists themselves. Normally so many aspects of an ethnographic research situation are beyond the fieldworker's control that it can appear as if 'the field' were a cultural given. But in many ways the situations that anthropologists experience in the field are ones that they themselves have played a role in shaping. This is manifestly true with respect to such practical and political factors as linguistic skill, the kind of lifestyle one maintains, and one's social affiliations within a community. But it is also true of more subtle psychological factors like the emotional qualities of the fieldworker's social relationships and the ways these reflect back on his or her own identity (see Devereux 1967). It is not only the fieldworker's intellect that is engaged in the course of fieldwork and the collection of ethnographic material. It is the entire person – heart, body, and mind – that shapes, apperceives, and interprets the situations that form the experiential basis of his or her knowledge of the culture.

Our attention was drawn to this process by a remarkable conflict of ethnographic interpretation in the history of anthropology. We did fieldwork in the late 1990s in the Mountain Arapesh region of Papua New Guinea, a region which had been previously studied in the 1930s by Margaret Mead and Reo Fortune. Although Mead and Fortune did their fieldwork as a married couple working together as partners (as did we), the portrayals of the culture they subsequently offered differ from one another in striking ways. In her best-selling book "Sex and temperament in three primitive societies" (1935), Mead famously concluded that Mountain Arapesh culture embodies a nurturing, maternal, and peaceful ideal for both sexes. Fortune, on the other hand, objected to this description, and in obscure publications, manuscript fragments, and a great mass of letters he countered it with a view of Arapesh culture that emphasized the brutal politics of interlocality rivalry, adultery capers, and warfare (Fortune 1939, Dobrin and Bashkow 2006). In effect, then, we found ourselves following in the footsteps of two trained observers who were in the same place at the same time, yet who interpreted the culture in opposing ways. How could this have happened? In inquiring into this problem, we have encountered a great deal of scholarship that approaches the matter in terms of anthropologists' intellectual predispositions and the personal and historical influences that directed their interpretations. This conventional 'intellectual history' approach is of course fully sophisticated in emphasizing that anthropologists perceive and interpret their field experiences through any number of filtering constructs, such as prior assumptions, cultural blind spots, personal inclinations, and theoretical models and

aims. But in the rest of this paper, we suggest that even though this approach is highly fruitful and absolutely necessary, it is still only part of the picture. We will be making a programmatic argument about the historiography of ethnography.[2]

The 'intellectual history' approach to Mead's Arapesh ethnography: its achievements and limitations

Mead's writings would seem to make a banner case for the utility of the intellectual history approach to understanding past ethnographic interpretations. Her interpretation of Arapesh culture in "Sex and temperament" is part of a triad of New Guinea case studies that has a conceptual architecture remarkably parallel to the three case studies developed in the book "Patterns of culture" (1934) by Mead's close friend, Ruth Benedict. Mead's triad also reflects her participation in a romantic triangle with Gregory Bateson, whose Iatmul field site was near to Mead and Fortune's Tchambuli. In particular, as we argue elsewhere, each of the three cultural case studies in Mead's book is in key respects a 'writ-large' expression of the core personality traits Mead discerned in herself and the two men as she analyzed their relationships.[3] Mead actually formalized these correspondences between the cultures she studied and her small circle of friends in a fourfold typology which she called the 'theory of the squares'.[4] She used this scheme to categorize the cultures she studied and read about, as well as virtually all of her friends, placing them along two axes, Northern-to-Southern and Turk-to-Fey, that Mead conceptualized as coordinating inborn temperaments with physiological traits. Even into her old age Mead regarded 'the squares' as her most original and important theoretical contribution, even though she feared publishing it at the time she was developing it (in the 1930s and '40s) due to its similarities to Nazi race theory.

In sum, there are a number of good reasons why Mead's interpretation of the Arapesh might be explained by appealing to factors like intellectual predisposition and influence. As for filtering, it may be enough to point out that "Sex and temperament" has been criticized since the time of its earliest reviews for the 'perplexing discrepancies' that exist between numerous ethnographic details she reported and the larger interpretation that she claimed for them.[5] For example, although one of Mead's main theses in the book is that the Arapesh are peaceful, nurturing, and averse to aggression, in an early review Richard Thurnwald compiled an impressive list of ethnographic details in

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2 We expect to develop and more fully exemplify this argument in subsequent publications on Mead and Fortune's fieldwork among the Arapesh.

3 Bashkow (2003), Bashkow and Dobrin (in prep.). See also Boon (1985).

4 Mead (1972:154–158), Banner (2003), (Sullivan 2004)

5 Thurnwald (1936:664). See also Lohmann (2004).

Mead's book that would seem to vitiate this conclusion, including among them 'quarrels over women', 'man and wife attacking each other with axes', 'men beating their wives', 'a quarrel which followed the abduction of a woman', 'a mother trying to strangle her baby and stepping on the head of another', 'violent, unreasonable rages', and the regular 'resort to sorcery' within a system of institutionalized hostilities.[6] Such discrepancies have not resolved themselves with the passage of time. They are central to Paul Roscoe's argument that in dismissing the importance of violence and warfare among the Arapesh, Mead 'got it wrong' (2003:586), a view that is also supported by our own fieldwork and research on the ethnohistory of Arapesh war alliances (Dobrin and Bashkow 2006). For better or worse, we are in a strong position to criticize Mead's image of the 'peaceful, nurturing Arapesh' as an interpretation that reflects her intellectual predisposition, including filtering tendencies, as much as it does 'the data'.

