

**“MORE LIKE FIGHTING THAN LIKE WAITING”:
MEAD, METHOD, AND THE PROPER OBJECT OF
KNOWLEDGE IN ANTHROPOLOGY**

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This paper examines the critical reviews of Margaret Mead's and Reo Fortune's early books and Mead's responses to them. It argues that these reviews demonstrate a consensus about proper anthropological practice and the proper object of anthropological knowledge. Mead's response was to go the offensive. She demonstrated her competence in the traditional fields of anthropology through her authorship of *Kinship in the Admiralty Islands* (Mead 1934) and her ability to generate "pure" knowledge and to provide historical context in *The Changing Culture of an Indian Tribe* (Mead 1932a). Most significantly, however, Mead challenged the consensus about the proper object of anthropological knowledge by arguing for a broadened and more subtle understanding of what constitutes "culture" in her 1933 article "More Comprehensive Field Methods." Mead's work broke new ground in anthropology and more closely resembles contemporary anthropology than the boundaries of the discipline her critics were trying to police.

Introduction

In January 1933, Ruth Benedict wrote a worried letter to a mutual friend about Margaret Mead and her husband, Reo Fortune. Mead and Fortune had been in New Guinea for fifteen months, doing fieldwork in poor health under difficult conditions. However, it was not so much their physical health but their (and, presumably, most particularly Mead's) states of mind that greatly concerned Benedict. Mead had recently written to her a

despairing letter about their professional futures (Library of Congress: Margaret Mead Papers [LOC: MMP], Add. III box S3, Mead to Ruth Fulton Benedict, letter dated June 16, 1932). It was apparently this letter to which Benedict had reacted:

They [Mead and Fortune] care so much for approval, and so much of it is withheld just because people are chagrined by being outdistanced, and find it easier to refuse to believe than to recognize that work can be done more quickly and thoroughly than they could do it in the field. Then there are non-anthropologists who feel themselves challenged by MM's and Reo's work as they weren't by the old stodgy monographs, and one and all refuse to believe. So they come out a little at the end of the horn, and the pity of it is that they want recognition so much. They have only to bide their time and rest their case on good work. But they are impatient and feel more like fighting than like waiting. (LOC: MMP, box O38, Benedict to "Dear Isabel," letter dated January 11, 1933)

There is surely no other anthropologist, either living or dead, whose fieldwork has been subjected to the kind of sustained critique, revisiting, and methodological microscopy than Margaret Mead's. Her early ethnographic research has been extensively reviewed, revised, and, at times, reviled through ethnographic research, review of her field notes, or both. On top of this, reviews of these reviews are now also common (e.g., Lipset 2003; McDowell 2005; Yans 2004), while the debate about the veracity of Mead's versus Freeman's claims about the nature of Samoan adolescent sexual behavior seems unlikely to die, despite promises to the contrary (Caton 2000; Shankman 2001; for examples published since 2001, see Côté 2005; Francis 2005; Shankman 2009).

Rancorous criticisms of Mead's work go back to the original publication in 1928 of *Coming of Age in Samoa* and, as Benedict's letter indicates, did not stop there. A sense of professional embattlement engulfed Mead and Fortune during their seven years together and, as I have argued elsewhere, contributed to the demise of their marriage (Molloy 2008; see also Thomas 2009). Benedict exaggerated the lack of recognition, as Mead was the best-known and certainly one of the best-selling anthropologists in the United States at the time. Yet Benedict was indeed accurate in her assessment of the professional acrimony directed at the pair. Despite her later reputation for overconfidence, Mead was deeply upset and affronted by the antagonism with which both her books and Fortune's were received by many of their anthropological colleagues.

However, rather than retiring or, as Benedict advised, waiting, Mead fought back. This paper explores these early criticisms and Mead’s response to them, explicating her strategies to defend both her and Fortune’s work and to maintain her position in a discipline hostile to women and to her particular scholarly focus. I argue that the criticisms of Mead’s and Fortune’s books reveal a consensus about acceptable anthropological practice and, perhaps more important, about the proper object of anthropological knowledge. Mead’s response demonstrates her determination to prove her competence in terms of that consensus. But perhaps more important for the future of the discipline, Mead also vigorously defended her expansive and challenging vision of what it is that anthropologists should study.

The first part of this paper considers twenty-four anthropological reviews of Mead’s and Fortune’s books published between 1928 and 1936. Their work is considered together for two reasons. First, they had a very strong sense that their research was a single project, unified by common field sites, methodology, and theory. An attack on one of them was seen as an attack on both. Second, some critics used one of them to criticize the other in these reviews. Therefore, Mead’s and Fortune’s work was entangled not only in their own view of it but also in the minds of their most vociferous anthropological critics.

