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Special Issue

The Gang of Four:
Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead
in Multiple Contexts

Vol. 32, Nos. 2/3-June/Sept. 2009

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PACIFIC STUDIES

# THE GANG OF FOUR

## GREGORY BATESON, RUTH BENEDICT, REO FORTUNE, AND MARGARET MEAD IN MULTIPLE CONTEXTS

*Guest Editors*
Gerald Sullivan
Sharon W. Tiffany

*Dedicated to the memory of Mary Wolfskill*

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SPECIAL ISSUE

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*Articles*



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Reo Fortune, and Margaret Mead in Multiple Contexts

Vol. 32, Nos. 2/3 June/September 2009

## GUEST EDITORS' NOTE

This special issue has its roots in several conferences, held between 2001 and 2005, to mark the Centennials of Margaret Mead (2001) and Gregory Bateson (2004); among these were five sessions at the Association for Social Anthropology in Oceania (ASAO) conferences between 2001 and 2005. Several contributors to these earlier conferences participated in a formal symposium, *The Gang of Four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in Multiple Contexts*, which convened in February 2005 during the annual meetings of ASAO on Lihu'e, Kaua'i Island, Hawai'i.

The papers from that symposium, which comprise this special issue, seek to understand the complex and multifaceted legacy of a unique group of anthropologists. Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead knew each other personally and intellectually. They all worked with or wrote about peoples of Oceania and beyond at a time when the anthropological endeavor was seen as an important social and intellectual contribution to the understanding of human cultures.

This group of anthropologists and their mutual encounters produced a wealth of books and essays that explored diverse topics, many of which prefigured contemporary disciplinary concerns. These groundbreaking publications include Bateson's *Naven* (1936); Benedict's *Patterns of Culture* (1934) and *The Chrysanthemum and the Sword* (1946); Fortune's *Sorcerers of Dobu* (1932a), *Omaha Secret Societies* (1932b), *Manus Religion* (1935), and *Arapesh* (1942); and Mead's *Coming of Age in Samoa* (1928), *Growing Up in New Guinea* (1930), and *Sex and Temperament in Three Primitive*

*Societies* (1935); and Bateson's and Mead's *Balinese Character* (1942), among other works. This is a considerable volume of research and writing. More importantly, the scholarly corpus produced by these four anthropologists remains a touchstone for Oceanists, in particular, and anthropologists and historians more generally. Margaret Mead's publications still have the capacity to inspire public interest, while anthropologists and historians continue to revisit Mead's enormous body of published and unpublished work (Molloy 2009; Shankman 2009a; Sullivan 2009; Tiffany 2009; Yans 2009). Academic interest in Mead has increased, inspired in part by Derek Freeman's (1983, 1999) famous attacks on Mead's analysis of Samoa (Shankman 2009b), while others have taken issue with her work in Melanesia. Gregory Bateson's *Naven* (1936) remains an inspiration for those who seek to understand the processes of relations (Guddemi 2006, 2007, 2009; see also Yans 2009). Ruth Benedict's classic, *Patterns of Culture* (1934), is required reading on college campuses, while her corpus of work inspires contemporary rereadings and analysis (Schachter 2009; Tannenbaum 2009). Reo Fortune, often viewed as the most obscure member of the Gang of Four, has long exerted influence, especially among *Kula* scholars and those who work among the Arapesh (Dobrin and Bashkow 2006, n.d.; Lohmann 2009; Munn 1986; Roscoe 2003).

As anthropological myth would have it, in late 1932 and early 1933, all four of these anthropologists were present in the mosquito room on the Sepik River for a moment every bit as iconic and transformative as Bronislaw Malinowski's exile in Kiriwina. Granted, Benedict's presence took the form of a draft of *Patterns of Culture*, but was no less significant for all that. In this moment, alliances shifted and new lines of work emerged (see also Yans 2009).

These four scholars—frequently categorized as members of the so-called "School of Culture and Personality"—are, in the editors' opinion, too often treated as if their work is completely understood thereby. In developing such a canonical approach, present-day anthropologists have too often neglected elements of the work, its influence, and the influences upon it. We hope that by drawing attention to some of these more neglected elements we can contribute to a reevaluation of the Gang of Four's work and we can suggest something of its breadth and continuing importance. We can learn as much, we think, from what has fallen through the cracks—from what has been forgotten—as from that which has been remembered and sometimes remembered too well.

Like the iconic, transformative moment in the mosquito room on the Sepik River, certain essays are easily placed in Oceania. Paul Shankman deconstructs Freeman's published narrative of his criticisms of Mead's

work, focusing on the absence of any reference to Mead in Freeman's master's thesis on Samoa. Sharon Tiffany rereads Mead's ethnographic bestseller, *Coming of Age in Samoa*, and finds a nuanced and multivocalic text. Phillip Guddemi examines Bateson's influence on a trio of Oceanic scholars: Roger Keesing, Robert Levy, and Roy Rappaport. Other essays in this collection range more widely while still referring to Oceania. Gerald Sullivan continues his analysis of Mead and Bateson's scientific project, following Mead's enquiry about the interactions between children and their caregivers, beginning with a memorandum Mead wrote in 1938 while working with Bateson among the Iatmul. Caroline Thomas provides Fortune with a much needed biography, examining not only Fortune's time in Oceania, but also the fallout from his break with Mead and, by extension, Benedict. Other papers concern earlier work, later developments, or both. Roger Lohmann considers Fortune's first book, *The Mind in Sleep* (1927), with Fortune's later ethnographic work in mind. Nicola Tannenbaum, a specialist on the Shan, examines Benedict's wartime study of Thailand, itself a part of the national character studies that can be traced back to the mosquito room moment and to Benedict's *Patterns of Culture* (1934). Judith Schachter looks to Benedict and the connections between her early study of Mary Wollstonecraft and *The Chrysanthemum and the Sword* (1946). By examining scholarly reviews of Mead's and Fortune's early ethnographies, as well as Mead's responses thereto, Maureen Molloy examines a past consensus of anthropological visions, methods, and tasks, as well as Mead's extensive role in articulating visions, methods, and tasks much closer to our own. John Gilkeson, an historian, looks to another important anthropologist of the period, Clyde Kluckhohn, and to the ethical implications of the sort of multicultural and multifaceted approaches developed in no small part by Bateson, Benedict, Fortune, and Mead.

Thus, for good reason, the works and lives of the Gang of Four (the editors' affectionate term for this quartet) continue to draw the attention of scholars working in several disciplines and genres. This attention is reflected in the current proliferation of biographies (Banner 2003; Lapsley 1999; Young 2005); interpretive collections (Caffrey and Francis 2006; Janiewski and Banner 2004; Tiffany 2005); academic works recently published or in progress (Crook 2007; Gilkeson n.d.; Lutkehaus 2008; Molloy 2008; Sullivan 1999); and numerous journal articles—many of which have been authored by contributors to this collection, and by others (e.g., Guddemi 2006, 2007; Ness 2008; Silverman 2005; Sullivan 2004, 2008; see also Yans 2009).

Our task in this special issue, then, is threefold: to examine the significance of the work by these four anthropologists, both individually and collectively; to examine the influence of Bateson, Benedict, Fortune, and

Mead upon other scholars (e.g., Gilkeson 2009; Guddemi 2009; Molloy 2009; Shankman 2009a; Thomas 2009); and to consider elements of their respective research that are often glossed over or forgotten (e.g., Lohmann 2009; Schachter 2009; Sullivan 2009; Tannenbaum 2009; Tiffany 2009). Virginia Yans, an historian and long-time student of the Mead Papers, considers what these essays taken together say about how anthropologists view their past in the present moment.

The coeditors of this project also have personal reasons for putting together this special collection. Gerald Sullivan first travelled to Bali in the late 1960s. He read Gregory Bateson's 1937 article, "An Old Temple and a New Myth," while still an undergraduate. Ultimately, his pursuit of Bateson, hence Mead, led him to the Margaret Mead Papers archived in the Manuscript Reading Room of the Library of Congress. His first day working with those papers was also Derek Freeman's last day; this would have been about 1992. Sullivan began a study of Mead and Bateson's Balinese materials, beginning with the village census for Bayung Gede, the highland village that had been Mead and Bateson's primary field site off and on from March 1936 until February 1939. Realizing that he did not know why Mead and Bateson had collected the materials they had, Sullivan read their research proposals from the Mead Papers. What Sullivan found was big science 1930s style, but big science of which he had not been previously aware. Mead and Bateson's scientific project has been the focus of Sullivan's research ever since. He met Sharon Tiffany in that same Manuscript Reading Room in 1995. His work now concerns not just the Gang of Four, but also the intellectual world within and against which Mead and Bateson's research grew, as well as the worlds of highland Bali in which these two worked so tirelessly.

Sharon Tiffany first read Mead's *Coming of Age in Samoa* (1928) and Benedict's *Patterns of Culture* (1934) as an undergraduate. Tiffany took a copy of *Coming of Age in Samoa* (in addition to *Social Organization of Manu'a*; Mead 1930 [1969]) to the field, where she conducted her doctoral research during 1969–1971 on Samoan social organization in the islands of Upolu and Savai'i in Western Samoa, with occasional side trips to American Samoa. During that first, transformative period of fieldwork, Tiffany met her fellow student and future colleague, Paul Shankman, a contributor to this special issue. Mead's texts inspired Tiffany's fieldwork in many ways: to observe the social processes of domestic relations and kinship ties for mobilizing labor power and resources for ceremonial redistributions of wealth; and to understand the dynamics of land and chiefly title disputes, based on Land and Title Court case files. Like Mead, Tiffany also received a *taupou* (ceremonial maiden) title, and she occasionally danced for her chief's visiting relatives and guests. Tiffany made over a dozen return field

trips to Samoa over a period of some twenty years. Tiffany's interest in the South Seas and popular media eventually led her to the Mead Papers at the Library of Congress in 1995, where she met Gerald Sullivan and other Mead scholars over the years. Her interest in gender and representation in the South Seas continues.

Many participants contributed their time and comments to the 2005 ASAO discussions and subsequent preparation for publication. We are, therefore, deeply grateful to the following persons: Mary Catherine Bateson for her continued interest and support of our endeavors; Phillip Guddemi, Nancy Lutkehaus, and Nancy McDowell for their incisive questions and commentaries during the 2005 ASAO symposium's proceedings; and Dale B. Robertson and Phillip McArthur, former and current editors in chief, respectively, of *Pacific Studies*, for their commitment to this project. The ASAO symposium in 2005 drew many interested participants, some of whom were unable to contribute essays to this special issue; their presence and comments are gratefully acknowledged. We would especially like to thank ASAO and its membership for kindly providing us with a venue for our mutual explorations. We dedicate this special issue to the late Mary Wolfskill, former Head, Reference and Reader Service Section of the Manuscript Division at the Library of Congress. Without Mary's efforts, encouragement, and friendship we would likely not have undertaken this project.

*Gerald Sullivan*
*Sharon W. Tiffany*
*September 2009*

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# INTRODUCTION: ON FOUR ANTHROPOLOGISTS AND THEIR HISTORIES

Virginia Yans[1]
*Rutgers University*

This introduction discusses ten essays concerning Pacific anthropologists Gregory Bateson, Reo Fortune, Margaret Mead, and their colleague, Ruth Benedict. The intellectual and personal engagement of these four social scientists with each other is well known and heavily documented in archival collections: they worked as "before-the-text" collaborators in fieldwork and in formulating culture theory. My Introduction highlights the epistemological and textual strategies the contributors to this special issue employ to describe the "Gang of Four." These essays demonstrate how a group of contemporary anthropologists think about the past and the history of their discipline. As a remedy for the disruption of disciplinary consensus and the declension in anthropology's scientific authority, ten scholars explore their ancestral past and occupy an epistemological middle ground or "vital center" between objective scientific authority and postmodern challenges to it. A revival of interest in anthropology's history appears to be related to a renewed interest in anthropology's potential to inform social change.

This collection consists of ten essays concerning four anthropologists: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead. Their research and writings spanned half a century of Pacific studies, beginning in the interwar period. Bateson, Fortune, and Mead began fieldwork during the 1920s. Mead initiated her fieldwork in Samoa in 1925 and continued her Pacific area research into the post–World War II era with one return visit to the Arapesh resettled at Hoskins Bay, New Britain; one to Bali; and two return trips to Pere village in the Admiralty Islands. Fortune began his research among the Dobu of New Guinea in 1927, and Bateson among the

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Baining of New Guinea in 1927. After their marriage, Mead and Fortune collaborated on Manus Island from 1928 to 1929 and in New Guinea during 1931–1933. Mead and Bateson later joined forces as husband and wife in Bali and New Guinea from 1936 to 1939. Benedict drew upon the field research of her three younger colleagues for her classic book, *Patterns of Culture*, published in 1934. She herself produced three "culture at a distance" studies of the Pacific region: *The Chrysanthemum and the Sword* (1946), her influential postwar analysis of Japan; and two short works on Burma (1943a) and Thailand (1943b [1952]) (Tannenbaum 2009; Schachter 1983, 270; 2009). These four anthropologists enjoyed rich and complex intellectual and personal relationships with one another. The husband–wife team expeditions of Fortune–Mead and Bateson–Mead, as well as Mead's professional exchanges and personal intimacy with Ruth Benedict, are well-known examples (Banner 2003; Caffrey and Francis 2006; Howard 1984; Lipset 1980).

This group of essays joins a recent revival of interest in the four anthropologists (Crook 2007: 121–54; Dobrin and Bashkow 2006; Stagoll 2004; Sullivan and Tiffany 2009). The Mead and Bateson Centennials of 2001 and 2004 caught the attention of anthropologists and other specialists who reviewed new and old editions of the four anthropologists' works in print and discussed their careers at conferences (Roscoe 2003; Tiffany 2005b). The centennials offered a welcome celebration, a palliative for weary American cultural anthropologists recovering from Derek Freeman's (1983) widely publicized attacks upon their studies. Surprisingly, given Freeman's critique of the accuracy of Mead's Samoan fieldwork, few anthropologists at the time investigated or verified her Samoan field notes and related documents available to researchers at the Library of Congress. According to disciplinary convention and habit, anthropologists prefer fieldwork to library studies. Some years passed before Samoa scholars and other Pacific ethnographers consulted the Mead Papers archived at the Library of Congress (e.g., Côté 1994; Lutkehaus 2008; Orans 1996; Shankman 2009a, 2009b; Sullivan 1999, 2004b; Tiffany 2001, 2005a, 2009). Symptomatic of what appears to be a historical turn for the discipline, several authors of the following essays are among those who have consulted the Mead Papers and smaller archival collections belonging to Bateson, Benedict, Fortune, and Clyde Kluckhohn (Gilkeson 2009; Guddemi 2009; Molloy 2009; Schachter 2009; Shankman 2009a; Sullivan 2009; Thomas 2009; Tiffany 2009). They draw upon a vast corpus of correspondence, fieldwork notes, manuscript drafts, correspondence, film, and photographs. Predicting their historical and scientific value to future researchers, including future fieldworkers who might wish to consult them, Mead systematically preserved

field notes, visual documentation, and correspondence relating to her Samoan efforts and to her collaborative work with Fortune, Bateson, and those who followed her to New Guinea—including Theodore Schwartz, Lenora Forestal, Lola Romanucci-Ross, and Fred and Barbara Roll. Mead included her personal papers as well. The Mead Papers and South Pacific Ethnographic Archives document almost thirty years of her collaborations with Ruth Benedict and a lifetime of personal and professional correspondence with many other social scientists. The Mead Archive is an unsurpassed collection of social science history. It holds an extraordinary record of the history of Pacific anthropology.

The four anthropologists featured in this volume helped lay the foundation for that field. However, the authors of these essays, some of them seasoned Pacific fieldworkers, set their sites on a different project, namely, the history of anthropological theory and practice as it manifested in four very different careers. Their subject is not Pacific peoples, but anthropologists who studied the Pacific. These histories are, to be sure, histories of a certain kind, and readers may reasonably require of them some ethnographic inspection, a useful inquiry to which I will return.

## Four Anthropologists in a Mosquito Room

We move now to the subjects at hand: the Gang of Four. This term, now a folkloric reference to Bateson, Benedict, Fortune, and Mead, originated in casual conversation between the coeditors of this volume at a seminar convened by the Association for Social Anthropology in Oceania (ASAO) in 2003: Sullivan and Tiffany agreed that a commonly used designation for the four anthropologists—the "Culture and Personality School"—constituted "badly digested . . . [and] assumed understandings" about their collective and individual work. It is widely known, as Sullivan states, that the four individuals were "very involved with each other," but their various collaborations with and influences upon each other, including their distinctive ways of thinking, remain surprisingly unexplored (E-mail correspondence to Yans from Gerald Sullivan, November 17, 2006, and May 24, 2009). In 2003, Sullivan and Tiffany used the Gang of Four as a kind of place marker for this volume and the larger intellectual project they hoped to initiate. The term continued to circulate, resurfacing now as a title for the present collection.

The title evokes these four anthropologists' complicated interpersonal and intellectual collaborations, including their occasionally competitive designs for the discipline's proper direction. Further, as multiple published references suggest, the term Gang of Four conjures a charged and

romantic—and some would say—constitutive moment in anthropology's history: an "event" in which all four were implicated (Crook 2007; Lipset 1980, 136f; Stocking 1986, 3; Sullivan 2004a, 2004b). This event occurred, as mythic occurrences so often do, in a space ordinarily situated outside of time—the fieldwork site. Something extraordinary happened, or so the myth tells us, in a screened mosquito room on the Sepik River (Boon 1985). George Stocking (1986, 3), the prominent historian of anthropology, called the Sepik River encounter between Bateson, Fortune, and Mead one of the discipline's "great moments." Actually, the event in question took place over three months of conversations, commencing soon after Mead and Fortune had moved on from their Arapesh and Mundugumor studies and located (with Bateson's assistance) among the nearby Tchambuli. Bateson—then eight months resident among the Iatmul people—was, according to Mead (1972, 209), "floundering methodologically," even as her marriage with Fortune grew increasingly problematic. Benedict, Mead's former mentor and intimate companion, was present during these conversations—but only figuratively—in the form of a text, a draft of her forthcoming work, *Patterns of Culture*. Benedict's ideas, according to Mead, Bateson, and Fortune, informed the three-way "mosquito room" conversations that followed (Bateson 1979; Mead 1972: 194–222; Thomas 2009).

Mead's (1972: 194–222) autobiography inscribed the mosquito room scene into anthropology's memory. As Mead framed her recollection, and as it has been subsequently remembered, personal biography and anthropology conspired in that now legendary space and moment to produce refinements in the culture concept and in anthropological theory:

> [C]ooped up together in the tiny eight-foot-by-eight-foot mosquito room, we moved back and forth between analyzing ourselves and each other, as individuals, and the cultures we knew and were studying, as anthropologists must. Working on the assumption that there were different clusters of inborn traits, each characteristic of a different temperamental type, it became clear that Gregory and I were close together in temperament— represented in fact a male and a female version of a temperamental type that was in strong contrast with the one represented by Reo. . . . The intensity of our discussions was heightened by the triangular situation. Gregory and I were falling in love (Mead 1972, 216).

The three anthropologists hammered out the beginnings of a new paradigm for understanding sex and temperament. They revised Benedict's

configurationist scheme of culture as personality "writ large" with a four-part typology used to describe the individual and his or her culture. The four-part scheme allowed Bateson, Fortune, and Mead to conjoin typologies/character (Mead's interest) with process/relations (Bateson's concern) and, moving beyond Benedict's configurations, to explain how social processes generated different, preferred typologies or characters; that is, a particular culture's gestalt (Crook 2007; Sullivan 2004b). Acknowledging the importance of heredity and sex differences, they assigned heavy significance to childhood socialization practices as a means to accomplish culturally prescribed sex roles and other behaviors. Mead and Bateson created the "squares hypothesis," a shorthand summary of the "personality" of different cultures and of specific individuals, including themselves (Lohmann 2009; Thomas 2009; Sullivan 2004a, 2004b). Mead's 1935 book, *Sex and Temperament in Three Primitive Societies*, described three of their variations; the fourth was yet to be found in Bali.

The Fortunes' marriage, already compromised upon arrival in New Guinea, collapsed (Thomas 2009). A new love affair began, and a new kind of fieldwork was soon to commence. Mead divorced Fortune and married Bateson in 1936. On their honeymoon, as the two set out for a three-year fieldwork expedition to Bali and New Guinea, Bateson corrected the publisher's proofs for his book, *Naven: A Survey of the Problems Suggested by a Composite Picture of the Culture of a New Guinea Tribe Drawn from Three Points of View* (1936). Bateson's work, considered a prototype of his lifelong involvement with cybernetic systems, prefigured his skepticism concerning induction as the most fruitful scientific method (Guddemi 2009; Stagoll 2004, 1038). In Bali, Bateson and Mead developed elaborate, coordinated fieldwork strategies incorporating both observational and reflexive commentary, a grand, pioneering experiment in visual anthropology that included thousands of still and cinematic images, and a more developed theoretical approach joining Bateson's interest in "process" to Mead's interest in "temperament" (Crook 2007; Sullivan 1999). After leaving the field, they wrote the book *Balinese Character: A Photographic Analysis* (Mead and Bateson 1942) and produced a series of ethnographic films on character formation, including *Trance and Dance in Bali* (Mead and Bateson 1951). These were collaborative, coauthored works, a break from their previous individually authored texts.

In retrospect, what actually took place in a Sepik River mosquito room and the significance of what happened there remains open to debate. Much of the event's received meaning finds its source in Mead's autobiography. By her accounts, the Sepik conversations represented a great disciplinary divide. Together these ambitious young fieldworkers challenged the way

that culture, personality, and gender roles would be understood, the way fieldwork would be conducted, and the way ethnographies would be written. Unsurprisingly, a disgruntled Fortune, betrayed by his wife and a male colleague, remembered the scene differently: he complained that the squares theory and Mead's Arapesh descriptions were not scientifically sound. Fortune's dissenting opinion could be attributed to his bitterness at a hurtful double betrayal. Still, as Fortune was quick to assert then and in subsequent published Arapesh ethnographic works, unlike what he considered Mead's and Bateson's wild psychological speculations, his objections rested upon empirical field research (Fortune 1939, 26; Lipset 2003; Thomas 2009). Subsequent critics of culture and personality studies offer still another appraisal: if the mosquito room represents a disciplinary divide, they see it as a wrong turn, and eighty years later, psychological anthropology is, indeed, a marginal field.

Still, as this volume's editors proposed, what actually happened among these three ethnographers and Benedict, their geographically distant mentor, justifiably merits greater attention. The foursome's continuing conversations with each other, documented in unusually rich archival collections, offer a rare opportunity to examine a surprisingly understudied subject in the history of science. Romantic, dramatic, and mythologized, the exchanges among this quartet of anthropologists may exemplify actual "before-the-text" collaborative processes, conflicts and agreements that can lead to hypothesis formation. Such a chapter remains to be written in the history of anthropology (Kennedy 1955: 26–33).

Readers may be surprised, as I was, to find that despite encouragement from this issue's editors to examine the four in concert, the authors gathered herein are not particularly concerned with what actually transpired in the mosquito room, nor with the Gang's collaborations as a foursome, or even consistently with the interwar period or "golden age" of Pacific anthropology. Nor, excepting discussions differentiating Benedict's and Mead's approaches to the individual and culture (Sullivan 2009), and of Benedict's financial support of Fortune's research and Mead's efforts to distance him from the American academic scene (Thomas 2009), is there much interest in how each anthropologist related to the ongoing work of any of the other three.

If not the four in collaboration, then, is there some other discernible, shared project animating our authors? Reflection upon the surface and subterranean levels of these seemingly disparate essays suggests a possibility. These essays tell us how some contemporary anthropologists think, specifically how they think about their discipline's past. How, then, does this "gang of ten" frame their discussions of the Gang of Four?

## What Kind of History Is Being Done Here?

These essays are concerned with the careers of individual anthropologists, with their writings, their fieldwork, sometimes with a solitary ethnographer's influence upon later generations, and occasionally with individual biographies. Individual author/agents, ethnographer/heroes, great men ("some of whom," as Regna Darnell 2006, 214, points out, "were women") occupy the pages of these essays. And, the autonomous, objective scientific research community progressing toward "better" science survives here, as well; it lives in these texts, seemingly quarantined from both post-Kuhnian and intertextual obliterations.[2] All these essays reveal a preference for examining anthropology from its interior and not, as some postcolonialists, political economists, and historians would prefer, within a broader political and economic context.[3] As others have observed, this internalist narrative implies a kind of scientific autonomy from external influences, itself a validation of anthropology's claims for scientific objectivity (Novick 1988, 533).[4] Given anthropology's enthusiastic embrace of various postmodern strategies, particularly cognitive relativism—and the particular kinds of cultural relativism and historicism so often accompanying cognitive relativism—our authors' dedication to such conventional intellectual and disciplinary history is surprising.[5]

George W. Stocking's (2004) discussion of both the history of anthropology as a field, and of his own efforts to accommodate late twentieth-century questioning of disciplinary scientific authority offers a useful perspective on how these authors position their reflections upon the Gang of Four. Stocking's commentary on disciplinary formation and its textual reproduction makes explicit some of our authors' assumptions and strategies. If I understand Stocking and our essayists correctly, they struggle to occupy a middle ground or "vital center," even as they accommodate the challenges of cognitive relativism and hermeneutic strategies into their reconstructions of the four ancestors and anthropology's past. In writing of historians engaged in similar struggles, intellectual historian Peter Novick (1988, 628, 626) astutely describes such efforts now visible across the disciplines as "restriction through partial incorporation." Stocking, for example, acknowledges that emphasis upon intertextuality at the expense of individual actors, writers, and authors has migrated to other scholarly areas, including anthropology. But, as he correctly suggests, "the situation . . . is different in the history of anthropology, which even when dealing with movements of thought has by and large focused on specific anthropologists" (Stocking 2004, 237). Stocking, a practitioner of conventional intellectual history (and its objectivist strategies), discusses his own efforts

to accommodate new "pluralistic," and "multi-contextual approaches" that "may enrich historical understanding." Demurring from both the "new historicism" (literary theory's gift) and "paleo historicism" (in the sense that Marxism and other major meta-systems have been spoken of as "historicist") Stocking (2004: 235, 238–39) remains dedicated to his own "non-evaluative" and "empirical" "personal credo."

Stocking lays out his own guidelines for writing the history of anthropology, some of which we see practiced within this collection:

> Insofar as possible, prior present interests should not be allowed to distort the interpretation of surviving evidences of the historical past; . . . the most important of these evidences are the words and actions of individual anthropologists; and . . . an understanding of these thoughts and actions may be enriched by considering them, in a conceptually eclectic manner, within the complexity of their various contexts: the inner psychological, the social interactional (institutional and disciplinary), and the cultural historical (Stocking 2004, 238).

In other words, "pluralistic" approaches are admitted, but scientific objectivity is never abjured. The author is not dead; the quest for objectivity marches on; biography and archival verification continue as viable textual strategies for reconstructing the past; and present-day interests must be held separate from these investigations.

Writing fifteen years after Stocking's perorations, our authors are still engaged in a Stocking-style accommodation—a middle of the road, "epistemological 'vital center'"—as a remedy for the disruption of disciplinary consensus and the declension in anthropology's scientific authority (Novick 1988, 628).

And, as it turns out, how anthropologists think about their past—in this instance how the ancestors, including the Gang of Four, are figuratively portrayed—may reveal something of how negotiations surrounding the vital center are proceeding early in the twenty-first century. There is, it appears, more at stake than the conflicts between objectivity and cognitive relativism evident in Stocking's commentaries, notably, the question of how these epistemological issues relate to anthropology's moral responsibility. This is a particularly troublesome issue for anthropology at this time and, I believe, this collection of essays moves some of its authors to look to the past, even to the exercise of examining anthropology's past, for inspiration (Molloy 2009; Schachter 2009; Tannenbaum 2009). Whereas postmodern critiques of anthropology defined the Gang of Four and other "believers" in "truth"

and the search for truth to be a problem, middle-ground accomodationists find in the ancestors, and in historical practice, potential solutions. Perhaps a new kind of historical turn has arrived.

## The Way We Think about the Way We Think

At this point, some historical reflection upon anthropology's earlier turns might be useful. How, after all, did we get here? World War II and the late twentieth century offer points of departure and comparison for the current accomodationist effort. The objective is to highlight alignments and realignments between scientific objectivity, cognitive relativism, and ethical relativism. How did anthropology's ethical mission and its thinking about the past as an antidote to anthropology's current ills figure into these confabulations (Guddemi 2009; Lohmann 2009; Sullivan 2009)?

Twenty-five years ago, commenting upon the shattering of a "used to be" consensual social science, Clifford Geertz (1983) sketched the synchronic arrival of attacks on "mainstream social science," the appearance of a variety of postmodern interpretive and critical strategies, and the "blurred genres" proliferating across all disciplines. Geertz asked four questions then of social science thought that prove useful in clarifying the meaning of the current ruminations about anthropology's past now before us: How do this volume's ten contributors go about knowing what they want to know? What do they want to know? What do they propose to do with what they know? Is something "happening," as Geertz (1983, 20) wondered, "to the way we think about the way we think"?

As I read them, the hybrid essays we have before us suggest a positive response to the last question. The other three questions will be dealt with in turn.

How do the authors in this issue go about knowing what they want to know? Several essays appear to be "blurred genres," demonstrating an affinity with the postmodern project Geertz described. For example, consider the authors' reliance upon both objectivist and interpretive readings of ancestral ethnographic canons: Benedict's *Patterns of Culture* (Schachter 2009; Sullivan 2009); Mead's *Coming of Age in Samoa* (Shankman 2009a; Tiffany 2009); *Sex and Temperament in Three Primitive Societies* (Sullivan 2009); Bateson's *Naven* (Guddemi 2009); and Fortune's writings on warfare (Lohmann 2009; Thomas 2009). Our authors use different reading and rereading strategies. These strategies involve very different projects and very different kinds of historical praxis. One, archivally based, uses textual remnants initially produced in the field or near the time that ethnographies were being written; it seeks to find the original intent of the

ethnographic texts being interpreted. The other approach finds satisfaction in reinterpretation of classic ethnographies irrespective of their original historical context. The first finds its heritage in historical practices emulating systematic scientific investigation; the second, a hermeneutic exercise, questions the value and the possibility of such discovery. The first is concerned with discovering the presumably stable meaning of the text, whereas the second takes as its object the relationship of readers and rereaders to the canonical texts and/or the location of ethnographic canons within contemporary ethnographic discourse. In accomodationist style, sometimes the two kinds of readings coexist blissfully within the same essays, untroubled by their epistemological contradictions. The authors in this volume, then, have decided that they know how they want to go about knowing what they want to know.

Unlike the post-1960s reconfiguration of social thought Geertz described, and in some ways antagonistic to that approach, the emphasis in several of these essays nonetheless moves in empiricist and humanistic directions. That is, for the most part, our authors adopt the strategies of what Geertz called "mainstream" scientific investigation and objectivity (Gilkeson 2009; Schachter 2009; Shankman 2009a; Sullivan 2009; Tiffany 2009). Their insistent reliance on archival sources and investigation, for example, implies a rejection of the postmodern antiscience heresy and a faith in the possibility of recovering the "true meaning"[6] and original intent of the four anthropologists at the time they wrote their ethnographies. These essays resonate more with Stocking's accommodation than Geertz's blurriness.

Acknowledging that the agenda before us is both pluralist and accomodationist, it is useful to ask of both historically inclined empiricists and postmodernists a question: What do they want to know about the past? (We are assuming, of course, that each group thinks they can know something about it, a proposition some postmodernists do not accept.) In my view, Stocking's accommodations answer for the historical objectivists. In the rest of this Introduction, I have chosen James Boon, dedicated hermeneutic analyst, as a second interlocutor. Conveniently, Boon (1985, 1999), both a cognitive relativist and critic of conventional or positivist historical strategies, has used the Gang of Four for his own orations.

With few exceptions, as we shall see, our writers weigh in (regardless of Boonian protestations) as dedicated and conventional intellectual historians. Certain themes regularly inflect these essays: ancestral lineages, influences, schools, or paradigms (Gilkeson 2009; Guddemi 2009); the center and the periphery of the discipline (Guddemi 2009; Lohmann 2009); and even biography (Schachter 2009; Guddemi 2009). They want to know about the anthropologist/author, but also about the discipline itself, each conceived as natural analytic constructs.

As Boon (1999, 74) and other critics of these conventional historical approaches have suggested, linear, internal narratives populated with casts of scholar-characters moving toward some holy grail of scientific consensus results in "artificially centered" histories of disciplines and movements within disciplines—made to appear inevitable through acts of retrospection.

I read the theme of "center and periphery," a leitmotif in these collected essays, as a coded reference to disciplinary consensus and authority. Our essays offer two innovative, nonlinear examples of original uses of this construct; both uses question Boon's presumption that the narrative itself creates centers that are not there, or are there only in retrospect. Writing about Fortune, for example, Molloy's use of book reviews of his work suggests that something like a disciplinary consensus actually existed. This conclusion results from her essay, regardless of her stated purpose of focusing upon the individual anthropologist's published works. Further, as Guddemi's essay shows, Bateson's generally accepted (and voluntary) "out of center" position implies that a center must have existed.[7]

Actually, the Gang of Four itself offers an interesting opportunity to examine the central and peripheral boundaries of the anthropological discipline. Each of the four ancestors was occasionally or consistently a marginal figure and transported to the discipline's outskirts for different reasons. Gregory Bateson's peripatetic career and transdisciplinary interests, his move to cybernetics, as well as his studies of alcoholism, schizophrenia, evolutionary theory, and communications theory, compromised his position within academic anthropology even as he garnered considerable attention outside of it. Reo Fortune, initially trained in a different national tradition, never successfully integrated himself into the American scene, even though he ultimately earned his doctorate at Columbia. Ruth Benedict scaled the boundaries between the center and periphery of the discipline. Her work and reputation still command respect today (Boon 1999: 23–42; Young 2005). However, Benedict owed her academic position at Columbia, where she worked without salary for several years, to Franz Boas, her mentor. Identifying herself as a person who did not fit in, as she surely did not, she wrote about deviance, and, according to some, it was her sense of herself as being deviant that drew her to cultural relativism (Banner 2003; Caffrey 1989: 254–55; Novick 1988, 144). Despite Boas's recruitment of women to anthropology at Columbia, both Mead and Benedict felt that their sex rendered them marginal. Margaret Mead, a museum anthropologist, did not hold a full-time academic position until late in life. Early in her career Mead failed to win an academic appointment at Harvard because the university's president did not want women on the faculty (Sullivan 2008, 221). Recognized as one of the twentieth century's most prolific fieldworkers, the

memorialized Mead nonetheless occupies an ambiguous position among anthropology's giants (M. C. Bateson 2005). Despite her shortcomings, Benedict's original contributions and her intellectual depth are widely acknowledged. Mead was lionized; Fortune has been virtually ignored; and Bateson is idolized, primarily outside the discipline of anthropology.

Phillip Guddemi (2009) describes an interesting inversion of the usual measure of "influence" upon a field, demonstrating the plasticity of the concept and the difficulties of identifying, even in retrospect, fixed, central, and peripheral disciplinary regions. Well after Bateson had apparently left academic anthropology behind, he went on to intentionally create a "liminal" position for himself vis-à-vis the discipline (Lipset 2005: 911–2). Unpredictably, Bateson's ideas migrated back into the discipline when a younger generation of Pacific anthropologists, including Roy Rappaport, Felix Keesing, and Robert Levy, recognized Bateson's theoretical power (Guddemi 2009).

With the exception of Judith Schachter and Caroline Thomas, our writers in this collection are not engaged in writing biography per se, a seeming concurrence with Boon's insistence that the biographical form narrates intellectual production as symptoms of a life. In Boon's (1999, 88) words, biography "does not explain the texts of those whose lives it presumes to reconstruct." As an alternative, Boon proposes a collaborative (not consensual) body of scholars and autodidacts as producers of the work and ideas that emerged from Bali in the 1930s. Collaboration, whether it is between husband and wife, investigator and subject, or jointly authored ethnographies, is common practice in the discipline. The larger organizing project of this collection, a Gang of Four, surely encourages notions of collective effort and joint authorship, but our authors prefer the modernist organizing framework of the individual anthropologist.[8] Clearly, the author as agent is not dead, but I would argue that this collection of essays is more appropriately described as "rehabilitative" or, as historians might put it, "revisionist" and not, as Boon would have it, "memorializing."

## Anthropology, Social Change, and Historical Praxis

This brings us to another question: What do our authors propose to do with what they know? Despite their pluralist eclecticism (and their recognition of it in the Gang of Four), these essays not only take up certain assumptions of scientific history, they use it to correct previous evaluations of the ancestors. Indeed, contributors to this collection express nostalgic yearning for the reformist "cultural watchdog" defender of values: those humanists

who, as Geertz (1983, 35) reminds us, were sent into exile along with social science experts and technocrats also dedicated to social change.

In these pages and in other publications, reconsideration of Mead's Samoan fieldwork (Shankman 2009 and Tiffany 2009); of Mead's Arapesh fieldwork (Roscoe 1995); of Mead's *Sex and Temperament* (Lipset 2003); of Mead's *Male and Female* (1949), and the "squares theory" (Sullivan 2004a, 2004b, 2009; Lohmann 2004; Roscoe 2003); of Bateson as scientist (Lipset 2005); and of Fortune as ethnographer and psychologist (Dobrin and Bashkow 2006; Molloy 2009; Thomas 2009) provide strong evidence of a rehabilitative project (see also Janiewski and Banner 2004). Mead and Bateson are recognized as pioneers of eclectic fieldwork. Mead is acknowledged here and elsewhere as an originator of a polyvocal ethnography that included women's voices (Schachter 2009; Tiffany 2001, 2005a, 2009).

Rehabilitation of each of the four ancestors implies reconsideration of the "Boasian" project to which Mead, Benedict, and Fortune—as Benedict's and Franz Boas's doctoral student—were connected. While Bateson ultimately eschewed what he considered to be Mead's social "tinkering," during the World War II era he joined scientists who were convinced that scientific knowledge could influence positive social and political outcomes (Yans 1986). This collection of writings about anthropology's past implicitly suggests a reinstatement of humanistic ideas, possibly even a reinscription of connections between scientific activity, praxis, and social commitment, a new turn that disables late twentieth-century critical anthropology's pairing of cognitive and ethical relativism and its rejection of Boasian humanism.

The humanism I find in these essays is not the kind initiated during Geertz's era of hermeneutic literary strategies. It is a pragmatic or "watch dog" humanism that Mead, Benedict, Franz Boas, and others of their generation paired with both objective science and cultural relativism (Gilkeson 2009; Yans 2004). In these pages and in recent discussions elsewhere, the nostalgia for pragmatic humanism is most pronounced in Mead's and Benedict's rehabilitations. Their dedication to a search for human values and for a better world redeems their shortcomings, including their blind dedication to the limitations of configurationist theory (Caldararo 2004; Gilkeson 2009; Schachter 2009; Shankman 2009a; Sullivan 2009; Tannenbaum 2009; Lutkehaus 2004; Molloy 2008, 2009). Mead and Benedict are both praised for their feminist sympathies (Schachter 2009; Tiffany 2009). Gerald Sullivan, one of this volume's editors, has described Ruth Benedict's kind of humanism as a concern for "conditions under which human freedom might flourish" (Sullivan 2008, 219; see also Young 2005, 89). I would also emphasize that the recent interest in Fortune's work

on warfare is not founded solely in ethnographic concerns but in humanistic ones (Dobrin and Bashkow 2006). As Roger Lohmann's essay in this volume demonstrates, even before Fortune left psychology for anthropology, he attempted to understand processes—in this case dreams—that encourage conformity to cultural norms. In analyzing his own dreams, Fortune reveals himself as deviant, a pacifist deeply opposed to the senseless violence of World War I (Dobrin and Bashkow 2006; Lohmann 2009; Molloy 2009; and Thomas 2009).

Looking back with twenty-first-century eyes, the conjoining of objectivity (and the scientific search for truth) with any sort of relativism—cultural or cognitive—seems oddly inconsistent. However, our Boasian ancestors, animated by historical circumstances, including twentieth-century warfare and Nazism's horrors, lived comfortably with the contradictions. As Peter Novick comments concerning Boas, Benedict, Mead, and others of their generation:

> Cultural relativism led directly to ethical relativism, since a non-judgmental attitude towards strange customs was its central core. It did not, logically, entail any form of cognitive relativism, and, indeed, a distinguishing hallmark of interwar social science was its combination of ethical relativism and objectivist empiricism. . . .
>
> Like historical relativism, cultural relativism had an instrumentalist, Progressive side which sought to make scholarship useful in the cause of social amelioration. One strand in cultural relativism—as in "historicism"—was a non-judgmentalism which, in a certain mode, was consistent with a purely spectatorial posture. . . . In practice Boas used cultural relativism as a weapon in his lifelong campaign against racism, Mead to criticize a competitive social order; Benedict thought that through cultural relativism we could "train ourselves to pass judgment on the dominant traits of our own civilization" (Novick 1988, 145, n. 16).

A review of a later, 1946 edition of Benedict's *Patterns of Culture* emphasizes the pressures brought upon cultural relativists and Benedict's response to the critique. The "Gold Star Mother," the reviewer Elgin Williams (1947) wrote, "is going to be reluctant about granting significance to Hitler's culture . . . and the remaining Jews of Europe . . . are going to be poor customers for gospels which hold that there are two sides to every question." Benedict and her colleagues had already clarified their stand by then, with Benedict writing of the war's disastrous proceedings: "'This

problem is beyond relativity'" (Williams 1947: 84–90; quoted in Novick 1988, 284).[9]

## History, Anthropology and Human Affairs

Now we turn to a question that Geertz did not ask: How is it that what our authors know they know is connected to what they want to do with what they know? The writers in this volume do not specifically address this question, but it is implied within their historical exercises and explicitly raises its head in the related controversies outside this volume concerning the Gang of Four.

Consider the recent anthropological controversy concerning Mead's and Fortune's disagreements over the Arapesh (Roscoe 2003). Fortune, of course, saw them as warriors, while Mead portrayed them as maternalistic. Elsewhere, anthropologist Niccolo Caldararo (2004) has recommended an historical reconstruction of what Mead knew and said at the time as a means for responding to her critics. Within the same essay, Caldararo takes another historical turn. He bemoans anthropology's failure to develop a long-term historical perspective on warfare that might advise on how to avoid it, a subject that Benedict (1939) thought worth considering (see also Young 2005, 95). Caldararo's words recall the Boasian social project: "Since we have decided to ignore the existence of any mechanics of social adjustment, we are left adrift unable to predict how societies will change and denying the existence of patterns in the past, in history, that can be used to frame actions in the present" (Caldararo 2004, 312). Naming anthropology's preference for an "atomized and fragmented study of components of culture" as a cause for this loss, he concludes: "ideologically, we have denied the value of history as a body of biased perceptions" (Caldararo 2004, 312).

Doing anthropology and doing the history of anthropology involves "framing" action in the present. The agenda and the argument are clear, not blurred; the case for historical objectivism and an ethical agenda for anthropology are clearly linked. The turn to history, indeed historical praxis itself, reconnects anthropology to human affairs.

The reasons for this reconfiguration in thinking about the past and, indeed, early twentieth-first-century anthropologists' reviving interest in it, may be located outside the internal disciplinary history that these essays emphasize. Today, as in the past, there is a crisis outside anthropology. We can look beyond the internal workings of the discipline itself to explain changes within it, including a new interest in the past, and a questioning of whether critical anthropology, the various postmodern strategies, and

their particular kind of ethical relativism are adequate strategies for our time. The Iraq war, torture, violations of human rights (including the rights of women and children), domestic policies favoring the production of wealth and protection of free markets before the well-being of citizens—each of these encourage reconsideration of what social scientists want to know and how they go about knowing it. A skepticism concerning the moral adequacy and social relevance of a postmodern social science enterprise dedicated to "analysis of symbol systems" the "anatomization of thought," text-building, and multiple perspectives emerges. Anthropology's "critical" position, it seems, is critical of the wrong things. Geertz's (1983: 34–35) triple epitaph: for the "cultural watchdog notion of what a humanist is;" for the death of the social scientist expert/reformer; and for empiricism comes into question. What we want to know, how we think we know it, and what we do with what we know—all of these may well be taking still another turn, and that turn appears to move anthropologists toward looking backward in their search for wisdom.