We have no doubt that a critique of Mead's ethnography in terms of such an intellectual history is warranted and indeed necessary. But we would like to suggest that it is not necessarily sufficient, and that we should not be content with it. One reason for this is that it disregards past anthropologists' own sense of what they were doing. Of course, Mead was attempting to produce a study of Arapesh culture, not an ethnographically-coded form of her own autobiography! And if she had doubted that the cultural patterns she described really existed 'out there', surely she would have thought twice before publishing so much evidence against her main argument. Indeed, as Roscoe notes, it is remarkable that 'she made no attempt to shovel [such evidence] under the carpet' (2003:585). When Mead insisted in the face of critical reviews that the ethnographic patterns she reported in her book were 'actually a reflection of the form which lay in [the] cultures themselves' (1950:i), she showed her conviction in the experiences she had in the field. But these were not – and could not be – unmediated experiences of 'the culture'. Rather, they were experiences of a particular lived world, the one she in part co-constructed through her relationships with Arapesh people during her fieldwork.

But another reason for moving beyond intellectual history has to do with the constitutive role of reading past work in the current life of anthropology. As we well know, the critique of previous scholarship has come to occupy a central place in the anthropological literature and in the training of the next generation of anthropologists. Ethnography today is taken to task on numerous counts, with classic works read specifically in order to problematise the project of ethnography itself. Ethnographies are said to inappropriately remove the people studied from history or overlook their participation in world and regional systems. They emphasize culture-internal uniformity at the expense of heterogeneity, hybridity, and contestation, thus oversimplifying a complex reality and rigidifying cultural differences into essentialised, often exoticised types. Ethnographies are criticized as ethnocentric, as responding to the concerns of the writer's culture or projecting onto the people studied Euro-American categories, 'myth models', or as-

6 Thurnwald (1936:665–666). See also Fortune (1939).

sumptions about society and human nature. Ethnographies are challenged on political grounds for portraying other societies in ways that support hegemonic ideologies such as the colonial concept of indirect rule or neoliberal individualism. And from an ethical perspective, fieldwork practices are scrutinized for faults such as the neglect of risks posed to the people studied, inappropriate social and sexual relations, exacerbation of conflicts, breaches of trust, and violations of intellectual property rights.

What is the anthropological lived world we create for ourselves through the practice of engaging in these kinds of dismissive critiques? We are concerned that we are creating an unproductive climate of unattainable goals that is inconsistent with our robust understanding of ethnography as a necessarily humanistic enterprise, a climate in which our relationship to our discipline's past has become adversarial, with the inadequacies of our intellectual ancestors continually emphasized and our disjuncture from them accentuated. For this reason we believe it is important to develop a more constructive form of critique, one that engages closely with the ethnographic particulars of the anthropologist's experiences while in the field. Because when we talk about Mead's ethnography solely in terms of her intellectual disposition and the influences of her western colleagues and friends – as juicy as all that is – we stop finding in her ethnography the lives of the Arapesh. We lose sight of the project that no literary critic of our discipline's archive would take on, the one that belongs to contemporary cultural anthropology itself: the project of understanding lived worlds of all kinds. And such worlds must ultimately include those which anthropologists themselves create in interaction over time with the people they study. We are thus interested in a form of reading past works that can reintegrate our critical discourse with an understanding of the ethnographic c o n t e n t – a project to which the anthropologist's fieldwork, even when questionable, must surely contribute.

Mead's Arapesh fieldwork as a co-constructed 'lived world'

And what might such a reading look like for Mead and Fortune's fieldwork? How did they construct the particular lived worlds of their fieldwork in the Mountain Arapesh region? Here, in summary form, are three key points that are emerging in our work on this case.

First, by posing the question of how these two ethnographers constructed the lived worlds of their fieldwork, we learn that their interpretive conflict stemmed in part from a marked difference in the way each participated in the extended Arapesh regional network that was constituted by travel and that was the medium for interlocality competition and political or warfare alliances. Indeed, even though the two ethnographers were working together, on a day to day basis they were often apart. As is well known, Mead's bad ankle confined her to their village field site of Alitoa for the full eight months of

their stay among the Arapesh. Because the Arapesh mountain terrain is steep and rugged she had to be carried into and out of the village at the start and end of her time there, and, hard as it may be to believe, we have clear evidence that she never stepped beyond the village perimeter. By contrast, Fortune travelled widely and frequently with parties of Arapesh people, on his errands as well as on their own, staying away from the village for long stretches of time. These travels took him past battlegrounds and on pathways that elicited from his Arapesh companions stories of great alliances, rivalries, and the politics of adultery that provoked men to war. There was nothing comparable in Mead's experience to make real for her this precolonial Arapesh culture of conflict, which found no place in her ethnography (Dobrin and Bashkow 2006).