Mead’s books were reviewed across a wide range of media—from local newspapers to scholarly journals; reviews of Fortune’s, as far as I can tell, appeared largely in professional publications. Although Mead was to build her career on the basis of popular responses to her work, it was the estimation of her anthropological colleagues that would define her professionally. I have chosen, therefore, to focus on reviews written by anthropologists for scholarly or learned publications. There were many reviews in the more popular periodicals, some written by anthropologists. Ruth Benedict, for example, reviewed two of Mead’s books in the *New York Herald Tribune*; Ralph Linton reviewed one in the *Madison [Wisconsin] Capital Times*. However, it was the *American Anthropologist*, the principal American professional journal, and *Man*, the principal British one, that were read internationally and considered the gold standard for reviews of scholarly anthropological work in the late 1920s and early 1930s. There were fifteen reviews of Mead’s and Fortune’s books published in these two journals between 1929 and 1935. In addition, there were three reviews in *Oceania*, two in the *Annals of the American Academy of Political and Social Sciences*, two in the *American Journal of Sociology*, and two in the *Saturday Review of Literature*, all by anthropologists. I include the *Saturday Review*, although it was not a professional journal because it did publish serious reviews, by anthropologists, of anthropological monographs and therefore can be presumed to have been read by professionals in the field.

A few of these reviews are purely descriptive rather than analytical. These include C. Darryl Forde's reviews of *The Changing Culture of an Indian Tribe* (Forde 1933) and *Omaha Secret Societies* (Fortune 1932a; Forde 1934) in *Man* and Alexander Goldenweiser's (1934) review of *Changing Culture of an Indian Tribe* in *American Anthropologist*. Of those that remain, eight are positive (Elkin 1934–1935; Hart 1933; Hogbin 1936–1937; Linton 1935; Redfield 1931; Tozzer 1933; Wedgwood 1935–1936; Seligman 1936), four are mixed (Fortes 1936; Linton 1936; Lowie 1929; Powdermaker 1935c), and nine are negative (Clarke 1931; Hart 1933; Kroeber 1931; Lowie 1933a, 1933b; Powdermaker 1935a, 1935b; Redfield 1929; Thurnwald 1936). On balance, therefore, Mead was right in her assessment that their work was unappreciated within the discipline. The criticisms in these negative and mixed reviews is consistent, giving us not so much a picture as a stencil of what constituted both acceptable anthropological practice and the proper object of anthropological knowledge in this "golden age" of anthropology.

The second part of this paper considers Mead's response to the most critical of these reviews. It focuses in particular on two pieces she wrote during this period: *The Changing Culture of an Indian Tribe* (Mead 1932a), and an article, "More Comprehensive Field Methods" (Mead 1933). In *Changing Culture of an Indian Tribe*, Mead addresses the critics' calls for historical context and pure knowledge and makes a methodological claim for the study of small-scale societies. However, it is in "More Comprehensive Field Methods" that Mead most clearly articulates her ideas about the meanings of anthropology's central concept of "culture" and the theoretical importance she attached to studying children. It prefigures her detailed photographically based research in Bali with Gregory Bateson in the late 1930s (Bateson and Mead 1942; Sullivan 2005).

Proper Anthropological Practice: Fieldwork, Language, and Rigor

Criticisms of Mead's and Fortune's books almost always began with reference to the duration of their fieldwork and its impact on language competence. By the early 1930s, they had become notorious for their relatively short periods of fieldwork: eight months in Samoa (Mead), six months in Dobu (Fortune), seven months in Manus (Mead and Fortune), and, most significantly for their American reviewers, only three months in the state of Nebraska, with no attempt to learn Omaha. Alfred Kroeber, for example, pinned his very critical review of *Growing Up in New Guinea* (Mead 1930a), discussed later in this paper, on his assessment that Mead provided clues rather than data because she had "only six months to learn

a language and enter the inwards of a whole culture, beside specializing on child behavior” (Kroeber 1931, 248). Similarly, Hortense Powdermaker, in a review of *Sorcerers of Dobu* (Fortune 1932b) in the *American Anthropologist* in October that same year, regretted that Dr. Fortune “could not have stayed another six months in Dobu, or gone there again, and given us some of the details possible from a longer period of residence” (Powdermaker 1935c, 724). She was particularly skeptical of Fortune’s contention that he had not used English after the first day and had learned the language by “contagion” (Powdermaker 1935c, 725). It must be said, however, that on the whole she found the book “exceptionally well integrated [and] of permanent value as a source book for those interested in Melanesia and in social anthropology” (Powdermaker 1935c, 724).