We can appreciate Gregory Bateson's and James Boon's insistence that the essays before us (and our reading of them) function on several levels. They are at once history in the European sense of narratives concerning heroes and heroines, as well as commemorative stories woven around events, heroes, relics, and ethnographic texts among these old remains. At times, these essays are hybrids of the two approaches to the past (Bateson 1937; Boon 1999). Bateson, writing during the 1930s and comparing Bali to the West, was interested in understanding how the uses and embodiments of the past—concrete symbols, such as a temple or myths about the gods, "magpie" collections of artifacts and documents or, in this case, a collection of essays—"function" in "uniting groups of people . . . all over the world" (Bateson 1937, 133). In our time, his daughter, Mary Catherine Bateson, riffs on her father's theme. Commenting on the use and misuse of ancestral figures like Mead as surrogates, Mary Catherine Bateson encourages looking beneath the surface of historical reconstructions. In both written texts and oral accounts, she observes, "It is common to use historical figures to represent the values that underlie an emerging understanding of history or as targets for criticism to dramatize flaws that still exist" (M. C. Bateson 2005, 165).

As usual, nothing is as it seems to be. Both the study of the ancestors and, indeed, the use of history are surrogates for something else. That is the way it has always been. Anthropology's late twentieth-century critique of itself attacked the monster (scientific anthropology) but emerged from the battle without a prize. Like the alligator under the bed, the monster was not there. And that is the way it has always been too.

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## NOTES

1. I would like to add a biographical note about myself as writer of this Introduction. I am a historian with a special interest in the history of twentieth-century anthropology. Mead and her colleagues have attracted my attention as individual biographies, as ethnographers, and as representatives of twentieth-century society and culture. My own "fieldwork" has taken place in the archives.

2. It should be noted that Shankman (2009a) does not portray a scientific community progressing toward greater "truth." Rather, Shankman offers detailed analysis of Derek Freeman's critique of Mead's Samoan research and demonstrates a less optimistic understanding of how science works.

3. Caroline Thomas (2009) does deal with Fortune and World War I, and Gerald Sullivan (2009) examines the social contexts of authors who influenced Mead's thinking at the time.

4. See also Boon (1999: 92–96) on grand narratives of the progressive unfolding of knowledge, biographical accounts, and other narrative devices.

5. See Novick (1988, 532, passim) for a discussion of this issue across the disciplines.

6. Lett (1997, 14), quoting Jarvie (1988, 429).

7. It is interesting to note in this connection that Darnell's (2006, 217) recent survey indicated that anthropologists do not think their discipline has a center.

8. Handler (2004) offers some examples on collaborators in anthropological practice.

9. See Yans (1986) on Bateson's, Benedict's, and Mead's acceptance of absolute values and their association with scientific engineering during World War II.

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# NARRATIVE, VOICE, AND GENRE IN MARGARET MEAD'S *COMING OF AGE IN SAMOA*[1]

Sharon W. Tiffany
*University of Wisconsin—Whitewater*

My contemporary reading of *Coming of Age in Samoa* explores Margaret Mead's experimental ethnography as a textual artifact whose social history may be interpreted within a framework situated in gender, time, and place. Mead's ethnography appeared to reinforce consumer-culture representations of female alterity and "free-love" in the South Seas, yet her text challenged these popular images with a radical counternarrative. Mead's case study approach to the problem of adolescence, as well as her fieldwork photographs, created a narrative and visual space that questioned the dominant anthropological discourse of her day. Mead's woman-centered book, combined with her publisher's astute marketing strategies, created a commercial bestseller that has acquired the status of "Ur-text" in anthropology. Eighty years after publication, *Coming of Age in Samoa* continues to generate both academic and public interest.

## Reading the Text

Published in August 1928, *Coming of Age in Samoa: A Psychological Study of Primitive Youth for Western Civilisation*, initiated Margaret Mead's career as an anthropologist, best-selling author, and public intellectual.[2] "At the time that I wrote it," Mead (1978b, 2) noted fifty years later, "I had no idea I was writing a book which would catch the imagination of the general public." Mead's anthropological research and publications, as well as her personal and professional relationships with

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Ruth Benedict, Reo Fortune, and Gregory Bateson, continue to generate both academic and public interest. In November 2001, the Library of Congress inaugurated the Mead Centennial Exhibition (Francis and Wolfskill 2001). Three biographies of Mead and Ruth Benedict were published between 1999 and 2005 (Banner 2003; Lapsley 1999; Young 2005). These works were supplemented with a collection of essays on the Mead–Benedict legacies (Janiewski and Banner 2004), a centennial special issue on Mead's contribution to Pacific ethnography (Tiffany 2005b), and selections from Mead's correspondence (Caffrey and Francis 2006). Additional analyses of Mead's work and life in the context of American culture and the public media have been recently published (Lutkehaus 2008; Molloy 2008). Mead's anthropological "Ur-text," a term I borrow from Janiewski and Banner (2004, 153), has also crossed academic boundaries and entered the "global cultural ecumene" (Appadurai and Breckenridge 1988, 5). A Google search of *Coming of Age in Samoa* (accessed August 2, 2009) resulted in 51,500 sites.[3] The fact that an anthropological text published eighty-one years ago (as this article goes to press) continues to generate such interest beyond the rarified domain of academe is a rare phenomenon indeed.

Despite the commercial success of Mead's first ethnography and her stature as a public intellectual, *Coming of Age in Samoa* (hereafter *COA*) served to brand Mead as academically suspect among anthropology's gatekeepers (Lutkehaus 2004; Molloy 2009; Yans 2004). A contemporary reading of Mead's book as a textual artifact must, therefore, consider the gender politics of women writing about culture from the margins of academe (Lamphere 2004; Rohatynskyji and Jaarsma 2000). Part of my task in this essay, then, is to consider *COA* as an experimental ethnography that created a discursive space privileging female lives and experiences.

Mead's work also has a social and intellectual context. Occasional references to Bronislaw Malinowski's ([1922] 1984) pioneering text, *Argonauts of the Western Pacific* (hereafter *Argonauts*), published six years before *COA*, allow us to contextualize Mead's awareness of her ethnographic role as "literary artificer" (Stocking 1983, 105). Mead (1969, xv; 1972, 159) commented many decades later that she had not read Malinowski's book prior to her fieldwork in Samoa (see also Sanjek 1990: 215–8). Yet, as we shall see, there are many narrative parallels between *COA* and *Argonauts*—ethnographies written during the sexual revolution of the "Roaring Twenties." Both authors studied psychology; both were concerned with social issues of the day, such as women's rights to contraception and divorce; and both wrote mythic texts that continue to inform the contemporary ethnographic enterprise.[4] Mead's text thus provides insight into the narrative structures of professional ethnographic writing emerging in the 1920s,

as well as an understanding of the historical contribution of *COA* to understanding current social issues of the day associated with the New Anthropology (Gilkeson 2009; Lyons and Lyons 2004: 155–215; Murray and Darnell 2000; Stocking 1992; Yans 2009).

## First Encounter

There is no "arrival story" in *COA*, unlike Malinowski's famous trope in *Argonauts* of being cast alone upon a remote beach in the Trobriand Islands—a trope that obscured the narrator's retreats to the local trader's compound for respite from his ethnographic endeavors (Malinowski [1922] 1984, 4; cf. Clifford 1986: 37–9, 42; Pratt 1986: 37–8; Stocking 1983, 108). Mead, however, offered no narrative artifice of a heroic castaway's "first encounter" with a pristine South Sea island. Rather, her commentaries—written over a span of fifty years—emphasized personal experiences and detailed observations of peoples and places in her correspondence, her Samoa Field Bulletins (group letters circulated to family and friends), and published works.

Mead's encounter stories begin with her self-representation as the financially impoverished neophyte who sailed off alone to Polynesia. After a two-week stopover in Hawai'i, Mead reached her final destination five days later. She noted a "cloudy daybreak" as the *S. S. Sonoma* entered the spectacular harbor of Tutuila Island. The harbor, set in a submerged volcanic crater, was filled with naval ships: the American fleet had arrived just hours earlier (Samoa Field Bulletin, no. 4, p. 1, August 31, 1925 [Library of Congress, Margaret Mead Papers, box N1]). Mead's early correspondence from Samoa contrasted the scenic beauty of the harbor area with the colonial presence. Writing on the same day of her arrival in American Samoa, Mead's Field Bulletin described her initial impressions of the port town of Pago Pago:

> The Navy have really done nobly in preserving the native tone; . . . only the arial [radio] stations and one smokestack really damage the scene. The presence of the fleet today skews the whole picture badly. There are numerous battleships in the harbor and on all sides of the island, mostly not in the harbor because they make the water oily and spoil the governor's bathing. Aeroplanes scream over head; the band of some ship is constantly playing ragtime. (Samoa Field Bulletin, no. 4, p. 1, August 31, 1925 [Library of Congress, Margaret Mead Papers, box N1], spelling in original; reprinted with editorial modifications in Mead 1977, 23)

Mead's description holds no promise of a pristine island world. Her arrival set off a swirl of gossip in the port town. In a letter to Ruth Benedict, written a few days after debarking in Samoa, Mead linked the local gossip to her ambivalent marital status and conspicuously absent husband, Luther Cressman:

> And this sweet little group of gossips are just seething with speculation as to why I "left my husband." Of course, they are sure I have. And I know I oughtn't to mind but it's so depressing to be greeted with suspicious unfriendly glances. (Mead to Ruth Benedict, letter dated September 11, 1925, quoted in Caffrey and Francis 2006, 54)

Mead (1931, 98) wryly noted a few years later that the "very polite and very disapproving" naval officers were the source of "many rumors about what I had come to Samoa for, and all of them said that I could not live with the Samoans."[5] Decades later, Mead revealed that the chain of disapproval over her presence in the Islands reached to the highest levels of colonial authority. She had received a "frosty reception" from the Governor of American Samoa, "an elderly and disgruntled man who had failed to attain the rank of admiral" (Mead 1969, xviii; 1972, 147).

Colonial personnel, presented as Mead's "'stock of straw men'" (quoted in Stocking 1983, 108), served to highlight the ethnographer's social distance from the local authorities and her disdain for their "ridiculous" and "frightening" tales about Samoans (Mead 1931, 98; 1972: 147–8). Malinowski's introduction to *Argonauts* contains passages critical of expatriate attitudes and behavior toward Trobriand Islanders (Malinowski [1922] 1984: 4–6; see also Lyons and Lyons 2004: 174–8; Stocking 1983: 108–9). Mead's text, by contrast, is silent on these issues, aside from formally acknowledging the cooperation of the Naval Commander, Owen Mink, and other medical personnel in assisting her fieldwork enterprise.[6] However, as her later publications indicate, Mead did not attempt to obfuscate her dependence on government personnel for help and favors, which included letters of introduction and commissary privileges (Mead 1969, xviii; 1972, 142, 147; 1977: 19–20, 31). In addition, medical authorities arranged for Butterfly, an English-speaking Samoan nurse, to provide free language instruction to Mead during her six-week stay in the port town, and they arranged for Mead's housing at the naval dispensary on Ta'ū Island in the Manu'a Archipelago (Mead [1928a] 1961: i–ii; 1931, 96; 1972: 147–51; 1977: 19–20, 24–5, 28–9). The same holds for Mead's fieldwork photographs, which show, for example, items of trade store cloth, tin wash tubs, and telephone poles on the Island of Tutuila (Tiffany 2005a).

Mead's subsequent versions of her arrival story emphasize her modest economic circumstances: "I landed in Pago Pago with four dollars and fifty cents in my pocket and a devout hope that a check would have arrived on the same boat," she wrote, three years after publication of *COA* (Mead 1931, 95).[7] Mead's living expenses during her fieldwork were paid from a National Research Council fellowship, doled out in monthly checks of $150, and dependent on the vagaries of shipping schedules (Mead 1972, 148; 1977, 19). Edward Sherwood Mead paid his daughter's round trip travel expenses to the South Seas, and Mead ([1928a] 1961, i) duly acknowledged his financial assistance in *COA*.

Mead's arrival story, expanded a half-century later in her 1977 introductory comments to the Samoa section in *Letters from the Field*, cited her inexperience: "But I myself had never been abroad or on a ship, had never spoken a foreign language or stayed in a hotel by myself. In fact, I had never spent a day in my life alone" (Mead 1977, 19). Accommodations in the port town were, at best, marginal. Mead ate solitary, "dreadful meals" served by the "sad-eyed" cook in a "ramshackle hotel" run by a "young half-caste," "amid a generally uncooperative atmosphere" (Mead 1931, 96; 1969, xviii; 1972: 147–8; 1977, 25). The hotel's claim to fame was its setting for *Rain*, Somerset Maugham's short story of missionary zeal and lust. Mead (1969, xviii; 1972, 147) saw a theatrical adaptation of Maugham's story in New York before her departure for the South Pacific.

### *"I Don't Dare Cry—and I Cry All the Time"*

By contrast to these scattered comments of ethnographic encounter, many of which were written decades after the fact, Mead's writing in *COA* presumes a self-assured author in control of the research project. The reader finds no hint in the text itself of the fieldworker's loneliness, self-criticism, and anxiety, the "*sine qua non* of ethnographic knowledge" (Stocking 1983, 106). One must, therefore, look to sources other than Mead's ethnography for such information: these include her rigorously edited autobiography, *Blackberry Winter*, and her Samoa Field Bulletins—the contents of which "were fairly evenly balanced between pain and pleasure" (Mead 1972, 151). In addition, there is Mead's published and unpublished correspondence, notable at times for its "heavy stress on points of difficulty" (Mead 1972, 151; 1977: 10–2). While in Samoa, Mead wrote almost daily letters to Ruth Benedict, her mentor, collaborator, and intimate partner, noting in *Blackberry Winter* that "[I]n my letters to friends I laid such heavy stress on points of difficulty that Ruth concluded I was having a hard and

disappointing time" (Mead 1972, 151). The two anthropologists' extensive correspondence, much of which the Library of Congress made available to scholars in 2001, provided an expressive outlet for Mead's homesickness and self-doubts while she was in the field (cf. Mead 1972, 142, 151; 1977, 12; Banner 2003, 240; Caffrey and Francis 2006: 54–7).[8] Benedict, writing from her own research site in Cochiti, New Mexico, described antidotes to the personal travails of fieldwork for Mead, newly arrived in Samoa:

> Develop all the expedients you can against weeping—companionship is only one of them. I've had excellent ones: they range from brushing your teeth and gargling your throat with every onset, to playing you're your own daughter for a year. (Benedict to Mead, letter dated September 5, 1925, quoted in Mead 1959, 301)

Mead's subsequent letter to Benedict, written from Ta'ū village in Manu'a, highlighted the emotional turmoil of the ethnographer's personal life and her fieldwork enterprise:

> I'm just unmitigatedly miserable and my head aches so I can't really think. It's the hottest day we've had this year. The room is full of flies, alive and dead. I have to beat off hordes of visiting Samoans. I don't dare cry—and I cry all the time.
>
> . . . I feel so helplessly beaten that I could almost decide to come home and give it all up. . . . I didn't half hope to be successful anyhow. And now—you can't get peoples' [Islanders'] inmost secrets out of them if you're on the verge of bursting into tears any moment. (Mead to Ruth Benedict, letter dated December 7–9, 1925, quoted in Caffrey and Francis 2006: 55–6)

A year before her death, Mead briefly revisited the topic of personal angst in the field, declaring in her 1977 Introduction to *Letters from the Field* that, "It would have made no more sense to broadcast one's miseries than to have cast messages onto the waters of the wide Pacific, hoping for some uplifting return three or four months later" (Mead 1977, 12). Emotional difficulties aside, her 1949 preface to the Mentor edition of *COA* provides one of the few published instances in which Mead, writing in the third person, described her fieldwork in Samoa as "plunge[ing] all alone into the language and customs of an alien people," while the ethnographer's "eagerness was very thoroughly tempered with apprehension" (Mead 1949, ix):

> For my first two months in Samoa, as I learned to speak the language, eat the food, and use and interpret the postures and the gestures of the people, I found myself often saying under my breath, 'I can't do it. I can't do it.' One day I noticed that I was no longer saying this in English but in Samoan, and then I knew that I could. (Mead 1949: ix–x)

Despite such tribulations, the anthropologist prevailed in her ethnographic endeavor. Publication of *COA* demonstrated that a woman who embarks on a quest "dares to reinvent herself" (Heller 1990, 1). The "White Princess of the South Seas" had indeed "collect[ed] out of the darkness stories never heard before" (Brand ca. 1926, 98; Heller 1990, 1; Tiffany unpubl. data).[9]

## Fieldwork

Mead set out for the South Seas in 1925 with a research project that had no precedent. Franz Boas, Mead's professor and mentor at Columbia, emphasized the innovative nature of Mead's fieldwork, both in his Foreword to *COA* (Boas 1928: iii–v) and in his earlier correspondence to Mead while she worked in Samoa:

> I am fully aware that the subject that you have selected is a very difficult one and is, I believe, the first serious attempt to enter into the mental attitude of a group in a primitive society. . . . I believe that your success would mark a beginning of a new era of methodological investigation of native tribes. (Franz Boas to Mead, letter dated November 7, 1925 [Library of Congress, Margaret Mead Papers, box B2]; online Côté 2004)

Mead reiterated Boas's comments about her pioneering fieldwork in subsequent prefaces to *COA* (Mead 1931: 94–97; 1973: vii–viii; 1977: 19–20) and throughout her career (cf. Murray and Darnell 2000: 563–5; Stocking 1983: 240–2). Writing for *Redbook Magazine* just months before her death, Mead returned to the theme of her Samoan research:

> [N]o one before me had tried to gain a scientific understanding of the life of young girls in another culture. I had to invent my methods as I went along and find out for myself whether my discoveries were genuine ones. Looking back, I think that took a certain steadfastness. (Mead 1978a, 40)

The introductory chapter in *COA* illustrates this "steadfastness." Emphasizing "this new experiment on the primitive adolescent girl," Mead cites both the lack of anthropological knowledge about the girl child and the importance of a woman-centered approach:

> Because I was a woman and could hope for greater intimacy in working with girls rather than with boys, and because owing to a paucity of women ethnologists our knowledge of primitive girls is far slighter than our knowledge of boys, I chose to concentrate upon the adolescent girl in Samoa. (Mead [1928a] 1961, 9)

Mead's ethnography, as well as her psychological training, would shape her subsequent research, as well as influence the work of her contemporaries who "attempted to deal with new insights emanating from Freud" (Gerald Sullivan, email communication to Sharon Tiffany, April 21, 2006; cf. Darnell 1977; Francis 2005; Gilkeson 2009; Molloy 2004; Sullivan 2004, 2009).

Most of the Introduction (chapter 1) of *COA* serves to familiarize the general reader with the anthropological perspective and its methods, and contrasts these with the dominant scientific paradigm of the "controlled experiment" (Mead [1928a] 1961, 5). Mead then addressed the study of adolescence as both a biological and cultural issue, casting her research problem as rhetorical questions: "Are the disturbances which vex our adolescents due to the nature of adolescence itself or to the civilisation? Under different conditions does adolescence present a different picture?" (Mead [1928a] 1961, 11).[10]

Describing *COA* fifty years later as "the least dated" of her books, Mead (1978b: 2–3) attributed the ethnography's "unexpectedly long life" to its subject matter. Noting that "the troubles that beset our adolescents in the 1920's are still with us," Mead (1978b: 2–3) alluded indirectly to the problematic issue of female sexuality. This topic would later become a focal point of academic and public debate with publication of Derek Freeman's (1983, 1999) contentious counternarratives about Mead's fieldwork in Samoa (Côté 2005; Shankman, 2009a, 2009b; Tiffany 2001, 2004). Indeed, Mead's proposed research had elicited criticism even prior to her arrival in Samoa. Herbert E. Gregory, then Director of the Bishop Museum, met with Mead during her stopover in Hawai'i and denigrated her project as the study of "'low things' like childhood and adolescence," (quoted in Mead 1969, xvi). Mead never forgot Gregory's comment, citing it forty-four years later in her 1969 introduction to the second edition of *Social Organization of Manu'a*.

Chapter 3 of *COA*, "The Education of the Samoan Child," illustrates Mead's interest in topics of little interest to mainstream anthropology of the day. The chapter begins with a literary hook: "Birthdays are of little account in Samoa" (Mead [1928a] 1961, 20). This assertion is immediately qualified in the next sentence by noting that birthdays of high status babies are different: "But for the birth itself of the baby of high rank, a great feast will be held, and much property given away" (Mead [1928a] 1961, 20). The chapter continues with a detailed discussion of breastfeeding and infant food, childcare, child nurses, and children's work—rare subject matter for ethnographies of the 1920s, but consistent with Mead's experimental writing about domestic life. Consider, for instance, the author's careful details about infant feeding, suggesting a cultural parallel with the bygone era of a rustic life way:

> [T]he food is either masticated by the mother and then put into the baby's mouth on her finger, or if it is liquid, a piece of bark cloth is dipped into it and the child allowed to suck it, as shepherds feed orphaned lambs. (Mead [1928a] 1961: 21–22)

Such closely observed descriptions in *COA* illustrate Mead's determination to create a narrative space for childhood and adolescence in ethnography, despite academic disparagement of "low brow" or "female" topics of sentiment and domesticity (Hirschfeld 2002; Lutkehaus 2004). Just two years after publication of *COA*, Mead had completed fieldwork with her second husband, Reo Fortune, in the Admiralty Islands (1928–1929) and among the Omaha of Native North America during the summer of 1930 (Molloy 2009; Thomas 2009). Meanwhile, *COA* remained a best-seller. *Growing Up in New Guinea: A Comparative Study of Primitive Education*, Mead's second ethnography based on her research in Manus, was published in 1930. Like *COA*, *Growing Up in New Guinea* received considerable media attention and mixed academic reviews (see also Molloy 2009; Tiffany unpubl. data).

Three years later, Mead published an impassioned argument for the study of domestic relations in the *American Anthropologist*, using ethnographic details based on her own fieldwork experiences. Mead pressed her case for greater ethnographic attention to the "unformalized aspects of culture" that center on the everyday lives of people (Mead 1933, 1). "In the traditional monograph," she declared, "it is still regarded as adequate to dismiss 'family relations' with a paragraph and 'child training' with a page" (Mead 1933, 1). Little anthropological research on these topics would occur until decades later with the publication of two

important multidisciplinary studies, *Mothers of Six Cultures* (Minturn and Lambert 1964) and *Children of Six Cultures* (Whiting and Whiting 1975; see also Gilkeson 2009).

## A Room of Her Own

Mead took two photographs of her room, the back porch of the naval dispensary occupied by the Chief Pharmacist's Mate, Edward Holt, and his family on Ta'ū Island (Mead 1972, 150; 1977: 36–37; Figure 1).[11] This domestic space—Mead's version of Malinowski in "the ethnographer's tent"—was crucial to her fieldwork enterprise (cf. Malinowski [1922] 1984: 6–8). Mead described her room nearly half a century later in *Blackberry Winter*:

> A lattice separated my bed from the dispensary porch and I looked out across a small yard into the village. There was a Samoan-type house in front of the dispensary where I was to work with my adolescents. A Samoan pastor in the next village presented me with a girl who was to be my constant companion . . . . (Mead 1972, 150)

Mead also mentioned her room in the Acknowledgments section of *COA*, emphasizing the value of her living area as "an absolutely essential neutral base from which I could study all the individuals in the village and yet remain aloof of native feuds and lines of demarcation" (Mead [1928a] 1961: i–ii; see also Mead 1972: 150–1; cf. Stocking 1983, 97).

Mead's photograph of her screened room on the veranda reveals a bed with mosquito netting, a curtained window (with the lower half covered with a pandanus skirt), a bark cloth-lined ceiling, and most importantly, a table, the sacred space for writing (Figure 1). There are pictures on the wall, including one of Franz Boas: "Between dances they [the visiting children] look at my pictures—I am going to have to put Dr. Boas much higher on the wall, his picture fascinates them" (Samoa Field Bulletin, no. 9, December 11, 1925, p.2 [Library of Congress, Margaret Mead Papers, box N1]; reprinted in Mead 1977, 40; see also Mead 1972, 153).[12]

Mead's ethnography does not indicate where or when she wrote. Other sources, however, reveal that she wrote prodigiously while in the field. She kept meticulous field notes and typed extensive Field Bulletins (archived at the Library of Congress) to share with a "varied, known and loved group of people" (Mead 1977, 9). Mead also managed an enormous personal and professional correspondence while in Samoa, receiving as many as seventy

FIGURE 1. **"Margaret Mead's Room at the Naval Dispensary." Untitled photograph by Margaret Mead, 1925–26, Ta'ū Island. (Photograph reproduced from the Margaret Mead Archives, Manuscript Division, Library of Congress, Washington, D.C. Courtesy of the Institute for Intercultural Studies, Inc., New York.)**

to eighty letters every six weeks by boat (Mead 1977, 9, 15). In her introduction to *Letters from the Field*, Mead (1977, 9) reminisced: "I would sit and stare at [the mail], spread out on my bed, bracing myself for whatever news they brought, whatever questions they raised."

Mead's engagement with her writing was, no doubt, evident to the Islanders who visited her, especially the children, who "gather[ed] to peek through the holes" of the bamboo screen dividing her room from the porch, and "chatter[ed] endlessly in Samoan about Makelita's various belongings" (Mead 1977, 37). We know, for instance, that Mead wrote in her room, despite repeated interruptions by visitors, and that she also wrote on the dispensary porch. At other times, Mead typed her bulletins "standing up at the sideboard" when she "was shooed out of the Dispensary" (Samoa Field Bulletin, no. 9, p. 2, December 11, 1925 [Library of Congress, Margaret Mead Papers, box N1]). The ethnographer's living space, then, was rarely as quiet as Mead's photograph in Figure 1 suggests.

Writing to her brother, Richard, Mead described the continual parade of Islanders in her room:

> It's always a gamble whether the next guest will be a grey beard or a child, a lunch bearer or a prize fighter, or a mother with a howling infant in her arms— they come for paper; for cigarettes, for rubber bands, for string, for candy . . . because they are my dear friends, to bring me bouquets; at all hours, and in all costumes. (Margaret Mead to Richard Mead, letter dated December 14, 1925 [Library of Congress, Margaret Mead Papers, box R7])

Mead's correspondence mentions her crowded domestic space—"half of the back porch of the dispensary quarters"—a situation that occasionally drove her to secrecy for writing time: "I have no trouble in gathering the girls about me: in fact I'm hiding in the Dispensary in order to write this letter in peace. My porch room is crowded from dawn to midnight with all and sundry maidens" (Mead to Franz Boas, letter dated November 14, 1925 [Library of Congress, Margaret Mead Papers, box I2]; reprinted in Mead 1977, 37; online Côté 2004). Evening was also the time for dancing in the ethnographer's living space:

> At night I push back the curtain which divides my room off at the other end, put away the chairs, push back the tables and there is plenty of room for a small *sivasiva*—dance. The young people bring their guitars and ukeleles and dance for me. A few new ones come every night and it gives me an excellent opportunity

> gradually to learn their names. (Samoa Field Bulletin, no. 8, p. 2, November 14, 1925 [Library of Congress, Margaret Mead Papers, box N1]; reprinted in Mead 1977, 37)

What we learn, then, from Mead's correspondence and Field Bulletins is the significance of the ethnographer's domestic circumstances to her fieldwork and writing. Much of the ethnographic information published in *COA*, including the material on children and dance in chapter 8, was observed and recorded in Mead's room—an agreeable place for young people to congregate. Yet, Mead's occupation of this space was also fraught with personal difficulties. Domestic tensions between Mead and Edward Holt escalated over time. The Chief Pharmacist's Mate eventually "declared his house off-limits" to the Samoan children and adolescents who crowded on the dispensary porch as Mead's research timetable was concluding (Banner 2003, 242). Mead's correspondence to Benedict expressed "relief" at departing the Ta'ū dispensary ahead of schedule (Banner 2003, 242).

## Science and the "Personal Equation"

Mead's text focused on a research topic that required her to spend considerable time with her young subjects (Mead 1972: 138–9, 144). Mead later referred to her age (twenty-three), diminutive stature (five feet, two-and-a-half inches), and bobbed hairstyle (which, she noted, Samoan teens also wore) as assets in her fieldwork with Samoan youth (Mead 1931, 95; 1969, 228; 1977, 19). Three years after publication of *COA*, Mead described her fieldwork experience in Samoa as a process of self-transformation and self-representation:

> I wasn't very sure how I would succeed in this strange kind of adventure, this adventure of shedding all one's own ways of eating, sleeping, talking, laughing, just as if they were an old skin instead of the most important part of one, and putting on the attitudes of a Samoan girl, as easily as if they were only a party dress. (Mead 1931: 95–6)

None of these reflections, however, appeared in *COA*. Mead's authorial voice ([1928a] 1961, 11) reiterates the research problem throughout her introductory chapter, asserting that ethnographic details were "selected always with a view to illuminating the problem of adolescence. Matters of political organisation which neither interest nor influence the young girl are not included" (Mead [1928a] 1961, 11). Five appendices, described as

"impersonal, cast in the mode of the technical book," present the author's scientific bona fides to her general readers and academic colleagues (Mead 1973, xi). Appendix 2 ("Methodology of This Study") emphasizes that the ethnographer's "generalisations are based upon a careful and detailed observation of a small group of subjects," supplemented with "case histories" (Mead [1928a] 1961: 260–1).

Mead's text also discusses the "personal equation" in drawing conclusions based on ethnographic data collected from a sample of sixty-eight girls between nine and twenty years of age (Mead [1928a] 1961: 260–1). Her conclusions are "the judgments of one individual upon a mass of data, many of the most significant aspects of which can, by their very nature, be known only to herself" (Mead [1928a] 1961, 261). Malinowski ([1922] 1984, 3) also addressed this issue, depicting the ethnographer as "his own chronicler and the historian at the same time." Mead's measured statement concerning the tension inherent in her fieldwork endeavor and the representation of that endeavor to Western readers differs in tone from Malinowski's famous, if rather muscular assertion: "In Ethnography, the distance is often enormous between the brute material of information . . . and the final authoritative presentation of the results" (Malinowski [1922] 1984: 3–4; see also Stocking 1983: 104–5). Like Malinowski's introduction to *Argonauts*, Mead's appendices further remind the reader that *COA* is a multilayered text, a story of Samoan adolescence *and* of anthropological research, in which the author acknowledges her interpretations as part of the ethnographic process (see also Sanjek 1990: 215–26; Sullivan 2005 and unpubl. data).

*Narrative Strategies*

Mead's book illustrates the challenges of writing a problem-oriented, woman-centered narrative that could be both literary and scientific. Ethnographies produced within mainstream anthropological discourse imposed an authoritative voice, framed in the language of objectivity and neutrality, in recording scientific data about the lives of other peoples (Behar and Gordon 1995). Mead understood the authenticating power of scientific language. She wrote in her 1949 preface to *COA*, for example, of "the special quality of that first attempt to see the life of a very different people, both as they saw it and as they could never see it" (Mead 1949, ix). Mead used the authorial voice in *COA* to persuade readers of her own ethnographic authority to observe, record, and write about her group of Samoan girls in a way that they themselves could not do: to weave "the ethnographer's magic" by transforming the experiences of Samoan girls into the reader's experience, thereby creating in this instance a text both scientific and literary (Stocking 1983, 106; Malinowski [1922] 1984, 6).

The first chapter of *COA* establishes Mead's ethnographic credentials to tell her story of the life course of Samoan girls, just as Malinowski used his first chapter in *Argonauts* to prepare the reader for the narrative of the *kula* journey that was to follow (Stocking 1983: 106–7). Mead explains how she contextualized the lives of Samoan girls whom she studied on Taʻū Island. She "gathered many detailed facts" about her subjects, noting that "these routine facts," presented in the book's appendices, "are only the barest skeleton" that must be clothed, as it were, with closely observed details of the girls' "family situations and sex relations, standards of friendship, of loyalty, of personal responsibility" (Mead [1928a] 1961: 10–1). Mead emphasizes that everything about these girls' lives differed from the experiences of Western readers, including the basics of food and shelter:

> All of her habits of life were different. She sat cross-legged on the ground, and to sit upon a chair made her stiff and miserable. She ate with her fingers from a woven plate; she slept upon the floor. Her house was a mere circle of pillars, roofed by a cone of thatch, carpeted with water-worn coral fragments. . . . Her food was taro, breadfruit and bananas, fish and wild pigeon and half-roasted pork, and land crabs. (Mead [1928a] 1961: 9–10)

This passage, among many in *COA*, provides domestic detail that creates an intimacy between the reader and the girls whose lives Mead shared as she "receiv[ed] their whispered confidences and learn[ed] at the same time the answer to the scientists' questions" (Mead 1931, 118).

Most of *Coming of Age in Samoa*, like Malinowski's *Argonauts*, is written in the active voice and present tense and, on occasion, shifts from the impersonal to the personal voice (e.g., Mead [1928a] 1961: 72–3, 115, 119, 178; cf. Geertz 1988: 8–11). In writing about child nurses, for instance, Mead switches from an authoritative, third-person narrative to the subjective first person:

> By the time Samoan girls and boys have reached sixteen or seventeen years of age these perpetual admonitions to the younger ones have become an inseparable part of their conversation, a monotonous, irritated undercurrent to all their comments. I have known them to intersperse their remarks every two or three minutes with, 'Keep still,' 'Sit still,' 'Keep your mouths shut,' 'Stop that noise,' uttered quite mechanically although all of the little ones present may have been behaving as quietly as a row of intimidated mice. (Mead [1928a] 1961: 23–4)

This passage serves important textual purposes. The author validates her fieldwork, grounded in extensive observations of adolescent–child relations. By situating herself in the narrative, Mead convinces her readers that she was strategically placed to hear and observe such interactions. Mead buttresses her commentary with pithy examples of adolescent remarks, creating a vivid social landscape for understanding the exotic yet all-too-human dynamics of child-tending in Samoa.

*Coming of Age in Samoa* is replete with narrative devices to keep the reader turning the pages. For instance, Mead frequently uses literary "hooks" at the beginning of chapters: "Until a child is six or seven at least she associates very little with her contemporaries" (chapter 5); "The first attitude which a little girl learns towards boys is one of avoidance and antagonism" (chapter 6) (Mead [1928a] 1961, 59, 86). Chapter 10 ("The Experience and Individuality of the Average Girl") and chapter 11 ("The Girl in Conflict") are noteworthy for their literary devices at the beginning. One hundred and thirty pages into the text, Mead enters the narrative in chapter 10 as the authoritative storyteller who reiterates the book's theme:

> [W]e come to the tale of the group of girls with whom I spent many months, the group of girls between ten and twenty years of age who lived in the three little villages on the lee side of the island of Taʻū. In their lives as a group, in their responses as individuals, lies the answer to the question: What is coming of age like in Samoa? (Mead [1928a] 1961, 131)

Chapter 11 begins with a rhetorical question about an apparently idyllic adolescent passage to adulthood: "Were there no conflicts, no temperaments which deviated so markedly from the normal that clash was inevitable?" (Mead [1928a] 1961, 158). Mead ([1928a] 1961, 158) responds that she "reserved for this chapter the tales of the few girls who deviated in temperament or conduct," adding that "in many cases these deviations were only charged with possibilities of conflict, and actually had no painful results." Chapter 11 focuses on case studies of eight "deviant" girls, divided into two categories: The first group consists of four girls who "demanded a different or improved environment, [and] who rejected the traditional choices" (Mead [1928a] 1961, 169). Each of the four "delinquent" girls in the second group "is maladjusted to the demands of her civilisation, and who comes definitely into conflict with her group, not because she adheres to a different standard, but because she violates the group standards which are also her own" (Mead [1928a] 1961: 171–2; see also Sullivan 2005). After

discussing these individuals, Mead's ([1928a] 1961, 183) conclusion parallels the chapter's beginning: "And here ends the tale of serious conflict or serious deviation from group standards."

Using the female life course as her narrative structure, Mead's text proceeds from birth, early childhood, adolescence, marriage and motherhood, to old age. The author clarifies her approach in the introduction, noting that she has no intention of including in *COA* the ethnographic "[m]inutiae" suitable for a more specialized text. Rather, she asserts:

> . . . I have tried to present to the reader the Samoan girl in her social setting, to describe the course of her life from birth until death, the problems she will have to solve, the values which will guide her in her solutions, the pains and pleasures of her human lot cast on a South Sea island. (Mead [1928a] 1961: 11–2)

Mead's text is most persuasive when the author engages the reader with a wealth of ethnographic observations centered on the domestic intimacies of young lives. By concentrating on children and adolescents (chapters 3–11), the life course narrative in *COA* falls short, giving married and mature women perfunctory attention in a single chapter of ten pages. Yet Mead's strategies enabled her to do what every good ethnographer must do: to make the exotic familiar by humanizing her subjects. Empathy with her youthful subjects is evident, for instance, in the fieldwork photograph that appeared next to the title page of the first edition of *COA* (Figure 2). Captioned "With Hibiscus in Her Hair," this girl's exuberant expression and her face, framed by hibiscus flowers worn over the ears, delights the viewer. Mead's photograph of this young woman, probably an informant, constitutes an important counternarrative to popular media constructions of the eroticized Polynesian body. This Samoan girl lacks the dreamy self-absorption of a Flaherty photograph; neither is she a commoditized exemplar of sensual eroticism (Tiffany 2004, 2005a). Rather, we see an Islander with a striking sense of personal charm and grace; she stands relaxed before Mead's camera, arms held across her chest in a graceful dance posture.[13]

*Case Studies*

Mead's discussion of child nurses enables the reader to enter the experiential world of five-, six-, and seven-year-old girls, "who trundle about upon their hips babies that are too heavy to be lifted into their arms" (Mead 1928b, 633). The reader empathizes with the child nurse's efforts at "coaxing, bribing, [and] diverting" a younger charge: "The little nurses are more

FIGURE 2. **"With Hibiscus in Her Hair." Title and photograph by Margaret Mead, 1925–26, probably Taʻū Island; printed in the first edition of *Coming of Age in Samoa*. (Photograph reproduced from the Margaret Mead Archives, Manuscript Division, Library of Congress, Washington, D.C. Courtesy of the Institute for Intercultural Studies, Inc., New York.)**

interested in peace than in forming the characters of their small charges and when a child begins to howl, it is simply dragged out of earshot of its elders" (Mead [1928a] 1961, 24).

Mead's description of Anovale (a pseudonym)—a low-status girl of about twelve to thirteen years of age and "on the verge of puberty"—could well apply to the child nurse in Mead's photograph (Figure 3):

> The girl is decidedly overworked [with five younger siblings to care for] and is always carrying a baby. They are quite poor and she never has any even passably respectable clothes. . . . Anovale is tall, angular, loud voiced and awkward, domineering towards all her younger relatives, obstinate, sulky, quick to take offense. She regards her playmates as so many obstacles to be beaten over the head. She has no interest in boys whatsoever, except as extra antagonists. (Mead to Franz Boas, letter dated January 5, 1926 [Library of Congress, Margaret Mead Papers, box I2]; online Côté 2004)

Mead's discussion in *COA* conveys how Samoan children, especially girls, are "disciplined and socialised through responsibility for a still younger one" (Mead [1928a] 1961, 24). Mead accomplishes this narrative task by describing the kinds of mischief and inconvenience a child can impose upon an older sibling by putting up a fuss. Child nurses will:

> succumb to some little tyrant's threat of making a scene, and five-year-olds bully their way into expeditions on which they will have to be carried, into weaving parties where they will tangle the strands, and cook houses where they will tear up the cooking leaves or get thoroughly smudged with the soot and have to be washed—all because an older boy or girl has become so accustomed to yielding any point to stop an outcry. (Mead [1928a] 1961, 24)

Mead's experimental narrative in this context evokes a vivid sense of immediacy combined with domestic detail, a technique that also serves to confirm her authorial legitimacy to readers. Only a keen observer who had "truly 'been there'" (quoted in Geertz 1988: 4–5) could describe the minutiae of daily life that the ethnographer experienced firsthand.

Mead's *being there* is evident, for example, in her contextualization of Mala (a pseudonym), who had just reached puberty:

FIGURE 3. **"Child Nurse." Untitled photograph by Margaret Mead, 1925–26, probably Ta'ū Island. (Photograph reproduced from the Margaret Mead Archives, Manuscript Division, Library of Congress, Washington, D.C. Courtesy of the Institute for Intercultural Studies, Inc., New York.)**

> She was a scrawny, ill-favoured little girl, always untidily dressed. Her parents were dead and she lived with her uncle, a sour, disgruntled man of small position. His wife came from another village and disliked her present home. The marriage was childless. The only other member of the house group was another niece who had divorced her husband. She also was childless. None showed Mala any affection, and they worked her unmercifully. (Mead [1928a] 1961, 178)

Mead's case study approach illustrates how issues of status, power, and gender affect a low-status, orphaned girl like Mala. A woman of rank accused Mala of petty theft. Thereafter, "[w]hen her [Mala's] name was mentioned, the information that she was a thief and a liar was tacked on as casually as was the remark that another was cross-eyed or deaf. Other children avoided her" (Mead [1928a] 1961, 179).

Mala also had a reputation for preferring boys and their play and for wearing her *lavalava* (wraparound skirt) like a boy. Villagers roundly condemned her: "'She really was a very bad girl. She stole; she lied; and she played with boys,'" who in turn, "teased her, bullied her, [and] used her as general errand boy and fag [i.e., a junior child who does minor chores for an older child]" (quoted in Mead [1928a] 1961, 179). Noting that "[s]ome of the more precocious boys of her own age" were beginning to show sexual interest in Mala, Mead goes on to speculate that Mala would likely spiral into sexual promiscuity and "sink lower and lower in the village esteem and especially in the opinion of her own sex from whom she so passionately desires recognition and affection" (Mead [1928a] 1961, 180). Framed in the context of a girl's "emotional needs" and impoverished domestic circumstances, Mead's ([1928a] 1961, 180) three-page discussion of this young "delinquent" enables the reader to understand why Mala—on account of her appearance, behavior, and transgression of gender boundaries—was considered a "bad girl" in the eyes of others, and likely to reconfirm her "badness" through increasingly risky behavior.

The innovative structure of Mala's case study approach prefigures postcolonial feminist theory, which critically examines the boundaries of knowledge and power in the process of writing ethnography. Mead's representation of Mala is an experimental shift toward engagement with the marginalized voice in which the author uses, for example, quoted remarks of what others said about this girl. Mead's description of Mala's body reinforces the underlying message of deprivation that this girl suffered in her daily life. Her "scrawny" body suggests an impoverished diet; her disheveled appearance implies familial indifference. Her truculent behavior suggests "unusual emotional needs and unusual home conditions" that left Mala "marooned

in a household of unsympathetic adults" (Mead [1928a] 1961, 180). Mead wrote passionately about Mala as one individual girl victimized by domestic circumstances and emotional neglect. Mala is by no means an essentialized stand-in for all "deviant" girls in Samoa (Sullivan unpubl. data).

Mead struggled with the problem of how to present her data while still in the field, noting, "I have to be able to marshal an array of facts from which another would be able to draw independent conclusions" (Mead to Franz Boas, letter dated January 5, 1926 [Library of Congress, Margaret Mead Papers, box I2]; online Côté 2004). Mead's correspondence to Boas expressed her concerns about the subjective element of her ethnographic material and the questionable value of shaping it in "a semi-statistical fashion:"

> I can probably write two or three times as much about each one of them [her informants] before I leave. But to fill such case histories with all the minutiae which make them significant to me when they are passing before my eyes is next to impossible. And the smaller the details become, the more dangerous they become if they are to be taken just as so many separate facts which can be added up to prove a point. . . . But how to use it? If I simply write conclusions and use my cases as illustrative material will it be acceptable? (Mead to Franz Boas, letter dated January 5, 1926 [Library of Congress, Margaret Mead Papers, box I2]; online Côté 2004)

Boas endorsed Mead's case study approach, responding that "a statistical treatment of such an intricate behavior as the one you are studying, will not have very much meaning" (Franz Boas to Mead, letter dated February 15, 1926 [Library of Congress, Margaret Mead Papers, box I2]; online Côté 2004). Mead's challenge was to present objective and subjective voices in a narrative that—as she phrased it many decades later—would also have "literary persuasiveness," citing the widespread appeal of Malinowski's ethnographic writing at the time (Mead 1976, 3). Like *Argonauts*, *COA* occupies a distinct discursive space, representing a pioneering topic in the ethnographic endeavor—in this instance, the girl child in the social context of her domestic life and bodily experiences.