Second, Mead and Fortune experienced Mountain Arapesh life in the form of a particular social world they in part created in interaction with the villagers through their fieldwork strategies. Here the primary issue is the way they set up their household and conducted their local exchange relations. The villagers had to be prevailed upon to build the anthropologists a house, something they were only willing to do once Fortune promised that the work would be richly reciprocated with foreign commodities they desired like matches and salt.[7] But the high expectations he thereby raised were soon shattered, no doubt unavoidably, and the anthropologists's relations with the villagers soon soured. Though the anthropologists were generous in providing food to workers during the period of active housebuilding, the villagers were disappointed by the small knives given out at the house completion feast, by the fact that many people received no gifts at all, and by the lack of any distribution of food for the housebuilders and their hosts to carry home with them. Mead recorded that, in the late afternoon as the feast was concluding, the women stood up and gave a 'long speech saying that they would now have to go to their distant gardens for food because all their supplies were exhausted' – this as evening was falling! – clearly expressing their dissatisfaction at Mead and Fortune's ungenerosity: you have left us empty-handed (Mead 1947:237). Mead was struck that every last native of the village cleared out immediately: 'The sun went down on our first night in the new house with Alitoa, the largest village of the Mountain Arapesh, absolutely empty, except for ourselves and our boys, all newcomers' (1947:237).

The presence of these boys who Mead and Fortune had brought from elsewhere to serve them was a further complicating factor in their village relationships. The boys' social status was awkward inasmuch as, being outsiders, they should have been hosted by the villagers but were instead being hosted – fed – by the whites. And it was the boys, rather than the villagers, who received the lion's share of the material benefits from the anthropologists' presence. Fearing jealousy, the boys frequently ran away; indeed, this is one of the main comedic themes in Mead's letters (see Mead 1977, MMP). Their social awkwardness also found expression in (and was further aggravated by) a continuing series of petty disputes, for example, when the boys hunted game (as they were charged to

7 MMP: Mead Bulletin Letter 1/15/32, p. 1 [N92:5]

do by Mead and Fortune) near the village without compensating the landowners. One of the main ways in which the villagers apparently registered their displeasure was by staying away from the village for weeks at a stretch, so that Mead's main informants, like the well-known Unabelin, were their boys and other outsiders – here again, the whites' village guests. As a result of this, Mead found herself interacting with people primarily in contexts that required them to be deferential, diplomatic, and humble. The markers of this in Arapesh discourse are gentle tone and an emphasis on agreeable themes like the importance of mutual help, nurture, and peace; and since these closely resemble core qualities of Mead's American construction of femininity, we infer that her dominant impression of Arapesh cultural temperament was formed in significant part from observing the deferential behaviour evoked in a village situation that she and Fortune did much to create.

Third, and finally, Mead, much more than Fortune, maintained in the field a strong orientation toward her own home frame of reference. When we began our research in the Margaret Mead Papers in the Library of Congress, we were more than impressed – we were amazed! – by the sheer volume of her output of letters from the field: it is a truly immense archive. Initially, we treated this letter-writing as an activity ancillary to her fieldwork, something she did over and beyond it. But as we soon came to recognize, her letter-writing was in fact a central part of her experience while in the field. Compounded by Mead's diary-keeping, the effort she and Fortune invested in the management of their supply stores, and the multitudinous note-slips that Mead typed and filed daily according to an anthropological system of categories, it becomes clear that she was intensely engaged in activities that continually oriented her to her home frame of reference: to the home plans and relationships that she maintained through her correspondence, to the intellectual currents in anthropology that she kept abreast of – and even tried to some extent direct – while remaining physically present in the New Guinea village. It is thus no accident that Mead's portrayal of the Arapesh is so brightly illumined by the conventional intellectual history approach that makes visible the influence of relationships stemming from the anthropologist's home culture. Indeed, the importance of those relationships is itself motivated by the lived world she constructed and experienced during her fieldwork.

We should note in closing that applying the notion of the lived world to the history of anthropology is not an easy thing to do. It requires knowledge of the anthropologist's actions in the field, as well as knowledge of the culture that was researched. It requires reanalysis of the anthropologist's participation in a series of unfolding intercultural encounters from the viewpoint of those studied. But the approach is worthwhile precisely because it draws attention back to the p a r t i c u l a r s of the field situation – 'the ethnography' itself. Even when the stance is critical of past work, the discourse is not inhibitory – it does not suggest that real ethnographic understanding is unattainable – but rather it is a productive cultivation of this understanding itself. This is in no way a negation of intellectual history, but its extension into the intercultural situation of fieldwork. For it is

here that, through an unfolding series of activities and relationships, the anthropologist's lived world of fieldwork is co-constructed by the anthropologist and the people his or her work helps us learn about.

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