The relationship between length of stay, language competence, and grasp of the “whole” culture emerges throughout these reviews as crucial to ethnographic credibility. Powdermaker was as scathing about Mead’s and Fortune’s fieldwork practice in private as she was in her published reviews. In a letter to Elsie and “Bronio” (Bronislaw) Malinowski, written while she was a visitor at Columbia, she wrote,

I wish New Ireland was a bit nearer. I am constantly seeing new aspects of some of my problems. I suppose this happens to everyone except Margaret Meade [*sic*] who after five months in Manus says that she saw and solved all problems connected with that island. You can put this down to my catty personality, but she really did say it, and what is more seems to believe it. (London School of Economics: Bronislaw Malinowski Papers [LSE: BMP], Stud/11, Powdermaker to Elsie and Bronislaw Malinowski, letter dated December 11, 1930)

Malinowski concurred with Powdermaker’s assessment. (LSE: BMP, Stud/11, Bronislaw Malinowski to Hortense Powdermaker, letter dated February 11, 1931).

The worst attack was a blast from Robert Lowie, who chose the occasion of his retirement as editor of the *American Anthropologist* to publish a think piece titled “Queries” in the spring 1933 edition. While the bulk of the article is aimed at the eminent men in anthropology, including Boas and Radcliffe-Brown, Lowie ended with a series of questions for “the younger generation” (Lowie 1933a, 296). These were clearly aimed at Fortune, whose *Omaha Secret Societies* (Fortune 1932a) he was reading for review. Lowie was having nothing of the newcomer’s attempt to “correct” the findings of an older generation of anthropologists—James Dorsey,

Alice Fletcher, and Francis LaFlesche. Lowie's queries of "the younger generation" were:

How does one master a native tongue in three or even six months?

Does an observation in 1930 necessarily take precedence of one in 1870, 1800, 1700?

How probable is it that a trained fieldworker can in a season or two plumb depths inaccessible to predecessors who have lived with the same tribe for years and speak its language perfectly? (Lowie 1933a, 296)

Lowie was to follow these "Queries" in the next issue of the *American Anthropologist* with what must be one of the most vituperative book reviews published in that journal. He began by contrasting the credentials of those whom Fortune presumed to correct with Fortune's own:

In 1871 J. O. Dorsey began among the Southern Siouans the series of observations which bore fruit in his *Omaha Sociology* (BAE-R 3: 205–270, 1884) and *A Study of Siouan Cults* (BAE-R 11:371–422, 1894). He was followed by Miss Alice Fletcher and Francis LaFlesche with their study on *The Omaha Tribe* (BAE-R 27:1911) based on twenty-nine years of "more or less constant intercourse." Several years ago Dr. Fortune paid a three months' visit to the Omaha. (Lowie 1933b, 529)

Lowie then condemned Fortune's book from beginning to end, accusing him of arrogance, ignorance of ethnographic relationships between Plains Indians cultures, willful misreading of the historical literature, and incomprehensible writing.

Lowie's was but the most critical of the reviews that linked truncated fieldwork and dubious language competence to lack of rigor. Kroeber, for example, implied that Mead's evidence in *Growing Up in New Guinea* (Mead 1930a) was so poor as to suggest that she was suppressing information that did not support "the vividness of her picture" (Kroeber 1931, 250). He compared her to Malinowski, the "other functionalist" (Kroeber 1931, 249) but one who had supplied "unusually saturated, detailed, accurate, well-integrated, and valuable" ethnographic information (Kroeber 1931, 250). In a final coup de grâce, Kroeber suggested that Mead had let down her own standards: "If she can learn to satisfy only herself, she should do finer and profounder works than Samoa and New Guinea" (Kroeber 1931, 250). Edith Clarke's review of *Growing Up in New Guinea*, published a

month later in the *Saturday Review of Literature*, was more restrained but equally damning. Again Mead was accused of conducting fieldwork too hastily and without adequate language skills. Internal contradictions in the book, such as Mead’s contention that the Manus were virtually untouched by European encroachment, were politely but firmly exposed, and her assessment of the emptiness of Manus children’s patterns of play was greeted with incredulity (Clarke 1931). Such criticisms were to persist. In 1936, Richard Thurnwald suggested that Mead’s *Sex and Temperament in Three Primitive Societies*, published in 1935,

might have gained had [it?] been completed in less of a hurry, and had the stay in New Guinea been of longer duration. . . . It should be realized that at least a year’s stay with one tribe is required to yield promising results. (Thurnwald 1936, 667)

Despite these criticisms of methodological inadequacy, there was also praise for Mead’s and Fortune’s practice of participant observation. Clearly, as late as the early 1930s, participant observation—now seen as the defining feature of anthropological method, despite its much-written-about limitations—was relatively new and rare, particularly in the United States (for the classic texts, see Clifford and Marcus 1986; Geertz 1988). Powdermaker, a practitioner herself, praised Fortune for “telling us when his material is based on firsthand information and when it is hearsay from an informant” (Powdermaker 1935c, 725). Similarly, Linton’s review of *Social Organization of Manu’a* (Mead 1930b) commented,

Most of the material was gathered by direct observation rather than from informants. Very few studies of this sort have been made by American ethnologists and the present work shows how much valuable material can be obtained by using this approach. (Linton 1935: 157–58)

However, none of Mead’s and Fortune’s American critics considered the possibility that total immersion in an indigenous community would have sped up both the process of language acquisition and a broader knowledge of the culture and its practices. The more common American practice of historical reconstruction based on interviews with elderly informants did not involve the same intensity of interaction with what anthropologists on both sides of the Atlantic were apt to refer to as “whole” cultures.