## "'Under the Palm Trees'"

*Coming of Age in Samoa* describes the intimacies of daily life among adolescent girls in the South Seas—a culturally charged landscape associated in popular culture with the romance and exotica of indigenous women's

sexualized bodies. Aware of the cultural and visual burdens that accompanied her fieldwork in Polynesia, as well as her subsequent writing about the South Seas, Mead sought to address the social issues that shaped her thinking at the time (Mead 1949, x; 1961, vii; 1973: vi–vii; see also Tiffany 2001: 22–4). These published commentaries consist primarily of a series of prefaces, written over a period of fifty years, for successive reprints of *COA*. Mead acknowledged the "young and hopeful world in the 1920's" that shaped her thinking and writing, noting as well the dark side of the cultural and social order of the interwar years: "the rebellion and self-criticism, the hatreds and the cynical despair," and the "rising totalitarianisms" that would eventually result in yet another catastrophic war (Mead 1961, vii; cf. Mead 1949, x; 1973: viii–ix).

Mead (1961: vii–viii) also reflected on the public reception of her ethnography, commenting that: "Those who saw American society in the 1920's as a rapacious and consuming monster greeted this book as an escape—an escape in spirit that paralleled an escape in body to a South Sea island where love and ease were the order of the day." *Coming of Age in Samoa* provided "satisfaction" to those who contrasted "'the primitive,' which was natural and delightful, with 'the civilized,' which was unnatural and repressive" (Mead 1961, viii). A few months before her death in 1978, Mead returned to this theme in an unpublished preface for a forthcoming special edition of *COA*:

> The book was a small bomb-shell. My account of adolescent sexual freedom in Samoa was embraced as a panacea in a country that had just given up chaperonage and had left adolescents to themselves. A long forgotten popular commentator pronounced: "Modern man has just one choice, Samoa or Calvary." (Mead 1978b, 2)

Such "either-or" statements, suggesting sexual liberation or repression, were common rhetorical devices in media reviews and public commentaries on *COA* at the time. They would assume even greater popular currency in the gender and culture wars of the 1980s and 1990s, and beyond (Tiffany 2001, 2004).

Perhaps the best known example of Mead's literary voice in *COA* is "A Day in Samoa." Chapter 2 conveyed, not only "the whole gentle rhythm of [village] life" (Mead 1965, 141), but also the South Sea romance of "lovers slip[ping] home from trysts beneath the palm trees or in the shadow of beached canoes, that the light [of dawn] may find each sleeper in his appointed place" (Mead [1928a] 1961, 14). "A Day in Samoa" turned out to be a marketing asset: As one reviewer enthused, "The author begins her

work with a description of Samoa that is more beautiful than any I have ever read of in this corner of the world" (Nat Ferber, *Review* of *COA*, unattributed newspaper clipping, September 1928 [Library of Congress, Margaret Mead Papers, box L3]). Mead had originally intended "A Day in Samoa" for inclusion in her more "technical" work, *Social Organization of Manu'a*, first published in 1930 and reprinted in 1969 with a new introduction. However, Mead decided that her essay was "too literary in character for the style of a Bishop Museum monograph!" (Mead 1969, xvii, punctuation in original; cf. Mead 1972, 165; 1976: 3–4).

The narrative purpose of "A Day in Samoa," according to Mead's (1965, 135) subsequent account, was to present "a fairly typical day in the life of the people" from the perspective of an ethnographer's day in the field. Chapter 2, then, is a constructed world, a literary device for "giving readers some sense of the tone and the pace of life as I [Mead] had experienced it" (Mead 1965, 135). The author invites the reader to share what the ethnographer herself experienced, and to preview what is to come later in the text.

*Coming of Age in Samoa* raised the hackles of many colleagues. Reo Fortune, Mead's soon-to-be-husband, read the manuscript before publication and criticized the redundancy of its conclusions (see Thomas 2009). Edward Sapir, nursing a personal vendetta against Mead, assailed *COA* in a 1929 issue of *The New Republic* as "'cheap and dull'" (quoted in Shankman 2009a, 206; see also Molloy 2004, 39). Mead's response figures in a letter written to Benedict in 1932, when Mead and Fortune were conducting fieldwork in Alitoa, New Guinea:

> F[rances] Phillips [Director of William Morrow and Co.] sent me a copy of the Blue Ribbon Samoa. Reo is really impressed with my having gotten into a popular edition, instead of disapproving which is a help. I have been rereading it at meals since he left [on a supply trip] and I find there is very little in it that I regret—the journalese of the first part of the introduction, I do. And all over again, I have decided that Edward's [Sapir] accusations of cheap and sensational are unfounded. What I don't understand is why the general public ever reads it at all. (Mead to Ruth Benedict, letter dated July 12–13, 1932, quoted in Caffrey and Francis 2006, 153; see also note 13, below)

Mead's letter is significant: Aside from some "regret" for informal language in part of the Introduction of *COA*, she remained pleased overall with the content of her book. She expressed anger at Sapir's virulent criticism of her and the ethnography, and wonderment at the book's commercial appeal.

Years later, *COA* continued to elicit disparaging reviews and commentaries. Peter Worsley (1957) questioned whether *COA*, as well as Mead's other ethnographies, were "science or science fiction." E. E. Evans-Pritchard (1962, 96) deprecated Mead's "chatty and feminine" narrative as an instance of a "Rustling-of-the-Wind-in-the-Palm-Trees" style of ethnographic writing (see also Lutkehaus 1995).[14] Indeed, fifty-five years after the publication of *COA*, anthropological reviews of Freeman's (1983) contentious analysis of Boas, Mead, and *COA* reiterated the "Fantasy Island" theme with titles such as "Love Under the Palm Trees" (Kuper 1983) and "The Shangri-La That Never Was" (Leach 1983). These provocative titles suggested that Mead's book could be dismissed as romantic fiction; in other words, *COA* was merely a "literary" work written *by* a woman *for* a female audience. Such critiques belittled both the author and her subject matter (see also Shankman 2009a, 2009b; Yans 2004).

Mead's innovative narrative presented an unacceptable straying from the boundaries of academic ethnography, in which "[t]he subjectivity of the author is separated from the objective referent of the text" (Clifford 1986, 13). Yet, in 1928 Malinowski praised Mead's ethnography precisely for its personal tone, stating that "the many more subjective touches which you [Mead] give to your work, the obiter dicta, and the glimpses into your Weltanschauung have always stimulated me and at times delighted" (Bronislaw Malinowski to Mead, letter dated September 22, 1928 [Library of Congress, Margaret Mead Papers, box L3]). Indeed, just one month earlier, Malinowski had written to Mead's publisher, praising *COA* as

> [A]n absolutely first-rate piece of descriptive anthropology. . . . The manner in which Miss Mead's field-work was conducted seems to be beyond cavil and criticism. Her style is fascinating as well as exact and the book provides excellent reading; convincing to the specialist, attractive to the layman. (Bronislaw Malinowski to William Morrow, letter dated August 22, 1928 [Library of Congress, Margaret Mead Papers, box S9])

William Morrow subsequently used Malinowski's comments as endorsements for marketing the book (Tiffany 2001, 23).[15] By contrast, Robert Redfield's (1929: 729–30) review of *COA* lamented the absence of a personal voice: "But Miss Mead is interested, one feels, in problems and cases, not in human nature. There is no warmth in her account. A little Malinowski, stirred in, would have helped, perhaps."

Mead's publisher considered chapter 7 ("Formal Sex Relations") to be "one of the most important" chapters in *COA* (William Morrow to Mead,

letter dated September 5, 1930 [Library of Congress, Margaret Mead Papers, box Q12]). Certainly the cover of the first edition highlighted the "love under the palm trees" theme that Mead herself wrote about in *COA* (Figure 4).[16] However, a careful reading of Mead's ethnography indicates

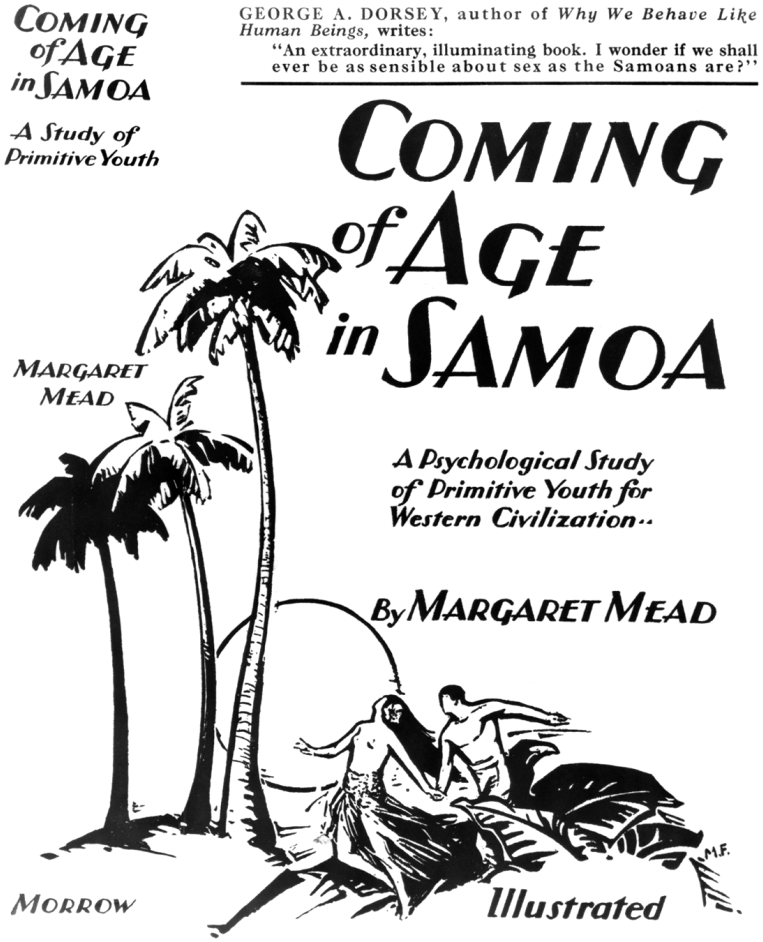


FIGURE 4. **Cover of the first edition of *Coming of Age in Samoa*, by Margaret Mead, published in 1928 by William Morrow and Company. (Reprinted by permission of HarperCollins Publishers, Inc. Jacket cover reproduced from the Margaret Mead Archives, Manuscript Division, Library of Congress, Washington, D.C. Courtesy of the Institute for Intercultural Studies, Inc., New York.)**

that the book's cover (Figure 4), which suggests the girl's initiative in leading her lover toward the palm grove, is somewhat misleading. According to Mead, youthful excursions at night typically involved groups, rather than individual couples:

> These clandestine lovers make their rendezvous on the outskirts of the village. "Under the palm trees" is the conventionalized designation of this type of intrigue. Very often three or four couples will have a common rendezvous, when either the boys or the girls are relatives who are friends. (Mead [1928a] 1961, 92)

Another distinctive feature of the first-edition dust jacket is the prominent display of George Dorsey's printed endorsement, which contrasted Samoan and American sexual mores (see Figure 4). A best-selling author and distinguished anthropologist of Native America in his own right, Dorsey had introduced Mead and her manuscript to the fledgling publisher, William Morrow. Dorsey subsequently proposed a new title for the manuscript, encouraged Mead to ask Boas to write a foreword to *COA*, and also wrote a glowing review of the book (Mead 1965: 122–5; Tiffany unpubl. data).[17] Mead (1965, 125) had little to say about the first-edition cover of *COA*, noting decades later that: "It went to press, I read proof, and saw a small printer's dummy of the table of contents, chapter one, and the jacket. This done, I sailed for Hawaii, glad to be on my way to the field in the Admiralty Islands" (see also Tiffany 2004: 157–8).

Despite Malinowski's and Dorsey's endorsements, Mead recognized that the sexual content of *COA* could jeopardize a return field trip to Samoa that she had tentatively planned for 1928. She eventually decided not to go for personal and professional reasons, opining that a second trip to the Islands could delay publication of *COA* for at least a year, "for it [the book] would alienate too many people down there [Samoa] and complicate my field work" (Mead to William Ogburn, letter dated April 27, 1927; Mead to Franz Boas, letter dated April 27, 1927 [Library of Congress, Margaret Mead Papers, box Q11]; cf. Thomas 2009).

### *Toward A Political Economy of Sex in Samoa*

Mead did not discuss explicit details of what constituted "sex" in *COA*, lacking, in part, a respectable, public vocabulary specific to female experiences (Lyons and Lyons 2004: 148–51). Her ethnography does indicate, however, that sex experience included "*play,*" such as "suggestive dancing," "salacious conversation," "salacious songs," and "tussling" (Mead [1928a]

1961: 148–9, 151). Other kinds of social interactions, such as watching others engaged in "sex" or sex "play," as well as "good-natured banter" and "teasing" about others' alleged lovers, comprised part of Samoan girls' sex experiences (Mead [1928a] 1961: 86–9, 136–8). In other words, "sex" did not necessarily mean intercourse (cf. Grant 1995).

Mead's narrative of the political economy of sex presents a complex social cartography of desire and constraint: The ethnographer elucidates the restrictions and limits, ranging from fear of going out at night, to issues of age, status, and domestic environment. Female sexuality is fraught with social and emotional uncertainties: the politics of status that surround the *taupou* (ceremonial maiden) and her chastity; the boy who waits until dawn for the girl who never shows up; the willful girl who elopes, with its attendant domestic and sociopolitical consequences for her high-ranking relatives; the low-status adults who ignore a girl's sexual adventures for fear of losing a valuable household worker (Mead [1928a] 1961: 92, 94, 98–104, 151–2). While Mead's ethnography appears to reinforce consumer-culture representations of "free-love" in the South Seas, her narrative, in fact, challenged popular images with a radical counternarrative of Polynesian female sexuality and its nuanced expressions situated in a specific social environment.

## Writing Gender and Ethnography

*Coming of Age in Samoa* remains an important experimental work in which Mead creatively used literary and scientific language—writing styles considered incompatible by her academic critics. Further, the presence of a gendered voice in *COA*, with its attendant message of female agency in the problematic domain of sexuality, prefigured more recent developments of reflexive and postmodern issues of gender in contemporary ethnographic writing. Mead's innovative case study approach created a narrative space for privileging the female experience in ethnography. *Coming of Age in Samoa* also served to legitimize public discussion of gender issues concerning both Polynesian and Western women's bodies and behavior.

Like Malinowski, the "'Reluctant Sexologist'" (quoted in Lyons and Lyons 2004: 183–4), Mead lamented public fixation on the exotica of Pacific Islander sexuality, noting that the topic of Samoan adolescent sexual experiences comprised only a small portion (68 pages) of her 297-page ethnography (Mead to Professor W. A. Brownell, letter dated March 10, 1930 [Library of Congress, Margaret Mead Papers, box I2]). Mead expressed these concerns to Dr. W. A. Brownell, a Psychology Professor who had assigned portions of *COA* in an undergraduate class. Basing her comments

on Brownell's discussion of his students' response to *COA*, Mead noted the students' "complete conviction that this is a book about sex, mainly about sex education and sex freedom. I have met this attitude in other people and it has always amazed me" (Mead to Professor W. A. Brownell, George Peabody College for Teachers, Nashville, Tennessee, letter dated March 10, 1930 [Library of Congress, Margaret Mead Papers, box I2]).

Despite her critics, *COA* cannot be dismissed as a *National Geographic*–style of adventure narrative with titillating commentary on indigenous sexual practices. Mead was neither "duped" by her informants (Freeman 1999) nor gulled by "the potency of the Western myth of uninhibited Polynesian sexuality" (Tcherkézoff 2001, 72), to cite just two interpretive scripts that anthropologists have sought to impose on her work. Mead steadfastly resisted demands that she update or revise *COA*, writing that the girls whose lives she recorded remained "living persons as they were known to me and to their friends and relatives, human in their lives and loves" (Mead 1973, xi).

In 1933, the International Conclave of Woman Writers and the International Congress of Women, convening jointly in Chicago, announced the best books by American women writers of the past one hundred years. Chaired by the editor of *Poetry Week*, "a book council of twenty-four American writers and literary figures and the faculties of sixty universities and colleges" selected a hundred titles for the list (Gifford Ernest, *Century's 100 Best Books by U.S. Women; Two Chicagoans on List*, unattributed newspaper clipping, ca. 1933 [Library of Congress, Margaret Mead Papers, box L3]). *Coming of Age in Samoa* shared the social science category of the hundred best books with two works now considered First Wave feminist classics: Margaret Sanger's *Woman and the New Race* (1920, with a preface by Havelock Ellis); and *Woman Suffrage and Politics: The Inner Story of the Suffrage Movement* by Carrie Chapman Catt and Nettie Rogers Shuler (1923). Presumably, Mead was delighted to be part of such a distinguished group of authors. Seventy-two years later, *COA* achieved "Honorable Mention" in a list of the "Worst Books of the 19th and 20th Century," published online in May 2005 by *Human Events*, a conservative weekly. Mead, no doubt, would be both amused and pleased to have her ethnography included with such luminaries as Auguste Comte, Charles Darwin, and John Dewey, among others, on the weekly's list. *Coming of Age in Samoa* continues to generate debate in the postmodern culture wars of academe, politics, and popular media. This is no small accomplishment for a work published in 1928, reprinted in several editions and languages over a period spanning more than eight decades, and dedicated to the girls of Ta'ū Island in American Samoa.

## ACKNOWLEDGMENTS

This essay is dedicated to the memory of Mary Wolfskill, former Head, Reference and Reader Service Section of the Manuscript Division at the Library of Congress. I also wish to thank the staff of the Manuscript Division for their generous assistance with my research on the Margaret Mead Papers at the Library of Congress. I am deeply grateful to Mary Catherine Bateson for her interest in my ongoing research with the Mead Papers and to the Institute for Intercultural Studies in New York for permission to reproduce photographs and materials from the Margaret Mead Papers at the Library of Congress. My special thanks to Kathleen J. Adams and Gerald Sullivan for their comments on earlier drafts of this essay; any errors of interpretation are, of course, my own.

## NOTES

1. An earlier version of this essay was presented on February 3, 2005 at the symposium, *Gang of Four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in Multiple Contexts*, during the Association for Social Anthropology in Oceania (ASAO) annual meetings in Kauai'i, Hawai'i.

2. All references to *Coming of Age in Samoa* are from the Morrow Quill Paperback edition of 1973, copyrighted 1961, unless otherwise noted.

3. Web site results for "*Coming of Age in Samoa*" or "Margaret Mead 1928", for example, are diverse. They include advertisements from booksellers, hotels, and tours; gay newsletters and articles about *fa'afine* (Samoan cross-gender roles); academic essays on Mead's work and life; public responses to Mead's intellectual legacy; the Mead–Freeman controversy; and conservative blogs filled with the rhetoric of "debunking" and "hoaxing." The Wikipedia site offers a fairly extensive discussion but dated bibliography on the Mead–Freeman controversy (accessed August 2, 2009). See Stover (2005) for extended discussion of Mead and online sources.

4. The publishing histories of these two works are remarkably similar. Malinowski and Mead received assistance from other well-known anthropologists of the day (Alfred C. Haddon and George C. Dorsey, respectively) in getting *Argonauts* and *COA* published. Both ethnographies were subsequently retitled (originally, *Kula: A Tale of Native Enterprise and Adventure in Eastern New Guinea* and *The Adolescent Girl in Samoa*), and both were published by commercial presses (Routledge & Kegan Paul and William Morrow, respectively) (Lyons and Lyons 2004: 155–215; Stocking 1983, 94, 106, 110).

5. Mead's 1931 publication, written for a young female audience, was one of ten "personal stories" by heroic women "who have traveled far or experienced some unusual adventure." The anthology included an essay by the famous aviator, Amelia Earhart, on her cross-Atlantic flight ("Review of *Adventures of 10 Women*," *Every Evening*, Wilmington, Delaware, October 10, 1931 [Library of Congress, Margaret Mead Papers, box L3]). Mead's essay for this anthology, described as "her life in the South Sea Islands with natives," is an important published source regarding Mead's thoughts about her Samoan fieldwork experience ("Review of *All True!—A Compilation*," *Oregon Journal*, Portland, Oregon, October 25, 1931 [Library of Congress, Margaret Mead Papers, box L3]).

6. Mead cited the assistance of American Naval authorities and medical personnel in her acknowledgments for *COA*. Upon returning to the United States, Mead wrote a two-page letter of thanks to the Navy surgeon general, stating that "the Medical Department was the backbone of my work" in Samoa. Her letter concluded with praise for the naval presence, noting that "the Samoans are exceedingly fortunate in the naval administration," by comparison to civil governmental "exploitation which is devastating most of the other South Sea Islands" (Margaret Mead Papers: Library of Congress, box I2, Mead to Dr. E. R. Stitt, surgeon general of the United States Navy, letter dated September 17, 1927).

7. Mead's check did not, in fact, arrive on the same boat as she had anticipated. Penniless, she was forced to remain in the port town for six weeks until her fellowship check arrived on the next boat, enabling her to settle her hotel bill (Mead 1972, 148).

8. Biographical accounts provide few details on the extent of Benedict's intellectual and editorial collaboration with Mead during the preparation of *COA* (e.g., Caffrey 1989, 100; Lapsley 1999, 170). Mead herself is vague on this point, citing Benedict's (and others') "criticism and assistance" in the Acknowledgments of COA (Mead [1928a] 1961, ii). A brief discussion of Benedict's influence on Mead's thinking about Samoa may be found in Mead's 1969 introduction to the reprint edition of *Social Organization of Manu'a*, which was dedicated to Benedict in the first edition of 1930 (Mead 1969: xvi–xvii). Further examination of the Benedict–Mead correspondence during and after Mead's Samoan fieldwork may provide further insights into the early working relationship between these two anthropologists.

9. In January 1978, just months before Mead's death, *Redbook Magazine* published an article based on Mead's Samoan fieldwork correspondence, excerpted in *Letters from the Field* (Mead 1977). The *Redbook* article, titled "The Reluctant Princess," was published during a resurgence of the feminist movement in the 1970s, contrasting it to the 1920s, when "at that time, women were doing things nevertheless and that she [Mead] just got up and got going'" (Mead 1978a; quoted in Helene Pleasants, Senior Editor of *Redbook Magazine*, to Rhoda Metraux, letter dated June 23, 1977 [Library of Congress, Margaret Mead Papers, box I301]).

10. The theme of culture and biology was rephrased for a publicity blurb for *COA* printed in *Morrow News Notes*: "In her book, Miss Mead shows the developing girl, as a constant factor in American and Samoan civilizations, but the civilizations are different. Is it the difference in those civilizations, not in the girls, that brings about the problems of adolescence?" (*Morrow News Notes*, August 29, 1928 [Library of Congress, Margaret Mead Papers, box L3]).

11. All photographs, with the exception of Figure 4, were taken by Margaret Mead in American Samoa between 1925 and 1926. Only one of Mead's photographs (Figure 2) is titled. All other photo titles are those of the author (Sharon W. Tiffany). Mead's fieldwork photographs reproduced in this article were retouched using the computer program *Adobe Photoshop Version 7.0* in order to reduce fading and discolorations and to lighten shadowed faces. The composition and subject matter of Mead's images have not been altered or edited in any way. Mead's second photograph of her room, which is not reproduced in this essay, reveals that she actually had two tables. The second, shown with a bark cloth cover and a row of books on top, faced the screened area of the veranda (Library of Congress, Margaret Mead Papers, box P25).

12. The pictures displayed on the wall of Mead's room in Ta'ū probably included some of Francis Flaherty's photographs of Western Samoa, published in the May 1925 issue of *Asia Magazine*, which was mailed to Mead in the field. Three of Francis Flaherty's published photographs, taken during the filming in 1923–1924 of Robert Flaherty's documentary, *Moana of the South Seas*, were eventually used for the picture interpretation test Mead administered to her Samoan informants (Mead [1928a] 1961: 290–1; 1969, xix; 1972, 154; see also Tiffany 2005a). The "picture-naming test" is described in Appendices 2 and 5 of *COA* (Mead [1928a] 1961: 262–5, 289–92). Mead's archived test is closed to public access, primarily to protect the identities and responses of her informants (Patricia A. Francis, email communication to Sharon Tiffany, June 24, 2004). To my knowledge, Mead's most extensive discussion about protecting her Samoan subjects' identities is contained in unpublished correspondence to Derek Freeman (Mead to Derek Freeman, letter dated November 6, 1968 [Library of Congress, Margaret Mead Papers, box I2]).

13. This fieldwork photograph, along with six other images of Samoan girls and women, appeared in the first edition of *COA* published in 1928 by William Morrow, as well as in Morrow's cheaper, mass-market Blue Ribbon Books reprint edition of 1930 (Frances Phillips, Director of William Morrow and Co., to Mead, letter dated October 27, 1931 [Library of Congress, Margaret Mead Papers, box I6]; see also Caffrey and Francis 2006, 399, n. 13). Mead selected a total of twelve fieldwork images for the first edition of *COA;* none of these photos were republished in subsequent reprints of her ethnography, including the 2001 Centennial reissue by Perennial, HarperCollins Publishers (Tiffany 2001, 2004). Reprint editions of *Argonauts*, by contrast, included Malinowski's numerous fieldwork photographs reproduced in the first edition (Malinowski [1922] 1984; see also Young [1998]).

14. While Evans-Pritchard (1962, 93) acknowledged Malinowski's *Argonauts* as "a classic of descriptive ethnography," he also criticized the book as "long-winded and written in a journalistic style."

15. Delighted with receipt, on the same day, of letters praising *COA* from Malinowski and sexologist Havelock Ellis, Morrow and his staff "almost staged a celebration when we read them" (William Morrow to Mead, letter dated January 11, 1929 [Library of Congress, Margaret Mead Papers, box I2]).

16. A copy of the first-edition cover of *COA* is archived in the Mead Papers at the Library of Congress (box L3).

17. Mead's original manuscript submitted to Morrow carried the academic title, *The Adolescent Girl in Samoa*—the same title as Mead's final report to the National Research Council's Board of Fellowships in the Biological Sciences, which had sponsored her fieldwork (Library of Congress, Margaret Mead Papers, box N1; online Côté 2004). Dorsey's positive review of *COA*, titled "Natural or Savage?" was published on September 2, 1928 in the *New York Herald Tribune* (Library of Congress, Margaret Mead Papers, box L3).

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# DEREK FREEMAN AND MARGARET MEAD: WHAT DID HE KNOW, AND WHEN DID HE KNOW IT?

Paul Shankman
*University of Colorado, Boulder*

Derek Freeman's published autobiographical statement and his biographers' account of his life report that, before he went to Samoa in 1940, Freeman was a cultural determinist strongly influenced by Margaret Mead's work. While in the islands, Freeman stated that he discovered that Mead was wrong about Samoan culture and felt responsible for refuting her work, thus establishing a linear progression in his critique of Mead from his own first trip to the islands to the eventual publication, some four decades later, of *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth* (1983). Interviews with Freeman suggest, however, that this narrative is incomplete and that the path he took was more circuitous and indirect. In fact, although Freeman had opportunities to do so, for more than two decades he avoided published criticism of Mead's work. This more complex narrative raises questions about what Freeman knew about Mead's work, when he knew it, and what he did with that knowledge.

## Introduction

Of the many issues in the Margaret Mead–Derek Freeman controversy, one of the most intriguing is the chronology of Freeman's critique of Mead's work on Samoa. At what point did Mead's work become a focus of Freeman's attention? How well did Freeman understand Mead's work on Samoa early in his career? And when did his critique of Mead move from the private sphere to the published academic sphere? Addressing these questions is of interest because Freeman's (1983) published autobiographical statement

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and Appell and Madan's (1988) biographical account are incomplete in this regard. Freeman himself has provided additional information in interviews with Frank Heimans (2001) and Hiram Caton (2002). These materials now supplement the published record and provide a more complete, although still imperfect, understanding of the young Derek Freeman's relationship to Mead and her Samoan scholarship.

Freeman's (1983) brief autobiographical account about his early involvement with Mead's work can be found in the Preface of *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth*. There he discusses the circumstances that led to his critique of Mead, commenting in the preface that it was "by accident that I have come to write this book" (Freeman 1983, xiii). This "accident" began in the late 1930s when, as an undergraduate at Victoria University College in Wellington, Freeman took courses with Ernest Beaglehole, who, he reminds us, had studied with Edward Sapir at Yale. Freeman noted that

> Beaglehole's anthropology was very similar to Mead's and it was this approach, stemming from the teaching of Boas, that I adopted when, with Beaglehole's encouragement, I decided to do ethnographic research in the Samoan islands. When I reached Western Samoa in April 1940, I was very much a cultural determinist. *Coming of Age in Samoa* had been unreservedly commended to me by Beaglehole, and my credence in Mead's findings was complete. (1983, xiii)

Freeman continues his account, stating that, while in Western Samoa, he mastered the language after two years of study and was adopted into a Samoan family in the village of Sa'anapu on the south coast of Upolu. After living for about fifteen months in Sa'anapu and having been conferred a princely title in the village, Freeman felt that he had come to know a good deal about "the realities of Samoan life" (1983, xiv). Only then, on the basis of considerable experience in the islands, did Freeman begin to question his confidence in Mead's findings, remarking that

> [i]n my early work [in Samoa] I had, in my unquestioning acceptance of Mead's writings, tended to dismiss all evidence that ran counter to her findings. By the end of 1942, however, it had become apparent to me that much of what she had written about the inhabitants of Manu'a in eastern Samoa did not apply to the people of western Samoa. After I had been assured by Samoans who had lived in Manu'a that life there was essentially the same

> as in the western islands, I realized that I would have to make one of the objectives of my research the systematic testing of Mead's depiction of Samoan culture. (1983, xiv)

At that moment, Freeman recognized his obligation to correct Mead's errors: "By the time I left Samoa in November 1943 I knew that I would one day face the responsibility of writing a refutation of Mead's Samoan findings" (1983, xiv). This responsibility, of course, culminated in the publication of *Margaret Mead and Samoa* in 1983.

Freeman's brief autobiographical account gives his critique of Mead a coherent narrative with a noble purpose. Freeman had sincerely embraced cultural determinism, handed down in lineal fashion from Boas through Sapir to Beaglehole to Freeman. Although he had initially accepted Mead's Samoan findings, based on his own research experience in the islands in the early 1940s, Freeman came to know the realities of Samoan life and felt it his duty to refute Mead, presumably in published form. This narrative gives Freeman's critique of Mead a sense of authenticity and, indeed, inevitability. Mead had not accurately portrayed the Samoa that Freeman had come to know. It was therefore appropriate that he would be responsible for correcting the ethnographic record. In contrast to an allegedly inexperienced, naive, and gullible Mead, Freeman overcame his own naïveté about Mead's work while gaining the ethnographic credentials to put forth his critique.

As convincing and plausible as this narrative seems, it is nevertheless incomplete and, in some ways, misleading. Freeman gives the impression that he knew Mead's work well, that he went to Samoa to do ethnographic research, that Mead's perspective guided his thinking for much of his time in the islands, that he had taken as his responsibility the refutation of her work when he realized her alleged error, and that this was a priority in his own work. However, the interviews of Freeman by Heimans (2001) and Caton (2002) complicate this account and, in some ways, subvert it. Freeman's path was less direct and more haphazard than the one presented in his published, retrospective account. For example, it now seems that Freeman could not remember whether he had read *Coming of Age in Samoa* before going to the islands. In the 2001 interview with Heimans, Freeman readily acknowledged that he knew almost nothing about the islands when he arrived there. And while Freeman stated that Beaglehole encouraged him to do ethnographic research, Freeman had limited training in anthropology and no undergraduate degree in any subject. He went to Samoa not as an ethnographic researcher but as a schoolteacher, and the research he conducted in his spare time, by his own account, was initially archaeological and curatorial rather than ethnographic.

It is not clear exactly when Freeman began to question Mead's work in a professional forum. Freeman privately critiqued Mead's research and expressed his personal dislike for her among his colleagues as a graduate student during the late 1940s and early 1950s. Yet he did not correspond with her until 1957, nor did he publish criticism of her work until 1972, neglecting a major opportunity to do so in his 1948 postgraduate diploma thesis on Samoan social structure at the London School of Economics and another opportunity to do so in 1964 in an article in the *American Anthropologist* on Samoan kinship and political organization. Indeed, Freeman seemed to be unaware of or unconcerned with Mead's (1930) professional monograph on Samoa, *Social Organization of Manu'a*, until much later in his career and to the detriment of his 1948 thesis. Thus, a closer look at Freeman's relationship to Mead's Samoan work through the 1960s raises the question, What did he know about Mead's Samoan research, and when did he know it?

## Right from the Start?

Freeman's interview with Frank Heimans on February 12, 2001, just prior to his death, is one source that provides more detail about Freeman's intellectual and personal journey during the early years of his career. Heimans (1988) had produced the documentary film *Margaret Mead and Samoa* with Freeman's collaboration. The film was, in part, a visual version of Freeman's book and articles on Mead, and while the film was quite critical of Mead, it added little to our knowledge of what Freeman knew of Mead's work in Samoa during the late 1930s and early 1940s. Heimans's interview with Freeman is therefore helpful in this regard. Freeman was also interviewed between 1985 and 1987 by Hiram Caton, a professor of history and politics at Griffith University and a colleague of Freeman's. Caton had become close to Freeman and edited *The Samoa Reader* (1990) during the early years of the Mead–Freeman controversy before becoming more critical of Freeman later (Caton 2002: v–vi; 2005). What do these interviews add to our knowledge?

According to the 2001 interview with Heimans, Freeman, as a young college student of seventeen, described himself as having been "intensely interested in the human condition" and of having "kind of anthropological interests from the beginning" (2001, 7). Yet Victoria University College in Wellington did not offer courses in anthropology. Freeman enrolled in psychology, economics, and philosophy: "But in 1937, a man called Ernest Beaglehole, who was a psychologist who had taken up anthropology, joined the psychology department and I came under his influence" (Heimans

2001, 7). As Freeman (1983, xiii) recounted in *Margaret Mead and Samoa*, Beaglehole had studied with Edward Sapir, Boas's brilliant protégé, after Beaglehole received his PhD from the London School of Economics. It was through Beaglehole that Freeman says that he learned the doctrine of cultural determinism advocated by Boas and Mead, and Freeman implies that Sapir was influential in the transmission of Mead's work to Freeman. But what Beaglehole learned about Mead from Sapir is unclear.

Sapir had been involved in a passionate love affair with Mead during the mid-1920s before she went to American Samoa. He had urged Mead to leave her husband, Luther Cressman, and to marry him. While in Samoa during 1925–1926, Mead rejected Sapir, and he became openly antagonistic toward her. In an interview with Caton (2002, 32), Freeman reported that anthropologist Weston LaBarre sent Freeman a letter stating that Sapir had labeled Mead a "pathological liar" while he was teaching at Yale. And in a letter to Ruth Benedict, written in 1929, Sapir had called Mead a "'loathsome bitch'" and "'a malodorous symbol of everything he hated in American culture'" (quoted in Banner 2004, 24). Furthermore, in a review published in the *New Republic*, Sapir referred to *Coming of Age in Samoa* as "cheap and dull" (1929, 279). In another article in another magazine, Sapir alluded to Mead and Benedict, deploring feminists and lesbians, whom he accused of being both "frigid and ambitious," and attacked "'emancipated women'" in general as "being little better than prostitutes" (quoted in Molloy 2004, 39). These articles shocked Mead and Benedict and ended their further collaboration with Sapir (Silverstein 2004, 152).

Given this antagonistic relationship, it is difficult to imagine that, when Beaglehole was at Yale, Sapir would have praised Mead and *Coming of Age in Samoa*. So it is doubtful that Beaglehole's high regard for Mead came directly from Sapir; more likely, it came from Beaglehole's friendship with her. After completing his doctorate in 1931, Beaglehole received a Commonwealth Fund Fellowship to travel to Yale to work with American psychological anthropologists, particularly Sapir. He later met both Benedict and Mead. As Freeman noted in his interview with Heimans, Beaglehole "became a personal friend of Margaret Mead's" (2001, 11); in fact, both Ernest and Pearl Beaglehole became lifelong friends of Mead.

Although Beaglehole may have conveyed the general idea of cultural determinism to Freeman, it was certainly not a unique perspective in anthropological circles.[1] Nor does it seem that Freeman fully embraced this doctrine, even before going to Samoa and despite his published statement to the contrary. In his interview with Heimans, Freeman remembered that he began to question cultural determinism as a result of an intellectual encounter with Jiddu Krishnamurti, the noted Telugu mystic, in 1939:

> [Krishnamurti's] view was a kind of Buddhist view. He was totally against all social conditioning which he thought made a mess of the mind, and was a master of meditation and you had to rid yourself of all the false ideas in religion and so on and kind of reach a pristine state from where you could make good choices. I was the literary editor of the student newspaper called *Salient* and we were specialising in debunking people. . . . I was sent along to debunk Krishnamurti, but he debunked me. I mean, I was hugely impressed by what he was saying. He was deeply questioning cultural determinist theory, and he was no relativist. He said that you could, by severe intellectual effort, win through to an enlightened state. (Heimans 2001, 11)

Freeman was so impressed with Krishnamurti's views that he took two weeks off from teaching in order to have private meetings with him. Krishnamurti encouraged Freeman to doubt cultural determinism *before* he was aware that Mead was allegedly wrong about both Samoa and cultural determinism.[2]

## Ethnographic Research in Samoa

Freeman had to work in order to support himself after his first year at the university, and he became a schoolteacher of young children in New Zealand for about two years in the late 1930s, earning a Trained Teacher's Certificate from Wellington Training College for Teachers in the process. On seeing a position in the Department of Education in Western Samoa advertised in the *Teacher's Gazette*, Freeman applied for and obtained it. As he noted, "[I]t was through teaching that I got to Samoa, you see" (Heimans 2001, 9). However, Freeman did not know much about Samoa by his own admission (Heimans 2001, 13). Furthermore, his desire to go to the islands was not animated by his specific knowledge of them so much as his alienation from his home country. As he explained to Heimans, "I wanted to escape from New Zealand society and from the whole suffocating atmosphere there" (Heimans 2001, 12).

Freeman was looking for new experiences in life, and the islands were for him "the most romantic and lush place"; they were "overpowering" (Heimans 2001, 13). Freeman stated in *Margaret Mead and Samoa* (1983, xii) that, with Beaglehole's encouragement, he decided to conduct "ethnographic research" in Samoa. Yet it is not clear how he learned to do so. Beaglehole did give Freeman a list of readings on the subject, but did this list include readings about Samoa? Again, Freeman recalled that he "didn't know much about it [Samoa] at all" (Heimans 2001, 13).

Had Freeman actually read Mead's work before arriving in Western Samoa in 1940? In his interview with Heimans, Freeman was asked, "When you arrived in Samoa, did you have Margaret Mead's book, *Coming of Age in Samoa*, under your . . .:

> Derek Freeman: "I sent away for it and I got the volume. It's called *From the South Seas* and it has *Coming of Age in Samoa, Growing Up in New Guinea* and *Sex and Temperament in Three Primitive Societies*. I had Boas' *General Anthropology* textbook. But I wrote to America and got them. I was already collecting books."
> Frank Heimans: "So you read Margaret Mead's book in Samoa itself?"
> Derek Freeman: "Yes."
> Frank Heimans: "Was that the first time you'd read it?"
> Derek Freeman: "I can't be clear about that. I probably did read it in New Zealand because Beaglehole was always talking about it. One, at any rate, knew what the message was." (Heimans 2001: 15–16)

The book that Freeman ordered, *From the South Seas*, was published in 1939, after Freeman had taken course work from Beaglehole and just before he went to Samoa.

Although he did not complete his undergraduate degree and had limited training in anthropology from Beaglehole, Freeman was interested in other cultures and in doing research. His teaching position in Samoa facilitated this desire. Working in the port town of Apia with young "half-caste" (part Samoan/part European) and European children, Freeman taught in the mornings and so had his afternoons free to practice the Samoan language and to learn about Samoan culture (Heimans 2001, 14). The research that he did initially was archaeological in nature, beginning at Seuao Cave. As Freeman recalled,

> I went and did archaeological work and found my first stone adze, which was a huge thrill. Then I would write down the story of it from the local Samoans when I'd got my Samoan well enough under control. I did a number of other studies like that. (Heimans 2001, 14)

Some of these archaeological accounts were later published in the *Journal of the Polynesian Society*. Freeman also collected artifacts for H. D. Skinner of the Otago Museum in New Zealand.

After visiting the village of Sa'anapu on the island of Upolu and being adopted by the Samoan family of a chief with whom he lived, Freeman began studying the village (Heimans 2001, 16); the chief gave him "secret information," including genealogies. Given Freeman's interest in social organization, it is interesting that he does not mention reading Mead's (1930) *Social Organization of Manu'a* during the interview discussion of his own fieldwork. The monograph would have been quite useful on this subject.

During his stay in Sa'anapu, Freeman acquired a title or, to be more precise, "the title of the heir apparent to the high chief, Anapu" (Heimans 2001, 17). This meant that Freeman could sit in the council of chiefs and listen to cases involving serious crimes, including rape. He also learned about the protection of young women and punishment of those who were engaged in sexual activity. Freeman regarded this village experience as "an extraordinary stroke of good luck, because now I was right in a Samoan family. See, Margaret Mead lived in a United States naval dispensary with expatriate Americans" (Heimans 2001, 15). Thus, Freeman believed that his authentic village experience stood in sharp contrast to Mead's allegedly inauthentic one.

Yet Freeman's most significant insights about Samoan sexual conduct came not from his intermittent visits to the village of Sa'anapu but rather from his time in the port town of Apia. On the boat to Samoa in 1940, Freeman met Dr. Hans Neumann, an Austrian doctor and refugee from Hitler. They became friends, and sometime later in the islands,

> [a]fter I had sort of established myself in Samoa and become a kind of local authority on the folklore and archaeological sites and I was speaking Samoan fluently and so on, he [Neumann] invited me to come up and live with him. He had a house, a very lavish house, opposite the hospital and I went up and lived with him there and quickly established ties with the Samoan nurses and became very friendly with one of them in particular. She quickly divulged what the Samoa system was, that she would go to bed with you but you must make her a promise that you would not attempt to deflower her, you see. It's not a sexual thing. I mean, they don't mind sex, but they want to be virgins when they marry, you see, for prestige reasons. Then I checked this out with other nurses and they all confirmed it. I then looked at the law cases and they all confirmed it and it was quite clear that Margaret Mead was totally wrong. (Heimans 2001, 18)

From his published account, Freeman gives the impression that his data on Samoan sexual conduct had come largely from his fieldwork experience in the village of Sa'anapu, and some of it did. But from the unpublished interview, it appears that critical information was obtained from Samoan nurses who were familiar with both Samoan and Western traditions while Freeman was living as a European in a European house opposite the main hospital in the port town of Apia. As sources of information, these port town nurses are perfectly acceptable, but Freeman's narrative of authentic knowledge gained from village experience now seems less compelling.

## Freeman's Knowledge of Samoan Sexual Conduct

Freeman's published autobiographical statement and his unpublished interview with Heimans suggest how systematic he was in obtaining data and reaching his conclusions about Samoan sexual conduct. Yet Freeman's data depicting Samoan sexual restrictiveness in the early 1940s are incomplete and misleading. The nurses with whom Freeman spoke sincerely believed that Samoan sexual conduct was restrictive and required virginity—particularly for women—a view that was and continues to be the public ideology of sexual conduct in Samoa. Yet between 1942 and 1945, a period that overlapped substantially with Freeman's first stay in the islands, World War II brought tens of thousands of American servicemen to the islands, where they engaged in sexual relationships with Samoan women and produced a sizable number of offspring.

As I have noted in other publications, the war years were a period of major change in the islands, which included a dramatic increase during this period in interethnic unions between American servicemen and Samoan women (Shankman 1996, 2001, 2004). W. E. H. Stanner, an anthropologist and postwar observer, found that

> [a] great deal of sexual promiscuity occurred between Samoan or part-Samoan women and American troops. Responsible Samoans said that actual prostitution was restricted to a very small group of women. Romantic, at least friendly, relationships were very common. One mission society reported that in Upolu alone there were 1,200 known instances of illegitimate children by American soldiers from Samoan girls. The official statistics were not revealed, but put the number of known illegitimate children much lower. Only a few incidents were caused by the jealousy of Samoan men, and not much was made of them by either side. Some villages were said to have set up a special curfew for their girls, and at

> Falefa (near Apia) no troops except officers on business were allowed to enter *fale* [houses]. With troops so widely dispersed in an area so densely settled it is impossible to prevent familiar association. Many soldiers regularly visited girlfriends within the villages, by no means only with single intention, but the entrance-gates to the airport, it was said, became known among Samoans as "the gates of sin." At least one *matai* [chief] was summarily expelled from his church congregation and from the society of the village on suspicion of procuring girls for prostitution. (Stanner 1953, 327)

The well-known author James Michener (1992) reported in a discreet but detailed manner his own participation in one such relationship. As a lieutenant, Michener was responsible for base security. Early in his Western Samoan tour, he found a base where, during the day, sixty to seventy-two American men were on duty, yet at night there were only six. Concerned about security, Michener learned that military vehicles took the men to villages at dusk, where they were dropped off to meet with their Samoan girlfriends for the evening. Michener saw firsthand that Samoans openly welcomed these evening arrangements. In the morning, the servicemen were picked up and returned to their base. Michener himself was invited by a high-ranking Samoan chief to enter into such a relationship with his daughter and father her child (1992: 38–40). As a result of his own involvement, Michener felt so compromised that he never reported these relationships to his superior officers.

These accounts from wartime Samoa indicate that relationships between American servicemen and Samoan women developed quickly and often, although many villages more distant from bases and roads had little contact with American troops. Where relationships took place, young women were allowed and even encouraged by their families to enter into them, with contact to a large degree under the control of parents and the village. There were relatively few overt conflicts between families and American troops. Although Samoans were perfectly capable of secluding their daughters and punishing them for affairs with Americans and for having children with them, for the most part they did not do so. This pattern of permissive sexual conduct during World War II is very difficult to reconcile with Freeman's portrait of a "severe Christian morality" and a culture in which he stated that "virginity was probably carried to a greater extreme than in any society known to anthropology" (1983, 250). It is also at odds with Freeman's assertion that major changes in Samoan sexual conduct did not begin to occur until the 1950s (1983, 350).

Because the wartime occupation of Western Samoa by Americans began in 1942, perhaps the best opportunity to view these changes would have been during the time period shortly before then and immediately thereafter. Freeman arrived in Western Samoa in April 1940 and departed in November 1943. He was, therefore, in a position to have observed or at least known of these relationships. Freeman was a teacher of part Samoan/part European children in Apia, who were the offspring of earlier Samoan–European unions. As a New Zealander whose country was the governing power in Western Samoa at that time, Freeman served in the Local Defense Force and later served in the Royal New Zealand Volunteer Naval Reserve for the rest of the war. It was at this time that premarital sexual activity in Samoa was perhaps most apparent. Yet, although Freeman had gained valuable insights into the restrictive public ideology of sexual conduct, he neglected the widespread phenomenon of permitted interethnic relationships. At this time, what Freeman emphasized was that he would "one day have to face the responsibility of writing a refutation of Mead's Samoan findings" (1983, xiv).

## Freeman's Knowledge of Mead's Samoan Work

Freeman remembered being critical of Mead's work immediately after the war, stating, "When I got back to New Zealand I reported this to Beaglehole, that I thought Margaret Mead had made an astronomical error and he sort of just laughed at me" (Heimans 2001, 19; see also Freeman 1996, 190). When he went to England to do graduate work after briefly revisiting Samoa in 1946, Freeman continued to criticize Mead's research and was remembered for his personal antipathy to her. Robin Fox, a colleague of Freeman, recalled that Freeman "seemed to have a special place in hell reserved for Margaret Mead, for reasons not at all clear at the time" (2004, 339). Fox also noted that "the rest of the British school seemed to see her [Mead's] fault as a case of whoring after cheap fame instead of doing a professional job of fieldwork" (2004, 339). However, at least one British social anthropologist, Raymond Firth, the foremost Polynesian expert of his generation, was not hostile to Mead and had favorably cited her work in his own. Firth became Freeman's adviser for his postgraduate diploma degree program in anthropology at the London School of Economics.

Since he did not have an undergraduate degree, Freeman was required to enter the postgraduate diploma program, which was part of a larger advanced graduate program. Freeman now had an opportunity to refute Mead's Samoan findings in his postgraduate diploma thesis on Samoa, which has been recently published (Freeman 2006). Titled "The Social

Structure of a Samoan Village Community" (1948), it remains Freeman's most extensive ethnographic report on Samoa, containing a very detailed description of the village of Sa'anapu. Had Freeman been interested in criticizing Mead, this would have been an appropriate place to do so, especially since Mead's *Social Organization of Manu'a* was the standard ethnographic work on Samoan social organization in English at the time. Both Freeman and Mead had written on the same topic, and *Social Organization of Manu'a* was Mead's professional monograph, in contrast to *Coming of Age in Samoa*, a popular trade book.

So where is *Social Organization of Manu'a* in Freeman's thesis, which runs to over 300 typewritten pages? It is nowhere to be found. It is neither cited nor discussed, nor are any of Mead's other works on Samoa, although some relevant works by other scholars are. Was Freeman unaware of *Social Organization of Manu'a*? Freeman biographer and historian Peter Hempenstall has noted the absence of *Social Organization of Manu'a* in Freeman's two seminar papers on Samoa, including one titled "On Samoan Social Organization." He concludes that "there is no indication that he [Freeman] was familiar with it, although he was in possession of Mead's *Coming of Age in Samoa* (Hempenstall 2004, 242). Moreover, Hempenstall (2004, 242) argues that Freeman's retrospective autobiographical account of his critique of Mead implies that he "recognized her flaws early on in his own Samoan days. There is no sign in this [seminar] paper that this was the case."

If Hempenstall is correct, then Freeman's early knowledge of Mead's work may have been limited solely to *Coming of Age in Samoa*. Yet Freeman, in his interviews with Caton, implied that he had read *Social Organization of Manu'a*. Freeman recalled that when he arrived at the London School of Economics, he already disagreed with Firth about Mead and Samoa (Caton 2002, 1). Freeman argued with Firth, stating that Mead's work had muddled and confused scholars studying Samoa and that it needed to be refuted because of its negative influence on the field (Caton 2002: 1–3). Firth did not agree. Moreover, Firth had cited *Social Organization of Manu'a* in his own ethnography *We, the Tikopia* (1936), and Freeman remembered that "there was no criticism of it" there (Caton 2002, 3), implying that Freeman had his own critique of *Social Organization of Manu'a* at the time.

If Freeman did know about and was critical of *Social Organization of Manu'a*, then the absence of any mention of it in his thesis and seminar papers would have been the result of a choice to deliberately neglect it. But why would Freeman do so? Could it have been the nature of the professor–student relationship he had with Firth? Freeman remembered

Firth's criticism of his work, but did he fear that Firth would reject his thesis if he criticized Mead? In an interview with Freeman, Caton raised this possibility:

> Caton: "Did you construe Firth's views on this matter as being more than friendly persuasion, did it contain to you any sort of professional threat?"
> Freeman: "Oh, not really I think he . . . he'd been, I'd been in a pupil–teacher relationship with him."
> Caton: "Yes."
> Freeman: "And he let me know what his views were, but by this time I was a professor at ANU [the Australian National University] and he had no constraints over me anymore. It's very much a personal matter." (Caton 2002, 2)

It is not clear from this interview whether Freeman felt pressure from Firth to refrain from criticizing Mead in his graduate work, possibly including his thesis. Nor is it clear why Firth and other faculty members would allow Freeman to omit any reference to Mead's work, especially *Social Organization of Manu'a*. Although Freeman would strongly criticize *Social Organization of Manu'a* for linguistic errors in his later publication (1972), his first mention of the monograph in print, to my knowledge, came in 1964, when he *favorably* cited it in his critique of Marshall Sahlins's and Melvin Ember's work on Samoan social organization, published in the *American Anthropologist* (Freeman 1964). This was a full sixteen years after his thesis was completed.

Because *Social Organization of Manu'a* was so relevant to Freeman's own fieldwork in Samoa and to his area of expertise in graduate school, his omission of Mead's work had theoretical and ethnographic consequences. For example, Freeman could have benefited from reading *Social Organization of Manu'a* on a theoretical level. In 1930, Mead discussed the distinction between social organization and social structure, a distinction that Freeman would also address in his 1948 thesis without reference to her work.

Mead's monograph could have also assisted Freeman in his own ethnographic descriptions of Samoan social organization. Thus, in *Social Organization of Manu'a*, Mead correctly identified Samoan descent as "bilateral" (1930, 18), whereas Freeman's thesis identified Samoan descent as purely or primarily "patrilineal" (1948: 72–73), an error he would later acknowledge (Appell and Madan 1988, 9). Interestingly, Freeman would

become well known for his analysis of cognatic descent groups among the Iban of Borneo and would win the Curl Bequest Prize in 1961 for his essay on the kindred. Yet in his thesis, not only did he not recognize Mead's early work on descent in Samoa, he may have missed an opportunity to have better understood bilateral descent and the kindred much earlier in his career.

Another instance where Freeman's thesis could have benefited from a closer reading of Mead is his analysis of the decline of the *taupou* system, which he discussed in some detail. Freeman's thesis cited Felix Keesing's essay in Oceania, "The Taupo System: A Study of Institutional Decline" (1937), the only publication of that era to deal exclusively with the decline of the Samoan system of institutionalized virginity. Keesing in turn cited Mead's account in *Coming of Age in Samoa*, among others, as demonstrating that the taupou system had undergone major changes in the nineteenth- and early twentieth-centuries and that it no longer existed in practice in most of Samoa by the 1930s. In his 1948 thesis, Freeman added his own ethnographic description of the decline of the taupou system in the village of Sa'anapu, including a three-page discussion that confirms the observations of Mead and Keesing but goes beyond them in ethnographic detail.

Freeman began by stating that "[t]he taupou system has now become virtually defunct in Western Samoa" (1948, 245). He then reported on the factors responsible for its decline:

> Principal among the reasons for this change has been the rigorous suppression of customs associated with it by the Christian missions. Economic factors have also operated. Like a *matai* [chief], a *taupou* is obliged to have her title ratified by the other lineages of her village community. This is established at a feast (*saofa'iga*) provided by the *taupou*'s lineage. Such a feast is a serious drain on a lineage's resources. Again, following the introduction of money into the Samoan economy, marked discrepancies have developed in the value of the property (*oloa* and *toga*) exchanged at marriage ceremonies. This has resulted in a situation in which a *taupou*'s lineage and village gain nothing from her marriage or formal election. (Freeman 1948, 245)

As a result, Freeman found that of the five *taupou* titles in the village of Sa'anapu, none were occupied in 1943. That is, none of the chiefs who could have appointed a *taupou* chose to do so. As for *taupou* marriages, they had become so infrequent that, as Freeman noted, "this type of marriage, now relatively rare, does not here concern us" (1948, 108).

When I published an article about the decline of the taupou system, citing Keesing and Mead (Shankman 1996), Freeman dismissed my argument as "all made of fantasy" (1998). At that time, I was unaware that Freeman's unpublished thesis provided excellent ethnographic support for earlier arguments made by Mead and Keesing, and, later, by me (see Shankman 2006). Keesing's article was also used by Raymond Firth, Freeman's thesis adviser, who devoted two pages of his textbook *Human Types* (Firth 1958) to the decline of the *taupou* system. And Keesing had, of course, discussed Mead's work. These intersections of knowledge about the decline of the *taupou* system make it all the more difficult to understand why Freeman's thesis did not include Mead's account in support of his own argument.[3] And they make it more likely that Hempenstall's assessment of Freeman's lack of knowledge of *Social Organization of Manu'a* is correct.

## Conclusion

Recent research by Judith Heimann (1999), James Côté (2005), and Hiram Caton (2002) has explored the immediate context, commencing about 1960, in which Freeman became more critical of Mead. Freeman, too, believed that this period was a turning point in his work on Mead (Heimans 2001, 4). Up to this point in his career, Freeman's record of publication suggests a pattern of avoiding Mead's work rather than addressing it and of avoiding her personally despite his privately expressed criticism of her research. Freeman did not begin his correspondence with Mead until 1957, and this was not initially about Samoa. Moreover, it was not until 1968 that Freeman wrote up and circulated his critique of Mead in an unpublished manuscript titled "On Believing Six Impossible Things before Breakfast." This manuscript became the basis of *Margaret Mead and Samoa*. However, Freeman did not send the essay to her despite his public confrontation with her over Samoa at a seminar in Canberra in November 1964. Again, Freeman chose to avoid Mead rather than providing her with a manuscript that he was circulating to other colleagues.

Freeman's first published criticism of Mead's work appeared in 1972 in an obscure note to the *Journal of the Polynesian Society* about apparent linguistic errors in *Social Organization of Manu'a*. The implication of this errata was that if Mead could not use the Samoan language properly, then how reliable could the monograph itself be? But Freeman did not directly criticize the monograph or *Coming of Age in Samoa*. It was not until 1983 that his full-blown critique of her Samoan work appeared in book

form, almost forty years after the date that he says he first realized his responsibility for such a critique.

There are a number of missing pieces in this story. The narrative that Freeman presented in *Margaret Mead and Samoa* about his early work on Mead now appears too neat and partial. It is a heroic narrative in which in-depth ethnographic research overcomes blind faith and in which determination and intellectual honesty triumph over shoddy scholarship. Yet, in attempting to undo the "myth" that Mead had allegedly created about Samoa, Freeman seems to have created misconceptions about his own biography and destiny.

While Freeman may have learned about cultural determinism from Beaglehole in college, as a result of his conversations with Krishnamurti in 1939, he became skeptical of it before going to Samoa. Freeman was also uncertain about whether he read Mead before going to the islands, although he did read *Coming of Age in Samoa* once he was there. He said that he had been encouraged to do ethnographic research in Samoa but did not mention that he was relatively untrained in anthropology and without an undergraduate degree as well as being unfamiliar with the islands. Nor did Freeman note that his first research was archaeological rather than ethnographic in nature.

Freeman's conclusions about Samoan sexual conduct were only partially based on research in the village of Sa'anapu in the 1940s, and Freeman neglected the interethnic unions taking place in the islands during World War II while he was there. Moreover, when Freeman had the opportunity to critique Mead's work as a graduate student during the late 1940s, he did not cite or discuss her work, to the detriment of his postgraduate diploma thesis. His professional critique of Mead would not emerge until decades later. So Freeman's path to his critique of Mead was more circuitous and indirect than his published narrative suggests.

The narrative Freeman created for himself contrasts with the one he created for Mead, whom he depicted as a young, naive, and gullible field-worker. While Mead was young, Freeman was more of a novice and much less of an anthropologist than Mead when he first went to Samoa (see Tiffany 2009). Further, on the subject of Samoan sexual conduct, he seems to have been vulnerable to the very weaknesses that he attributed to Mead. Freeman would become a careful observer of Samoa and an excellent scholar with a superior understanding of Samoan culture on many matters, but this mostly seems to have occurred separately from rather than in response to Mead's work.

If Freeman felt a professional responsibility in 1943 to refute Mead's work, why did he not do so until decades later? Indeed, why did he actively

avoid doing so for a considerable period of time? From this review of Freeman's early encounters with Mead's work, two possibilities emerge. Either Freeman knew of Mead's research and publications and, for reasons that remain unclear, chose not to recognize or critique them in written form early in his career, or he did not know very much about Mead's work, especially *Social Organization of Manu'a*, and therefore could not use it either to support his own work or to critique hers. Neither of these scenarios is in accord with Freeman's published version of the professional decisions he made early in his career regarding Mead's work. Future biographers of Freeman with greater access to his early writings may be in a better position to determine which of these two scenarios is more plausible or, for that matter, whether either is. It is sufficient for now to view Freeman's narrative as less a definitive account of his early relationship to Mead's work and more as a stimulus to further research.

## NOTES

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1. This is a relatively minor point, but it does indicate that there were personal and professional schisms in the so-called Boasian school and that there was less ideological conformity among Boas's students than Freeman had imagined (see Murray and Darnell 1998).

2. The philosopher had a powerful influence on Freeman; later in life, after more fully studying Asian religions, Freeman became, in his own words, "an evolutionary Buddhist" (see Freeman 2001).

3. It is also difficult to understand why, after including Keesing in his thesis, Freeman omitted this key source in his book *Margaret Mead and Samoa*, published in 1983.

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# OF EXTERNAL HABITS AND MATERNAL ATTITUDES: MARGARET MEAD, GESTALT PSYCHOLOGY, AND THE REPRODUCTION OF CHARACTER

Gerald Sullivan
*Collin College*

Some critics have thought of Margaret Mead's research in culture and personality as a mechanical reduction of character types to child-rearing techniques. However, a closer reading of her work reveals that, by 1938, Mead understood character to arise in the social and communicative interactions between caregivers and their charges. For Mead, techniques such as swaddling were powerful means by which caregivers communicated attitudes to children, but those same techniques were not sufficient to either form a child's character or produce an attitude within a caregiver. This article takes up the examination of this more psychologically dynamic Mead. It attends to her rare but express statement of the influence by the gestalt psychologist, Kurt Koffka, as well as to the similarities between Mead's thought and that of Kurt Lewin, another important gestalt psychologist.

## A Beginning, of Sorts

During his mid- to late 1930s course of lectures on "the psychology of culture" at Yale, Edward Sapir (1994, 181) contended that such a psychology "only arises in the relations of individuals." For Sapir (1994, 183), "In itself, culture ha[d] no psychology. It [was] just a low-tone series of rituals, a rubber stamping waiting to be given meaning by" individuals in their relations with and to one another. "[C]ulture [meant] nothing until the individual, with his personality configuration, [gave] it meaning" (Sapir 1994, 183). The analyst needed to understand culture in order "to know

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how to gauge the individual's . . . expressions of his reactions," but "the psychology of *a* culture [meant] nothing at all" (Sapir 1994, 181, emphasis in original).

Sapir's concern with the consequences of what might be called "the reification of culture" (cf. Darnell 1986, 158; Handler 1986, 136) can easily be traced back to Sapir's (1917) innovative response to Alfred Kroeber's (1917) contention that culture is somehow superorganic. During this course of lectures, Sapir (1994, 181) brought his concern to bear on the work of both Margaret Mead and Ruth Benedict. He contended that "the implication of . . ." their work, or what he called "much of the social psychological literature [then] being produced," [was] "a bit mischievous" (Sapir 1994, 181). A single mention of Mead aside, Sapir's discussion, as re-created by Judith T. Irvine, attended to Benedict and her 1934 book *Patterns of Culture*.[1] Sapir's criticism of Mead, at least on the grounds that he advanced during his Yale course, was misplaced, however; in that it has been repeated, Sapir was himself mischievous at best. This point shall become clearer over the course of this article.

Here, I continue a body of work dedicated to exploring Mead's scientific project and that project's multiple contexts (see Sullivan 1997, 1999, 2004a, 2004b, 2005b). Further, I have noted elsewhere Mead's long attention to the individual in culture, a notion traceable to Sapir (Sullivan 2005a). In this article, I focus on the dynamic relations of individuals—notably caregivers and young children—in Mead's thought. As in my previous work, I draw from Mead's unpublished papers—in particular a memorandum Mead wrote in 1938 while working with Gregory Bateson among the Iatmul at Tambunam, New Guinea. For Mead, the dynamic relations between persons not only gave rise to the variable psychologies of cultures and also did so in ways central to the possibility of the reproduction of character from generation to generation.

## Introducing Portions of a Memorandum

On March 21, 1938, Mead wrote a letter to Nolan Lewis, director of the New York Psychiatric Institute and Hospital and coordinator of field research for the Committee for Research in Dementia Praecox (hereafter CRDP). Mead's letter informed Lewis that she and Bateson were returning to New Guinea in order to gather ethnographic information of sufficient complexity that it could be compared with the materials they had already gathered in Bali.

The CRDP was by far the largest source of funds for Mead and Bateson's joint Balinese and subsequent Iatmul researches. A coalition of psychiatrists funded by the Masons, the CRDP's members were interested

in all matters having any bearing on dementia praecox, a set of psychoses better known today as schizophrenia (see Sullivan 1998, 72ff).

Lewis responded to Mead on June 23, 1938, indicating that "if . . . possible" the CRDP would "aid [Mead and Bateson] in obtaining material that may have a bearing on the subject of schizophrenia" (Library of Congress: Margaret Mead Papers [LOC: MMP], box N5, file 1).[2] Lewis also passed on a series of questions brought to his attention by Dr. Margaret A. Ribble, whom Lewis described as "a combination of psychiatrist, psychoanalyst and obstetrician . . . [and] who does a great deal of work with children."[3]

Dr. Ribble's questions would not likely strike current anthropologists as being ethnographically sophisticated; however, these same anthropologists may take these questions as revealing a particular local form of sophistication about rearing children.

Mead was "not quite sure whether [Lewis] meant these questions as suggestions [about what she] should keep an eye out [for], or as points upon which Dr. Ribble want[ed] immediate comparative comment" (LOC: MMP, box N5, file 1; hereafter Mead 1938). Mead may well have found certain questions pertinent to her own line of study. For example, "5. What swaddling is done and when? Is there actual limitation of movement and what is the immediate reaction?," and "6. How much physical contact with the mother takes place in the first months?" She may also have wished to keep in Lewis's good graces. Whatever her reasons, Mead composed a five-page, single-spaced, undated document (ca. August 29, 1938) that she titled "Memorandum in answer to Dr. Ribble's questions" (Mead 1938).

Mead (1938) answered these questions, referring in particular to "the range of [her] experience among Oceanic peoples: Samoa, Manus, Arapesh, Mundugumor, Tchambuli, Bali, Iatmul," as well as in passing to the Omaha, the subjects of Mead's only Amerindian study (Mead 1932; see Molloy 2008), and the Lepchas of Nepal, among whom Geoffrey Gorer had worked in 1936 (on the Lepchas, see Gorer 1938). Mead then added a postscript of sorts that includes the following passage:

> In primitive societies there is a greater congruence between external habits of caring for a child and the attributes developed in members of the society than there is among ourselves and this congruence is revealing, but should not I think be taken to mean that a *method* of suckling, for instance, is *ipso facto* capable of producing a definitive type of character formation. A good deal stronger case can be made out for the determinative nature of the condition of the child at birth. (Mead 1938, emphasis in original)

## First Excursus, or Concerning Swaddling

Mead wrote that she had not found swaddling among the peoples of the Pacific she had studied.[4] She made reference, however, to two potentially relevant cases:

> Balinese babies [were] carried in the sling which is bound around the babies' hips and which constrains the lower parts of the body. Balinese children seem[ed] to concentrate all their activity in their arms and walk later and show[ed] less kicking activity than [did] New Guinea babies. (Mead 1938)

This way of carrying a baby was similar to swaddling, as the baby's lower body was constrained, but as the infant's body was not fully wrapped, the child was left free both to reach and to grasp. Mead made no further comment in the memorandum connecting this technique for transporting young children to any further development of their character.

"The most significant material on swaddling" that Mead (1938) knew of "among primitive people" concerned the cradleboard, used among some Amerindian groups, including the Omaha. To Mead (1938), there seemed "reason to believe that part of the exceedingly self-constrained American Indian personality can be attributed to the cradleboard." She limited the significance of her comments about the use of cradleboards in two ways. First, "there [were] many other cultural agencies forming this character" (Mead 1938). Second, "some tribes," did "not have the cradle board," yet presumably many persons would still manifest "the exceedingly self-constrained American Indian personality" to which Mead (1938) referred. Hence, according to Mead, the technique in isolation was not determinative; a given personality type could derived from different, possibly multiple, sources.

Mead had expressed the view that specific techniques were not sufficient to generate a "definitive type of character development," with its corresponding pattern of habits, to her sister, Priscilla Rosten, in a letter written shortly before the 1938 memorandum to Dr. Ribble. Rosten had written Mead asking advice about raising her recently born son, Phillip. Mead replied, "It's important to remember that no single item of education has much effect in itself, unless it is backed up by attitudes, tones of voice, etc" (LOC: MMP, box R7, file 7, letter from Mead to Priscilla Rosten, dated July 15, 1938).[5]

Further, Mead contended that any particular technique or apparatus "is a mechanical device whereby an emotional attitude *can* be put over"; that

is, any particular technique or apparatus (e.g., a sling or using a cradleboard) was a means of communicating about the tenor of relations between caregiver and child (LOC: MMP, box R7, file 7, letter from Mead to Priscilla Rosen, dated July 15, 1938, emphasis in original). Mead's sister was to understand that any particular technique or apparatus may "have some effect on character structure" but only insofar as that particular technique or apparatus was "correlated with other parts of the educational system, and [was] congruent with them" (LOC: MMP, box R7, file 7, letter from Mead to Priscilla Rosten, dated July 15, 1938). Thus, for example "nursing babies standing, will reinforce the hostility of a hostile mother, but it does not make a mother hostile or a child undernourished, *in itself*" (LOC: MMP Box R7, file 7, letter from Mead to Priscilla Rosten, dated July 15, 1938, emphasis in original). Similarly, "any child that [was] swaddled" or like a "Balinese baby, carried in a cloth sling," would "probably show some effect in its gesture, but whether" any particular way of handling infants would "also effect it [the child] emotionally [would] be due to whether the swaddling [or using the sling was] congruent with parental attitudes" (LOC: MMP, Box R7, file 7, letter from Mead to Priscilla Rosten, dated July 15, 1938).

There is no evidence that I have found in Mead's corpus, published or unpublished, that Mead ever changed her mind on this point. Even Geoffrey Gorer would note more than once that swaddling was but one of the important "clues" to understanding Great Russians (Gorer and Rickman 1949, 129, 198, 216). According to Gorer, "It is *not* the argument of this study that the Russian manner of swaddling their children produces the Russian character" (Gorer and Rickman 1949: 128–29, emphasis in original). "[T]echniques of education," swaddling being an example, were not "the cause of [subsequent] adult behavior" (Gorer and Rickman 1949, 128), nor should Gorer's argument "be interpreted to mean that" these techniques were such causes (Gorer and Rickman 1949, 128). Rather, "[s]waddling" was a "device employed by adults to communicate with the child in its first year of life, to lay the foundation for those habits and attitudes which will be developed and strengthened by all the major institutions in Great Russian society" (Gorer and Rickman 1949, 129).

Mead, in her 1954 essay "The Swaddling Hypothesis: Its Reception," concurred, asserting that Gorer's argument had been misunderstood. Among such misunderstandings was

> an assertion that when a student of national character attempts to delineate the way in which swaddling by Russians communicates to an infant in specific ways which become part of his culturally

> regular character, this description is equivalent to saying that swaddling *per se* by members of any culture will have definitive predictable effects of the same sort on all infants, regardless of culture. (Mead 1954, 398, emphasis in original)

In making this argument, Mead (1954, 398) attempted to distance herself from notions she attributed to Abram Kardiner, in which the projection of "individual fears and hopes which themselves originate[d] in childhood experience" became manifest in "cultural forms."[6] Equally, Mead (1954, 398) tried to separate herself from suggestions she attributed to Kenneth Little, who in her opinion held "that the way to find out whether swaddling was an important element in Great Russian character [was] to trace swaddling as a single trait through a variety of cultures to see if it always has the same effect."[7]

For Mead, Gorer's argument had not reduced institutions to individual psychology in Kardiner's manner.[8] Nor had Mead (1954, 400) taken Gorer's essay to be a study of swaddling, as Little (1950) suggested, primarily on methodological grounds, it should be. Mead's version of Gorer's argument —presented in her 1954 essay and also in her article on national character studies (Mead 1953) published in Alfred Kroeber's (1953) volume *Anthropology Today*—stressed neither Russian civilization nor swaddling. Rather, Mead's discussion emphasized Russians as people who came to embody a disposition that took its significance within particularly Russian contexts. Gorer's argument, paraphrasing Mead's own description of *Balinese Character*, was "not about [Russian] custom, but about the [Russians]—about the way in which they, as living persons, . . . embody that abstraction which (after we have abstracted it) we technically call culture"(Bateson and Mead 1942, xii).

For Mead (1954, 399), Gorer, like others involved in national character studies, had attempted "to understand the complex process by which a child with an innate biologically given potential, exposed to a certain very complex cultural configuration, develops a character structure with observable regularities which can be referred to the experience of being reared in that culture. . . . [T]he forms of acceptance" that Russians display towards "a strong leader whether called Czar or Stalin" would be grounded "in the way children [were] reared to be members of Russian culture," but "the Russian institution of such strong leader[s] . . . [was] not to be attributed to swaddling" (Mead 1954, 398). Concomitantly, Russian emotional life had not taken the form of the "the exceedingly self-constrained American Indian personality" that Mead (1932) had observed among the Omaha.

By 1954, Mead had been consistent on this point, beginning with her memorandum for Dr. Ribble and her letter to her sister, for at least sixteen

years, if not longer. A technique, by itself, whatever its capacities to influence the development of an individual's character, was insufficient, by itself, to effect such a patterned development of habits.

## Returning to the Memorandum

As we have seen, Mead's (1938) memorandum for Dr. Ribble contended that "[a] good deal stronger case [could] be made out for the determinative nature of condition of the child at birth" than for any particular child-rearing technique. In doing so, Mead drew attention to the issue of whether a given child was either healthy or ill through early childhood and hence, by extension, whether the child's experiences of interacting with others were largely similar to or rather different from the experience of other children reared in accordance with the local pattern of the "external habits of caring for a child" (Mead 1938).

For Mead (1938), these "external habits" were "mere physical detail[s] such as holding the child a certain way, or putting it on a cradle board, or feeding it hard or pre-masticated food." Any set of such habits required support "by a great number of other details of cultural behaviour"—especially but presumably not only "the basic emotional set of the mother"—if those external habits were to "shape the child's personality importantly" (Mead 1938). This was true even in societies where "there [was] a greater congruence between external habits of caring for a child and the attributes developed in members of the society" than in ours (Mead 1938). "No mere physical detail" of suckling, of holding children was "*ipso facto* capable of producing a definitive type of character formation" (Mead 1938, emphasis in original).

But where such external habits were congruent with "the attributes developed in members of the society," such habits were, according to Mead (1938), "perhaps one of the most potent ways in which a culture shapes the growing personality of the child to the cultural emphasis." Congruence could breed a continuity of coherence, in no small part, because what Mead (1938) called "the basic emotional set of the mother" would derive to a degree from her own earlier experiences of those same external habits.

## Caveats

Although Mead did not mention such matters directly in her memorandum for Dr. Ribble, at least two caveats apply here. We may trace the first concern in Mead's unpublished papers to the spring of 1933 (Sullivan 2004b; for the original, see LOC: MMP, box S11, file 8). For Mead, the combination of external and internal forces and stresses acting on a given society

could change the social order in ways rendering "the basic emotional set of the mother" (Mead 1938) at odds with either the predispositions of the child, the world in which the child grew, or both.

By external forces, Mead indicated the presence of larger, politically, economically, or militarily more powerful societies. Such forces would certainly have included defeat in expansive colonial wars and its correlates, "pacification" and the effective ending of local warfare, as well as new forms of labor relations, which perhaps called some part of the population away to distant plantations. In one way or another, such forces had already begun to affect all the peoples among whom Mead had already worked; this point was not absent from her thought, though it was also not as well developed as some would like, except perhaps in her Omaha study (Mead 1932; see also Molloy 2008).

By internal stresses, Mead referred to the manifest emergence of significant recessive genetic forms within the breeding population as well as changes in local incentives toward endogamy. She not only explicitly mentioned the effects of the cumulative growth of available knowledge but also implied stresses deriving from what we would now call ecological, epidemiological, or dietary changes.

If these cumulative forces and stresses were sufficiently strong, then a society could cease to be integrated. In *Patterns of Culture*, Benedict (1934, 46) wrote of the tendency of cultures toward integration. Perhaps anthropologists have made too little of this notion of "tendency," for it implies, as Benedict (1934, 223ff) knew, that at times—under some circumstances—cultures, like persons, would not be terribly consistent in the patterns of their thought and actions. In 1935, Mead dedicated the final two lectures of her Columbia University seminar on the study of the individual in culture to such relatively dis-integrated societies and the circumstances of persons living therein (LOC: MMP, box J8, file 11; cf. box O40, file 7; on this seminar, see Sullivan 2005a; for a more recent discussion of ethics and life after cultural devastation, see Lear 2006). Much later, Mead (1959, 206), contrasted Benedict with W. H. R. Rivers, A. R. Radcliffe-Brown, Bronislaw Malinowski, and Claude Lévi-Strauss—all of whom had worked, if only briefly, among a "living people." Mead contended that Benedict

> never saw a whole primitive culture that was untroubled by boarding schools for the children, by missions and public health nurses, by Indian Service agents, traders, and sentimental or exiled white people. No living flesh-and-blood member of a coherent culture was present to obscure her vision or to make it too concrete, when, in the summer of 1927, she saw with a sense of revelation that it would be possible to explain the differences

> among the tribes of the Southwest or the Plains—both in what they had taken from one another and in what they had resisted—as one might explain the choices of an individual who, true to his own temperament, organized his life out of the myriad and often conflicting choices presented to him by a rich historical tradition. (Mead 1959, 206)

Mead and Benedict, both good Boasians versed in the literature of so-called salvage anthropology, were well aware that this tendency toward cultural integration was but a tendency.

A second caveat: Under some circumstances, a caregiver and a child would not have had the same experiences of "the external habits of caring for a child" (Mead 1938). For example, when and where (1) boys and girls are raised differently, when (2) caregivers are predominantly or exclusively female, and when (3) the child is male, a female caregiver and a male child would not have the same early-life experiences of the pattern of these external habits. The same could be said of male caregivers and their female charges. Mead (2001) later discussed these issues in *Male and Female*, contending that, under such conditions, relations between caregiver and child would be more complementary than symmetrical.

According to Mead's (2001, 59) later formulation, "each of the pair [in complementary relations was] seen as playing a different rôle, and the two rôles [were] conceived as complementing each other." Symmetrical relations, by contrast, required that "the mother behave[d] as if the child were essentially similar to herself, and as if she were responding to behaviour of the same type as her own" (Mead 2001, 59). Mead (2001, 361 n. 2) acknowledged Bateson's discussions of schismogenesis as the source of her terminology (e.g., Bateson 1936, passim).

For Bateson (1936), whether complementary or symmetrical, schismogenic interactions tended toward characteristically cumulative, intensifying, and eventually climactic, even socially destructive encounters, unless someone introduced a contrasting and emotionally defusing form of behavior.[9] By 1938, when she responded to Dr. Ribble's questions, Mead would have been well aware of Bateson's terms and their pertinence for describing a "psychology of culture" arising "in the relations of [and between] individuals" (Sapir 1994, 181). Either complementary or symmetrical interactions would have given the relations between caregivers and young children their characteristic trajectory and emotional tenor.

Mead's 1938 memorandum did not directly refer to her theory of the squares. Most discussions of the squares have noted that, while in New Guinea in 1933, Mead, Bateson, and Reo Fortune read and discussed a

draft of Benedict's (1934) *Patterns of Culture*. Encouraged by Mead's (1972, 217) own account, these discussions have tended to see the squares hypothesis as a continuation of Benedict's analyses and, to a lesser extent, of Carl Jung's theory of psychological types.[10] Less widely noted is the point that Bateson (1979, 192) later viewed these same discussions and his subsequent "descriptions of Iatmul men and women" as leading "away from typology and into questions of process." Mead's discussions in *Sex and Temperament in Three Primitive Societies* (1935) and *Balinese Character* (Bateson and Mead 1942) were crucially organized not only around matters of temperamental types but also around questions of process and development. This is apparent in the very order and architecture of her analyses.

## Second Excursus, or Initial Comments on Mead, Benedict, and Gestalt Psychology

I have discussed elsewhere Mead's and Bateson's knowledge of Erik Erikson's (1937) zonal-modal theory with its notion of progressively differential patterns of human development (Sullivan 2004b). I understood Mead as addressing questions of the production and reproduction of particular embodied personality forms deriving from (1) heritable psychological dispositions of the sort Mead called temperament; (2) temperament's cultural correlative, that is, ethos; and (3) patterns of psychological habit, or what Mead termed character. Character, in this sense, developed over the course of a people's lives from the conjoined interaction of their temperament, the accidents of their life, and the local patterns of culture within and against which they lived those lives. Unlike in xerography, the production and reproduction in these processes from which character derives do not necessarily, perhaps cannot, yield perfect copies.

I suggest that a fuller exposition of these matters would require looking back to Kurt Koffka's book *The Growth of the Mind* (1927). In *An Anthropologist at Work* and later in *Blackberry Winter*, Mead (1959, 207; 1972, 125) wrote that she had read Koffka. She lent her copy of Koffka's book to Edward Sapir and discussed the book with both Sapir and Benedict before she left for Samoa in 1925. Mead's comments are unusual not so much because she refers to Sapir and Benedict or their developing interest in "pattern" or even their shared interest in gestalt psychology. Rather, Mead's references to Koffka are unusual because they were among the very few instances in her (auto)biographical and historiographic writing in which Mead mentioned reading a book—any book—much less that "echoes" of a book "came" "into [the] discussions" she had with Bateson, which led to

the formulation of the theory of the squares (Mead 1959, 207). By contrast, Mead (1972, 124, 217) only implied that she had read Jung's (1921) *Psychological Types*. Nor did she provide a source for the terms "temperament" and "character," though she adapted these terms to her purposes from the work of William McDougall (Sullivan 2004b).[11] Mead provided no reference that I have been able to find for the source of the concept of "psychological load," of which she was so fond. That source was June Etta Downey's (1924) psychological work.[12] We are, therefore, not at liberty to dismiss Mead's debts to the gestalt psychologists too quickly or easily.

In *The Growth of the Mind*, Koffka (1927) introduced a child psychology organized around the concept of *struktur*. For Koffka, such structures initially arose as the infant's nervous system adapted itself to the wider world and that world's shifting stimuli through the infant's active perceiving of and responding to that world. The structures themselves were organizations of apperception, including both the perceiving individual and the stimulative world in a single whole not reducible to its parts. Both the infant's shifting capacities and the perceptible characteristics of the stimulus crucially influenced the processes of the infant's ongoing development, a point that I shall return to below. For the moment, it is sufficient to note that for the gestalt psychologists, such structures were, as Maurice Merleau-Ponty (1964, 117) noted, "those wholes articulated by certain lines of force and giving every phenomenon its local value" within "the configurations of the perceptual field." Put another way, such structures were the world rendered psychologically real for any individual or group of persons living that world.

Koffka (1927, ix) and his translator, Robert M. Ogden, together agreed to translate *struktur* as "configuration." They hoped thereby to avoid problems posed by the "very definite and quite different meaning in English and American psychology" that using the term "structure" would have occasioned, given the "controversy between *structuralism* and *functionalism*" in American and British psychology of the era (Koffka 1927: xv–xvi, emphasis in original).[13]

Benedict had also read *The Growth of the Mind*. She owed a general debt to Koffka and the gestalt psychologists (Caffrey 1989: 151–52, 154). Benedict began using the term *configuration* in much the gestalt psychologist's manner by the early 1930s.

In *Patterns of Culture*, Benedict argued that cultures may exhibit a tendency toward integration through processes of both selection and adaptation of available or borrowed "traits" to their diverse, respective purposes. Mead (1935: v–xiv) reprised Benedict's argument concerning integration in the introduction to *Sex and Temperament*.

As part of her argument, Benedict wrote of the gestalt psychologists, albeit not of Koffka by name:

> The *Gestalt* (configuration) psychology has done some of the most striking work in justifying the importance of this point of departure from the whole rather than from its parts. *Gestalt* psychologists have shown that in the simplest sense-perception no analysis of the separate precepts can account for the total experience. It is not enough to divide perceptions up into objective fragments. The subjective framework, the forms provided by past experience, are crucial and cannot be omitted. The "wholeness-properties" and the "wholeness-tendencies" must be studied in addition to the simple association mechanisms with which psychology has been satisfied since the time of Locke. The whole determines its parts, not only their relation but their very nature. Between two wholes there is a discontinuity in kind, and any understanding must take account of their different natures, over and above a recognition of the similar elements that have entered into the two. The work in *Gestalt* psychology has been chiefly in those fields where evidence can be experimentally arrived at in the laboratory, but its implications reach far beyond the simple demonstrations which are associated with its work. (Benedict 1934: 51–52)

Benedict's view was psychological insofar as it implied a human interiority complimentary to, if not exactly of the same order as, the surrounding lived world. For her, personalities and cultures bore metaphorical similarities without being of the same order. Yet Benedict's version of human, as opposed to cultural, psychology was neither particularly nor necessarily dynamic.

Writing to Mead on August 3, 1938, Benedict showed interest in a possible book project for Mead concerning the differences between the sexes:

> The way to approach it may be very well be through the phraseology of the zones, and it would be worth trying, but the zones have never really clicked for me. I suppose it's because the zonal discussions are all mixed up with a series of stages through which the human life cycles progress, and it seems harder to me to disentangle the salient points than to begin over and stick just to the conditioning without any particular use of what's been said about zones. (LOC: MMP, box 5, file 9, Benedict to Mead, letter dated August 3, 1938)[14]

In this context, "zones" must be understood to have referred to theories of the sort developed by Erikson (1937) and specifically to those parts of the body—classically oral, anal and genital—through which the child's capacity to exert control of his or her body and, to some extent, the broader world moves as the child's mind, in Koffka's sense, and body develop. By sticking with "conditioning" and by suggesting that "the character of [Iatmul] *tamberan*[15] organization works itself out in their character formation exactly as one would expect," Benedict (LOC: MMP, box 5, file 9, Benedict to Mead, letter dated August 3, 1938) had aligned herself with the behaviorists in psychology on the one hand and with the emerging culturalists in anthropology on the other. Benedict chose to explain mind from the vantage of the stimulus or, put another way, by reference to external habits alone.

For this reason, Mead (1946, 428) would later describe "Benedict's theory" as "the most culturally based theory of personality" from among those scholars now loosely and somewhat erroneously grouped together as "the culture and personality school" (cf. Sullivan 2005a). Benedict did "not rely upon any assumption of systematic differences in temperament or constitution, nor upon any theory of limited possibilities." (Mead 1959: 546–47 n. 21).

According to Mead (1946, 481), Benedict treated "culture over time as analogous to personality." Culture, through the "selection of certain types of behavior and the rejection of others" over long periods of time, could but need not necessarily obtain a greater consistency or integration than would necessarily be found "in the life history of a single individual" (Mead 1946, 481).

Benedict's patterns arose "not so much in the interpersonal relations of individuals as in the formal elements of culture," such as "religion, myths, formal speeches, [and] magic" (Mead 1946, 481). Thus, Benedict's thought referred neither to bodily processes as such nor to the behavioral interaction of people with one another but rather to understandings of the external world as both imagined and therefore lived.

In her letter to Mead of August 3, 1938, Benedict (LOC: MMP box 5 file 9) described her own attitude toward such a dynamic psychology as potentially "very wasteful." In the next passage, she also wrote,

> As soon as [Mead could, she] must write a book on childhood conditioning. People don't understand and there's no one but you to write it. When I want points I have to go back to my notes on the course you gave at Columbia. It's a book that would just roll off your pen and you probably won't believe until you get back to civilization how much it's needed. (LOC: MMP, box 5, file 9, Benedict to Mead, letter dated August 3, 1938)

## Third Excursus, or Further Comments on Mead and Gestalt Psychology

It is not entirely clear that Mead ever wrote the book on "childhood conditioning" that Benedict had hoped for, though that book-in-the-mind may have been *Male and Female* (Mead 2001). Mead's understanding of this subject at the time of Benedict's letter would have informed the memorandum she wrote that same month, August 1938, in response to Dr. Ribble's questions. Mead later listed the gestalt psychologists, notably Koffka and Kurt Lewin, as among the psychological influences on national character studies. If influence by or, more likely, a confluence of interest and opinion with the gestalt psychologists is to be found, in Mead's thought of the 1930s the memorandum would be a likely place.