The consistency of the equation between length of fieldwork, language acquisition, and academic rigor in these reviews demonstrates that, by the

early 1930s, there was a strongly held consensus about some methodological principles among anthropologists in the United Kingdom and the United States. Long periods of fieldwork—although not necessarily as a participant—and language fluency had become the bedrock of anthropological practice. Anthropologists were also likely to focus on a single culture or group of cultures. So it also seems likely that Mead's and Fortune's practice of moving from one group to another and across widely separated culture areas (Polynesia to Melanesia to Plains Indians and back) over relatively short periods of time exacerbated their colleagues' disapproval. Mead's and Fortune's condensed fieldwork and confident conclusions could be seen as implicitly undercutting those who had spent years studying a single indigenous culture.

The Proper Object of Knowledge of Anthropology

Mead's focus on specific problems rather than full ethnographies was a source of comment right from the beginning of her career (see also Tiffany 2009). In the first review of *Coming of Age in Samoa* (Mead 1928), Lowie commented that

Dr. Mead deliberately set herself a task distinct from the traditional ethnographer's. Ignoring the conventional descriptive pattern, she concentrated on the individual's reaction to his social setting,—specifically, the adolescent girl's adjustment. (Lowie 1929, 532)

Lowie was not critical of this unorthodox approach and believed that “[d]ealing with problems incomparably subtler than those which usually engage the ethnographer's attention, she [Mead] has . . . illustrated a new method of study that is bound to find followers and to yield an even richer harvest” (Lowie 1929, 534). However, Lowie remained skeptical of some of Mead's conclusions, especially her contention that the patterns she recounted were not the result of colonization but were indigenous. He suggested that a historical approach to Samoa, similar to that which other American anthropologists had developed for the Plains Indians, might yield different conclusions. This was a gentle review, perhaps suitable for the first published work of a newly credentialed professional. However, in his skepticism of her “ethnographic present,” his call for an account of historical change, and his recognition of the difference of Mead's approach to the ethnographic norm, Lowie presaged the more critical reviews of her next popular book, *Growing Up in New Guinea* (Mead 1931).

By far the most serious and critical review of Mead's work was Alfred Kroeber's review of *Growing Up in New Guinea*. In May 1930, Mead wrote to Kroeber to ask if he would review the book for the *American Anthropologist* (LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated May 26, 1930). In a private letter written less than a month after the book's publication, Kroeber praised her for having “sharpened your technique” (LOC: MMP, box I4, Kroeber to Margaret Mead, letter dated October 23, 1930) but chided her for “touching lightly on the culture in order to protect your [Mead's] husband [Reo Fortune]” (LOC: MMP, box I4, Kroeber to Margaret Mead, letter dated October 23, 1930). Mead responded instantly, informing him she had not been “as forebearing as your first impression” (LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated October 30, 1930) with regard to Fortune's work and laid out their division of labor:

Reo will do . . . the ethnology of the culture to set beside my special study, using my notes in addition to his much fuller material. But I couldn't possibly have done all the special work which I wanted to do and also have done the complete ethnology. I did do most of the social organization and the material culture—a little sketchily, while Reo did the religion and economics in full textual detail. He also did most of the formal work on the language. (LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated October 30, 1930)

She then, perhaps mistakenly and certainly not tactfully, outlined her thoughts on the limited nature of American anthropology, developed over the summer while she was working in Nebraska on the Omaha:

The summer was good for our souls and bad for our dispositions. It's [*sic*] chief function, as far as I was concerned was illumination, about the Indian, and also about American field methods and points of view. One realizes so much more vividly why the American emphasis is historical and not functional, why the best understanding can be gained though a study of different integrations of the same trait in different tribes, rather than the study of inter-related traits in one tribe. We had moments of despair [*sic*] when nicely constructed hypotheses day after day were formed only to collapse before the dead wall of a vanished culture. (LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated October 30, 1930)

This was a view of American anthropology that she had begun to promulgate. While in Nebraska, Mead had written to Malinowski that she was

beginning to understand why the American school has historically stressed history rather than function.

a. because you can't do function decently, and b. because all function is obscured by the hodge podge of traits borrowed from hither and yon. (LOC: MMP, box N19, Mead to Bronislaw Malinowski, letter dated August 9, 1930)

After she returned to New York, Mead publicly dismissed American anthropology. She was quoted in a newspaper interview as saying that “[t]here isn't any fieldwork left in the United States, so one has to go to Africa, South America, the Pacific Islands or Siberia” (LOC: MMP, box L3, Dr. Margaret Mead, n.d., 1931, unprovenanced newspaper clipping). The implication that American anthropology was a kind of deficient option made necessary by the condition of its native peoples was not a sentiment likely to endear herself to the “big men” of American anthropology, and their reviews must be seen, at least in part, as their revenge.