Mead maintained contacts with leading gestalt psychologists over the years, including both Koffka and Lewin. Further, she attended the gestalt psychologists' Christmas conferences in 1935 and 1940, and Bateson joined her at the latter conference (see Gilkeson 2009).

On January 3, 1936, Mead wrote to Bateson concerning primarily her happiness with the world and about her plans for her impending ship journey to Batavia (now Jakarta, Indonesia), where they would meet prior to going on to Bali.[16] In this letter, she described having lunch with Koffka at a conference of gestalt psychologists:

> I got quite a little about the Gestalt point of view, especially about the Lewin approach which they call topology. I think I can use it to show the relationship between personality and social structure, [Radcliffe-]Brown's kind of social structure I mean, not yours. (LOC: MMP, box S1, file 6, Mead to Bateson, letter dated January 3, 1936)

Bateson described Radcliffe-Brown's notion of structure as referring to "*society*," where the "units" of study were "human individuals . . . linked together in groups," a description that accords well with Radcliffe-Brown's own subsequent description of his position (Bateson 1936: 25–26, emphasis in original; cf. Radcliffe-Brown 1940: 190–91). Bateson (1936: 25–26) also developed an idea of what he called cultural structure, wherein "details of [human] behavior" were "linked into a 'logical' scheme." Bateson (1936, 26) conceived of these two sorts of structure as the same sorts of "phenomena," albeit studied "from two different points of view."

Mead published *Cooperation and Competition among Primitive Peoples* in 1937 but had effectively completed the manuscript before she sailed for

the Dutch East Indies (modern Indonesia) in 1936. She explained to Bateson that in this book she had "still used [Radcliffe-]Brown's definitions" in that book because she wrote "if I try to use yours [Bateson's], I come to points we haven't discussed, and then I don't know how to say it" (LOC: MMP box S1, file 6, Mead to Bateson, letter dated January 3, 1936). While she clearly looked forward to working on their shared vocabulary, Mead also implicitly suggested that one of the subjects that she and Bateson would have to address was "the relationship between personality and social structure" (LOC: MMP box S1, file 6, Mead to Bateson, letter dated January 3, 1936).

Mead and Bateson's proposals of 1935 and 1936 for their Balinese researches built on the theory of the squares and, ultimately, on their earlier discussions with Reo Fortune while the three were among the Tchambuli during 1933.[17] These proposals held that personality arises in the conjunction of (1) the person's constitution or temperament, meaning his or her inherited, innate disposition; (2) the conditioning or training which the person experienced, organized in accordance with the specific culture's regularities; (3) those accidents peculiar or particular to his or her life; and (4), in Bateson's addition, the person's reaction to this conditioning and those accidents of experience. The terms of these 1935–1936 proposals clearly prefigure Mead's (1954, 399) subsequent description of what the national character studies would later attempt to understand.

Such a conjunction of temperament, culture, character, accident, and reaction would likely have left, as Mead phrased it in 1935, "the factors with which the student has to deal are too complex and too incapable of control," rendering "[a]ll attempts to study the individual within society, in regard to his good or poor functioning, . . . nugatory (LOC: MMP, box N5, file 1, "A Plan for the Study of the Origins of Mental Disorders with a View to Isolating the Cultural and the Biological Factors," Mead's research proposal submitted to CRDM, September 12, 1935).

Lewin, like Koffka before him, went a step further in the ongoing discussion of the relationship between biology and psychology, especially social psychology:

> The sterility, for example, of the always circular discussion of heredity and environment and the impossibility of carrying through the division . . . of the characteristics of the individual begin to show that there is something radically wrong with their [both hereditarian and environmentalist] fundamental assumptions. (Lewin 1935a, 40; cf. Koffka 1927)

For Lewin (1935a, 41), psychology, like other disciplines studying such matters, was in a transition away from Aristotelian views concerned with the internal teleologies of "single isolated objects." For that earlier psychology, such "single isolated objects" would have been individual human beings considered without reference to their environment, no matter how dynamic their development (Lewin 1935a, 41).

Lewin (1935a, 41) called the emerging understanding within the human sciences "a Galilean view of dynamics," using an analogy with the physics of motion. Such a transition, he hoped, would lead the human sciences to understand that "[*t*]*he dynamics of the processes* [were] *always to be derived from the relation of the concrete individual to the concrete situation* (Lewin 1935a, 41, emphasis in original). In this view, understanding the "momentary condition of the individual" required attention to "the mutual relations of the various functional systems that make up the individual" (Lewin 1935a, 41). At another level, when concerned with "the psychological structure of the situation," Lewin's view established the possibility of a multiperson or properly social psychology (Lewin 1935a, 41). While Mead would have thought Lewin's ideas about "the momentary condition of the individual" similar to her own notions of temperament and character, Mead also suggested to Bateson that Lewin's approach to "the psychological structure of the situation" was comparable to Radcliffe-Brown's concern with "*society*," where the "units" of study were "human individuals . . . linked together in groups" (Lewin 1935a, 41; LOC: MMP box S1 file 6, Mead to Bateson, letter dated January 3, 1936; Bateson 1936: 25–26, emphasis in original; cf. Radcliffe-Brown (1940: 190–91). For his part, Bateson (1936: 175–76, emphasis in original) was "inclined to regard the study of *the reactions of individuals to the reactions of other individuals* as a useful definition of the whole discipline which is vaguely referred to as Social Psychology."

In Lewin's (1935b, 43, italics in original) view, any stimulus would possess "*an adhesion with certain reactions*." That is, not only would the stimulus, whatever it might be, and the stimulated living being become conjoined in these processes, but also the processes would lead on toward further reactions. Such stimuli would have included, for example, what Mead (1935) called "the external habits of caring for a child" and "the basic emotional set of the mother."

According to Lewin (1935b, 48), "[i]n every process *the forces in the inner and outer environment are changed by the process itself*." Lewin (1935b, 48) continued, contending that "the totality of the forces present in the psychical field" controlled any interactive process. Lewin's "attractive object"—that is, the stimulus whatever it might be—controlled "the

direction of the [interactive] process" only insofar as this object remained part of "the totality of the forces present in the psychical field" (Lewin 1935b, 48). The same caveat implicitly applied to the person or persons being stimulated:

> This change of the forces controlling the processes may, however, be of very different degrees in different processes, so that in many processes this change [was] not essential to the course of the process itself, while in others the course of the process [was] fundamentally influenced thereby. (Lewin 1935b, 48)

Lewin's thought, then, is not far removed from Bateson's idea of schismogenesis, save that in Lewin's version, unlike Bateson's, there is no sense that such encounters must culminate in some emotive, violent, or orgasmic climax (see Sullivan 2004c; cf. Bateson 1936, 175ff). In Lewin's version, the course of any particular interactive processes would depend on the specific qualities of all the entities—persons or any other forces—within the psychical field or, put another way, all those forces and entities considered together.

To the extent that no two persons are exactly alike, if only because they are not the same person, any developmental process they underwent, while perhaps quite similar, would differ from person to person, if only in some small detail. But we may also infer from this, as Mead and Bateson would most probably have done, that, to the extent that two persons grew up responding to the same general set of culturally organized stimuli, their personalities would likely become similar, albeit not absolutely or even necessarily obviously so.

## Breast-Feeding, as an Example

As Mead well knew, Erikson's zonal-modal psychology described a series of divergent developmental sequences "of a large variety of different types of character structure," which, for Bateson were "as met with in different cultures" (Bateson 1949, 38, n. 2; cf. Erikson 1937). The first of Erikson's zones was the mouth because this was the first of several portions of the growing infant's body over which the infant could exert control of itself and, thereby, control over some small portion of its environment. The modes of an infant's possible engagement with some object could differ. The infant could hold something in his or her mouth, refusing to let it go; could allow objects or substances to pass into and out of his or her mouth; or could refuse access thereto. Erikson (1937) was not overly concerned with the

qualities of these objects or substances as such. In this respect, Erikson's psychology differed from that of Koffka and Lewin.

Koffka's psychology, like Lewin's, eschewed explanations couched solely in terms of nurture, or the only apparently more adequate but equally flawed terms of nature alone. Rather, Koffka preferred to remind us that reference solely to either nurture or nature was insufficient, as both nurture and nature were operationally intertwined and, therefore, necessary. Thus, in discussing why suckling is a complicated, instinctive act and not a reflex, he contended, in part,

> The movement [i.e., suckling] depend[ed] upon the stimulus in the sense of being adapted to it . . . because the act of suckling [was] regulated directly by the formal characteristics of the stimulating object. Thus the position of the lips in suckling must be different according as it [was] the breast nipple, a rubber nipple, an adult's finger, or the child's own finger which [was] being sucked. (Koffka 1927, 87).

Even as suckling was one of those "modes of behaviour . . . which originate neither in experience or in deliberation," its movements depend "upon the stimulus" to which the movements and the infant, considered as a whole, adapt (Koffka 1927, 87). Adaptation, here as elsewhere in Koffka's work, must be understood as an activity undertaken by the adapting entity. Koffka's discussion addressed processes by which the nervous system assumes the shape we would now want to call something like the embodied mind, the mindful body, or, with Gerald Edelman, "higher order consciousness" (Edelman 2004, 97ff).

We must note that neither Lewin nor Koffka, in this specific context, discussed suckling as a social interaction occurring in culturally variable and culturally specific contexts. From Mead's developing point of view, the specific qualities of breast-feeding—considered as a social interaction occurring in culturally variable and culturally specific contexts—necessarily involved a further dimension, for Mead a child adapted not just to the qualities of the nipple but also to those of the enculturated woman or women (or even men) whose nipple(s) the child suckled. Suckling would be like any other technique or apparatus, a means or "devise whereby an emotional attitude can be put over" or communicated between caregiver and child, as Mead would explain to her sister and as Gorer would subsequently note (LOC: MMP, box R7, file 7, letter from Mead to Priscilla Rosten, dated July 15, 1938; Gorer and Rickman 1949, 129). By extension, therefore, the child would also have begun to adapt him- or herself to the

local cultural structure, in Bateson's (1936: 25–26) sense, wherein "details of [human] behavior" were "linked into a 'logical' scheme" by which and through which these persons lived.

## Breast-Feeding Woman as Already Enculturated

In her memorandum for Dr. Ribble, Mead (1938) did not distinguish the types found in her squares hypothesis but rather what she called an "active, aggressive . . . type of personality" from, by implication, a form of passive personality typified by the Balinese. Mead (1938) was well aware of what she called the "gross differences" of culture and "the question of whether the child sle[pt] in its mother's arms, or in a separate cradle." Still, Mead was inclined "to think the most significant difference [between these two types of personality] is whether the child's body" was "held *off* from the mother's body, *out* in the hand, *up* on the shoulders"—as it was among what she called "[t]he active cultures"—or "whether the child's body is held against the mother's body, curved relaxed, adapted to the mother's postures," as among the Balinese (Mead 1938, emphasis in original).

How the child's body was held or carried had implications for the sorts of interactions that would become characteristic between the mother and child:

> [M]ethods of carrying in which the baby [was] hung from the mother's back in a bag or basket, or carried in the outstretched hand, or flung face down over the shoulder, or set on the shoulder, or carried on the back with the arms clasped around the neck or set on the shoulder round the neck, all mean[t] that the act of giving the child the breast [was] definite and ha[d] a beginning and an end, noted by the mother as well as the child. (Mead 1938)

Under these circumstances, when children wished to feed, they must, by some means or other, command the attention of their mothers, and, concomitantly, the "women tend[ed] to stop their work to suckle a child, wait[ed] impatiently until its hunger [was] assuaged, and then [went] back to work" (Mead 1938). Each partner in this relationship was deliberate in his or her actions, while the repeated interaction potentially paired upset against impatience, especially if the mother did not enjoy nursing.

As we have seen, Mead would explain to her sister, Priscilla Rosten, that "nursing babies standing, will reinforce the hostility of a hostile mother, but it does not make a mother hostile or a child undernourished, *in itself*" (LOC: MMP, box R7, file 7, Mead to Priscilla Rosten, letter dated July 15,

1938, emphasis in original). So, too, having to wait might upset a baby already prone to being upset or evoke impatience in an impatient mother, but it need not make the baby upset or the mother impatient in and of itself.

Not so with mountain Balinese "babies [who were] carried in a cloth sling from birth up to the age of two or so, with the breast always there . . ." (Mead 1938). "The sling . . . [was] bound around the babies' hips. This position "constrict[ed] movement in the lower part of their body" and pinioned the right hand against or behind the caregiver's body (Mead 1938). "The baby [hung] securely from the sling, the mother's hands [were] free and the baby [could] suckle as the mother pounds rice for instance" (Mead 1938). Such babies "habitually [fell] asleep still suckling gently" (Mead 1938)—upset rarely paired with impatience; indeed, neither partner needed to disturb, much less command, the other's attention at all.

These external habits of Balinese childcare were part of an analogic set in which "all through babyhood, the child is fitted into a frame of behavior, of imputed speech and imputed thought and complex gesture, far beyond his skill and maturity" in which "he [would] be echoing" words and gestures (Bateson and Mead 1942, 13). The words would have "already been said, on his behalf and in his hearing, hundreds of times" (Bateson and Mead 1942, 13). As the child assumed postures or made gestures, such as extending a right yet pinioned hand to receive anything or learning a dance, the child's body was directly manipulated into position by others. These external habits, though powerful and suggestive of the forms of the resultant personalities, had been "supported by a great number of details of cultural behaviour, the most significant of which is the basic emotional set of the mother" (LOC: MMP, box R7, file 7, Mead to Priscilla Rosten, letter dated July 15, 1938). Thus, those habits could prove "capable of producing a definitive type of character formation" (Mead 1938).

This propensity toward mutual emotional unobtrusiveness became further complicated by "a series of broken sequences" initiated by the caregiver and a series "of unreached climaxes" experienced by the child (Bateson and Mead 1942, 32):

> The mother continually stimulate[d] the child to show emotion—love or desire, jealousy or anger—only to turn away, to break the thread, as the child, in rising passion, [made] a demand for some emotional response on her part. When the baby fail[ed] to nurse, the mother tickl[ed] his lips with her nipple, only to look away uninterested, no slightest nerve attending, as soon as the baby's lips close[d] firmly and it be[gan] to suck. (Bateson and Mead 1942, 32)

A Balinese caregiver may well have initiated interpersonal engagements, "stimulat[ing] her child to active response[s]" (Bateson and Mead 1942, 152), evoking thereby anger, fear, or flirtatious desire in the child—only to turn away once the child began to respond emotively to their caregiver's stimulating activity. In response, "the more directly assertive children of both sexes" tended towards tantrums; "the more passive of both sexes" were more likely to sulk in response to the failure of their "attempt[s] to introduce some sort of climax into the sequences of everyday interpersonal" engagements with their mothers or other caregivers (Bateson and Mead 1942: 155–56). Children of either sex might respond in either way, depending on their temperament.

According to Bateson and Mead (1942, 155), Balinese "[a]dults usually [did] not respond to either the sulks or the tantrums of their children." The adult had already learned "the Balinese habit[s] of feeling and titivating [tidying or stimulating] the skin, . . . introversion" of fantasizing that the "body as made of separable parts" and "avoidance of inter-personal climax," all of which, Bateson and Mead contended, could draw the adult's attention away from the child and back on the self (Bateson and Mead 1942, 151). It was "the child who has not yet learned the drawbacks of responsiveness and the satisfactions of Balinese gaiety" (Bateson and Mead 1942, 151).

## First Coda: Narcissism

In the Balinese case, according to Mead, the mutual emotional unobstrusiveness that obtained between caregiver and child as described briefly just now was both supported by and congruent with a maternal attitude to which Bateson and Mead (1942, 152ff) accorded the rubric "narcissism."

Narcissism, understood as the turning of life energies turned back onto or into the self, was far from irrelevant to then current theories of dementia praecox. Eugen Bleuler (1911), a leading Swiss psychiatrist and for many years Jung's supervisor, had developed a theory of the group of schizophrenias organized around a dissociation of the self from the surrounding world and, in some cases, from the self's emotional responses thereto. Among the mechanisms that Bleuler (1912) proposed were "ambivalence," a term he coined to designate the diffusion, even utter dissipation, rather than the unity, of emotion, and "negativism," that is, the refusal to engage emotionally.

Paul Federn (1928) had contended that some degree of narcissism was necessary.[18] But if engagement with the self came to exclude emotional engagement outside the self, narcissism could take on a pathological caste.

Federn's analysis of narcissism is not unlike Jung's commentary on introversion. For Jung, introversion was necessary if there was to be any integration of mental life, but excessive introversion led away from the world (Jung 1921; see also Sullivan 2004b). According to her squares hypothesis, Mead termed a generally narcissistic adaptation to life "fey" (Sullivan 2004a, 2004b).

For Bateson and Mead, the characteristic encounters of Balinese life and custom gave rise to fey persons and to complementary ethos; in a Western context such an adaptation could, as the concerns of eminent psychiatrists reveal, yield persons at significant, even humanly destructive, odds with the tenor of their society. For present purposes, then, Mead's analysis of the development of Balinese character was at least as relevant to the study of dementia praecox as another study funded by Lewis and the CRDP that focused on shy children in Saskatoon, Saskatchewan, Canada.

## Final Coda, or Back to Where We Started

Koffka died in 1941. During World War II, Bateson, Benedict, Lewin, and Mead all served the Allied efforts, with Lewin and Mead both joining the Committee on Food Habits (for Benedict's war effort, see Schachter 2009). Lewin died in 1948; Benedict followed in 1949. Mead and Bateson lived separately during much of the war before divorcing in 1950. Neither Bateson nor Mead obtained faculty positions with teaching responsibilities for a number of years.

The synthesis of anthropology and gestalt psychology that these scholars together might have generated—had they lived and prospered together—largely failed to gather adherents. Their joint interest in psychologies that "arise in the relations of [and between] individuals" (Sapir 1994, 181) has been too readily forgotten.

Sapir continued to criticize both Benedict's and Mead's work until his death in 1939. Regna Darnell (1990, 429 n. 7) holds that "Sapir's former students . . . did not see Mead as relevant to Sapir's work or as close to him personally." Mead played no part in the memorial volume for Sapir, edited by Leslie Spier, Irving Hallowell, and Stanley Newman (1941).

Unlike Mead and Bateson, many of Sapir's students and younger colleagues did obtain teaching positions; they furthered Sapir's legacy as well as his critiques of others' work. One might argue about whether Sapir's criticisms were apt when applied to Benedict; as we have seen, they had no reasonable application to Mead. Sapir's mischief has thus been compounded.

## NOTES

I presented earlier versions of this article to the Department of Anthropology seminar at the University of Notre Dame, South Bend, Indiana, on October 17, 2003; the 2003 American Anthropological Association meetings in Chicago on November 19, 2003; and the 2005 Association for Social Anthropology in Oceania meetings, Lihue, Hawai'i, on February 6, 2005. Quotations from the Margaret Mead Papers appear courtesy of Mary Catherine Bateson and the Institute for Intercultural Studies. Virginia Young first introduced me in a serious way to the subject of Mead's scientific program; I greatly appreciate her continuing encouragement, kind comments, and insights. None of my work would have been possible without the friendship and assistance of the late Mary Wolfskill, former head of the Reference and Reader Service Section of the Manuscript Division at the Library of Congress, and her colleagues. I thank Mary Catherine Bateson and Patricia A. Francis for their support and aid. Although I have not quoted from the papers she provided me, Lizette Royer of the National Psychological Archives at the University of Akron was also of great assistance.

1. Whether Sapir (1994, 181) was correct in his contention that Benedict conflated the configurations of culture with "the psychology of culture . . . [arising] in the relations of individuals" is well beyond the scope of this paper. So too is any question of whether or how Sapir's (1994, 183) contention that culture was "just a low-tone series of rituals, a rubber stamping waiting to be given meaning by" individuals can be squared with his nearly contemporaneous observation that

> [i]t is exceedingly difficult, if not impossible, to teach a native to take account of purely mechanical phonetic variations which have no phonemic reality for him. (Sapir 1994, 183; 1933, 48)

For the moment, one might wonder why, if phonemes have psychological reality for speakers, rituals and other social forms would not have such a reality for those who live them out.

2. As in all my previous work, I proceed as a prudent editor when quoting from work that Mead or her correspondents had not prepared for publication. I make modest corrections of spelling and grammar, but only where these do not change the plain meaning of the text. Mead's correspondence and unpublished materials used underlining to show emphasis, indicated in this paper as italics.

3. Margaret (or Margaretha) Antoinette Ribble (1890–1971) was a British pediatrician who pioneered working with children and their emotional problems before Melanie Klein and Anna Freud developed child analysis (Gail Donaldson, pers. comm., May 24, 2006). Dr. Ribble published two books, *The Rights of Infants* (1948) and *The Personality of the Young Child* (1955). I am also indebted to Michael Sokal for his assistance in researching Dr. Ribble's death date.

4. I am indebted to Ira Bashkow for his suggestion to include a discussion of the swaddling hypothesis; any errors in interpretation are perforce my own.

5. My thanks to Patricia A. Francis for bringing this letter to my attention.

6. Abram Kardiner (1891–1981) is perhaps best known for the seminar that he and Ralph Linton organized in New York City during the early 1940s to apply psychoanalytic

insights to ethnographic materials collected by Linton, Cora Du Bois, and Clyde Kluckhohn, among others. Kardiner's book, based on the seminar *The Psychological Frontiers of Society*, appeared in 1945.

7. Mead refers to Kenneth Linsay Little, an anthropologist who worked among the Mende of Sierra Leone and on issues relating to race in Great Britain. Little became professor of social anthropology at the University of Edinburgh in 1965.

8. For an elaboration of Mead's argument concerning individuals in cultures, see Sullivan (2005b).

9. On the dynamics of such encounters as a theme in Bateson's thought, see Sullivan (2004c).

10. On the squares, see Sullivan (2004b); see also Banner (2003, 238ff), Boon (1990, 186), Gewertz (1984), Lapsley (1999, 222ff), McDowell (1991, 293ff), and Molloy (2008). On Reo Fortune's response to the squares, see Thomas (2009).

11. William McDougall (1871–1938) was a member of the Torres Straights Expedition and later professor of psychology at Cambridge, Harvard, and Duke universities. He was one of the founders of social psychology, a term now more associated today with Kurt Lewin.

12. June Etta Downey (1875–1932) received her PhD at the University of Wyoming and later served there as professor of psychology. Downey's notion of load concerned psychological inertia: the ongoing preservation or attenuation of some previous mood, disturbance, or tension.

13. Histories of psychology have traced notions of structuralism to E. B. Titchener's (1908) work, for example, on the perception of phenomena prior to the interpretation of such phenomena and the so-called imageless thought controversy (see, for example, Kroker 2003). Accounts of the dispute between the structuralists and functionalists, including Mead's teacher, Robert Sessions Woodward, can be found in many of the standard histories of American psychology. On Mead's study with Woodward, among others, see Francis (2005).

14. I am indebted to Virginia Young for bringing this letter to my attention and for providing me with a copy. Patricia A. Francis found the Library of Congress box and file citation for me; any error of interpretation is perforce mine. I have deliberately left Benedict's phrase "human life cycles" in the plural. Changing this to the more conventional singular "cycle" would, in my view, alter the plain meaning of the text.

15. The *tamberan* is a men's cult practiced in parts of New Guinea. Mead (1935) referred to *tamberan* among both the Arapesh and the Tchambuli. Bateson's (1936) work concerned *naven* rather than *tamberan* ceremonial among the Iatmul.

16. I am indebted to Patricia A. Francis for bringing this letter to my attention.

17. For the originals, see LOC: MMP, box N5, file 1, and box N6, file 2.

18. Paul Federn (1871–1950) originally trained as a pediatrician before he met Sigmund Freud in 1902. Federn subsequently taught at the Vienna Psychoanalytic Institute.

Freud later appointed Federn as his personal deputy. Federn, like Freud, emigrated to avoid Nazi persecution but to the United States instead of Britain.

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# CLYDE KLUCKHOHN AND THE NEW ANTHROPOLOGY: FROM CULTURE AND PERSONALITY TO THE SCIENTIFIC STUDY OF VALUES

John S. Gilkeson
*Arizona State University*

This essay examines Clyde Kluckhohn's relations with Ruth Benedict, Margaret Mead, and Gregory Bateson in two contexts: the school of culture and personality, and the Conference on Science, Philosophy, and Religion in Their Relation to the Democratic Way of Life convened during the Second World War. Kluckhohn strongly identified with the Boasian tradition. Enlisting in the Boasian campaign to make Americans more "culture-conscious," Kluckhohn joined Benedict and Mead as a public intellectual. In this capacity, Kluckhohn sought to clarify the concept of culture and to widen its currency, emphasized the affinity between anthropology and psychiatry, and, after 1945, searched for the integrating principles of cultures.

## Introduction

In 1949, Clyde Kluckhohn published *Mirror for Man: The Relation of Anthropology to Modern Life*. His book, which won a $10,000 prize offered by McGraw-Hill for the best popular book on science, was a "manifesto" of "the New Anthropology." Popularized by Ruth Benedict and Margaret Mead, the New Anthropology was, in the words of the critic Robert Endleman, "anthropology with a message"—the message that anthropologists now commanded "the knowledge needed to reform the world." The New Anthropology had been popularized, between the world wars, by Margaret Mead, who instructed the educated public on such problems as adolescence, child rearing, and gender roles. Ruth Benedict "forged" the

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"link" between studying "the exotic primitive" and solving "the problems of modern society." Whereas Benedict marshaled anthropological knowledge to shatter Americans' "ethnocentric ethical conceptions," Kluckhohn hoped to derive the "ultimate values" with which social scientists could promote a peaceful postwar world. (Endleman 1949: 285–6, 290).

In *Mirror for Man*, Kluckhohn declared that anthropology was "no longer just the science of the long-ago and far-away," it was "an aid to useful action." Thanks to the "all-embracing" or holistic character of their discipline, anthropologists occupied "a strategic position" to determine which "factors" would "create a world community of distinct cultures and hold it together against disruption." Only those experts who, like anthropologists, were "singularly emancipated from the sway of the locally accepted" could surmount the apparently "unbridgeable gap" between "competing ways of life" by laying bare "the principles that undergird each culture" ([1949a] 1985: 286–7). On the heels of the publication of *Mirror for Man*, Kluckhohn appeared on the cover of the January 29, 1949, issue of the *Saturday Review of Literature*, proclaiming that anthropologists now had "the beginnings of a science whose principles are applicable to any human situation."

Described by a reviewer as a "prophet" of the New Anthropology (Mishkin 1949, 15), Kluckhohn captured anthropologists' exuberance and heady optimism, born of their wartime service and access to policymakers, that they would play prominent roles in postwar reconstruction. No longer practitioners of what Clifford Geertz (2002, 3) has described as "an obscure, isolate, even reclusive, lone-wolf sort of discipline," they would increasingly participate in "multi- (or inter-, or cross-) disciplinary work" and "team projects," lavishly funded by philanthropic foundations and, in some cases, by federal agencies, dedicated to solving "the immediate problems of the contemporary world." This enthusiasm proved infectious. For a brief moment, anthropology loomed as "the reigning social science" in the eyes of many political scientists, family therapists, historians, and American studies scholars (Pye 1973, 65; see also Berkhofer 1973 and Weinstein 2004).

In what follows, I examine Kluckhohn's relations with Benedict, Mead, and Gregory Bateson in two contexts: the school of culture and personality and the wartime Conference on Science, Philosophy, and Religion in Their Relation to the Democratic Way of Life. Although technically not a Boasian—having studied anthropology with Father Wilhelm Schmidt in Vienna, Robert Marett in Oxford, and Alfred Tozzer at Harvard—Clyde Kay Maben Kluckhohn (1905–1960) nonetheless strongly identified with "the Boasian tradition" (Handler 1995: 80–1). He worshipped Boas as his

"anthropological god" (Kluckhohn to Robert H. Lowie, letter dated October 20, 1945 [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.3]), and "derived a major part of [his] theoretical orientation from" Benedict (Kluckhohn 1949c, 18). Kluckhohn joined Benedict and Mead in communicating anthropologists' findings to the educated public, and agreed with Mead and Bateson on the fundamental importance of biology to anthropology (Kluckhohn, *Comments on Persons Nominated for Consideration at the Center for Advanced Study in the Behavioral Sciences as of February 23, 1954* [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.6]; Kluckhohn 1951a: 121–2). In addition, he developed a "close full friendship" with Alfred Kroeber, with whom he attempted to fix the meaning of the culture concept (Theodora Kroeber 1970, 201; Kroeber and Kluckhohn 1952). Kluckhohn's closest intellectual affinity among the Boasians, however, was with Edward Sapir, and it is this affinity that helps to explain Kluckhohn's eventual turn away from psychoanalytically informed culture and personality to the linguistically informed study of values (Stocking 1996, 8).

Like Benedict, Kluckhohn came to anthropology through affection for the American Southwest. In 1922–1923, while recuperating on a ranch near Ramah, New Mexico, from the rheumatic fever that interrupted his freshman year at Princeton, he became "fascinated" by a nearby band of Navajo. Kluckhohn's academic interests then lay in the classics: he had studied Latin in preparatory school. Learning to speak "passable" Navajo, he explored Navajo Country on horseback. After resuming his studies at the University of Wisconsin in 1924, Kluckhohn majored in Greek and, as a Rhodes Scholar, read classical archaeology in Corpus Christi College, Oxford. He then briefly studied at Harvard Law School, before traveling extensively in Europe, learning French and German, and studying anthropology in Vienna with Father Wilhelm Schmidt, a member of the *Kulturkreis* (culture circle) school of diffusionism. Returning to Oxford, Kluckhohn worked with Robert Marett, a specialist in comparative religion. From 1932 until 1934, Kluckhohn taught physical anthropology at the University of New Mexico and, as an associate of the School for American Research, directed archaeological excavations in Chaco Canyon. In 1934, he went to Harvard on a grant from the Rockefeller Foundation to study physical anthropology and archaeology. Shifting his interests to social and cultural anthropology, Kluckhohn cut such a brilliant figure that Harvard kept him on after he earned his degree in 1936. Appointed assistant professor in 1937, he became associate professor in 1940 and professor in 1946—the same year in which the Department of Social Relations was established (transcript of Ann Roe's interview with Clyde Kluckhohn, 1950 [American

Philosophical Society, Anne Roe papers, B/R261]; Kluckhohn, *Autobiographical Sketch*, ca. 1946 [Harvard University Archives, Clyde Kluckhohn papers, HUG 4490.7]).

## Culture and Personality

Although he had been analyzed in 1931 by Eduard Hitschmann—one of Freud's earliest disciples in Vienna—Kluckhohn did not overcome his "ambivalence" toward psychoanalysis until 1939 (Parsons 1973: 30–1). He first made his name as an outspoken critic of "the almost morbid avoidance of theory" in anthropology and archaeology. Until anthropologists and archaeologists made their "postulates" and "canons of procedure" explicit, and hence subject to "systematic criticism," their findings, Kluckhohn (1939, 1940) insisted, would not have any scientific standing.

Although his dissertation had been library-based, Kluckhohn soon earned his spurs as an ethnographer. In 1936, he began fieldwork among the Ramah Navajo that would continue until his death. Inspired by the social psychologist, John Dollard, whom he had known from his freshman year at Wisconsin, and by Edward Sapir, with whom he studied the Navajo language in New Haven in 1936–1937, Kluckhohn decided to follow a representative sample of Navajo children "through time" as they "acquired" their culture in "a needed experiment" to correct "the flat, one-dimensional quality" of most ethnographies at that time (Kluckhohn 1949b, v). In doing so, he became one of the pioneers of "long-term field research" in American anthropology (Foster et al., 1979, 7).

While kinship and social organization bored Kluckhohn, he paid close attention to the details of Navajo religion, ceremonialism, and beliefs. Concerned with individual variation, he documented the frequency of the behaviors he observed, sought to determine the extent of individual participation in ceremonies, and indicated whether his sources were informants' statements or his own observations. From the outset, Kluckhohn was strongly oriented toward the life-history method, which John Dollard defined as "a deliberate attempt to define the growth of a person in a cultural milieu and to make theoretical sense of it" (Dollard [1935] 1949, iii). Quick, however, to acknowledge the usefulness of statistical analysis, Kluckhohn put his generalizations, whenever possible, on a quantitative basis (Lamphere and Vogt 1973). His concern with documenting variation, combined with his life-history orientation, may explain Benedict's disparaging allusion to "Kluckhohn's counting noses" (Benedict to Margaret Mead, letter dated January 30, 1939, cited in Young 2005, 74).

Believing that "multiple observations" and "approaches" would eliminate any "distortions" stemming from personal bias or from the "stereotyped fashions" prevalent in the fieldworker's discipline, Kluckhohn engaged in a number of cross-disciplinary collaborations (Kluckhohn 1949b, vi). With the physiologist Leland Wyman, he compiled a taxonomy of Navajo rituals; with the psychiatrist Dorothea Leighton, he produced two books on the Navajo for the Indian Education Research Project; and with the biological anthropologist James N. Spuhler, he studied Navajo genetics (Kluckhohn and Wyman 1940; Kluckhohn and Leighton 1946; Leighton and Kluckhohn 1947; Spuhler and Kluckhohn 1953). It is small wonder, then, that the 1940s and 1950s were known as "the Kluckhohn era" in Navajo studies (Witherspoon 1975, ix).

As an undergraduate at Wisconsin, Kluckhohn had taken his first course in psychology from Norman A. Cameron—at that time a "brass instrument" behaviorist who had little use for Freud. Kluckhohn, however, "stopped ranting about Freud's anthropological errors" when he discovered the "unconscious" during his analysis in Vienna (transcript of Anne Roe's interview with Clyde Kluckhohn, 1950 [American Philosophical Society, Anne Roe Papers, B/R261]). In 1939, Kluckhohn accepted a fellowship from the Carnegie Corporation. This allowed him to study psychology and psychiatry with Ralph Linton at Columbia, to present material on the Navajo in the seminar on culture and personality run jointly by Linton and the psychiatrist Abram Kardiner, and to participate in Sandor Rado's seminar on psychoanalytic theory at the New York Psychoanalytic Institute (Charles Dollard to Kluckhohn, letter dated March 3, 1939; Ralph Linton to Kluckhohn, letters dated 9 and February 13, 1939, and March 26, 1939 [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.3]; see also Kluckhohn 1944a 1989, 237*n*). While in New York, Kluckhohn became one of Mead's numerous protégés (Lagemann [1989] 1992, 166; Sahlins 1984, 1).

In 1941, Kluckhohn collaborated with O. Hobart Mowrer and Henry A. Murray in offering a cooperative seminar on "socialization" modeled on the Linton–Kardiner seminar. Mowrer, an experimental psychologist, had come to Harvard in 1940, after six years at the Yale Institute of Human Relations, where he worked with John Dollard and others on integrating learning theory and psychoanalysis. In 1944, Kluckhohn and Mowrer outlined a "conceptual scheme" for culture and personality that fused "concepts" and "postulates" drawn from anthropology, learning theory, and psychoanalytic theory (Kluckhohn and Mowrer 1944; Mowrer and Kluckhohn 1944). Kluckhohn, like Mead, had more use for Kurt Lewin's field theory than did Mowrer (Kluckhohn to Norman A. Cameron, letter dated October 24, 1944

[Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.3]; Margaret Mead to Kluckhohn, letter dated December 10, 1943 [Library of Congress, Margaret Mead Papers, Box C10]; Kluckhohn to Mead, letter dated December 28, 1943 [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.3]).

Kluckhohn found the clinician Henry Murray, Director of the Harvard Psychological Clinic and developer of the Thematic Apperception Test, more congenial than Mowrer. Murray's press–need formulation (in which "press" designated the "temporal gestalt of stimuli" ascertainable by a fieldworker, and "need" designated the informant's motivation) provided Benedict with the "psychological ground-work" to go "beyond relativity," to search for the "fundamental social and cultural arrangements" that "minimize[d] hostility and conflict (aggression)" (Benedict to Murray, letter dated July 30, 1944, cited in Caffrey 1989, 305; Benedict, reply to *Questionnaire from the Conference on Science, Philosophy, and Religion, 1943*, cited in Young 2005, 100). To Kluckhohn, Murray was "the great humanist" whom he himself aspired to be. Kluckhohn thus followed Murray in "defining humanistic social science as the systematic study of 'the whole man'" (Kluckhohn to Murray, letter dated July 18, 1944, cited in Robinson 1992: 294–5). In 1948, Kluckhohn and Murray published *Personality in Nature, Society, and Culture*, the first collection of readings from the periodical literature in culture and personality, designed in part to teach "social science to psychiatrists" (Kluckhohn to Roger Shugg, letter dated May 8, 1948 [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.7]). As Kluckhohn and Murray explained in their Introduction, "all research in [the] field [of culture and personality] is in the last analysis directly or indirectly oriented to one central type of question: What makes an Englishman an Englishman? an American an American? a Russian a Russian?" (Kluckhohn and Murray 1948, xiv).

When the American Psychiatric Association invited Kluckhohn in 1944 to assess psychiatry's impact on anthropology, he credited Benedict, Mead, and Sapir with having promoted a "rapprochement" between the two disciplines. Kluckhohn perceived in Benedict's work an "attitude" that could "only be described as 'psychiatric.'" He attributed Mead's standing as "possibly the best-known anthropologist in psychiatric circles" to her "field data," tests of psychiatric problems in the field, and "idiom," which psychiatrists "found intelligible." However, it was Sapir, according to Kluckhohn, who had done the most to make "possible some real fusion between the two disciplines." The "tough insights" Sapir drew from psychiatry had "forced" anthropologists to reconstruct their "postulates." Thanks to Sapir's "conceptual refinements," anthropologists were no longer able to regard

individuals as the "more or less passive carrier[s] of tradition," or culture as "a superorganic, impersonal whole" (Kluckhohn 1944b: 597, 600–603).

In 1945, Kluckhohn evaluated the use of personal documents in anthropology for the Social Science Research Council's Committee on Appraisal of Research. He drew on his collaboration with Dorothea Leighton to discuss the "interpersonal" aspects of fieldwork. Urging anthropologists "to take more account of the 'human' side of their materials," Kluckhohn suggested that they act as "a blank screen" on which informants could project their own lives. Kluckhohn was convinced that until anthropologists learned how to "deal rigorously with the 'subjective factors' in the lives of 'primitives,'" their work would remain "flat and insubstantial" (Kluckhohn 1945e: 86, 122, 162–3; Bashkow 1991: 189–90).

Kluckhohn was also convinced of the existence of "certain affinities" between the anthropologist and the psychiatrist. Both were interested in "total personality" and "the whole man." Both were practitioners of disciplines that were "innocent of statistics," "observational" as opposed to "experimental," and "holistic." Finally, fieldwork was, to Kluckhohn's mind, as "fundamentally revealing" of the relationship between the anthropologist and informant as analysis was of the relationship between the psychiatrist and analysand. Thanks to the influence of psychiatry, Kluckhohn (1948: 440–1, 1956a, 906) thought, anthropologists were gaining "a better understanding of and control over their principal instruments—themselves."

What Kluckhohn most wanted from psychoanalysis was "a theory of raw human nature." Like Benedict and Mead, he had earlier considered Freudian theory "strongly culture-bound," and had found the work of "culturalists," such as Erich Fromm and Karen Horney, more congenial than that of the orthodox Freudian, Géza Róheim. By the late 1940s, however, Kluckhohn's own views were converging with Róheim's. The culturalists, Kluckhohn had come to believe, went too far in discounting the influence of biology and in paying more attention to cultural differences than to cultural "universals." Besides, Kluckhohn's fieldwork among the Navajo had convinced him of the "astonishing correctness" with which Freud had depicted a number of universal "themes in motivational life." While the "expression" and "manifest content" of these themes varied from culture to culture, "the underlying psychologic drama," Kluckhohn believed, "transcend[ed] cultural difference." It was now time for anthropologists to turn their attention from the differences among cultures to the similarities (Kluckhohn and Morgan [1951] 1962: 350–1; Wolf [1964] 1974, 36, 39).

Like Benedict and Mead, Kluckhohn had studied "Culture at a Distance" during the Second World War. While working alongside Benedict in the Foreign Morale Analysis Division of the Office of War Information during

1944–1945, Kluckhohn came to appreciate her uncanny ability to "saturate" herself in library materials and to grasp "the essential dynamics of Japanese personality and culture" without having engaged in fieldwork (see also Tannenbaum 2009 and Schachter 2009). Then, while serving as a consultant to the American occupation forces in Japan in 1946–1947, Kluckhohn was "astonished to discover" how well "he knew what was coming in unformalized situations," thanks to his conversations with Benedict and his reading of her book, *The Chrysanthemum and the Sword*, published in 1946 (Kluckhohn 1949c: 18–9; Benedict 1946).

Like Benedict and Mead in the Columbia University Research in Contemporary Cultures, Kluckhohn attempted, after 1945, to refine methods that had been hurriedly improvised during the war. As Director of the Harvard Russian Research Center from its inception in 1948 until 1954, Kluckhohn sponsored research intended to be at once interdisciplinary, experimental, coherent, and "cumulative," and to incorporate the methods and insights of the behavioral sciences. Together with Alex Inkeles and Raymond Bauer, he analyzed more than 400 life-history interviews with "displaced" Soviet citizens and some 2,000 questionnaires in the Harvard Project on the Soviet Social System. However, when, Kluckhohn stepped down from the directorship of the Russian Research Center in 1954, the methods and insights of history, economics, and political science had largely eclipsed those of the behavioral sciences (Kluckhohn 1949d; Bauer, Inkeles, and Kluckhohn [1956] 1960; Inkeles 1973; Lagemann [1989] 1992: 174–75; Mead and Métraux 1953).

By 1954, Kluckhohn could point to the "considerable improvement in communication" that had occurred "between psychoanalysts and anthropologists" since the late 1920s. Yet, as he admitted, work in culture and personality "suffered" from being too "fashionable," with too many publications in the field "hasty, overly schematic, and indeed naive." Still, the "underlying notions" of culture and personality seemed to Kluckhohn "basically sound." By then, however, Kluckhohn's "central interests" lay elsewhere (Kluckhohn 1954a, 961; 1954b, 693). Although he continued to review work in the field for professional journals, and for the *New York Times* and the *New York Herald Tribune*, Kluckhohn turned his attention to the linguistically informed study of values.

## The Conference on Science, Philosophy, and Religion

Kluckhohn's move toward Freudian orthodoxy, interest in the scientific study of values, and turn to linguistics become more understandable when placed against the backdrop of his participation—along with Mead,

Benedict, and Bateson—in the wartime symposia of the Conference on Science, Philosophy, and Religion in Their Relation to the Democratic Way of Life. Founded in 1940 by Louis Finkelstein, provost of the Jewish Theological Seminary of America, the Conference mobilized American intellectuals in a democratic crusade against fascism by sponsoring annual interdisciplinary and ecumenical symposia. The Conference was a product of what Philip Gleason (1992) has called the "democratic revival" of the late 1930s and early 1940s, an "ideological reawakening" in which American intellectuals responded to the rise of totalitarianism abroad by affirming the American way of life as a normative democratic culture. For the more religiously inclined participants like Finkelstein, the point of the annual symposia was to ground democratic ethics in moral absolutes. For Mead and Benedict, the point was to develop "a wartime theory of democratic culture." For Kluckhohn, the annual symposia provided a stage on which he could rehearse themes that, after 1945, he presented to the educated public in publications like *Mirror for Man* (Gleason 1992, 165; Yans-McLaughlin 1986, 208).

Kluckhohn first participated in the Conference on Science, Philosophy, and Religion at the second annual symposium in 1941 when he commented on Mead's paper, "The Comparative Study of Culture and the Purposive Cultivation of Democratic Values." Kluckhohn did not dissent from Mead's disavowal of any "finished blue print of the future of the absolutely desirable way of life." He also endorsed Mead's recommendation that social scientists devote themselves "to a direction, not a fixed goal," and "to a process, not a static system" (Mead 1942: 67–8; Yans-McLaughlin 1986, 209).

Where Kluckhohn did differ from Mead was in urging social scientists to search for "ultimate" values based on the scientific study of mankind's "universal needs" and of the "varied ways" devised to meet those needs. Were there, he asked, "certain cultural features which remain constant in those cultures which give high value to the individual?" If these features could be discovered, they could be "incorporated" into American culture to enhance the democratic way of life. Kluckhohn (1942, 76) was thus "slightly more optimistic" than Mead that social scientists could chart "aims" as well as "general direction."