In the spring of 1931, Kroeber published his review of *Growing Up in New Guinea* in the *American Anthropologist*. The review is exceedingly clever and crafted for maximum effect. It begins with fulsome praise of Mead's ability to “swiftly aperceiv[e] the principal currents of a culture as they impinge on individuals, and [to delineate] these with compact pen-pictures of astonishing sharpness” (Kroeber 1931, 248). Mead's “near-genius,” he wrote, was essentially aesthetic, but, he pointed out, “a piece of work need not be ethnographically unreliable because it is aesthetically effective. And an artist Margaret Mead surely is” (Kroeber 1931, 248). He then ceded the points she made in her letter, including the spread of functionalism to the “heart of the Boas school” (Kroeber 1931, 248) and avowed that it is a method that can be “most effectively applied to healthily living cultures” (Kroeber 1931, 248). Functionalism, he concurred, is not applicable to societies such as the Hopi, Zuni, or Navaho, which are not still “essentially native in their fabric” (Kroeber 1931, 249).

Having given these points so graciously, he attacked. But Kroeber's criticisms were not limited to the methodological issues raised in the previous section. He challenged her basic approach to ethnography, raising the question of whether her work actually was anthropology. Specifically, he objected to Mead's focus on the present, her concern for the practical applications of her findings, and her subordination of the Manus to American problems. Like Lowie, he deplored that functionalists like Mead lacked “any serious sense of historical problems obtruding themselves, of every culture necessarily having a historical dimension” (Kroeber 1931, 249). This, he implied, was because Mead was, in fact, a sociologist with an eye

to “the practical and the present,” while anthropologists were more concerned with “pure understanding and the past” (Kroeber 1931, 249). Mead’s “sharp interest in the America of 1930” was a shock to “an ingrained anthropologist, who all his life has been schooling himself to see his own culture really on one horizon with all others” (Kroeber 1931, 249).

C. W. Hart’s review of the English edition of *Growing Up in New Guinea* (Mead 1931), published a year later in *Man*, is remarkably similar in detail to Kroeber’s but even more dismissive.¹ Like other critics, he referred to Mead’s six-month period of fieldwork. For Hart, the duration of fieldwork was not the defining problem with the book: “residence for a period of time among a native community does not make a person an anthropologist” (Hart 1932, 146). Rather, he argued, anthropology is defined by “the sort of phenomena investigated” (Hart 1932, 146) and “the sort of generalizations attempted in any published material” (Hart 1932, 146). Mead’s focus on the effect of culture on the individual, rather than on “what the culture was,” rendered her book something other than anthropology—suggesting perhaps a new field of comparative social psychology (Hart 1932, 146). While Americans might hail her as a leading anthropologist, the English would query whether she was “an anthropologist at all” (Hart 1932, 146). However, he was not content simply to relegate Mead to this new field. Like Kroeber and Clarke, Hart condemned the ethnographic section for “oversimplification and unjustifiable dogmatism” (Hart 1932, 146). The appendices, he wrote, suggested that Mead went into the field with “an anthropological *Notes and Queries* on the one hand and a psychological *Notes and Queries* on the other” (Hart 1932, 146), each inadequate to the task. Hart also followed Kroeber in using Fortune to criticize Mead, referring in the review to “the more careful and judicial Mr. Fortune, trained in English methods of scientific research” (Hart 1932, 146). Hart’s (1933) complementary review of Fortune’s *Sorcerers of Dobu* (Fortune 1932b), published a year later, likely served to exacerbate tensions in Mead’s and Fortune’s crumbling marriage.

Fighting

These responses to *Growing Up in New Guinea* horrified Mead. By the early 1930s, she was well used to her public status as girl prodigy; she was, therefore, shocked at the accusations of ignorance, incompetence, and intellectual dishonesty. In a private letter to Kroeber, she struggled to contain her outrage with a man who was able to wield his influence over both her and Fortune’s access to funding and jobs in the United States. She thanked him for “all his kind words of commendation in the review”

(LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated May 1, 1931) and told him that the review had

taught me how incredibly naïve I have been in my reactions to previous criticisms. . . . To discover that [my colleagues] thought me so lacking in method, so deficient in ethnological training as to be making flimsy generalizations without having done the kinship system, or understood the economic arrangements or the religious ideas, was a real revelation to me. (LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated 1 May 1, 1931)

Although treading cautiously on other criticisms in the review, she challenged him directly for his references to Fortune. Implicitly contrasting Kroeber's churlishness with Fortune's gentlemanly behavior, she pointed out that Fortune had "always shared honors so scrupulously and generously with me that it makes me very unhappy to have had such a comment appear in a review of my work" (LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated May 1, 1931). Kroeber, evidently, wrote an apologetic letter, and relations were patched up although never fully repaired (LOC: MMP, box C3, Mead to Alfred L. Kroeber, letter dated October 30, 1931).

While the most disturbing criticisms were those that attacked the pair for lack of rigor, these were also the easiest to deal with.² Mead's response to Kroeber and Clarke's critiques of *Growing Up in New Guinea* was simply to negotiate (or force) a new division of labor with regard to the Manus material. Fortune had originally been committed to writing a "complete ethnology of the Manus culture" (Mead 1942, 293). He now limited himself to Manus religion, promising a book on language in the future, while Mead took over writing up the detailed interrelationships between kinship and economics. Fortune and Mead delayed their trip to New Guinea for six months in order that she could write *Kinship in the Admiralty Islands* (Mead 1934). Nevertheless, the book was written very quickly and on the go—one suspects largely on shipboard. They left New York on August 25, 1931, for Vancouver, where they sailed to New Zealand. After a brief visit with Fortune's family, they proceeded to Sydney to consult with A. R. Radcliffe-Brown and other colleagues and then proceeded on to New Guinea. From Karawop on December 8, 1931, Mead sent the typescript of *Kinship in the Admiralty Islands* to Bella Weitzner in New York (LOC: MMP, box I6, Mead to Bella Weitzner, letter dated December 8, 1931).

Reviews of *Kinship in the Admiralty Islands* demonstrate that Mead's response to the charge of "scrappy" evidence had been effective: the book

was described as “a most valuable kinship study” (Zeligman 1936, 325) and “so well-documented” (Seligman 1936, 327) that it “must have required immense patience and skill” (Seligman 1936, 325). A. P. Elkin came close to acknowledging the doubts that had been expressed about Mead’s competence: his review in *Oceania* opens with the observation that

[r]eaders of Dr. Mead’s book and articles on the development of the social personality in various societies will welcome this work showing that underlying those most interesting analyses there is a thorough comprehension of the social organization of the people concerned. (Elkin 1934–1935, 490)

Mead took up the issue of methodology in *The Changing Culture of an Indian Tribe* (Mead 1932a), her least-known ethnography of this period. Mead’s study of the Omaha of Macy, Nebraska, was undertaken in the summer of 1930, in part to placate Clark Wissler, who had received a grant to fund a study of contemporary Native American family life. Benedict had obtained a grant for Fortune to study Omaha vision quests, enabling Mead to conduct her own research covertly, by pretending to be simply accompanying her husband. She disguised the identity of the tribe by calling them the Antlers in the book.

In the book’s introduction, Mead seems more intent on warding off attacks from sociologists rather than anthropologists. However, the book indirectly addressed issues raised by her anthropological critics, namely, that her work was not science, that it was applied rather than “pure knowledge,” and that it ignored history. Wissler’s foreword explicitly claimed the study as science rather than any form of applied or “ameliorative” work (Wissler 1932, iv). Mead’s introduction is principally a methodological defense of the study of small-scale societies in light of “the tendency to identify science with quantitative methods and to accept no social data without their probable error and standard deviation” (Mead 1932b, 6). The ethnologist’s job, Mead explained, is either to reconstruct historical social form (a function she attributed to most American anthropology) or to determine the relationship between “original nature and social environment” (Mead 1932b, 6). She designated these two ethnological tasks as historical and sociological or social psychological.³ While an ethnologist cannot offer statistical significance as a defense of her findings, she can, Mead argued, offer the “homogeneity” of a “complete culture, and the interrelation and functioning of its parts” (Mead 1932b, 10). However, the student of a “transitional primitive culture” is at a disadvantage: homogeneity and the smooth articulation of interacting parts have gone, but numbers are still too small to satisfy the sociologist’s need for statistical significance.

The paradigm for the ethnological method in such cases, she argued, must be drawn then not from the social sciences but from medicine, specifically pathology and psychiatry. These disciplines present “each case in detail because of its power to illuminate our knowledge of . . . physiology . . . and of the human mind” (Mead 1932b, 15). Mead used metaphors of disease and contagion throughout the book itself, writing, for example, that “every delinquent [Omaha] girl is a plague spot, a source of infection to the other girls” (Mead 1966, 196).

Mead also went on the offensive against her fellow anthropologists, criticizing American colleagues who had insisted that culture contact “preserve some shadow of the peaceful diffusion between cultures that are evenly matched” (Mead 1932b, 5) rather than acknowledging the very real power imbalances such as those that the Antler faced. She disavowed British studies “immured from use by serious students in a wealth of invective against imperialistic policies or missionary influences” (Mead 1932b, 5). This study, Mead told her readers, had a carefully considered methodology, was robustly situated in a medical paradigm, eschewed invective in the service of pure contribution to knowledge, and was realistic, objective, and balanced.