Replying to a questionnaire from the Conference in 1942, Kluckhohn identified the principal "evil of our world" as the lack of a secular "faith" that could "give clear meaning and purpose to living," yet be compatible "with what we have learned of our world by 'scientific methods.'" Anthropologists were agreed on the necessity of religion conceived as a symbolically enacted "system of common purposes," but no such system,

Kluckhohn insisted, should be based on "supernatural sanctions" (Kluckhohn, reply to *Questionnaire from the Conference on Science, Philosophy, and Religion*, dated December 7, 1942 [Library of Congress, Margaret Mead Papers, box E59]). As a lapsed Protestant who had once "flirted" with becoming an Episcopalian priest and who had even considered converting to Roman Catholicism, Kluckhohn now stood resolutely opposed to revealed religion. He had come to believe that "scientific humanism" was "the only hope for American culture" (Kluckhohn 1941a: 174–5; transcript of Anne Roe's interview with Clyde Kluckhohn, 1950 [American Philosophical Society, Anne Roe Papers, B/R261]). Kluckhohn was one of a number of anthropologists who played prominent roles in the Kulturkampf waged in the first half of the twentieth century by what David Hollinger (1996) has called the "American liberal intelligentsia" to "de-Christianize" America's public culture. By the early 1940s, this Kulturkampf had taken on an anti-Catholic animus, owing to the Concordats the Vatican had reached with fascist regimes in Italy and Germany, and to Catholic support for Franco's insurgency in Spain (McGreevy 2003: 166–88).

During the winter of 1942–1943, Kluckhohn circulated among his friends a "manifesto" entitled, *A Declaration of Interdependence: A Creed for Americans as World Citizens*. In this manifesto describing his vision of a world made "safe for differences," Kluckhohn dismissed "the American Century" proclaimed by Henry Luce in *Life* magazine in 1941 (Luce 1941) as nothing more than a prescription for "imperialistic American domination of the world." Kluckhohn's new world order, in contrast, would guarantee the world's peoples the right to "live according to their own values and traditions." Kluckhohn then threw down the gauntlet: Americans must choose. They could either "waste" the "potentialities" of millions of men and women by beating "a frightened retreat to some single standard," or they could reorient American culture around the principle of "orchestrated heterogeneity" (Kluckhohn, A Declaration of Interdependence: A Creed for Americans as World Citizens. Version 1b, dated January 17, 1943 [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4990.3]). Among the sources from which Kluckhohn borrowed some of his ideas was Mead's 1939 Introduction to *From the South Seas*. "We are at a crossroads," Mead (1939: xxx–xxxi) wrote, "and must decide whether to go forward towards a more ordered heterogeneity, or make a frightened retreat to some single standard which will waste nine-tenths of the potentialities of the human race."

In "Anthropological Research and World Peace," a paper given at the Conference's fourth annual symposium in 1943, Kluckhohn characterized anthropologists as "tough-minded" social scientists who insisted on the

"stupidity" of "unlinear attack[s]" on the problems of the contemporary world, yet criticized attempts to view those problems "too exclusively in the light of reason." The distinctive contribution anthropologists could make to world peace was, in conjunction with sociologists, psychologists, and psychiatrists, to emphasize such nonrational elements in human life as "sentiments" and "unconscious assumptions." Indeed, for Kluckhohn, "the central problem of world peace" was to "minimize and control aggressive impulses" (Kluckhohn 1944c: 143–5, 149).

In "Group Tensions: Analysis of a Case History," a paper given at the Conference's fifth annual symposium in 1944, Kluckhohn drew on *Navajo Witchcraft*, his newly published inquiry into the sources of aggression among the Navajo, to locate "the conditions for universal sociopsychological processes" in "the uniformities of human neurological equipment" and "the universality of the great dramas of human life (birth, renewed dependency, death)" (Kluckhohn [1944a] 1989, 1945a).

During this symposium, Kluckhohn dissented from the call issued by his Harvard colleague, the sociologist Pitirim Sorokin, for an international order based on "universal adherence" to "values long since discovered by great religious leaders and thinkers." Kluckhohn took issue with the implication that the "integration" of mankind would be achieved only by subordinating "*all* men to values which are *all* the same" (Kluckhohn 1945b: 216–7, italics in original). In addition, he denounced the claim advanced by "official Christianity" that it was "the *only* perfect faith to which all humanity must be converted" as "one of the most dangerous threats to world order" (Kluckhohn 1945c: 297–8, italics in original).

Finally, Kluckhohn agreed with the Harvard political scientist Carl Friedrich that the culture concept could not "form the firm cornerstone of a unified social science." There was simply too much disagreement among "specialists" over the concept's "philosophical and methodological implications." Here Kluckhohn revealed perhaps his primary reason for collaborating with Alfred Kroeber on their 1952 compendium of culture: to fix, as best they could, the concept's meaning. Kluckhohn also objected to "cultural determinism," which, to his mind, was just "as false as every other unilateral 'ism.'" While agreeing with Friedrich that "the pooling of 'psychological' and 'anthropological' knowledge" in culture and personality had "only barely opened up," Kluckhohn extolled the promise of analyzing "culture structure." If anthropologists could "dissect out" patterns in explicit (or overt) culture, they could arrive at the "integrating principles" of the "implicit culture" without having to rely on vague "intuition" (Kluckhohn 1945d: 628–9; Kroeber and Kluckhohn 1952).

Here Kluckhohn touched on what he considered his most important contribution to anthropological theory—the concept of implicit culture. Implicit culture designated the integrating principles of a culture. Because informants were largely unconscious of them, these integrating principles had to be "inferred" by the anthropologist. Kluckhohn borrowed the concept of implicit culture from Ralph Linton's unpublished lectures and then refined it by drawing on Benedict's notion of "unconscious canons of choice" (Kluckhohn 1964, 145; Herskovits 1961, 130). Indeed, Kluckhohn thought that when Benedict spoke of "patterns" in her celebrated book, *Patterns of Culture* (1934), she meant the "configurations" or unconscious patterns of implicit culture, rather than the behavioral patterns of explicit culture. For Kluckhohn, as for Benedict, patterning suggested the "regularity," as opposed to the "randomness," of culture (Kluckhohn 1941b: 117, 126–8; Benedict 1934). From Edward Sapir, Kluckhohn learned how anthropologists could "infer" patterns. Like Sapir, he believed in the existence of "linguistic universals" (Sapir 1927).

## The Comparative Study of Values in Five Cultures

Kluckhohn designed the Comparative Study of Values in Five Cultures Project to discover the integrating principles of implicit culture. Supported by $200,000 in grants from the Rockefeller Foundation, some forty researchers from Harvard and nine other institutions, as well as practitioners of seven different disciplines, conducted a series of researches between 1949 and 1954 among the Navajo, Zuñi, Mexican Americans, Mormons, and Texans in the Ramah area.

The project was one of the great undertakings sponsored by the Harvard Department of Social Relations, which had been founded in 1946 by Kluckhohn, Parsons, Mowrer, Murray, and the social psychologist, Gordon Allport. Before the Second World War, all had been members of the "Levellers," an interdisciplinary group interested in promoting "basic social science" at Harvard (Parsons 1949, 1973: 32–3) In 1943, Kluckhohn sent Mead and Bateson a copy of a proposed curriculum for "a unified teaching of the social sciences" at Harvard. Bateson recommended that students be exposed to scientists' "ways of thinking," but that they be trained in "qualitative" rather than "quantitative" techniques (Bateson to Kluckhohn, letter dated January 18, 1944 [Library of Congress, Margaret Mead Papers, Box O3]). In 1944, Kluckhohn and Parsons attempted to bring Mead and Bateson to Harvard, but James B. Conant, then President of Harvard, balked. Conant, as Kluckhohn phrased it, "didn't want to commit himself

to women professors on the Harvard faculty" (Kluckhohn to Mead, letter dated October 31, 1944 [Library of Congress, Margaret Mead Papers, box C16]).

The Five Cultures Project sought to explain why the values of these adjacent cultures differed, even though each culture had adapted to the same environment, had been exposed to the same streams of diffusion from "generalized" American culture, and—with the exception of the Texans who migrated to the region in the 1930s—had interacted with each other for two generations (Kluckhohn [1951c] 1962, 395). The project's ultimate objective, however, was to develop a "unified theory" and a common "set of methods" for the scientific study of values. Although more than sixty books and articles eventually issued from the project, it produced neither a unified theory nor a common set of methods. Instead, the whole effort was soon forgotten after Kluckhohn's death (Dumont 1980: 212–3; Powers 1997; Vogt and Albert [1966] 1970: 1–5]).

For help in comparing cultures and identifying cultural universals, Kluckhohn turned to linguistics, the social science discipline that, in his eyes, most resembled the natural sciences "in rigor and elegance." As Franz Boas and Edward Sapir had contended, language approached "pure culture" in illustrating "regular and patterned selection among a limited number of biological possibilities." Language was also that aspect of culture in which "order and predictability" had been "most successfully demonstrated." Inspired by Claude Lévi-Strauss, "the most brilliant and innovating anthropologist alive today," and by the linguist Roman Jakobson's analysis of the "distinctive features" of languages, Kluckhohn searched for cultural equivalents of the phoneme—basic units of culture comparable across cultures (Kluckhohn 1955, 347; Kluckhohn to Kenneth Setton, letter dated October 27, 1959 [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.6]; Fischer 1973). Kluckhohn's untimely death of a heart attack in July 1960 cut short this attempt to develop a cultural "grammar" that would allow anthropologists to describe and compare cultures as "parsimoniously" as linguists did languages. His turn to linguistics illustrates the way in which the discipline provided models not only for the school of culture and personality, as David Aberle has noted, but also for the scientific study of values. Kluckhohn derived from linguistics the characteristics of selectivity, patterning, and the largely unconscious nature of implicit culture that he emphasized in his later work (Kluckhohn 1951b, 1956b; Aberle 1960).

The Second World War spurred Kluckhohn's embrace of an international order founded upon cultural diversity and, as its concomitant, American culture reorganized around orchestrated heterogeneity. The

Cold War, which pitted the United States in ideological warfare with the Soviet Union, heightened the urgency that Kluckhohn felt for formulating "a good five-cent ideology" that Americans could both articulate to themselves and communicate to foreigners. Such "a positive, clearly defined national faith" seemed essential, not only to offset Communism's appeal as a secular religion but also to supersede the "competitive individualism" and outmoded "Horatio Alger economic and achievement values" that Kluckhohn ([1950] 1962: 328–31) deplored. By 1957, Kluckhohn thought that he had detected the emergence of "a 'new' set" of American values, the "most pervasive" of which was "the weakening of the Puritan ethic with its demands for exhibitionistic achievement, unbridled 'individualism,' and competition" (Kluckhohn 1958, 204). "Heterogeneity," he believed, was fast becoming "one of the organizing principles of American culture" (Kluckhohn 1958: 196–7; Morison 1958, 407).

## Conclusion

At the time of his death, Kluckhohn had just begun a well-earned respite from teaching, thanks to a multiyear grant he received from the Ford Foundation in 1957. Kluckhohn looked forward to synthesizing his many years of fieldwork among the Navajo, shaping the summary volumes of the Comparative Study of Values in Five Cultures Project, and preparing a new edition of *Navaho Witchcraft*. He also hoped to make some progress on two books—one on "anthropological theory," the other on "anthropological studies of modern civilizations" (Kluckhohn to Bernard Berelson, letter dated June 1, 1957 [Harvard University Archives, Clyde Kluckhohn Papers, HUG 4490.6]; Parsons 1973, 36). By then, Kluckhohn had earned a well-deserved reputation as fieldworker, theorist, promoter of interdisciplinary and multidisciplinary projects, and prophet of the New Anthropology.

Kluckhohn's wide-ranging interests well equipped him to represent anthropology, not only to professional colleagues in other disciplines but also to educated Americans. As a public intellectual, Kluckhohn exemplified a long and rich tradition in American anthropology stretching from his anthropological god, Franz Boas, to his student, Clifford Geertz (Stocking [1979] 1992: 92–113; Ortner 1997). Influenced more by Benedict the theorist, than by Mead the popularizer, Kluckhohn endeavored to make Americans culture-conscious. Like Benedict, he wanted to go "beyond relativity." For Benedict, going beyond relativity meant finding the social and cultural arrangements that fostered social cohesion; for Kluckhohn, it meant searching for the organizing principles of cultures. Hence his turn, after 1945, to linguistics. As a practitioner of culture and personality,

Kluckhohn was interested in discovering the personality characteristics that distinguished Americans from other peoples; as a student of values, in discovering the organizing principles that distinguished American culture from other cultures. Although Kluckhohn died before he could produce the authoritative account of the Navajo that would have constituted his legacy, he should be recalled, nonetheless, as an eloquent spokesman for anthropology's unique position in the American academy as "the most scientific of the humanities, the most humanist of the sciences" (Wolf [1964] 1974, 88).

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# DREAMS OF FORTUNE: REO FORTUNE'S PSYCHOLOGICAL THEORY OF CULTURAL AMBIVALENCE

Roger Ivar Lohmann
*Trent University*

Reo Fortune's first book, *The Mind in Sleep*, published in 1927, considers dreams in which one's attitudes contradict waking opinions. Fortune was keenly aware of his own conflicting perspectives and abhorred ethnocentrism. He argued that rejected beliefs remain subconsciously. Individuals hold contradictory beliefs using two capacities: logical and emotional representation and connection. The former is more accessible when awake, the latter in dreams. Fortune's subsequent ethnographic studies attended to dreams and ambivalence. His theory encouraged his rejection of stereotyping by the "culture and personality" school of thought, and can be used as a model of cultural ambivalence with ongoing anthropological value.

## Curious Dreams and Fortune's Theory of Dual Culture in Mind

Reo Fortune was among the most accomplished fieldworkers and prolific ethnographers of the twentieth century. However, it could be argued that he is most remembered through his association with two anthropologists. One was Margaret Mead, his one-time wife, with whom Fortune shared intense field experiences and conflicts at home and in print (Fortune 1939, 26; Mead [1938, 1940] 2002; Roscoe 2003). The other was Ruth Benedict ([1934] 1953), whose use of Fortune's materials made the "paranoid" Dobuans far more widely known than Fortune's own writings. Biographical and historical work on Mead and Benedict has reproduced an image of Fortune as macho and defensive (Banner 2003: 255–6, 318; Thomas 2009). Without questioning the legitimacy of these impressions by some of his

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associates at certain points in his life, here I look to what we can learn about Reo Fortune's personality and scholarly motives before his fateful association with Mead and Benedict. The fact that Benedict, and later Gregory Bateson, were competitors for Mead's love, added emotional complications to Fortune's scholarly relationships with each of these figures in distinctive ways (see Molloy 2009 and Thomas 2009). Here I am interested in who Reo Fortune was before life threw him the curve ball of meeting Mead, which helped motivate him to leave psychology in favor of anthropology (for other factors, see Gray 1999; Thomas 2009).

My main source is a little-known book on dreaming that Fortune published at the age of 24, titled *The Mind in Sleep* (1927). It was a work in progress when he met Margaret Mead on a ship from Australia to Europe in 1926 (Mead 1972: 169–80). While he anonymously included some of Mead's dreams in it (Banner 2003, 264; Lapsley 1999: 149–50), he also included six of his own that reveal much about his views before his association with Mead. We have here a glimpse of the young man's trials, ideals, and scholarly persona.

The man who emerges from this book is an emotionally sensitive, brilliantly thoughtful character who devoted considerable effort toward tackling difficult moral issues, particularly those involving conflicting views and perspectives. We learn that Fortune was a liberal, an agnostic, a biological materialist, an antinationalist, and a pacifist. He was also keenly aware of the perspectives of others and how differing views influenced relationships. A widely read scholar with a strong foundation in psychology, he fearlessly and respectfully challenged the orthodoxies of intellectual giants like Sigmund Freud ([1900] 1965) and W. H. R. Rivers (1923), the Cambridge psychologist and ethnologist. Fortune focused on dreams in which one acts or thinks in ways contrary to one's waking views to theorize about dreams without one's own theory influencing dream content. This perspective provides an opportunity for us to see some of Fortune's struggles with competing drives surrounding the First World War (also called the Great War), his abandonment of Christianity, and his love of scholarly order.

Fortune's theory of self-contrary dreams pointed his ethnographic work to problems of dreaming and ambivalence and encouraged his rejection of stereotyping. Though of little influence when published, Fortune's book prefigures some developments in the psychology and anthropology of dreaming. Finally, *The Mind in Sleep* provides a model of cultural ambivalence and dynamism of continuing usefulness by showing how individuals can hold contradictory cultural views simultaneously.

## Fortune's Theory in *The Mind in Sleep*

According to Fortune, some of each person's consciously held beliefs are shadowed with conflicting beliefs on the subconscious level, accessible in dreams. Going through life, new attitudes are learned and developed, but older convictions persist below the surface. For example, the religious convert may, deep down, remain attached to a former belief system. Intrigued with the idea that people apparently hold conflicting views simultaneously, Fortune investigated dreams in which the ego exhibits attitudes that have been rejected or repressed in waking life.

Fortune disagreed with Rivers' argument that Freud's dream theory was self-fulfilling. Dreams in which a person holds opposite views to those of waking life, Fortune (1927, ix) argued, showed that "it is easily possible to exaggerate the influence of waking theory on dreams of the type under review." In anthropological terms, *The Mind in Sleep* is concerned with conflicting elements of culture in each person's mind and argues that dreaming allows for the expression of these incongruous views without harming social cohesion. In dreams, ideas that have been rejected in the conscious, social, cosmological, and moral systems remain and find expression.

This raises questions about how and why rejected beliefs persist, and why they may be accepted again, at least temporarily, in dreams. Fortune answered that these ideas retain emotional force, even when they have been rejected by logical or pragmatic concerns; they resurface in dreams because of the emotional tenor of dreaming.

Dreaming and waking are characterized by different kinds of thinking. Waking cognition connects ideas in terms of spatiotemporal relationships and reason, but "in dreams," Fortune (1927, 34) wrote, "association by common affect frequently supersedes association by contiguity and logical similarity." In "affective association," dream images and scenarios are connected or juxtaposed because they evoke a common feeling. Fortune considered affective association to be *the* distinctive characteristic of dream thought. It dominates dreaming when it is least disturbed by waking attitudes. As one nears waking, conscious thought patterns invade. When the conscious attitudes disagree with repressed ones, the intrusion of waking thought into dreams engages various mechanisms of censorship. These disguises complicate the affective associative symbolism of dreaming. Fortune thus disagreed with Rivers' view that symbolic censorship does not occur in dreaming. Fortune (1927, 18, 20) considered his own theory closer to Freud's in this regard, though he rejected the idea that deeply repressed attitudes are necessarily or usually sexual.

While Fortune's theory is detailed and provocative, with implications for both psychology and anthropology, it did not make much of an impression on either field. Mead (1972, 177) attributed this to Fortune's using part of his fellowship funding to subsidize its publication as a trade book, such that it "never got any scientific hearing at all." Social anthropologist Jackson Steward Lincoln (1935, 170) made no reference to it in his ethnological monograph on dreaming, though he did mention Fortune's Omaha ethnography ([1932b] 1969). Fortune himself, unfortunately, did not develop the ideas laid out in his book in his later writing, nor did he make explicit use of his theory in his own ethnographic work.

In *The Mind in Sleep*, Fortune (1927, 4) analyzed several dreams in which repressed tendencies find expression, including when "an agnostic dreams with belief in Christianity, [and] a pacifist with hatred of former enemy nations."

Since remembered dreams are usually those that occur shortly before waking, when censorship has come into play, Fortune argued that in order to reach awareness they must escape censorship via three "methods of evasion" (1927, 21). Fortune called the first of these "surrogation," in which a relatively repressed experience or attitude (the "submergent") finds "subsequent expression by merging and confounding it with an object of less repressed" but otherwise similar feeling ("the surrogate") (Fortune 1927, 20). In other words, it is "the process of confounding submergent and surrogate by association through common affect" (Fortune 1927, 21). For example, in one dream Fortune recounted that his repressed hatred of Germany during WWI was expressed by links between studying in that country as a gesture of peace, and being in a hated former school.

Fortune's second method of evasion is "envelopment," when a dream experience of an unrepressed attitude evokes the appearance of an affectively similar, repressed attitude. Fortune illustrated envelopment with one of his dreams in which he found himself in a library advocating agnosticism. This represented his waking views. However, his submergent Christianity appeared undisguised when a disturbance broke out, and he gave a speech on the virtues of Christianity as a way of quelling the disorder. He associated Christianity with an ordered and just universe, for which he longed in spite of contrary evidence. The repressed attitude, like a wolf in sheep's clothing, moves freely among the flock of conscious propriety in the dream scenario, smuggled past the censor's shepherd in its disguise of affective association with the uncensored attitude. Thus in envelopment, "the submergent obtains unsymbolic, undistorted release when the surrogate is confounded with [one's waking views]" (Fortune 1927, 46).

Third, in "substitution," two layers of symbolism hide the repressed attitude. In one of Fortune's examples, he dreamed of flowers (the censored version or manifest content), which, through punning association and common colors, he interpreted upon waking to represent the national flag (the surrogate), which in turn, represented extreme patriotism (the submergent). In substitution, the highly repressed submergent and the less repressed surrogate both remain out of the dream's manifest content. Dream characters or things are substituted for repressed others based on punning or other similarities, and the disguise is so thorough that these dreams do not evoke much emotion. Substitution occurs when the attitude dreamt is highly repressed and censorship maintains control (Fortune 1927: 64–65). When this happens there is "displacement": "the image of the manifest content that has the greatest importance in connection with the latent content appears relatively inconspicuously" (Fortune 1927, 57). "Displacement appears to occur when repression is so heavy as to keep both submergent and surrogate in latent content" (Fortune 1927, 66).

With his interest in the censorship of unacceptable attitudes, and interpreting dreams to find definite meanings, Fortune may initially appear to be a Freudian. However, Fortune countered Freud's view that the censor, all-powerful in waking life, is weakened when we are asleep. Rather, he held that the censor continues to operate in sleep, but repressed material can slip past it into awareness because dreaming represents "a new mode of associative thinking, whereby objects are connected, not by the logical relations of contiguity or cognitive similarity that obtain in waking thought, but rather by similarity of affect" (1927, 15).

While it now seems unwarranted to claim that dreams are unique in this way, the linking of ideas through affective similarity is certainly prevalent in dreaming. Fortune and Rivers both accepted Freud's distinction between manifest and latent content; but both rejected Freud's wish fulfillment theory of dreaming on the grounds that nightmares do not depict events that we want to occur (Fortune 1927: 16–17). According to Fortune, however, Rivers did "not accept the theory of the symbolic evasion of the censorship in dreams. Rivers believe[d] that the confused imagery of dreams may be explained as regression to a lower more infantile level of thinking" (Fortune 1927, 19). By contrast, Fortune argued that waking thought organizes emotions around objects, while dreaming thought organizes objects around emotions. Censorship is "the return of waking consciousness upon a type of thought which actively resists such a return" (Fortune 1927, 31). Since Fortune considered affective connection the basis of dream scenarios, it followed that when censorship was strong, there was little manifest affect; when censorship was weak, affect was high (1927, 72).

The units of waking cognition are "sentiments." Fortune (1927, 88) defined a *sentiment* as *"an organized system of emotional tendencies grouped about an object and the idea of an object."* The units of dreaming cognition, by contrast, are "constellations." A "*constellation is a collection of ideas perceived in hallucinatory fashion as objects, disrupted from the sentiments of which they form the core, and regrouped about an emotional tendency which their respective sentiments have in common*" (1927: 88–89, italics in original). Fortune implied that constellations, which are unconscious in waking life, often contradict sentiments, are more resilient than sentiments, and preserve ideas that have been rejected or repressed.

For Fortune, constellations associate representations in "unconscious thought" (unavailable in waking life but accessed in dreaming) through "association by common feeling tone." Common affect is the primary means by which the mind in sleep connects representations to form constellations. When conscious ideology begins to invade the dream as one approaches waking, affective constellations are replaced with "surrogate constellations":

> *The surrogate constellation is formed by two ideas, perceived in hallucinatory fashion as objects, disrupted from the sentiments of which they form the core, and regrouped about an emotional tendency which their respective sentiments have in common, but which is more strongly repressed in one sentiment than in the other.* (Fortune 1927, 89, italics in original)

Surrogate constellations connect representations through "verbal similarity" (puns), and "simulacral association" (assigning attributes of one event to another) (Fortune 1927: 101–4). Thus, Fortune concerned himself with both pure dream cognition, characterized by affective association, and mixed dreaming/waking cognition, characterized by simulacral association. Because of their distinctive modes of connecting ideas and feelings, they allow the expression of attitudes repressed by the conscious mind as it confronts physical reality and its sociocultural and logical constraints.

Fortune's book concludes with the suggestion that dreaming evolved to balance individual and social needs:

> The organization of emotions around an object is the normal mode of working of the waking mind. The organization of objects about an emotion, on the other hand, is a normal mode of working of the dreaming mind. (Fortune 1927, 88)

These two modes evolved "to keep incompatible suggestion separated" (Fortune 1927, 88). By this Fortune meant preventing conflicts between

one's personal views and dominant views in one's culture. Fortune called greater openness to enculturation "suggestibility." Humans need to have strong suggestibility in order to achieve good social agreement, but this threatens individuality. Dreaming evolved to allow individual contrariness to express itself in emotionally satisfying ways without endangering sociality (Fortune 1927: 85–86).

Fortune evidently considered a degree of individual resistance to suggestibility to have conferred a selective advantage in human evolution, though he did not spell out what this would have been. Subsequent research has shown that dreaming is common to all mammals, except possibly egg-laying species, indicating that the evolutionary origins of dreaming must be sought long before the hominid line and its reliance on culture evolved (Hunt 1989, 26).

Regardless of how dreaming evolved in human ancestors, Fortune's explanation offers the anthropologically useful observation that dreaming can function socially by allowing people to express views that go against those prevailing in the surrounding society. Curiously, his own recounted dreams do just the opposite: they express majority views lurking in the mind of a social rebel. The agnostic becomes a Christian, the pacifist becomes a warrior, and so on. Evidently, it can go both ways: the waking view can either agree or disagree with the majority view in the surrounding culture, and the dreaming view can disagree with the waking view. In both types of internal conflict, Fortune's formulation, like other psychodynamic approaches, has important implications for a sophisticated anthropology of cultural storage and transmission, because it offers a composite model of the individual as a culture-bearing being (Hollan 2000). Such work reminds us that to be enculturated in one's tradition is not simply to agree with it. People are often almost literally of two minds: one might agree with others in dreams but not when awake, or vice versa.

Fortune's ideas about dreaming, though not directly influential in psychology or anthropology in his own time, and apparently only indirectly influential in setting his own ethnographic agenda, are nevertheless echoed in subsequent and current scholarship. I mention two examples. First, psychologist C. G. Jung ([1945] 1960) took an interest in the same sorts of dreams as Fortune considered. Jung labeled them "compensatory dreams" and saw them as functioning to restore intrapsychic balance by calling attention to neglected aspects of personality that are striving for expression (Koulack 1993).

Second, anthropologist Michele Stephen proposed a model of memory and dreaming that resembles Fortune's ideas about verbal and emotional knowledge, each using distinct memory systems:

> [One] organizes information in terms of verbal categories and semantic understandings and [the other] records and organizes all information according to its emotional significance. . . . What is usually available to waking consciousness is only the semantic/language register of memory. (Stephen 2003, 97)

Here, then, is an elaboration compatible with Fortune's model of dual belief systems that provides an opportunity for ambivalence in each individual's self-image and cultural repertoire. Each person has, as it were, two cultures, one based on emotional connections, and the other based on logical connections among representations. The dialectic between dreaming and alert consciousness brings the two alternately into view (see Lohmann 2003: 206–7).

## Fortune's Minds

Fortune revealed much about his personality, beliefs, and attitudes in the course of demonstrating his psychological ideas about dreaming. Six of the sample dreams he provided are his own. Here I recount them and his commentary about them in order to capture a glimpse of his youthful attitudes.

### *The Dream of the German Broil*

In this dream, set in the time of the Great War, Fortune was trying to decide whether to attend college in Germany or France (Fortune 1927: 5–22). In the dream, he had relatives in both places, and both countries were located in his hometown in New Zealand. The French college was in a flat area in the middle of town, affording convenient access. The German college was perched atop a high hill outside of town, accessible only by tram. He decided, in spite of the inconvenience, to attend the college in the enemy nation, Germany, in order to show his "freedom from the narrow nationalistic prejudices then convulsing the world" (Fortune 1927, 6). Upon arrival in Germany, he was ignored and ridiculed by his relatives, his former school principal, and others. Upon taking the tram to campus, he had only English money to pay, which was rejected. Finally, the driver took him to the college, only to crash the tram through the front door, and throw the hapless Fortune into a giant metal basket hung from the ceiling, before withdrawing and taking the tracks with him, leaving no way of escape. Fortune was harassed by various officials, and then thrown out the door.

Fortune explained that the night before he had this dream, he had read an article advocating passivism and the removal of nationalistic bias and insulting, ethnocentric descriptions of Germans from schoolbooks to promote international understanding and dialog. Fortune agreed strongly. The principal of the school he was attending had disapproved of his "pacifist activity" (Fortune 1927, 9), and had berated him in his office, beside a wastebasket that resembled the dream basket in which he experienced similar feelings of "humiliation and distaste" (Fortune 1927, 10). Caroline Thomas has identified Fortune's hated school as the

> Teacher's Training College in Wellington, which Fortune attended at the same time as he was doing his B.A. at Victoria College. The principal of the former was also a lecturer at the latter. Fortune would have been around nineteen years of age at this time. (Caroline Thomas, personal e-mail communication, August 27, 2005; quoted with permission)

The German college appeared similar in architecture and environment to his old school. Fortune hated the institution in real life because he found much of the lecturing there inadequate, and he resented the required attendance at these inferior lectures. These feelings, suppressed at the time, came out in the dream.

Fortune (1927, 13) related this dream narrative to his theory by explaining that his unconscious hatred of Germany found "expression by cloaking itself in a conscious hatred." The dream was not really about his former school, however. Fortune felt that his feelings toward it were less repressed than his hatred of Germany during the First World War as a schoolboy. He had later rejected and repressed these feelings, only to have them fanned by reading about anti-German textbooks. That Fortune had taken a strong stand rejecting nationalistic ethnocentrism is made clear in a footnote. "In the year of this dream I acted as secretary (honorary) to a student body that was raising funds (by manual labour) for student relief in Germany" (Fortune 1927, 13, n. 1).

In The Dream of the German Broil's narrative, and Fortune's contextualization and analysis, we learn that Fortune was rebellious, even as a child. He revealed himself to have high standards, not only for his own behavior, but also for others'. He criticized the quality of instruction at his school. He found their disapproval of his pacifism—already present as a child—difficult to bear. He was "humiliated" by authority figures' rejection of his moral stance against nationalism. Later, as a young man at university, he read critiques against nationalism and hawkish violence. He was a student activist and a leader, organizing support for his counterparts in Germany,

whom he regretted having hated during the war. Fortune was an active, dynamic *participant* in his society and culture. He was enculturated to British nationalism, and then, critically assessing official and majority views, he broke with them and sought to convince others to change their views as well. He did this not only as a student, but also as an author. Fortune's passages are infused with his controversial views in a remarkably frank and open manner.

*The Dream of the Library Vandalism*

The second of Fortune's shared dreams expressed anxieties over his responsibility as a student employee for his college library's late operation and closing. In the dream, he acted in his actual capacity at his college library at night. He secretly let in some friends who, to his horror, smashed the stained-glass windows and left. Fortune then danced barefoot with the vandals in celebration. One of them remarked that his reputation was such that he had nothing to lose, and Fortune became anxious that he, in fact, did have reputation and position to lose. Contextualizing his narrative, Fortune wrote that the chief librarian had recently reprimanded him for forgetting to turn out the lights, and though Fortune felt intimidated, he exhibited nonchalance, and later feared that his attitude would be reported to higher authorities (Fortune 1927: 23–28).

In this narrative, we see a conflict between wanting to rebel while continuing to value tradition and order as a foundation for life. He wanted reputation and status within proper society, yet he also wanted to cut loose. His dilemma, it would seem, was how to let his individuality, in Burridge's (1979) sense of a critical and exemplary reshaper of society, shine without destroying the order that he loved. His very interest in dreams that highlight contrarian views is a symptom of this.

*The Dream of the Library Disorder*

In the next dream, Fortune was again in the library, engaged in an argument about religion with the chief librarian. While they were distracted, a student gave a loud speech in favor of agnosticism, drawing a crowd. Fortune saw this as a threat to the peace and order of the library, which it was his job to maintain. After silencing the speaker, he stood on a chair and gave his own soapbox speech on the "great truth and advantages of the Christian religion" (Fortune 1927, 41). Fortune recounted this dream because in waking life he had, in fact, rejected Christianity and become an agnostic. His reasons for this were not because he found supernatural explanations untenable. Rather, he reasoned that believing in Christianity

would compel him to accept that God is a demon for allowing injustice in the world. Therefore, he preferred to see evil as the result of a "blind force" (Fortune 1927: 42–43).

For Fortune, agnosticism was associated with disorder and lack of direction. In the library disorder dream, he was a Christian because this repressed part of his personality was affectively associated with his open dislike of "disorder" in the library (Fortune 1927, 43).

Fortune's father, Peter, was a clergyman and named his son "Reo" after *te reo* "the Word" in Maori. According to Caroline Thomas:

> Peter Fortune had been a missionary in China but at the time of Reo's birth he was an ordained minister posted to the small town of Coromandel which had a relatively large Maori population. He was only there about a year before the family moved and over the next 10 years or so the family changed parishes about 4 or 5 times. Peter Fortune abandoned the church sometime around 1918 when Reo was 15 and it is probably then that Reo developed his agnostic beliefs. (Caroline Thomas, personal e-mail communication, August 27, 2005; quoted with permission)

Fortune thus grew up in a household in which cultural difference and religious ambivalence were central. Coming of age in New Zealand, he rejected both the ethnocentrism and absolute religious belief that guided Christian missionary work. This personal history would have prepared him well for the ethnographic career he was about to undertake.

A recurring theme of order/tradition versus disorder/individuality appears in Fortune's dream narratives. Order/tradition provides structure, beauty, meaning, and social cohesion, but it also stifles creativity and denies new knowledge. Disorder/individuality provides novelty and exuberant personal expression, but it also maims security and staid grandeur. The young Fortune struggled to discern what their best balance should be. He seemed to settle, uneasily, for individuality in the same way Margaret Mead was to do in vacillating between tradition versus freedom in gender roles (Lohmann 2004, 127). Yet Fortune's fascination with traditional culture, and how individuals act under its sway, was to help motivate his several future ethnographic projects.

*The Dream of the Pike Attack*

In this dream, Fortune found himself in a kill-or-be-killed wartime situation, where his pacifism and internationalism melted before necessities of the moment. Fortune's (1927, 47) description of this dream is brief:

> We were to storm a large house manned by German soldiery. We were outnumbered badly, but we had one advantage. The pikes that we carried were somewhat longer than the rifle and bayonet. I was concerned that my pike was not so sharp as my neighbour's. Then I found myself in a rush. In a minute I was in a room alone with my back to the wall facing eight or nine Germans. I felt a wave of fear sweep over me. But I killed them off and found myself outside gulping in the clean air.

Fortune explained that the previous evening he had attended a pacifist speech with which he had strongly agreed. In his dream, his pacifism was mocked by pragmatic doubts. This dream illustrated that it is well to be a pacifist in theory, but when one has to go with one's fellows against those who have become enemies, personal dissent can blunt one's weapon, and one can be killed. Instead, pacifist Fortune defended himself without a thought, killing several men. Again there was the fear of being different from others in his culture, which made him vulnerable, yet by toeing the line and doing what had to be done, he and his side vanquished.

*The Dream of Stopes*

Fortune offered an extended account of this dream and the background information needed to understand its characters. In sum, it is a dream in which he was introduced to a woman character from an erotic French novel that he had recently read. He arranged to meet her, but she did not appear. He then met some people he knew from his old school on the playground, including some girls whom he knew well and a younger boy named "Stopes," whom he barely knew. The playground was flooded; he told them that he had played cricket under worse conditions. After some people walked by, he found himself helping his former house master cut branches off a pine tree (Fortune 1927: 52–56).

Fortune was interested in this dream because it initially appeared to have no affective or personal significance, but he realized that it had merely seemed that way because the real meaning was so successfully disguised. The boy, whose name was not really *Stopes*, had a name similar to the author of a sex manual in his college library that Fortune had loaned out the day before. Fortune's choice of names becomes clear when we realize that the founder of the first birth control clinic in England was Marie Stopes (Lapsley 1999, 155). Another part of the sex manual author's name was that of a woman who had rejected Fortune. He had been reading the French novel at the same time as this misadventure, and the woman who failed to meet him in his dream had also jilted the hero in the novel.

Fortune accepted the Freudian symbolism of the feminine, watery field and the castration anxiety of chopping branches off a phallic tree. He had heavily repressed the idea of sex with the woman who had rejected him. But the book so circuitously connected with her by its author's name was explicitly about sex. Here, according to Fortune, was a dream about a would-be girlfriend, though she never appeared in its manifest content.

From this dream we learn of an occasion when Fortune had been unlucky in love, and that he had nonsexually idealized his object. It would appear that while Fortune was exploring sexually liberal attitudes in waking life, he retained prudish feelings. There is little wonder that Mead's introducing him to her own free-love beliefs challenged Fortune's sensibilities, in spite of his consciously held degree of liberality (see Thomas 2009).

*The Dream of Irises*

Like the Dream of Stopes, this dream had a simple manifest content, but the context and analysis Fortune provided is complex, because he saw deep repression as retaining its true meaning beyond consciousness:

> I am speaking at a public gathering in favour of the Labour Party. Then I am climbing, climbing, continually climbing a long ladder. At the top I mount into a great cluster of large red roses, very fragrant and extending away a great distance on either side. I descend the ladder. Half-way down is a long bed of white lilies. I descend through them and come at the foot of the ladder to a great bed of irises, deep blue irises, extending, a solid mass of colour, as far as I can see in either direction. (Fortune 1927, 58)

Fortune explained that he did, in fact, support the Labour Party, which he saw as rejecting uncritical patriotism. Some days earlier, he had engaged in a heated argument with supporters of the Conservatives, who had angrily accused him of being disloyal when he suggested that expenditures to the royal family might be regarded as "a national extravagance" (Fortune 1927, 59). Immediately before the dream, he had read a piece of conservative literature that described Labour as disloyal. Fortune (1927, 61) recalled an event six months earlier when the former premier, "a strong Imperialist," had died. The premier represented a form of patriotism that Fortune found distasteful. At the time, Fortune was teaching at a school, and was obliged to honor the deceased by setting the flag at half-mast. He had to ascend a ladder and nail the flag into correct position. Fortune used this background

to interpret his dream as actually about the British flag: the flowers were the correct colors and the blue irises are known as "flags." The dream represented to Fortune a reversal of his waking views: he had given a patriotic speech (though in favor of the antipatriotic Labour Party), and he wallowed in the beauty of the flag, though he rejected flag waving.

Part of Fortune accepted conservative attitudes prevailing in the surrounding culture, though on the surface he held views more typical of liberals. Rather than opposing the individual to the collective, Fortune opposed the views of two sectors of his society. He was both a Labour supporter and a loyal citizen of the British Empire. On the surface he rejected the mindless patriotism of the latter, yet on a deeper level he reveled in it. He also rejected the ostensible disloyalty to tradition of the Labour Party, while also recognizing that this was an inherent part of its message. Such are the dilemmas of living socially, as a member of groups with conflicting views.

## Continuities and Breaks between Fortune's Psychology and Anthropology

Fortune entered anthropology with a strong psychological foundation and a fascination for both dreams and the presence of what one might call contradictory "alleles" of culture in any individual's mind, each with its own kinds of dominance and recessiveness. What traces did this leave in his later anthropological work? Looking at his subsequent monographs, three on Oceania (Dobu, Manus, and Arapesh) and one on Native North America (Omaha), a subtle influence can be seen in Fortune's awareness of dreams and the ambivalences of individuals arising from cultural and social contradictions. However, despite their potential to enrich both his accounts and his explanations, Fortune's earlier theoretical interests are not systematically followed up. I see several possible explanations for this, each coming into play at different points. First, he may have seen the task of ethnography as fundamentally different from psychology. Second, simply documenting social, cultural, and linguistic systems may have consumed so much of his time in the field that there was insufficient opportunity for him to explicitly use and test his theory. Dobrin and Bashkow (2006) and Molloy (2009) all attest to the intensity of his cultural immersion in fieldwork. Third, he may simply have moved on to pursue other interests. I examine these major works for signs of how the younger, psychologist Fortune influenced the slightly older, post–Margaret Mead, anthropologist Fortune.

In *Sorcerers of Dobu* ([1932a] 1963, 181), Fortune noted that Dobuans believed personal souls leave the body during dreams, and that dream

images are spirits (cf. Lohmann 2003). Dobuans saw magical incantation as the ultimate cause of successful outcomes, even inspiring love (Fortune [1932a] 1963, 97). Some magic required dreaming: "In all love-magic the spirit of the magician is exhorted to go forth in the night to influence the spirit of the beloved" (Fortune [1932a] 1963, 237). A witch "does all of her work in spirit form while her body sleeps, but only at the bidding of the fully conscious and fully awake woman and as the result of her spells, it is said" (Fortune [1932a] 1963, 150). Not only were attacks perpetrated in (lucid?) dreams, but people experienced victimhood in their dreams as well, as when a woman

> would wake from a nightmare convinced that the flying witches were chasing her spirit and were just outside baulked by her spirit's luck in getting home before them. Then the night would be hideous with a ghastly yelling or alternate high and low shrieking, expressing such fear in its very sound as to be contagious enough to myself who knew its origin. (Fortune [1932a] 1963, 152)

Vivid here is Fortune's portrayal of how social interaction and personal experience, during both waking and sleeping life, led Dobuan people to accept beliefs about dreams and spirits (cf. Lohmann 2000). This approach is consistent with the themes of individual adaptation to surrounding culture discussed in *The Mind in Sleep*. There is, however, no discussion of Dobuan types of dream symbolism or how dream narratives might be used as a window on contradictory beliefs of the individual struggling to fit in with surrounding society. Instead, *Sorcerers of Dobu* is a description of how the sociocultural system works.

Fortune ([1932a] 1963: 43–62) did not gloss over rough spots and departed from ideal, synchronic images of social structure in his description of the "functioning of the system." The glitches he described show that the social system did not purr along like a well-oiled machine, but was rather embodied in individuals facing psychic and social ambivalences. Marriages were seldom smooth and happy, and people were torn by conflict between natal and affinal loyalties. Fortune also noted an inconsistency in the creation myth and how people coped with this:

> In the beginning of time various human persons *emanua nidi*, changed into birds. Thus birds came to be. Inconsistently enough, various birds hatched eggs from which issued the first human beings upon earth.

> In truth, the Dobuan does not push hard upon logic in his account of Creation. He does not notice that one legend conflicts with another. (Fortune [1932a] 1963: 94–95)

Fortune ([1932a] 1963, 126) similarly noted that Dobuans made alternating use of incompatible explanations for the efficacy of magic without concern.

In *Sorcerers of Dobu*, Fortune depicted incompatible beliefs and marveled at native acceptance of these as unproblematic, but did not explore the question of *how* they accomplished this. He did not explore intrapersonal ambivalence over holding contradictory beliefs, which is a centerpiece of *The Mind in Sleep*. It may be that he did not see his job as an ethnographer of a primitive people to go into these sorts of questions. Furthermore, doing research into dream symbolism and personal ambivalences in these exotic field settings would have required rich knowledge of each informant's personal quirks, life history, and both idiosyncratic and symbolic associations circulating in the local culture. As Waud Kracke (1987, 1999) has observed in his psychoanalytic studies of dreaming in Amazonia, an intimacy is required between ethnographer and informants that may not have been easily possible, given constraints of time, linguistic competence, and the other ethnographic work that Fortune faced (see also Spiro 2003).