In the first chapter (“Retrospective Sketch”), Mead answered her critics’ charge that she had ignored historical change. She drew on the work of Dorsey, Fletcher, and La Flesche to construct a history of Antler (Omaha) society in three phases: a traditional phase in which the culture was stable and functionally integrated; a post-Allotment phase during which Antler (Omaha) culture was “attenuated” (Mead 1966, 30), “the shadow of the rich complexity of their former lives” (Mead 1966, 29), but still coherent and in a state of “slender equilibrium” (Mead 1966, 30); and, finally, its current state of disintegration, brought on by the “onrush of white settlement” (Mead 1966, 30) after the Antlers had received alienable title to their land.⁴ While attending to the idea of history, Mead’s account ignored 150 years of postcontact change that had seen the Omaha displaced from the east to the northwest of the Missouri River and decimated by smallpox and intertribal warfare.

Perhaps Mead’s most interesting response to the criticisms of her work is the article “More Comprehensive Field Methods” (Mead 1933), which she sent to the *American Anthropologist*, her home journal but one that was proving hostile to her work. The article is explicitly a response to Kroeber’s review. “More Comprehensive Field Methods” is mistitled; the article is not so much a statement of methods as a stake in the ground as to what constitutes culture. In this article, Mead argued for a broadened, more subtle definition, inclusive of what she termed the “inexplicit” or “unformalized” aspects of culture.

She began by arguing that “[t]he history of ethnographic field work has been also the history of widening definition of which departments of human life are to be regarded as culture” (Mead 1933, 1). This definition, however, had not widened far enough:

[A] monograph would be condemned . . . [if] the ethnographer has failed to find out whether there was circumcision. . . . But a complete ignorance of the way in which a child is weaned or the position in which a child is held while being suckled . . . may be omitted with clear ethnographic conscience. (Mead 1933, 1)⁵

Moreover, she accused fellow ethnographers of too often describing only “the conspicuous, the conventional or the bizarre” (Mead 1933, 2) and ignoring the taken-for-granted or unformalized aspects of culture, especially those that relate to childhood. Mead continued,

Reviews of my two studies have revealed very clearly two facts: first, that many anthropologists are far from clearly realizing that child behavior or sex attitudes are as much a part of culture, are as distinctly and as elaborately patterned as are religious observances; and second, that they have no very definite conception of how such inexplicit aspects of culture are to be studied. (Mead 1933, 9)

Mead’s argument turned on the distinction between formalized or ritualized aspects of culture and those that she labeled unformalized or inexplicit, that is, those aspects of culture that are often not even recognized by the people themselves. She pointed out that “only formal [aspects of culture] can be obtained from informants in a dead culture” (Mead 1933:4), thus claiming for herself a more complete ethnographic practice than those who studied “dead” or “broken” cultures. The study of the unformalized aspects of culture, she argued, required more depth and rigor. One must have “a knowledge of the language, a much more extended entrée into the lives of the people, a much more complete participation in their lives (Mead 1933, 7). This is because each generalization must be the result of a myriad of systematic observations of behaviors, some of which may vary from formal accounts given by informants and some of which native informants will not even be conscious.

What may be formalized in one culture, she argued, may not be in another, so no hard-and-fast rules can be developed for how to proceed. However, informal but patterned behaviors are as influential in shaping the individual as are the highly regulated and ritualized. The final section

of the article outlined explicitly the methods she used in studying the children of Samoa and Manus, including the kinds of case controls she attempted to use to ensure her conclusions were robust.

In the concluding paragraph, Mead returned to the question of the “object of knowledge.” She rejected the idea that the “type of problem” she studied, or her “delimitation of theoretical inquiry,” excluded her from anthropology (Mead 1933, 15). Ethnologists, she argued, study the “cultures of primitive peoples” (Mead 1933, 15). In order to carry out that task, ethnologists must:

extend the present, narrow, accidental and inadequate rubrics under which most investigators have been accustomed to collect and present their data.

. . . [T]he whole of man’s life is determined and bounded by his culture and . . . every aspect of it, the inexplicit, the unformulated, the uninstitutionalized, is as important to an understanding of the whole, as are the traditional institutions about which it has been customary to center inquiry. (Mead 1933:15)

Waiting

Reviews of Mead’s and Fortune’s books reveal a disciplinary culture at the center of which was a commitment to extended periods of fieldwork, often with a single society, and that mandated linguistic fluency. Anthropology (or ethnology as Mead was wont to call it) was not, however, a discipline based, as it is now, on participant observation. The work of Margaret Mead and Reo Franklin Fortune made a critical contribution to the normalizing of intensive “immersion” fieldwork, especially in the United States, where the emphasis had been on salvaging what could be known of precontact cultures, largely through interviews with elders.