Fortune's book on Native North America, *Omaha Secret Societies* ([1932b] 1969), is a rich and sophisticated account of both thriving and faded religious practices and beliefs in a tribe facing poverty and acculturation. Fortune ([1932b] 1969, 5) gave the role of dreaming in social life some attention, noting first that dream visions are not linguistically distinguished from waking ones. His ethnographic attention to dream narratives here secured him a recognized place in the ethnology of dreaming (Lohmann 2007).

Fortune quoted Small Fangs, who told of having been drawn to a place by a sweet smell (indicating a supernatural presence). Here, he saw one snake writhing over another snake he had dismembered shortly before, "doctoring the cut snake" (Fortune [1932b] 1969, 56). Small Fangs took this as a vision conferring healing ability, and accordingly, when his wife Lea was ill twenty years later:

> I told Lea about the snakes and told her to dream about it. She did dream that those snakes came to her and said to her to eat peyote and she would get well. So when she came home they had a peyote meeting and gave her peyote tea and she felt happier. (Fortune [1932b] 1969, 57)

Fortune related this story to illustrate how people who were not members of secret societies kept their visions quiet, except within the family, to avoid subjecting themselves to magical danger from affronted secret society members. Omaha people had less faith in private visions than in those officially sanctioned by the secret societies. Here we have a situation of ambivalent and semiprivate challenge to social hierarchy and dogma, yet Fortune did not take this opportunity to explore the point using the theory of dreaming and cultural ambivalence that he had developed a few years earlier.

This being said, Fortune did not shy away from points of individuality and ambivalence in his general description of how Omaha secret societies and visionary power operated. He noted that secret society members awed their audiences with "miraculous" displays, including supposedly sucking pathogenic fluids from patients' bodies without breaking the skin. When these practices were revealed to initiates as "tricks," they faced a contradiction:

> The initiates . . . realising that their affected 'materialisations' were not material miracles but solemn dramatisations only, a fact unknown to and strictly kept secret from the non-initiates, were free to believe that all was *batho*$^{n}$, unseen influence.
>
> . . . [I]n some initiates the process led to a heightening of religious feeling, [and] in others it led to a degradation. (Fortune [1932b] 1969, 4; see also Tuzin 1980)

Thus, Fortune depicted the Omaha individual as possessed of complex, changing, and contradictory beliefs. However, he did not make exploration of this complexity central to his work, which focused on documenting the society rather than individuals. Fortune had adopted a more typically anthropological perspective.

Turning to *Manus Religion* (Fortune 1935), we find a similar treatment of dreaming and simultaneously holding contradictory views. Fortune stressed the ambivalent feelings Manus people felt toward their "Sir Ghost—each household's protective ancestor represented by a skull. This ambivalence arose, in Fortune's analysis, because they expected the impossible from him: no accidents and indefinitely long life. When a Sir Ghost "failed to protect" his ward, and the household head died, the skull was removed from its place of honor, destroyed, and cast into the sea, to be replaced by another. Far from being straightforward protectors, spirits—including one's own Sir Ghost and those of other households—were also understood to be a common cause of illness and loss of life. So the ambivalence the Manus felt toward ghosts was the same as their ambivalence

toward living personalities whom they saw as both helpful and harmful to their own interests.

Reminiscent of his earlier point that dreams reveal submergent attitudes rejected in waking life, Fortune referred to Manus dreams as showcasing "a deeper attitude" of distaste toward one's own protector ghost, who on the surface was honored as benevolent. In this connection, one man told Fortune that he "dreamt of his Sir Ghost saying to another, 'Now let's go kill a good man,' and both laughed at the project" (Fortune 1935, 21). Fortune expected to find ambivalent and complex attitudes in individuals toward hegemonic dogma and decorum.

For the Manus, something seen in a dream was a real occurrence, so this was a revelation of hidden truth, confirming an air of suspicion that Sir Ghosts are not to be trusted. However, this did not contradict the belief that a Sir Ghost could also protect: "Manus children are not subjected to religious pressure. Faith and belief are taken for granted" (Fortune 1935, 5). This seems a far cry from an agnostic pacifist's struggles with God-and-country militaristic rhetoric in New Zealand during the Great War. Deeper ambivalences of this sort may simply not have come to Fortune's attention during his stay among the Manus.

Another point of religious uncertainty explored by Fortune among the Manus is their beliefs surrounding the causes of death. Sins of the living could motivate a judgmental Sir Ghost to punish the household with illness, and if the sin was not confessed and reparations paid, death of a member might result:

> In this way popular opinion becomes standardised, and sin is generally stressed as the cause of death, mortal sin, not ghostly malice, although individual oracles are continually making individual exceptions to try to save sinners' faces, and their own faces also. (Fortune 1935, 24; see also p. 56)

So here are ambivalences over the ways one assigns cause and effect for others', as opposed to one's own, matters. Fortune was clearly aware of the rough spots and inconsistencies in both his own and Manus models of their society, though he did not explore these issues through intimate dream analysis.

Nevertheless, Fortune's attention was firmly fixed on ambivalent beliefs among the Manus, as still another example shows. He reported that the Manus incompletely borrowed religious and magical beliefs of the neighboring inland Usiai people, including notions of "*tchinal* . . . mischievous land ogres . . . [and] magical familiars of the land dwellers of the Great

Admiralty, the Usiai" (Fortune 1935, 60). While the Usiai held these beings in earnest regard, the Manus considered them ridiculous characters and declared their disbelief. Yet

> in cases of serious illness, the Manus make use of *tchinal* derived exorcism of *tchinal* derived black magic. This use is generally secondary to the use of the customs of the Sir Ghost and ghost cult in order of trial, an order that is also an order of faith. The extraordinary thing, considering the legends, is that there is any place at all for the *tchinal* derived magic. (Fortune 1935: 60–61)

Here Fortune identified beliefs and practices that were explicitly denied or ridiculed under normal circumstances, possibly because of their foreign source and challenge to local tradition. Fortune described this ambivalence in terms of conflicting practices, turned to as a last resort. Such moments of desperation reveal (or produce) beliefs and attitudes that are otherwise submergent or denied. Fortune did not, however, explore these matters theoretically for his Manus ethnography as he did in *The Mind in Sleep*.

Finally, Fortune (1935, 254, 264) mentions the use of dreams by Usiai seers employed by the Manus as diagnosticians. Noting that for the Manus dreams represented accurate visions that might cause interpersonal conflicts, Fortune did not analyze them in terms he had laid out in his dream book.

Fortune's last book, *Arapesh* (1942), focuses on descriptive linguistics, supplemented with Arapesh texts and literal translations. This monograph reveals Fortune's remarkable skill, not only as an ethnographer, but also as a linguist. He does not deal with the problem of dreaming and cultural ambivalence, evidently because his efforts focused on other problems. However, in his 1939 article, "Arapesh Warfare," Fortune does take seriously the problem of reconciling individual and collective goals, to which he had attributed the evolutionary origin of dreaming in *The Mind in Sleep*:

> A balance was struck between individual values and collective values. . . . A war was promoted by individual initiative in the first instance, when one man coveted another man's wife. . . . The woman had to be the wife of a man of a foreign locality. She had first to be seduced and to be found willing to run away from her husband. Her seducer had to possess the support of his clan, moiety, and locality in arranging for her elopement to himself. (Fortune 1939: 26–27)

It would not do to stir up fights within one's own group. Moreover, to force a foreign woman to enter her husband's group as a bride would be, in Arapesh belief, to court her husband's death by sorcery. While Fortune depicted Arapesh individuals as needing to confront their sometimes conflicting egocentric and sociocentric desires, he did not turn to Arapesh dreams as a window on this dynamic.

In the same article, Fortune challenged Mead's ([1935] 2001) generalizations that Arapesh are profoundly nonviolent, lack warfare, and select "a *maternal* temperament, placid and domestic in its implications, both for men and women" (Fortune 1939, 36). Fortune's evidence is compelling, based on Arapesh narratives, transcribed and translated with great linguistic skill (Roscoe 2003).

In his ethnographies, Fortune portrayed people as changeable individuals, working within their social systems to balance personal and collective goals, which are easily at odds.

## Fortune's Resistance to Stereotyping

Retrospectives on early work of the culture and personality school demonstrate that researchers' personalities, concerns, and interpersonal relations shaped their personified depictions of societies (Dobrin and Bashkow n.d). Fortune's early study of dreams reveals a distaste for stereotyping people and groups. His intellectual position was that individuals, embedded in dynamic social life, are comprised of multiple, changing attitudes that are shaped by an ongoing internal dialog. When awake, this internal dialog is based on logical connections and spatial contiguities, but when dreaming, the inner discourse is based on affective association, in which emotional connections have precedence. Fortune argued that an individual's cultural repertoire exists in two forms that are dynamic and sometimes contradictory: (1) logical-waking, which is dominant and socially attuned, and (2) affective-dreaming, which is recessive and egocentrically attuned. While he did not make it his business as an ethnographer to trace out these processes in detail, it is possible that this sophisticated position mitigated against his adopting the stereotyping excesses that marred the early culture and personality school's otherwise valuable achievements. Most particularly, Fortune's temperament and intellectual position, and not merely his resentment as a spurned husband, led him to reject two of Margaret Mead's formulations: her unpublished "squares" or fourfold personality typing (Banner 2003: 326–33; Sullivan 2004; Thomas 2009) and her generalized gender types in *Sex and Temperament in Three Primitive Societies* ([1935] 2001).

Mead's squares typology comprised four personality types labeled Northerner, Southerner, Turk, and fey, that one might simplistically gloss as egocentric, sociocentric, domineering, and nurturant, respectively. It was born in the famous conversations among Fortune, Mead, and Bateson in the Sepik field of New Guinea in 1933, at the same time as an intense flirtation developed between Mead (Fortune's wife) and Bateson. This certainly would have made for bad affective associations in Fortune's mind, probably both in waking and in sleep. Beyond this, the characteristics of the model would make it anathema to his assumptions and preferences, carefully and logically worked out in researching his dream book.

Fortune realized that the squares system's classifications were subjective and bad science when Mead changed her classifications of particular people depending on the health of her relationships with them. In the intense emotions and "tropo" psychology that gripped the group in the oppressive Sepik heat, Fortune struck Mead, following which she miscarried (Banner 2003: 335–6). In her understandable anger, Mead labeled Fortune with negatively valued, masculine terms from the squares model like possessive, jealous, and aggressive. Fortune's behavior at the time doubtless reflected consternation over the appearance of a charming rival who, in apparent collusion with his wife, threatened his marriage.

Caroline Thomas (2009, 307) quotes one of Fortune's letters of 1934 in which he wrote that when Mead labeled him a sadistic "Northerner," he felt himself not only negatively judged, but also the victim of what anthropologists now call "othering." I suspect Fortune's theory of the changing, conflicted individual vis-à-vis social pressure, and his stance against smug nationalism, would have biased him against the squares model, even had it not been used as a weapon against him.

After Fortune's marriage to Mead ended, Mead published her influential and groundbreaking *Sex and Temperament in Three Primitive Societies* (Mead [1935] 2001). As Gerald Sullivan has pointed out, the idealized gender types Mead identified in the perfectly contrasting Arapesh, Mundugummor, and Tchambuli correspond to her squares types:

> Mead and Bateson used the same system of categories to compare various societies; hence the ethos of each of the societies mentioned in *Sex and Temperament* should be understood as a representation of one or more of Mead and Bateson's types. The men and women of the Arapesh generally exemplified the maternal, or Southern, position as those of the Mundugumor generally exemplified the paternal, or Northern position. Tchambuli women were usually Turks; Tchambuli men were most often a variation on feys. (Sullivan 2004, 195)

Unsurprisingly, then, Fortune was among the published critics of *Sex and Temperament in Three Primitive Societies*. He wrote, somewhat awkwardly:

> Although the theory of Arapesh social culture having the one, uniform tendency, so called maternal, remains a hypothetical creation, it is not proper to assume that the Arapesh must be conceived either in terms of that hypothesis, or in terms of alternative hypothesis. It is better to make no hypotheses. (Fortune 1939, 37)

Fortune's intellectual style shines through in his attack on Mead's generalized depiction of Arapesh personality. In Mead's writing on the Arapesh, in spite of rich description, she tended to rhetorically use idealized scenarios as though they were data, and made strong generalizations even when her own data contradicted these (Lohmann 2004, 112). Fortune criticized the accuracy of such blanket generalizations, and in the lines that follow, drew on specific data to support a more flexible picture of Arapesh culture-in-practice.

Though Fortune and others have pointed to Mead's tendency to ride roughshod over the details, Mead's central point in *Sex and Temperament*, that gender is not determined by sex alone, stands as a monumental achievement in anthropology (Lipset 2003). But Fortune's aversion to an etic, generalized picture of the cultures of both groups and individuals is clear. Lise Dobrin and Ira Bashkow (2006, 146) have shown that in comparison to Mead, Fortune exhibited an emic, empathetic, and particularist approach, and generally eschewed subordinating ethnographic data to theoretical frameworks.

Ruth Benedict's *Patterns of Culture* ([1934] 1953) inspired the intense discussions within the Sepik love triangle. While the book is a towering achievement in anthropology, it problematically likened cultures to individual personalities. Among the difficulties with early culture and personality studies was the tendency to reify cultures and stereotype cultural configurations. Ironically, Benedict used Fortune's ethnography of Dobu as one of her exemplars. Susanne Kuehling (2005: 136–7) studied Dobu seventy years after Fortune and critiques his account, but calls Benedict's boiled down version of Dobuan paranoia a "travesty." Thomas (2009) has confirmed that in private, Fortune himself was similarly critical of Benedict's use of his materials. Fortune's pre-Mead dream-life and self-analyses indicate that before his fateful association with Mead, Benedict, and Bateson, he tended not to think of either individuals or groups in terms of

stereotypes, but rather saw them as continually learning and changing, and holding multiple and contradictory views simultaneously.

## Conclusion

Reo Franklin Fortune was a complex, changing personality whose early psychological theorizing lent subtlety to his subsequent ethnographic work. However, he did not treat his theory of dreaming as a set of hypotheses to be tested in the ethnographic field. Fortune's theory of cultural ambivalence and his personality are consistent with his rejection of blanket characterizations of people and peoples.

Fortune's psychological theory of dreaming, though dated and imperfect, is a provocative and sophisticated anthropological theory of cultural ambivalence. Among its valuable implications is the point that the dynamic cycling of culture in individuals takes place in both waking and dreaming consciousness, as well as in the groggy zones in between. Fortune's *Mind in Sleep* deserves a second chance among contemporary psychological anthropologists.

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# REDISCOVERING REO: REFLECTIONS ON THE LIFE AND ANTHROPOLOGICAL CAREER OF REO FRANKLIN FORTUNE

Caroline Thomas
*University of Waikato*

Reo Fortune's legacy to anthropology has been overshadowed by his relationships with friends and colleagues, many of whom came to see him as difficult. Professional differences and personality clashes pervaded his career. Despite this, he was once regarded as the foremost anthropologist of his era. Fortune's contribution to anthropology is reflected in his major publications and journal articles that illustrate the diversity and complexity of his fieldwork.

## Introduction

Reo Franklin Fortune's position in anthropology is problematic. Despite his proximity to important figures in anthropology during the 1920s, 1930s, and 1940s, he never attained the status accorded many of his peers. Fortune was unsuccessful in securing a permanent academic position until 1947, when he was appointed to the University of Cambridge, where he remained until retirement in 1971. Yet, though established in an academic institution, his problematic relationships with colleagues and students marginalized him in terms of recognition. Fortune's difficulty in public speaking and his erratic approach to teaching exacerbated his liminality so that he always remained on the fringes of greatness.

There are three reasons why Fortune deserves to be reconsidered. First, he was an immensely productive writer with four books in eight years: between 1927 and 1935, he published a work on dreams and three

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ethnographies of which, perhaps, his most famous are *Sorcerers of Dobu* (1932c) and *Manus Religion* (1935) (see also Lohmann 2009). Second, he was a New Zealander—one of many, including Diamond Jenness, Raymond Firth, and Te Rangihiroa (Peter Buck)—who informed anthropology during the early part of the twentieth century. However, Fortune lacked the institutional base that Jenness achieved in Canada, Firth in London, and Te Rangihiroa at Hawai'i and Yale. Third, Fortune was a central figure in anthropology during the interwar years of 1926–1939 because of his strong links to both American and British anthropological traditions. He was trained by some of the most significant figures in British social anthropology. These included Alfred Cort Haddon, Bronislaw Malinowski, and Alfred Reginald Radcliffe-Brown. His marriage to Margaret Mead and his connections to Franz Boas, Ruth Benedict, and Gregory Bateson helped Fortune establish ties with those who were central to anthropology in both Britain and the United States at that time.

The published literature about Reo Fortune's life is small. There are a few obituaries (Gathercole 1980; Lawrence 1980; Young 1980); a chapter by his niece, Ann McLean, in a book about early anthropology in the Papua New Guinea highlands (Hays 1992); and Wardle's (2004) entry in the *Biographical Dictionary of Social and Cultural Anthropologists* (Amit 2004). However, the only material of substance published since his death exists in works about Mead, Benedict, and Bateson (Banner 2003; Caffrey 1989; Grosskurth 1988; Howard 1984; Lapsley 1999; Lipset 1980; Modell 1983) and in Mead's own writings, such as *Blackberry Winter* (1972) and *Letters from the Field 1925–1975* (1977). The Australian historian Geoffrey Gray (1999) recounts Fortune's relationship with John Hubert Plunkett Murray (later Sir), the lieutenant governor of Papua from 1906 to 1941. Fortune conducted his first fieldwork on Dobu in 1927–1928 and courted controversy by refusing to meet with Murray in the field. The result was an exchange of letters that ended with Murray distrusting most anthropologists and Fortune being labeled a troublemaker. Francoise Héritier (1999), writing on incest, attributes comments to Fortune that were actually made by Mead. Most contemporary literature either ignores or sees Fortune as an adjunct of Mead. Fortune's relationship with Mead, Benedict, and Bateson has been well documented in the literature, while his relationships with others, also influenced to some extent by these three, have not (Banner 2003; Howard 1984; Lapsley 1999; see also Lohmann 2009; Molloy 2009; Sullivan 2004). This paper, using material from archival sources, is one interpretation of Fortune's relations with others within the context of his professional career.

Fortune's interpersonal relationships were at times stretched to breaking points. His marriage to Mead foundered, and his friendships with colleagues and mentors were often strained. Through gossip and misunderstandings, his reputation as "difficult" became accepted as truth. However, Peter Worsley and Peter Gathercole, who knew him as a colleague and mentor, spoke fondly of Fortune, describing him as a breath of fresh air, affable, and charming (Worsley 1989; Gathercole, pers. comm., December 8, 2003).

I shall begin with an overview of Fortune's life and then relate various relationships that illustrate changing perceptions of Fortune as a friend and colleague and conclude with comments on his legacy to anthropology.

## Biographical Notes

Reo Franklin Fortune was born in Coromandel, New Zealand, on March 27, 1903, and died in Cambridge, England, on November 25, 1979. He was awarded his MA with first-class honors from Victoria University College in 1925 for a thesis titled "Dream Problems." In 1926, he won a traveling scholarship that enabled him to travel to England to continue his studies at the University of Cambridge. The following year, he published his first book, *The Mind in Sleep* (1927b); completed his thesis for the diploma in anthropology; and commenced his first fieldwork—on the Island of Tewera in the D'Entrecasteux Archipelago off the coast of Papua. In 1928, Fortune married Mead, and over the next five years they conducted research in five different cultures until their marriage broke down while they conducted research in the Sepik area of New Guinea. In 1932, Fortune published *Sorcerers of Dobu* and *Omaha Secret Societies* along with an article in the *Encyclopaedia of Social Sciences* on incest (1932a), and in 1933 he applied unsuccessfully for the chair of anthropology at the University of Sydney. In 1934, he applied for various positions, including the chair in sociology at Cambridge, again unsuccessfully, and returned to New Guinea in 1935. Fortune's ethnography *Manus Religion* was published this same year, and in 1936 he and Mead were divorced. In the years following the divorce, Fortune married his former love, Eileen Pope. He also held various academic and governmental positions: at Lingnan University, China (1937–1939); Toledo, Ohio (1940–1941); Toronto (1941–1943); government anthropologist to Burma (1946–1947); and, finally, lecturer at the University of Cambridge (1947–1970), where he remained until his death in 1979. Fortune was to publish only one more book during his lifetime, *Arapesh* (1942).

Perhaps some of the recognition Fortune craved came in 1951 when he was awarded the Rivers Medal for anthropological work in the field. This medal was instituted in 1923 in honor of William Halse Rivers Rivers, a former president of the Royal Anthropological Institute who, along with Charles Seligman and A. C. Haddon, had conducted the first major anthropological field expedition to Papua and New Guinea during the latter part of the nineteenth century. In 1974, Fortune was also made an honorary fellow of the Association for Social Anthropology in Oceania.

That Fortune came to make a career in anthropology was probably as much a result of his meeting Mead as it was with his disillusionment with the teaching of psychology at Cambridge. He found the system at Cambridge isolating. Access to a suitable area for study was difficult to obtain, as the rooms available in the laboratory of the Department of Psychology were allocated to others. He resorted to studying in the psychology library, the anthropology library, and his room. He was also without the financial means to entertain other students, which would have been useful for building his network of social and intellectual contacts. Finding the people in anthropology to be more sociable and disillusioned with psychology, he switched disciplines (Library of Congress: Margaret Mead Papers [LOC: MMP], box R4, Fortune to Mead, letter dated October 12, 1926). According to Bateson, Thomas Callan Hodson, a reader in anthropology at Cambridge, was reputed to have remarked that he [Hodson] had "rescued" Fortune from psychology and "saved him from himself" (LOC: MMP, box R2, quoted in Gregory Bateson to Mead, letter dated February 6, 1934). Anthropology also provided an introduction to Bateson, who was at that time preparing for fieldwork in the mandated territory of New Guinea. The different social, economic, and intellectual backgrounds of these two men were to be an important factor when they came together in the Sepik area of New Guinea in 1932.

## First Fieldwork

Fortune's journey to the Sepik began five years earlier, when he arrived in Australia in 1927 to take up field research under the auspices of the Australian National Research Council (hereafter ANRC). He and Mead were not yet married, and functionalism was in its infancy. A. R. Radcliffe-Brown (or "Brown" as he was then known) had recently arrived in Australia to take up the newly formed chair in anthropology at the University of Sydney. Radcliffe-Brown also held the position of chairman of the Committee on Anthropological Research of the ANRC—the body that determined funding for anthropological research within Australia, Papua,

and the mandated territory of New Guinea. Fortune was granted funding to conduct research in Tikopia, but Radcliffe-Brown had decided that Fortune was to go to the Gilbert Islands. Fortune declined and chose instead to go to Fergusson Island and Dobu Island in the D'Entrecasteaux Archipelago. Finding Dobu contaminated by missionaries, he then chose to go to Tewera Island, about thirty nautical miles northeast of Dobu.[1]

Fortune's disagreement with Radcliffe-Brown did not stop there. Radcliffe-Brown also strongly disagreed with Fortune on anthropological issues, believing that there was no place for psychology within the anthropological framework. Fortune described Radcliffe-Brown as totally antagonistic to "(1) [Franz] Boas' influence . . . (2) Theories of first origin. (3) Psychology," distrusting of Mead on the first count and of him (Fortune) on the third (LOC: MMP, box S1, Fortune to Mead, letter dated September 19, 1927). Fortune's letters to Mead from this time indicate that they both hoped still to be "given" Tikopia, but news that Raymond Firth had also laid claim to this region ended any chance. Radcliffe-Brown was at this time also seeking an assistant for the department, and Fortune came under consideration. However, Firth got both Tikopia and the assistant position. Fortune got Dobu and, on completion of his fieldwork, Mead.

Despite Fortune's theoretical differences with Radcliffe-Brown, he came to his defence when J. H. P. Murray, the governor of Papua, took exception to a letter from Fortune. Honesty and concern for the well-being of the Dobuans led Fortune in 1928 to express himself in terms that Murray considered "deranged" but that reflected the anthropologist's distrust of colonial administrators. Fortune saw missionaries and colonial authorities as a threat to the autonomous well-being of indigenous peoples and made this clear in his correspondence with Murray. As far as Fortune was concerned, the idea of making the Islanders adhere to the "European mould of law" was quixotic as well as "the attempt to fit an impossibly resistant material into an alien mould" (National Archives of Australia [NAA] Series A518/1 Item A806/1/5; NAA, Series CRS G69 Item 16/41 Folios 1–22). Fortune was, as Gray (1999) quoted him, "[b]eing honest to my science." But the damage was done. Fortune's reputation in Australia would always be tainted by this episode. Radcliffe-Brown continued his support for Fortune throughout the 1930s, despite his earlier dispute with Murray, and praised Fortune to Mead and to A. C. Haddon. Mead and Fortune had provided hospitality to Radcliffe-Brown when he was in New York, and he wrote to Mead in 1931, saying:

> I have been reading the proof of Reo's monograph on the Omaha and am very pleased with it. Please offer him my congratulations.

> I do hope that by this time he knows that I regard him as one of the very few first-class anthropologists round the world. (LOC: MMP, box B15, Radcliffe-Brown to Mead, letter dated November 2, 1931)

Radcliffe-Brown continued by saying how Fortune had done so much better than many expected him to and how Haddon, who had been doubtful, was very pleased when informed of Boas's approval of the Omaha work. Later, Radcliffe-Brown, at the instigation of Mead, was to be instrumental in Fortune being offered a position at Lingnan University in China in 1936. Fortune at first refused, hoping to be selected for the chair at Cambridge, but when this did not eventuate, he accepted the offer in China.

## Margaret Mead and Ruth Benedict

In 1928, Fortune returned to New Zealand to await Mead's arrival. Their marriage took place at Auckland on October 8 before they set sail for Sydney and their joint fieldwork in Manus. However, the marriage seemed doomed even before it began. Fortune had already expressed doubts about Mead's truthfulness regarding living with her first husband, Luther Cressman. Fortune's concerns centered on how Mead had explained this to Louise Rosenblatt, her former roommate at Barnard College. Rosenblatt was also in Paris when Fortune met Mead there in 1926. He wrote,

> That however altruistic your motive its execution repelled me immeasurably . . . [and] [a]s an example of twisting things—"you'd not want to touch a divorced woman"—your interpretation of my revulsion. . . . If you refer to this further when you're with me Margaret I'll feel tempted to strangle you—Come forgiving it—or else get out—one thing or the other—thoroughly—I'll stand no further misinterpretation and unfair slight in that matter. (LOC: MMP, box S1, Fortune to Mead, letter dated April 9, 1927)

Even as she sailed for Bremen in 1927 to meet Fortune in Berlin, Mead had reservations about their relationship. Writing a conciliatory letter to Cressman one week and then another the following week—after just three days with Fortune—Mead said they had no future together at all (Banner 2003, 262; Howard 1984, 103). Mead returned to New York and filed for divorce from Cressman. Fortune, meanwhile, was completing his diploma while sailing to Sydney. He was not to see Mead for another year, and

during that time their letters were not particularly romantic in the traditional sense, instead being filled with plans for their first combined research. Mead read and critiqued Fortune's thesis for the diploma in anthropology at Cambridge. He, in turn, proofed her forthcoming book, *Coming of Age in Samoa* (1928), and apologized for being critical. He suggested that "the redundant citation of points already made in the pages entitled 'Conclusions' is not excusable and leaves a bad taste rather than otherwise" (LOC: MMP, box S1, Fortune to Mead, letter dated August 19, 1927). Mead considered returning to Samoa (see also Tiffany 2009). However, Fortune suggested that they could go there in their own time later, "unless you prefer to work there alone—in which case I suggest waiting till I appear to appreciate you less than I appear to now" (LOC: MMP, box S1, Fortune to Mead, letter dated September 19, 1927).

Marriage to Mead gave Fortune entry to academic circles in the United States. Mead and Benedict had been instrumental in securing a fellowship for Fortune at Columbia University under Boas, where he was to complete his doctorate. His thesis was to be the first chapter of *Sorcerers of Dobu*. The correspondence between Fortune and Boas is sparse. However, Boas appears to have been sufficiently impressed with him that he recommended an extension of the fellowship and supported Fortune's future applications for funding. This resulted in *Omaha Secret Societies* (Fortune 1932b), his return to New Guinea in 1935, and eventually *Arapesh* (Fortune 1942).

More important, however, was the influence of Benedict, whose intellectual relationship with Boas ensured continued support for Fortune through the Social Science Research Council at Columbia. Fortune often turned to Benedict for assistance in finding funds. With monies secretly supplied by Mead, Benedict sought the support of Boas, usually with success.

Mead had supplied the initial funding for Fortune's trip to the Sepik. Under the guise of an anonymous donor, she had offered $3,000 to fund Fortune's research. Whether he was aware of this is not known. Mead suggested that should the matter come to light, her father would be known as "the anonymous donor" (LOC: MMP, box A4, Mead to Edward Sherwood Mead, letter dated October 16, 1930). After the marriage failed, Mead used her own money to fund research that would keep Fortune out of the United States. She used the guise of an anonymous donor, once more enlisting the assistance of Benedict and, indirectly, Boas.

Ruth Benedict was, however, the one constant in Fortune's anthropological career. While there is no indication of when Benedict and Fortune first met in person, it is probable that it was during Benedict's visit to Paris

in 1926. Their correspondence began in 1926 when Benedict wrote to Fortune expressing her pleasure in reading his article "The Psychology of Dreams" (Fortune 1926):

> It is an excellent piece of work and even I who am the merest amateur in the subject can appreciate its quality. I congratulate you heartily on it. If you were nearer than across the ocean I could have much conversation with you about it. It's stimulating. (Alexander Turnbull Library: Reo Fortune Papers [ATL: RFP], MS-Group-0923: 80-323-444, Ruth Benedict to Reo Fortune, letter dated October 25, 1926)

When Benedict wished to use the Dobuan material in her own work, she wrote to Fortune requesting permission to do so. He replied, "Of course use the Dobuan material if it's really good enough" (quoted in Mead 1959, 329; Fortune to Benedict, letter dated November 21, 1929). However, Fortune subsequently regarded the way in which his Dobuan ethnography was used as a travesty. His personal copy of *Patterns of Culture* (Benedict 1935) is heavily marked with comments. Where Benedict stated in her acknowledgments that "the chapters have been read and verified as to facts by these authorities," Fortune noted,

> Verified as to facts. What are such when misinterpreted [and] . . . the pouring of the pig's fat over one of the men of dead man's village is obviously a bit of horseplay, but Benedict is so determined that the Dobuans shall be merely dour and jealous psychopaths that she takes the perfectly straight forward statement "in this happy manner the locality pulls together its forces when death strikes it," and interprets it as a piece of irony. (ATL: RFP 80-323-078, quoted from Fortune's annotated copy Benedict's *Patterns of Culture* 1935)

Fortune remained silent and never publicly challenged Benedict on her use of the Dobuan material. Several years later, when a French edition of *Patterns of Culture* was being prepared, he did write to her, "I am not happy about your *Patterns of Culture* thesis. The use of the Dobuan negatives is conditional in my not being cited in support of it in any way" (LOC: MMP, box R5, Fortune to Benedict, letter dated June 24, 1948).[2]

Benedict encouraged Fortune to continue writing while she edited and arranged for the publication of all his major anthropological works. It was also Benedict who passed Fortune's doctoral thesis at Columbia in 1932.

She ensured that he had funds whenever possible and appears to have understood Fortune in a way that few others did. She cared greatly for him, even after his marriage to Mead ended. Perhaps it was her own relationship with Mead that allowed her to empathize with Fortune. After all, they had both loved and lost the same woman who, while professing her love for them, seemed to regard them more as acquisitions. Fortune confided in Benedict. It was to her that he related his feelings regarding Mead.

In perhaps the only account, from his perspective, of what happened in the Sepik, Fortune wrote to Benedict in 1934,

> I don't know much of what you think of me after Margaret's done with talking of me. Margaret's always represented me as butting into her affairs too much and spoiling them, as you probably know. So that it was high time I stood aside. What I regret is that I was not in a position to stand aside easily and politely—not on the Sepik River. (LOC: MMP, box R5, Fortune to Benedict, letter dated as "end of October" 1934)

Because there was £250 invested in new field equipment and charges in reaching their field site, Fortune felt that he could not just up and leave and, more so, that he could not leave alone. Mead had become hysterical, blaming all her "failed" relationships on Fortune. And then came the "Race" business (LOC: MMP, box R5, quoted in Fortune to Benedict, letter dated as "end of October" 1934):

> I was a member of an alien Race to you, Luther [Cressman], G[regory Bateson], and Margaret—I being called Northern, sadistic etc. and you all Southern and masochistic; a lot of stuff about sex perversions, horoscopy, [*sic*] twins (we are all twins with one twin absorbed into the umbilical cord of our births), analysis of the Holy Family in Race, Margaret sorting out medicine chest into bottles of medicine for one race and bottles for another race for several days. (LOC: MMP, box R5, Fortune to Benedict, letter dated as "end of October" 1934)

Fortune apparently tried to get the key to the medicine chest from Mead but failed despite the fact that he had been suffering from malaria for ten days. Meanwhile, Mead continued "sorting medicines insanely and having hot baths every half hour lest she die—a fear she had." Fortune also remarked that he had retained "one or two documents of the hysteria . . . they reveal the form of the stuff tho' little of the intensity or of the feeling"

(LOC: MMP, box R5, Fortune to Benedict, letter dated as "end of October" 1934). Fortune described abandoning the camp and how Bateson had decided to come with them, believing that Fortune was a danger to Mead. According to Fortune, the boat journey from New Guinea was quiet and reasonably peaceful until they approached Sydney, whereupon Mead once again lashed out at him, realizing that their arrival in Sydney could mean her losing Bateson. Fortune was deeply hurt but "did what seemed right to me . . . which doesn't alter the fact that I'm fond of her, care about her—in a way" (LOC: MMP, box R5, Fortune to Benedict, letter dated as "end of October" 1934).

When Mead left Fortune in Sydney in 1933 and returned to the United States via New Zealand, she did so with the knowledge that she would always remain vulnerable to criticism from Fortune, whether it was through published material or verbal reports. By going to New Zealand, she was able to present her side of the story to Fortune's family, but three years later, when Fortune himself returned to New Zealand, his version of events differed considerably from hers. Fortune's sister-in-law, Shirley, wrote to Mead in 1936 saying, "I suppose very naturally, and you will understand this better than I do, the two accounts don't tally" (LOC: MMP, box B9, Shirley Fortune to Mead, letter dated August 26, 1936).

Mead was apparently angry that Shirley had questioned her version of why she had left Fortune. In response, she wanted Shirley to know that she could make or break Fortune's career. Although she did not explicitly say that she would stop assisting Fortune, the inference is there. Mead claimed that she was the only person who could help Fortune: he "has owed his whole scientific support to wires that I have been able to pull" (LOC: MMP, box B9, Mead to Shirley Fortune, letter dated October 3, 1936). Mead continued,

> I have some feeling that your letter is based partly on a feeling that I lied to you, in order to set myself in a good light and Reo in a bad one. What possible use that could have been to me seems difficult to discover. I went to New Zealand because Reo said he wouldn't go there and have to explain why I hadn't come. It meant leaving Sydney earlier than necessary and it meant expending a lot of money and suffering considerable retrospective misery, to make that stop in New Zealand. (LOC: MMP, box B9, Mead to Shirley Fortune, letter dated October 3, 1936)

After their marriage ended, Mead wrote to Fortune's brother, Barter,

> If he should want to marry he will probably have to take up some other and related occupation—like teaching psychology for instance. Unless he should marry Dorothy Anabaldi [*sic*]. Doesn't she inherit a farm when her father dies? (LOC: MMP, box R5, Mead to Barter Fortune, letter dated July 8, 1935)

Dorothea Arnaboldi was in fact a cousin of Fortune, and the family had expected that she and Reo would marry. However, Fortune backed out because of a perceived problem with consanguinity. "He felt that they were too closely related to risk having children" (Melda Brunette [Fortune's niece], pers. comm., April 14, 2007).

In order to protect herself, Mead utilized her network of friends and colleagues to ensure that Reo Fortune remained as far away as possible. She wrote to Shirley Fortune, "There is not one single person with any power in the anthropological world who is going to try to get Reo a job, or get his stuff published, unless I push them" (LOC: MMP, box B9, Mead to Shirley Fortune, letter dated October 3, 1936). Fortune, in turn, wanted nothing further to do with Mead, while she maintained her belief that the theory of the squares, which she had devised in the Sepik, was, in fact, scientific.[3]

The squares theory was based on the four points of the compass, each sector being representative of a different temperament (see also Sullivan 2004). For example, northerners were cold, domineering, and sadistic; southerners were hot, submissive, and masochistic. Mead had placed Fortune in the North and herself and Bateson in the South. Fortune repudiated this, calling it dishonest. It was the way in which Mead used the squares theory that possibly hurt him the most. Initially, Fortune thought that Mead had lost her mind, that the events in the Sepik were brought about by figments of her imagination, and he hoped that when they returned to civilization, she would once more be the Margaret he knew and loved. He thought that giving her space and time to come to her senses would resolve their conflict. But when he attacked her "science" in his letters to her, Mead could not see that he honestly saw it as "bad science" and took it as a personal assault.

## Life after Margaret Mead

Fortune was to remain in Sydney for some time after Mead left in 1933, undecided as to where he might go next. Most of his friends in Australia were also Mead's friends, and their correspondence clearly shows that of the two, Mead appears to have been the more charismatic. Caroline

Tennant Kelly had befriended Fortune on his first stop in Sydney, so it was only natural for him to introduce Mead to her. After the events in the Sepik, Mead was to make full use of this friendship by using Kelly as her intermediary in securing information about what, where, and when Fortune was doing. If he coughed or appeared distracted, Kelly wrote dutiful letters to Mead advising her of what was happening. In return, Mead, requesting absolute secrecy, secured Kelly's cooperation in securing signatures for the divorce papers. In addition, little of what was happening in the Department of Anthropology at the University of Sydney escaped her knowledge. All comings and goings were duly reported back to Mead. Having been persuaded of the validity of "the squares," Kelly began to categorize all around her accordingly, but she also had doubts. She wrote to Mead, "You know Margaret I have spasms of absolute pro Reo-ism. . . . Is it that we have created a Reo of our imaginings or is it that we become fogged when we try to penetrate the Northern Square? (LOC: MMP, box B9, Caroline Kelly to Margaret Mead, undated letter, ca. May 1934).

While Benedict encouraged Fortune to continue researching and writing in the years immediately after his separation and divorce from Mead, she discouraged him from returning to the United States. When Fortune enquired about positions at Columbia and Duke, Mead wrote saying that money was unavailable and that positions were hard to find (LOC: MMP, box R4, Margaret Mead to Reo Fortune, letter dated May 14, 1934). The United States was in the middle of the Great Depression, and it was also Mead's desire to keep Fortune as far away as possible. As stated previously, Mead used her own money and Benedict's help to secure a grant that would see Fortune return to New Guinea; this grant was an extension of project 46, "Research in New Guinea." In a report to the Council for Research in the Social Sciences (CRSS), Boas stated, "In 1934 on his second field trip he was the first anthropologist to go into the newly opened, not yet pacified 'Gold fields' region of New Guinea. A Major part of his monograph on the Purari tribe has been turned in to the Department" (Columbia University Archives [CUA]: CRSS 1925–1968, box 6, folder 46, Professor Franz Boas, Research in New Guinea). Presumably, this was the Kamano material that Benedict had read but rejected: "It would be a mistake to publish these three slight chapters on Purari as a separate publication. The whole detailed monograph should appear at once and make all the material available together" (LOC: MMP, box S1, Benedict to Fortune, letter dated December 1, 1936). Although Fortune was to send Benedict a revision of this material including diagrams of the material culture (clothing and weapons), it too was returned to him with Benedict's

note saying, "I shall be very glad to have the completed and edited manuscript whenever you get it done" (LOC: MMP, box R5, Benedict to Fortune, letter dated May 7, 1940).

Unfortunately, Fortune never completed the Kamano manuscript. His attention had now been taken by a teaching position in China, a new wife, and new fieldwork among the Yao of southern China. However, when he sought funding while in China, he turned once again to Benedict, who attempted to have his previous grant (project 46) extended. In her memorandum to the CRSS, Benedict recommended that $1,000 be appropriated "to be used in financing ethnological study of Chinese village communities" (CUA: CRSS, Benedict to the CRSS, memorandum dated May 10, 1937). However, when the matter was brought before the members of the Committee on Anthropology and Sociology at the CRSS, only Boas gave his support. Benedict had to find another source. However, the Japanese invasion of China intervened in any plans Fortune had for research, and he was forced to flee.

In 1941, Benedict wrote to Alfred L. Kroeber,

> I'd do a lot to save Reo, but it would have to be out of my own pocket for he has fallen down badly on writing up his last two-years field trip which was arranged for him under Columbia's auspices, and I would not feel I could urge any Foundation to risk write-up money even if I knew a Foundation which might give it. (Bancroft Library [BANC]: MSS, CU23, box 33, Benedict to Kroeber, letter dated August 6, 1941)

Benedict referred here to the Kamano material, which Fortune seems to have abandoned at this point in his career. Perhaps more important, throughout the difficult years following his separation and divorce from Mead, Benedict remained his friend, even when Fortune thought that she was his enemy and accused her of being an agent of Mead and Bateson. In 1937, Fortune wrote to Benedict,

> It might occur also to a friend of mine—of any reality—that I do not wish to be reported upon to Margaret Mead and Bateson—however curious they may be—that I regard their curiosity into my state of mind, doings etc. as impertinent and mean. . . . I would prefer your friendship myself. (LOC: MMP, box R5, Fortune to Benedict, letter dated April 28, 1937)

Paramount to Fortune's relationships were loyalty and honesty, and these qualities had been sorely tested with events in the Sepik.

On his return to England in 1933, Fortune attended Malinowski's seminar series. Fortune also presented a paper at Cambridge that Bateson dutifully reported back to Mead. Fortune's (1934) unpublished manuscript "A Critical Anthropology," which posed the question "where, and to what extent, anthropologists should stand behind native cultures, push their claims and throw his [*sic*] personal influence into their championship" was, perhaps, a reflection on his encounter with J. H. P. Murray in New Guinea and the repercussions that followed from this. Malinowski provided the means for Fortune to live in London, lending him money that Fortune repaid from his living expenses that came with grant monies (ATL: RFP, MS-Group-0923, Malinowski to Fortune, letter dated October 20, 1938). Further, when Fortune applied in 1934 for a position as assistant in ethnology at the Colombo Museum in Ceylon, he asked Malinowski to provide support for his application. Malinowski wrote a glowing letter of recommendation for both Fortune and Ralph Piddington but concluded by backing Piddington, who was one of his own doctoral students. Of Fortune, Malinowski wrote,

> Dr. Fortune is a brilliant young anthropologist who most likely will make for himself a career at one of the world's great universities, and whose ambitions are set that way. Even if you could secure his services, I should be afraid that any time he might be lured away by some outside call. . . . I am going to support him as strongly as I can for the Professorship at Cambridge or Oxford. (London School of Economics, Bronislaw Malinowski Papers [LSE: BMP], box Malinowski/7/9, Malinowski to the [unnamed] Director of the Colombo Museum, letter dated February 4, 1936)

However, when it came time to throw his support behind Fortune for the Cambridge chair in 1937, Malinowski wrote to A. C. Haddon, asking him to take up the task, as two of Malinowski's former students, Raymond Firth and Audrey Richards, had already asked Malinowski to support them for the same position. Nevertheless, Malinowski did write Fortune a general letter of recommendation at some stage in which he stated, "Theoretically, Dr Fortune has shown in his many articles an originality of outlook which promises to place him among those who will build the anthropology of the future" (LSE: BMP, Malinowski/7/22, Malinowski, Letter of Reference for Reo Fortune, undated [ca. 1937]).[4]

Between 1933 and 1937, Fortune applied for numerous positions—in anthropology at Sydney, Cairo, Ceylon, and Cambridge and in psychology

at both Victoria and Canterbury University Colleges in New Zealand. Despite seemingly glowing references, he remained unsuccessful and finally accepted a position at Lingnan University in China that he had previously declined, much to Mead's chagrin. As mentioned earlier, Mead had interceded with Radcliffe-Brown in an attempt to secure a position for Fortune in China. This position was the result. He settled in well at Lingnan (1937–1939) and may have remained there longer had not war broken out, making it impossible to continue.