This salvaging mission also marked the anthropological community’s consensus about what anthropologists properly studied, that is, “whole cultures.” Mead did not invent problem-focused ethnology; if anyone can be credited with that, it must be Boas, who sent her to Samoa specifically to study adolescent girls. Other anthropologists had studied and written on specific aspects of culture, at the very least in journal-length articles. Nevertheless, the standard anthropological monograph in the 1920s and up at least until the late 1930s was, as Ralph Linton put it in his review of Fortune’s *Manus Religion* (Fortune 1935), “description of . . . culture as a whole” (Linton 1936, 498). The kind of focused work that Mead and

Fortune produced was regarded either as “not anthropology” or as “excellent within . . . bounds” (Linton 1936, 498), depending on the reviewer. Mead’s other foci, on the individual and on lessons for modern America, took her even farther outside the bounds of normative anthropological practice of the 1920s and 1930s. However, what really marked the objects of Mead’s work as distinctive were its foci on children, on sex, and, as she would put it, on the unformalized aspects of culture.

In *Blackberry Winter*, Mead wrote that she and Bateson decided to work in Bali because they believed Balinese culture would complete the square: the schema of culture/personality types they had developed on the Sepik in the austral summer of 1932 (Mead 1975, 216). However, it is clear from “More Comprehensive Field Methods” that the theoretical groundwork for the kind of fieldwork they would do had already been set out by Mead in 1931, before she had met Bateson. The link between these “marginal” topics and Mead’s extension of the definition of culture to these nuanced practices and behaviors was not accidental. She was interested in how people become members of their cultures, and her studies of children, normally excluded from most of the formal or ritualized aspects of culture, had led her to the subtle but distinctive ways in which individuals learn to “be” their culture.

It is perhaps significant that this version of culture, growing out of the seemingly insignificant, the detail, the mundane aspects of life is so different from the more immediately influential characterisations of culture: Sapir’s high-modernist definition as those elements that most emphatically represent a group’s “genius” or “distinctiveness” (Sapir 1924) and Kroeber’s culture as “superorganic” (Kroeber 1917). Mead’s is truly a domestic version, but one that has much more relevance today than those more grandiose schemes.

The experience of reading the reviews of Mead’s books is much like reading the books themselves—one tends to verge from enjoyment and delight to disbelief and disappointment. Much of the criticism of her work is justified. Her books are full of evidence that contradicts her interpretations as well as vast and unsubstantiated generalizations. At the same time, Mead’s books have deliciously literate passages, acute images, and the sense that if she was not always right in the details, she somehow often seems just right in the larger picture. The reviews are similarly complex. Right and righteous in their exposure of her flights of fancy, lack of consistency, and rigor, the worst of the them are also dead wrong in terms of the subject matter of anthropology. What Mead and Fortune did and studied is much closer to current anthropological practice than the boundaries that their critics were trying to police in the mid-1930s.

So Benedict was right. Mead's ethnographic and methodological defenses have largely passed into obscurity. However, her broadened definition of culture, her attention to the quotidian, and her foci on gender, children, sex, and the domestic form an enduring legacy that forever changed the face of anthropology.

NOTES

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1. The first English edition of *Growing Up in New Guinea* was substantially different from the first American edition. In particular, part 2 has a different structure, with most of the material on American fatherhood omitted and references to English culture and literature included (Mead 1931).
2. Much later, Mead also tackled the issue of language competence in an article for the *American Anthropologist* titled "Native Languages as Field-work Tools" (Mead 1939), in which she laid out a method for learning key phrases, key questions, and so on and distinguished between the kinds of situations in which interpreters could be used as opposed to those in which the anthropologist could get by with less "virtuosity." Lowie (1940, 81) wrote a scathing reply, but it must be said that Mead's article is a good "how-to" primer for anyone entering a period of fieldwork with no knowledge of the local language.
3. At this point in time, Mead was still somewhat ambivalent about her professional identification. She saw "primitive" cultures as natural laboratories from which the modern world could extract knowledge useful for its problems. She believed that the task of anthropology was to extract that knowledge in service of the disciplines focused on the modern West—education, psychology, sociology, and history. This attitude was, of course, premised on the belief that "primitive" cultures would inevitably die out.
4. Alice Fletcher conducted the first tribal census of the Omaha in 1883. The occasion was a request by the Omaha that their 300,000-acre reserve be divided into individually owned allotments, a request made in the hope that such entitlement would prevent a further rumored displacement to Oklahoma. The Omaha Allotment Act (1882) included a twenty-five-year moratorium on onward sale of the land (Barnes 1984).
5. See Tiffany (2009) for a discussion of Samoan "child nurses" and Sullivan (2005, 2009) for elaboration of how Mead developed these ideas in her subsequent research.

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