But where was he to go? Fortune wrote to Malinowski, who suggested that he would be better served by contacting Kroeber in California. However, American universities, post-Depression and nervous of an impending war, had few positions to offer. On his arrival in California, Fortune stayed at the University of California, Berkeley, for some weeks, attending seminars and giving one or two informal talks to students before eventually securing a position at the University of Toledo, Ohio, in 1941. Although this position was part time, he had hopes of it becoming something more. Unfortunately, ignoring the advice of Kroeber that publication of his papers "Social Forms and their Biological Basis" (Fortune 1941a, 1941b) would offend the Puritan sensibilities of midwesterners, Fortune went ahead and published, thereby effectively ending any chances of continued employment at Toledo (BANC: MSS, CU23, Fortune to Kroeber, letter dated July 4, 1945).

The published versions of the articles were marked "R. F. Fortune, Toledo," without naming the university, as its authorities had requested he remove the name of the university from the papers. These two papers, recorded as "current issues" in the *American Ethnological Review*, referred to (1) codes of sexual conditioning among tribes in New Guinea, comparing patrilineal societies with matrilineal ones, and (2) the relationship between war and diet. In the former, Fortune stated, "Where inheritance, succession and descent inhere in the male line, orgasm of the clitoris is tabooed and sex fore—play and after—play directed to that end is also tabooed," whereas in the matrilineal group, these things were regarded as a common part of sexual relations (Fortune 1941a, 571). In simple terms, the biological needs of the male in patrilineal societies take precedence over the needs of females to the extent that female satisfaction is tabooed, thereby establishing a code of behavior conditioned in a similar manner to Pavlov's dogs. Likewise, the reverse is true of matrilineal societies.

In his latter paper, Fortune (1941b) suggests how the type of diet conditions the stomach and gut in such a way as to determine social responses to war and peace. Those who maintain a light diet with a high metabolism indulge in a fast alternation between war and peace, whereas those whose

diet is heavy and with a slow metabolism have a slower alternation between war and peace. In both papers, the essence is the link between biological conditioning and social conditioning. However, it was the discussion of sexual mores rather than the ideas behind it that was deemed offensive to university officials in particular and to midwesterners in general.

It was also at this time that Fortune traveled around the Midwest attending conferences, ostensibly without invitation, and according to Benedict "had gone off on tangents in anthropological arguments that had left them thinking he was probably deranged" (LOC: MMP, box B1, Benedict to Mead, letter dated July 20, 1941).

Fortune next moved to Toronto, where he found himself in the Department of Anthropology with Thomas McIlwraith and Charles William Merton Hart. Hart and, presumably, McIlwraith were no strangers to Fortune, with Hart having been the subject of a rather scathing unpublished letter from Fortune to the editor of *Man* in which he questioned Hart's conception of anthropology (LOC: MMP, box R4, Fortune to the Editor of *Man*, letter dated September 8, 1932). Hart had written a review of Mead's *Growing Up in New Guinea* in which he wondered "whether [Dr. Mead] can be called an anthropologist at all" (Hart 1932, 146) and Fortune had sprung to her defense. But here, as in Toledo, Fortune's controversial writing proved to be problematic. His article "Arapesh Maternity" published in *Nature* (Fortune 1943) outraged McIlwraith and Hart, but Fortune failed to understand why. Fortune wrote to his wife, Eileen,

> I got another copy of the August 7, 1943 number in which I published an article those brainless sops at Toronto raised a storm in a tea-cup about. On re-reading it I see nothing in the article to justify their behaviour. It was I'm certain largely malicious. (ATL: RFP MS-Group-09213, Reo Fortune to Eileen Fortune, letter dated 1945)

This article may have been only a part of the problem. Edmund Carpenter, who was also at Toronto around this time, recalled asking McIlwraith whether it was true that Fortune had been fired for suggesting to his mainly female class "that the unique human feature of face-to-face sexual intercourse might have influenced human development" (Carpenter, quoted in Howard 1984, 267). McIlwraith was reported to have agreed in the affirmative. Anecdotal evidence also tells that Fortune challenged McIlwraith to a duel with weapons of his choice from the museum's collection or, perhaps more seriously, chased McIlwraith around the museum with a tomahawk

(Levin, Avrith, and Barrett 1984). It is more likely that Fortune challenged McIlwraith to identify weapons in the collection, but whatever had happened, Fortune became extremely uncomfortable remaining in a department where he no longer felt welcome. Fortune took refuge by enlisting in the Canadian armed forces and served as a Royal Canadian Auxiliary Forces War Services supervisor in England from 1943 to 1945.

By the end of World War II, Fortune was once again jobless and accepted the position as government anthropologist to Burma. His time there was to be short, as in 1947 he finally achieved a permanent position at Cambridge. A colleague from the same department, Glyn Daniel (1986: 199–200), in his autobiography *Some Small Harvest*, described Fortune's appointment as "a disastrous appointment and we suffered as a result for many years." In the beginning, Fortune felt that he was doing well. He received an MA from Cambridge shortly after his arrival as well as a letter from President Score at SouthWestern University of Texas with prospects for a job there in the future (ATL: RFP MS-Group-0923, Reo Fortune to Eileen Fortune, letter dated December 10, 1947). However, Score died in 1949, and no further offer was made from SouthWestern. Fortune's wife, Eileen, had yet to join him in England, and his letters to her indicate that he felt confident in his new role. However, his self-confidence and his relations with Raymond Firth were to be sorely tested by what Fortune saw as Firth's arbitrary interference between a student and his supervisor.

Peter Lawrence had applied for funding through the Australian National University (ANU) to conduct fieldwork in Papua New Guinea, and it was Firth who had conducted the interview as ANU's London representative. Firth agreed that Lawrence should receive the fellowship and that Fortune should be appointed his supervisor. The dispute that ensued between Firth and Fortune was acrimonious and changed their relationship, with Fortune feeling demeaned and his authority irreparably undermined by Firth's interference. Fortune had wanted Lawrence to conduct research in the highlands of New Guinea, whereas Firth, determined that the Madang area would be cheaper and more accessible for Lawrence. Firth had also provided Lawrence with a rail pass to enable him to attend lectures at the London School of Economics. Fortune interpreted this as being made a condition for funding and accused Firth of trying to poach students. Firth also preferred to take advice from the Australian anthropologist Ian Hogbin, who had just returned from New Guinea. Fortune saw this as a slight, suggesting that his knowledge of New Guinea was outdated. The authorities at ANU sided with Firth and even went so far as to make further funding for Lawrence conditional on Firth approving Lawrence's progress. Although Fortune did eventually attempt to proffer an olive branch, it was too little,

too late (London School of Economics: Raymond William Firth Papers [LSE: RFP], Folder 312, Reo F. Fortune).

In 1954, Fortune applied for the new chair of African studies at Rhodesia University College. Firth was named as a referee and responded generously in his official reply, praising Fortune's intelligence and contribution to anthropology, but expressed doubt on his organizing abilities and lack of administrative experience. In a personal note to Walter Adams, secretary of the Inter-University Council for Higher Education in the Colonies, Firth was more explicit, saying,

> I have not felt that I could open my mind fully because some years ago I suffered from one of his obsessive notions and our relationship has never got back on the old footing. Indeed I am a bit surprised that he gave me as a reference. I do not think I am alone in this. McIlwraith of Toronto, and [E. E.] Evans-Pritchard [of Oxford] have both had something of the same trouble. I think there is no doubt that Fortune, in all ordinary matters a reasonable man, is inclined at times to fly off the handle if he thinks he has not had his due. . . . I think you should consult privately a man like [Isaac] Schapera, who, as far as I know, has never been involved. (LSE: RFP, Box Firth/8/30, Firth to Walter Adams, letter dated October 29, 1954)

There is no doubt that Fortune had difficulty in coherently expressing his ideas. Bateson recalls that "the curious zigzag violent progression of Reo's mind—talking nonsense this way and that with all the time a sound idea unexpressed behind it all and insisting on his nonsense till finally the idea comes to the surface" (LOC: MMP, box S1, Bateson to EJ [Ethel John Lindgren], letter dated February 27, 1936). According to Bateson, "[William] Blake and Reo would probably have understood each other" (LOC: MMP, box S1, Bateson to EJ [Ethel John Lindgren], letter dated February 27, 1936). It was this inability to articulate what he was thinking that probably contributed to Fortune's reputation for being difficult.

On the other hand, Kroeber was pleasantly surprised, when Fortune visited Berkeley in 1941, to find that his manner had improved from when he first met him in 1930. In a letter to a colleague, William Lloyd Warner, Kroeber wrote,

> Our reaction to his personality is more favorable than it was eight or ten years ago. Lowie agrees with me in this. He is more fluent, at any rate definitely less jerky in a manner. He gave our students

> a talk yesterday which was very vivid and which they lapped up. (BANC: MSS, CU23, box 177, Kroeber to [William Lloyd] Warner, letter dated March 20, 1940)

By the 1950s, the pendulum had swung the other way, with Jean La Fontaine remarking that she and Nur Yalman attended Fortune's presentations at Cambridge "not because we could really follow them, but because we liked him and felt someone ought to go to his lectures" (La Fontaine 1982). Peter Worsley (1989) suggested, "In a bizarre way, he of course had some very powerful insights. One never knew whether he was being serious or crazy or just thinking beyond one." Gwilliam Iwan Jones (1989) recounted how his students at Cambridge in the 1950s decided that Fortune "thought in Dobuan" (Macfarlane 1982).

Whatever his thinking, Fortune published little after his relationship with Mead ended. Apart from the *Arapesh*, which appeared in 1942, his subsequent publishing consisted of short articles. Eileen Fortune allegedly extended an invitation to Mead to come live with them in the hope that she would inspire Fortune once again (Howard 1984, 431). It is my belief that Fortune's reluctance to publish was, in part, a result of Benedict's rejection of his Purari manuscript in 1935 and again in 1940. Although she remained on friendly terms with Fortune and supported his applications for funding, she resisted pressuring him to complete his work, perhaps because she too began to believe the impression that he was "deranged" (LOC: MMP, box B1, Benedict to Mead, letter dated July 20, 1941). Benedict's death in 1948 removed the one person who may have been able to motivate Fortune to complete his Purari work.

In addition, Fortune did not wish to enter into a publishing "war" with Mead over her interpretation of their fieldwork materials. Fortune's differences with colleagues were compounded by his refusal to indulge in academic politics, resulting in collegial perceptions of him as paranoid as the Dobuans he wrote about.

## Fortune's Legacy

Although Fortune came to be seen as eccentric and, at the worst, mad, his contribution to social anthropology is significant. *Sorcerers of Dobu* (Fortune 1932c) remains one of the classics of the functionalist school and is often cited in works relating to homicide (Daly and Wilson 1988), kinship (Lévi-Strauss 1969; Parkin 1997), exchange theory (Foster 1993; Gudeman 1986; Sahlins 1972; ), the *Kula* (Uberoi 1962), and misogyny (Gilmore 2001). Fortune's book remained the only published ethnography of Dobu

until 2005 (Kuehling 2005). Although Kuehling (1998, v) initially described Fortune's portrayal of Dobu as a "caricature," she tempered this in her published work with the admission that her "comments on his study are based on a different discourse of interpretation" (Kuehling 2005, 2). By contrast, Young's (1980) obituary of Fortune regards both *Sorcerers of Dobu* and *Manus Religion* as remarkable works.[5]

When Fortune revised *Sorcerers of Dobu* in 1963, he was constrained by the printing process, which allowed him to insert new material only by removing either text or placing material on the blank spaces that existed in the original. Consequently, he substituted one section with another, retaining the same pagination. In the original, pages 241 to 249 are concerned with the dominant sex attitudes of the Dobuan and present a clear portrait of Dobuan sexual morality. The revised text (pages 241–249) contains a seemingly irrelevant critique of Malinowski and lacks the detail of the original.

*Manus Religion* (Fortune 1935) used what Mead called "event analysis," a form of "situational analysis more than twenty years before it was 'discovered' in Africa by Gluckman and his colleagues" (Mead 1972, 199; Young 1980, 89). The American anthropologist Rodney Stark (2003, 372) described Fortune's book as a "distinguished study of the Manus of New Guinea."

Mead remarked that "A Note of Some Forms of Kinship Structure" (Fortune 1933), published in *Oceania* in 1933, was "the kind of thing on which a man could found his career" (Mead 1972, 215). Thirty-six years after publication, Nelson Graburn sought permission to reprint Fortune's 1933 article, describing it as "one of the most crucial contributions to the development of modern structural anthropology" (ATL: RFP, MS-Group-0923, Nelson Graburn to Fortune, letter dated May 2, 1969).

While *The Mind in Sleep* (1927b) and *Omaha Secret Societies* (1932b) were much overlooked, Roger Lohmann (2009) has recently revisited the former, while George Devereux regarded the latter as a "corrective investigation" throwing new light on a previously neglected aspect of Omaha society, thereby providing the impetus for future corrective studies. As Devereux (1967: 223–24) reported, "Fortune's seminal contribution to the initiating of this new policy should not be forgotten, no matter how great (or small) a role his personal penchant for the night-side of cultures may have played in it."

Lise Dobrin and Ira Bashkow's (2006) article makes extensive use of Fortune's Arapesh publications (1939, 1942) and his unpublished manuscripts, finding "his ethnographic work immensely insightful and unfailingly accurate" (Ira Bashkow, e-mail comm., August 31, 2006).

While historians of anthropology have often overlooked Fortune, his works endure. The recent interest shown in examining Fortune's contribution to anthropology is, perhaps, indicative of a Reo Fortune revival.

## NOTES

This paper began as a paper submitted for presentation at the symposium "Gang of Four: Gregory Bateson, Ruth Benedict, Reo Fortune and Margaret Mead in Multiple Contexts" during the Association for Social Anthropology in Oceania (ASAO) 2005 annual meetings in Kauai, Hawai'i.

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1. Fortune referred to areas as "uncontaminated" by missionaries, so I have assumed that he would have used this word if talking about Dobu.

2. The punctuation is as Fortune wrote it. It is unlikely that Benedict replied to this letter, as she was in Europe from mid-July until September and died shortly after her return to the United States.

3. For discussion on the squares, see Banner (2003: 328–408), Lapsley (1999: 221–44), Lohmann (2009), and Sullivan (2004).

4. I do not know if this letter was sent to anyone or just given to Fortune. There is nothing in the file to indicate what Malinowski did with this letter.

5. For a discussion of reviews of Fortune's work, see Molloy (2009).

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# "MORE LIKE FIGHTING THAN LIKE WAITING": MEAD, METHOD, AND THE PROPER OBJECT OF KNOWLEDGE IN ANTHROPOLOGY

Maureen Molloy
*University of Auckland*

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

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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 La Flesche 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 dispair [*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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# WRITING LIVES: RUTH BENEDICT'S JOURNEY FROM BIOGRAPHICAL STUDIES TO ANTHROPOLOGY

Judith Schachter
*Carnegie Mellon University*

Ruth Benedict's early biographical essays illuminate her anthropology and, especially, the humanism that guided her choice of subjects, her style of writing, and her goals in the profession. I examine the biographical essays, written in response to World War I, and then assess the contribution of *The Chrysanthemum and the Sword*, written at the end of World War II. With the speech she made to the American Anthropological Association in 1947, Benedict completed the circle, proposing a new anthropology that would include the emotions, ethics, reasoning, and experiences of individuals. A response to the behaviorist social science of the times, Benedict's anthropology drew on the lessons she had learned as a biographer about the creativity that emerges under congenial conditions and the human capacity for reshaping conditions that are uncongenial.

## Introduction

During World War I, Ruth Benedict drafted three biographical studies, her response, she said, to the horror of war. Thirty years later, she published *The Chrysanthemum and the Sword*, a response to World War II. Prompted by cataclysmic upheavals, these writings have more in common than first appears. The similarities in purpose and style between writing a life and writing a culture establish Benedict's importance for contemporary anthropology. The biographer's representation of the driving force of an individual trajectory informs the anthropologist's attempt to present the terms by which life is lived in diverse settings. The lesson Benedict teaches pertains

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to the discipline in the twenty-first century, as anthropologists dismantle the boundaries of "society" and dismiss the coherence of "culture."

Benedict's journey (1946, 1) began with her experiments in biography and culminated in a portrait of the "most alien enemy" the United States ever fought in an all-out war. In the intervening years, she pursued the question that had prompted her biographical inquiries: how can an individual, a culture, and, ultimately, humanity effectively and responsibly respond to a looming crisis or to relentlessly disastrous conditions? The question lay behind her anthropological inquiries, a source of her examination of varying cultural configurations. The anthropologist who was intrigued by the merits of a tightly integrated configuration, the driving force behind an enduring culture, grew out of the biographer who had been fascinated by the passionate conviction that propelled an individual life.

From first to last, Benedict wrote in order to make an impact. She was not satisfied until her writings reached a public and radically altered the perceptions of her audience. While the biographical essays of World War I do not predict what she would do in the subsequent endeavors of her life, they do provide a way of reconsidering the book she wrote during World War II—the highly successful and much disparaged *The Chrysanthemum and the Sword*. Benedict (1946, 1) said it was an "assignment," but Japan in fact was an ideal case for her. In "experiencing vicariously" the delicate and militaristic culture, Benedict entered a new phase of her anthropology, the phase that would have brought her compelling concern to the level of humanity, had she lived beyond 1948.

I begin with a discussion of the biographical essays to explore Benedict's claim that the study of lives constitutes an effective response to a raging world war. She viewed biography, with its intimacy of writer, subject, and reader, as a genre with a particular capacity for transforming a reader's perceptions. Next, I turn to the last book she wrote, as a mature anthropologist and servant of the American government during World War II. The study of Japanese patterns of culture remains provocative, condemned for its compact portrait of a complex nation and, at the same time, recognized as crucial to subsequent scholarly and popular accounts of Japan. Pro or con, readers marvel at the persuasiveness of an anthropological study done at a geographical, cultural, and political distance and admit its indisputable staying power.

In conclusion, I move from an examination of *The Chrysanthemum and the Sword* to a consideration of Benedict's 1947 farewell speech to the American Anthropological Association, "Anthropology and Humanism." Together, these two pieces bring Benedict's anthropology full circle, back to the early writings, back to World War I, and back to an evolving interpretation of the value of anthropology.

## The Biographical Studies

In November 1914, Benedict jotted an entry in her journal. "My pet scheme," she wrote, "is to steep myself in the lives of restless and highly enslaved women of past generations and write a series of biographical papers from the standpoint of the 'new woman'" (Mead 1959, 132). She had married Stanley Benedict six months earlier, and throughout the summer she had dreaded the inevitable coming of war. Why at that moment, newly married and horrified at the swirling global disaster, did she decide to write biographies of Mary Wollstonecraft, Margaret Fuller, and Olive Schreiner?[1] She chose the three, she informed a prospective publisher, because they were "leaders and pioneers"—a phrase handwritten in over the crossed-out sentence "women who were all, perhaps, leaders of a future generation rather than their own" (Ruth Fulton Benedict Papers, henceforth RFBP). The draft of a foreword shows the connection between her choice of subjects and the war that was raging in Europe. "Today the great adventure is in womanhood. No other calling, except perhaps world-statesmanship, has in it so much of the untried, so much of the still undiscovered; none is called upon to face such wholesale reversal of conditions; none to surmount such bewilderment of the soul" (RFBP).

These sentences indicate the heart of her biographical studies. They reveal Benedict's fascination with those who venture into a place where rules are not scripted, norms not developed, and the patterns of culture tangled. The comparison of Wollstonecraft, Fuller, and Schreiner to world statesmen puts the emphasis where it belongs: on boldness, courage, and action. For Benedict, the women represented a daring she wished for herself, for her contemporaries, and for the statesmen who would construct a postwar world. She treated the three not as founders of a feminist movement but as "pioneers" in a quest for freedom and fulfillment that transcended gender. Given this approach, Wollstonecraft's feminist manifesto, *A Vindication of the Rights of Woman* ([1792] 1988), receives scant attention. Benedict focused not on the writer but on the person whose spirit chafed against an era.

"Adventures in Womanhood," the proposed title for the triadic study, forefronts action. Not one to take titles lightly, Benedict relished the word adventure, with its connotations of youthful exploration and its echo of a children's story. She portrays a person whose spirited resistance to suffocating conventions preserved her "soul" and made her a model for future generations. Notes indicate that Benedict regarded Fuller and Schreiner through the same lens, the books they wrote less important than the adventures they lived. Without a geometry, the axioms untested, the three cut

new pathways, stretching their capacities to the utmost. These were lives boldly ventured, not carefully charted.

"She lived with all the alertness of her brain focused upon the abrupt experiences of her life: the knowledge she won, the price she paid, her books may hint to us, but it is her life through which we understand" (RFBP). These words propose an approach to biography that emphasizes *experiencing* and courage in the face of the unexpected. Activity is the key and the act of constructing a life more important than the products of a life. That was the lesson Wollstonecraft's life taught, a lesson that Benedict posed for her own generation in a time of war, when enslavement threatened a wide scope of humanity. In her drafts, the particulars of Wollstonecraft's living enlarge to depict any person who exploits the human potential to reshape the givens of life.

While she was working on the biographies, Benedict read the American philosopher William James. As always, she jotted engaging ideas into her notebook. Referring to James, she wrote,

> [A]nd in picking out from history our heroes, and communing with their kindred spirits, in imagining as strongly as poss. [*sic*] what differences their individualities brought about in this world while its surface was still plastic in their hands . . . each of us may best fortify and inspire which creative energy [*in original*] lie in his own soul. (RFBP)

This unreferenced sentence provides a key to her view of what biographical writing could accomplish: creating empathy between reader and subject so that the strength, individuality, and willfulness of the subject's adventure inspire a similar daring in the reader. With his practical American spirit, James considered heroic any person who asserted will in the face of adversity. As biographer, Benedict responded to this notion of the hero in the common man.

In 1927, Virginia Woolf wrote an essay describing the "new biography" in terms similar to those Benedict had ventured in her notes. Woolf praised the transformation in biography between the nineteenth and the twentieth centuries. No longer restricted to the great and famous, modern biography honors the lives of those to whom fame and fortune may not accrue (Woolf 1927, 149). The new approach to telling lives leaves behind the hagiographies of the nineteenth century and the birth-to-death accounts written by dutiful chroniclers. If the living is as significant as the status of the person, Woolf continued, a biography of Mrs. Smith can be as significant as one of Shakespeare. The new biographer acknowledges the glory in

conquering conditions and in staking a claim to dignity, whether the biographical subject is housemaid or poet. While Wollstonecraft, Fuller, and Schreiner hold a place in history different from that of Mrs. Smith, Benedict composed her biographical studies to prompt affiliation and not awe in readers.

In the 1927 essay, Virginia Woolf insisted on the art that constitutes the new biography. No longer recorder of events or uncritical admirer, the biographer selects, considers, and arranges details: "in short, he has ceased to be the chronicler; he has become an artist" (Woolf 1927, 152). The art itself is new, referring to a modernism in which features may be fractured and highly abstracted—Picasso evoking Gertrude Stein through lines and cubes, for instance, or Virginia Woolf portraying her friend Vita Sackville West in the multicenturied life of Orlando (Woolf 1928). Woolf also had a workmanlike approach to biography-as-art, evident when she undertook the biography of her fellow Bloomsburyite Roger Fry (Woolf 1940). In embarking on that project, the biographer shunned the flamboyance of *Orlando* and worried over the connection between the facts of the life and the presentation of the man: Benedict's challenge in her "Adventures."

*Empirical Philosophy*

Woolf described a genre of life writing that downplayed chronology and emphasized significant moments. Faced with a messy box full of love letters, tailors' bills, and ticket stubs, Woolf sought for the clues these provided to Fry's character, much as Margaret Mead later did with Benedict's papers in *An Anthropologist at Work* (Mead 1959). Like Benedict, Mead shared the viewpoint of the modern biographer that a life could be better conveyed through testimonies of the subject than through a conventional framework. Mead, however, ensures continuity by inserting commentary; she is less the biographer in Woolf's—and in Benedict's—terms than she is the mediator between reader and subject. The *art* of biography described by Woolf in 1927 resonated more closely with contemporaneous visual portraitists than did Mead's compilation in *An Anthropologist at Work* (1959). A decade before Woolf described the genre, Benedict had struggled with the links between the facts and the portrayal of a life. The weight of detail obstructed the goal of evoking the person. Sensitive to biography's burden of proof, Benedict jotted in her notes that "biography is a shying horse before facts" (RFBP).

A surprising analogy, the shying horse ultimately jumps the traces, creating a moment of beauty in an artful gesture. The image of a shying horse also bespeaks the difficulty—almost the fear—of plunging into the available

data. Tailors' bills and ticket stubs remain stubbornly there, at once vital to and deflecting from the individuality a biographer must convey. Scattered in boxes and in archives, facts are meaningless until composed, yet the art must not obscure the *living*.

> What this really adds up to—fate, being exposed, what life means—I can't really say in the abstract. . . . Perhaps all I can try to do is illustrate it with examples. And that is precisely why I want to write a biography. In this case, interpretation has to take the path of repetition.

The sentences are not Benedict's but Hannah Arendt's, commenting on her decision in 1957 to write a biography of the nineteenth-century intellectual, Rahel Varnhagen (quoted in Weissberg 1997, 31). Arendt intended to create a portrait through the presentation of statements and incidents and to avoid probing behind the image her subject imparted. Nearly forty years after Benedict initiated her biographical project, Arendt delineated a similar purpose for biography: not simply to present character but also to propose a way of living. Arendt's phrase, the *vita activa*, or lived life, resonates with Benedict's "experiment in living." Both phrases wed biography to philosophy. Biography, too, argued an ethical stance, and this implied a relationship between writer, subject, and reader. The biographer must create a direct connection between subject and reader, facilitating full participation in another way of living.

Benedict used the phrase "empirical philosophy" to describe biography (RFBP). The phrase underlined her goal of using biography to transform the viewpoints of readers on the conduct of their own lives. The phrase also describes a method in which the biographer's immersion in facts is replicated for the reader in repeated illustrative examples. Furthermore, as Arendt claimed, such a method is the appropriate way of treating a subject, whose inner motivations cannot be known by an outsider. For the philosopher who tried biography, like the anthropologist who did as well, the genre provided a model for the understanding (*verstehen*) that effects a transformation in readers.

Years before she began the biographical studies, Benedict (then Ruth Fulton) stood entranced before the Opie portrait of Mary Wollstonecraft hanging in London's National Portrait Gallery. A memory that explains the choice of subject, the experience also reveals the ideal Benedict set herself when she started the work. She recalls being able to understand the whole of a person's life through the one moment in time a portraitist represents. She ended her five drafts of the Wollstonecraft essay with that memory:

> In the National Portrait Gallery hangs a picture of Mary Wollstonecraft, a picture of her as she was a few scant months before her death. I remember the child I was when I saw it first, haunted by the terror of youth before experience. . . . And the woman in the little frame arrested me, this woman with the auburn hair, and the sad, steady, light-brown eyes, and the gallant poise of the head. (Mead 1959, 519)

The last sentences affirm the goal of the biographical journey, to establish intimacy between lives of different measures.

Benedict called herself a child in the passage, although she made the trip to London just after college. As Benedict used the word, "child" refers to her instant and complete absorption in the life displayed. An ideal viewer or reader, the child willingly enters the world presented to her, unencumbered by a search for the how-do-you-know or by a suspicion of the artist's motives. Caught by the web of details and entranced by the pattern they make, the child trustfully suspends disbelief. "There is no moment of reasonable doubt" (RFBP).

There are, however, facts. Reality stuck to the biographer's venture, and Benedict did not confuse the form with a made-up story. Rather, her journal notes reveal her conviction that an artful presentation of facts can effect a suspension of disbelief. Like William James, whom she continued to read, Benedict considered that an individual confrontation with reality confirmed the capacity of all human beings to alter conditions.[2] By choosing biography, however, Benedict adopted a mode of presentation in which illuminations from a lived life—an experiment in living—replaced the axiomatic prose of the American philosophical tradition. Benedict tested the value of artfully composing fact in the biographical essays and tested her own ability to compose a portrait in which words would have the impact of the most striking visual rendering.

The biographical essays represented Benedict's effort to captivate a reader. Her goal was to provoke a transformation in understanding comparable to the best of philosophers and the most skillful of artists. Starting with a life study allowed her to grapple with problems of identification, interpretation, and illumination and to bring her discoveries to the discipline that would satisfy her purposes in writing.

## Patterns of Japanese Culture

The biographical project failed. Without the promise of publication, Benedict deemed the endeavor futile. "And more and more I know I want

publication," she wrote in her journal (Mead 1959, 135). The marriage collapsed: the chemical detective stories she wrote with Stanley Benedict did not bring wife and husband together. The war ended, and Benedict enrolled in anthropology courses at the New School for Social Research. What happened to biography?

The book she published in 1934, *Patterns of Culture*, presents an easy case for concluding that Benedict transferred the lessons of biography into her anthropology. Three case studies resemble the triadic structure of "Adventures in Womanhood," foreground to a fourth, implicit character. In *Patterns of Culture* (hereafter *Patterns*), the United States vies with the Zuni, Dobu Islanders, and Kwakiutl for attention. In "Adventures," the fourth character is a cohort of compatriots, facing the consequences of an unprecedented global conflict. The connection to biography is enforced by the notion *Patterns* bequeathed to the discipline, that culture is personality writ large.[3] At the same time, *Patterns* veers away from Benedict's approach to biography. The portraits are surrounded by three chapters that bring the voice of the social scientist forward, offering theories of the individual in culture equally significant to the discipline (see Sullivan 2009). The book she published in 1946, *The Chrysanthemum and the Sword* (hereafter *Chrysanthemum*), demonstrates the profound impact of biographical inquiry on Benedict's anthropology.

Like "Adventures in Womanhood," *Chrysanthemum* constitutes a writer's response to war and in doing so tests the efficacy of *writing*. Thirty years after the drafts of her biographical essays, the anthropologist applied similar techniques to the study of a nation the United States had just defeated. The famous first sentence, with its characterization of Japan as the most alien enemy the United States ever had, introduces the equally well-known series of "but also's" that vividly evoke the distinct character of the culture, at once studiously reserved and scrupulously polite, daunting in battle and delicate in its aesthetics.

The subsequent twelve chapters extend the characterization through a series of explicit and implicit comparisons with American culture. The persistent alternation between "us" and "them" pulls the reader from one pole of the strange to another, from recognition to alienation and back. This sort of comparison perpetuates Benedict's rhetorical habit of exchanging the "wildly exotic" with the "all too familiar" (Geertz 1988, 106). In the 1946 book, however, the exotic and the familiar change place rapidly and repetitively, disturbing the reader's impulse to identify with certain customs and to reject others. The anthropologist works not only to teach her readers that "we" are as alien and bizarre as "they" but also to prove the logic of

Japanese culture. Submerging readers in "the intensely human commonplaces" of daily living, Benedict offers them the vicarious experience that precipitates a deeply felt appraisal of familiar values and customs (Benedict 1946, 11).

Repetition is overwhelmingly Benedict's mode of interpretation in *Chrysanthemum*. She presents an abundance of incidents, quotations, and observations that illuminate the principles of Japanese life, building an image of enduring coherence and integrity. Like Arendt in *Rahel Varnhagen*, Benedict holds back from probing behind the image offered by her subject, letting the facts speak for themselves—but not without plan: like a casuist, Benedict piles example on example, closing the case through the reiteration of detail. Carefully selected and arranged moments build a picture of Japan that appears self-evident, tautological, and unimpeachable.

The very persuasiveness of the portrait has led critics to condemn *Chrysanthemum* as a stereotypical, superficial, and simplistic rendering (see Kent 1996a). Benedict's artful compilation of anecdote, observation, and testimony obscures the method in her study, and suggestions that she knew nothing of Japan are fueled by the fact that she could not make a field trip during the war. Those who embrace this point of view fail to recognize the innovative aspects of Benedict's method, in which the minutiae of daily life, reiterated, compose a portrait that slights neither the complexities of history nor the intricate sources of cultural coherence (see Tannenbaum 2009).

Despite criticism from non-Japanese as well as Japanese scholars, *Chrysanthemum* endures. Widely read in Japan and in the rest of the world, the book played a crucial part in a postwar peace and in breaking down suspicion of a nation once perceived as alien (see Fukio 2004). *Chrysanthemum* also contributed to the postwar assessment of anthropology. Recognizing the value of the book for Benedict's agenda for anthropologists brings me back to the biographical essays. Unlike fiction, biography has received little attention in assessments of anthropological writing.

Benedict wrote *Chrysanthemum* in approximately three months, a remarkably short time. She had gathered the data for reports on Japan she prepared for the American government (see Tannenbaum 2009). The difference between the reports and the published book is telling, in both senses of that word. Delivered in a straightforward, prescriptive manner, the policy-driven discussions of the motives and principles of Japanese culture do anything but captivate a reader. The extraction of themes from twenty films, for instance, or a memo on Japanese morale for psychiatrists present blueprints for action and not templates for understanding. In moving from reports to a study geared toward a wide audience, Benedict

returned to a mode of writing that absorbed rather than directed the reader. Immersion or, in her phrase, *vicarious experience* became the mode of convincing readers that Japanese customs are as natural as their own. Thirty years after struggling with five drafts of an essay, Benedict leapt the traces in 1946, the analogy of a shying horse no longer apt. In lucid and assertive prose, Benedict presented to the world the distinct character of a nation that for centuries had closed its face to the world.

In *Chrysanthemum*, Benedict shunned the scholarly paraphernalia of an explicit methodology and limited her footnotes and citations. Throughout the book, she accumulates examples without attribution. A quotation from a European traveler follows a passage from a children's story, a diary entry is partnered with an imperial decree, a moral emerging from the juxtaposition.[4] Lack of attribution or reference for a conclusion can deceive the reader: sources there were aplenty, as Pauline Kent shows in her scrupulous documentation of the material that gave rise to the final portrait.[5] An ancient tale, an Imperial Rescript a scrap of conversation, and a strictly pruned flower combine to reveal the long history and intense purpose of a culture.

Benedict achieved the goal that eluded her in the biographical essays. During the intervening years she had learned the art of composing stubborn facts into a figure whose features mesmerize, whose motivating energy is apparent, and whose fateful trajectory prompts empathy. Her attempt in the biographical essays to convey lived experience culminated in *Chrysanthemum*, where the writing emphasizes the action (*vita activa*) of individuals-in-culture that constitutes the human condition (Arendt 1958). In *Chrysanthemum*, *shame* provides the key to the relationship between individual and culture.

The first nine chapters of the book describe the duties and obligations, loyalties and sanctions, gestures of respect, and intermittent outbursts that constitute the *vita activa* in Japan. These behaviors, carefully documented, eventually coalesce under the concept of *shame*. Familiar and colloquial, the concept summarized the culture in one striking reference, providing a lasting tag to the patterns of Japanese culture. Not what Benedict intended, the aptness of *shame* for condensing complex details turned the concept into a descriptor. Like the "Apollonian" that brands the Southwest Pueblos, *shame* sticks to Japan. As Benedict used it, the concept is not adjectival nor does it refer only to the personality of the culture. In *Chrysanthemum*, *shame* refers to the driving energy that maintained Japan through the ages (see Modell 1999).

In the winter of 1941, Benedict delivered a series of lectures at Bryn Mawr College. Published in 1970 in the *American Anthropologist* as

"Synergy: Some Notes of Ruth Benedict," the lectures set parameters for the wartime study of Japan. In them, Benedict attempted to deal equally with the nature of human dignity and the conditions for freedom in any culture (see Young 2005). "We need to ask whether or not these social restraints are such that they add or take away from the individual's ability to conduct his life as he desires," she told her audience (Benedict 1970, 322). She borrowed the word "synergy" from medicine and religion to develop a theory of self and society that distinguished cultures on the basis of the opportunities offered for conducting life creatively. In *high-synergy* cultures, all elements combine to create conditions for the exercise of individual capacities. "The inmost nature of the reality is congenial to powers which you possess," wrote William James, anticipating Benedict's application of *synergy* to cultures (Gunn 2000).

Japanese culture had lasted for centuries in a tightly integrated configuration, a seemingly perfect example of *high synergy*. In *Chrysanthemum*, Benedict delved into the sources and the implications of this synergy and selected *shame* to pinpoint the character of the configuration and the consequence for individual living. More explicitly than the descriptive concepts in *Patterns of Culture*, *shame* links the motivating energy of social arrangements to the behaviors and the temperament of individuals. *Shame* elucidates the behavior of the Japanese, from emperor to schoolboy. At the same time, the concept of *shame* crosses cultural boundaries, a feeling as familiar to non-Japanese as to Japanese readers. *Shame* is a human concept, and its use underlines the humanism of her Japanese study, elaborated in her farewell speech to the American Anthropological Association.

"But their extreme statements nevertheless point out correctly where the emphasis falls in Japan. It falls on the importance of shame rather than on the importance of guilt," she wrote in chapter 10, "The Dilemma of Virtue." She continued, "True shame cultures rely on external sanctions for good behavior, not, as true guilt cultures do, on an internalized conviction of sin" (Benedict 1946, 222, 223). Here the Benedictian gesture of using a contrast to make a point obscures the evolution of the concept in her book. These sentences do not appear until page 222 of a 300-page book. *Shame* comes front stage late in the account, following the portrait of a remarkably consistent and well-integrated culture. Benedict does not begin with the notion of *shame*; she does not treat the concept as a framework (or lens) through which to read Japanese culture. Not a priori, *shame* emerges, appropriately examined in the next-to-last chapter of the book.

In *Chrysanthemum*, Benedict attends to the processes through which shame drives Japanese culture. She had come a distance from *Patterns of Culture*, and her development reflected conversations with Mead and

Gregory Bateson about the formation of culture-character (see Sullivan 2009). "The Dilemma of Virtue" also reflects her absorption in social psychology and wartime literature on socialization (Tannenbaum 2009). Benedict details the way in which *shame* is inculcated in the child through sanctions and norms. A misstep is punished, a moral tale repeated: the chapter describes parental training techniques and not the inner recesses of the Japanese psyche. Benedict resists the temptation to probe behind the scenes or to expose the hidden "channels of the soul" (Woolf 1927, 150) that no outsider can be privy to, whether biographer or anthropologist. Examples serve as explanation, and observed behaviors manifest the mechanisms that institute *shame* as the relationship between self and society in Japan.

*Chrysanthemum* attached *shame* to Japanese culture for decades to come. The reasons are as political as they are anthropological, for Benedict, like Mead, wrote to change policy and not just to contribute to a discipline (see Tiffany 2009). With equal commitment to anthropology's obligation, Benedict intended to transform the stance readers took in the world. Unlike Mead, she depended less on the contrast between cultures than on the exchange of cultural possibilities within. *Shame* effectively characterized Japanese culture because the emotion resonated closely with an interpretation of American culture. The affiliation between shame and guilt in *Chrysanthemum* facilitated the acceptance of *shame* as a guidepost for General Douglas MacArthur and the American army responsible for negotiating Japan's surrender.

Under the challenge of administering a conquered nation, American officers and officials seized on *shame* as a clue to baffling and incomprehensible behaviors. The emotion explained the excruciating sensitivity to the eyes of the world, the fierce humiliation when teased, and the scrupulous politeness in every gesture of a Japanese person. An effective occupation policy would do well to attend to even the slightest manifestation of *shame*: "In the reconstruction of Japan those leaders who have their country's future at heart could do well to pay particular attention to hazing and the custom of making boys do silly stunts in the post-adolescent schools and in the Army" (Benedict 1946, 278). The threat was great, *Chrysanthemum* warned, for *shame* determined the Japanese capacity to act aggressively or to collapse into lethargy and depression. An invasion of neighboring nations or a suicide equally demonstrated the power of shame. In the world of defeat, she told her readers, "People take the shame as seriously as ever, but it more and more often paralyzes their energies instead of starting a fight" (Benedict 1946, 164). Instances of shame, she added, are rightly avoided by the American army: "American administration of Japan under

General MacArthur has accepted this Japanese ability to sail a new course. It has not impeded that course by insisting on using techniques of humiliation" (Benedict 1946, 306).

Clifford Geertz (1988, 108) described Benedict's works as edificatory ethnography. Benedict certainly meant to instruct readers, and Geertz had the style of instruction right: edificatory. Her rhetoric is one of enlightenment, of opening the eyes of readers and altering their awareness. Readers included the Japanese, who, as Shannon (2004) notes, Benedict molded into anthropologists, along with her countrymen. Even the accounts of American administrative policy occur in examples rather than in prescriptive or hortatory prose: "It was the Emperor who called first upon General MacArthur, not MacArthur upon him, and this was an object lesson to the Japanese the force of which it is hard for Westerners to appreciate" (Benedict 1946, 309). "Edificatory ethnography" also points to a relationship that distinguished Benedict from Mead.

Benedict's anthropology depended on the achievement of empathy between reader and subject. She delineated her lessons with more authorial modesty than Mead but with the same purpose. While Mead confronted her readers with striking comparisons, Benedict drew her readers through oscillating contrasts to a conclusion. In Mead's (1928) *Coming of Age in Samoa*, the appeal of the "other" is unmistakable (see Tiffany 2009). The paradoxes and "but also's" in *Chrysanthemum* oblige readers to find their own terms for evaluating cultural styles.

At the end of World War II, as at the end of World War I, Benedict wrote for a world witnessing the "wholesale reversal of conditions" and a climactic upheaval in international politics (RFBP). By 1946, she had achieved confidence in the efficacy of writing for influencing the course of events. The impulse to instruct did not vanish, although the focus did. Her early examination of three women who resisted the constraints of the time evolved as an inquiry into the conditions under which any individual might thrive. Benedict had not forgotten the human costs of suppression, nor did she relinquish the idea of individual freedom as the only lesson worth teaching (see Young 2005). She wrote the lesson in sharply drawn cases, an accumulation of details that delineated the conditions under which a person is able to live a full, productive, and creative life, stretching individual capacities to the limit. The synergy lectures, combined with the study of Japan, pushed Benedict in the direction of a robust anthropological humanism, to give the discipline a new method and a new purpose. That was the brunt of her farewell speech to the professional association when she stepped down from its presidency in 1947.

## Anthropology and Humanism: A Conclusion

Benedict began her speech to the American Anthropological Association in a familiar way. Anthropology, she informed her audience, has no connection with the humanities. Just as the designation of "most alien enemy" presaged a narrative in which alien becomes familiar, the assertion of no humanities presages a speech in which humanities becomes the heart and soul of anthropology. She catches her audience off guard by drawing them into one interpretation—anthropology is a science, a social science—only to move them into an entirely different interpretation: anthropology shares subject matter, methods, and goals with philosophy, literature, and literary criticism. She asserted,

> To my mind the very nature of the problems posed and discussed in the humanities is closer, chapter by chapter, to those in anthropology than are the investigations carried on in most of the social sciences. (Mead 1959, 460)

Despite putting man at the center of studies of society, she continued, anthropologists exclude "human emotion, ethics, rational insight and purpose" from their works (Mead 1959, 461). In anthropological writings, too often man is simply a mechanical cog in the social system. To move the person beyond this spiritless position as a research object, Benedict told her audience, anthropologists must follow the great humanists.

After the war, Benedict returned to her position at Columbia, where she threw herself into the U.S. Navy–funded Cultures at a Distance Project. She retained her interest in *shame* cultures, influencing the approaches of that project, but it is her references to George Santayana and to Shakespeare in the 1947 presidential farewell speech that represent her vision of the future of anthropology. Citing Santayana as an exemplary philosopher, she illuminated the kind of anthropology she initiated in *Chrysanthemum*. In the speech, she quoted from the philosopher's *Platonism and the Spiritual Life*: "This natural harmony between the spirit and its conditions is the only actual one; it is the source of every idea and the sole justification of any hope" (quoted in Mead 1959, 466). The language echoes phrases from thirty years earlier, when Benedict read William James on the flourishing of human capacities under "congenial" conditions and admired Mary Wollstonecraft for refusing to submit to hostile conditions.

Japan stretched her capacities, and the book is an experiment whose geometry is incomplete. *Chrysanthemum* is an experiment in understanding the ramifications for individuals of a culture whose motivating purpose

led to defeat in war and an ashamed apathy in its population. The account of patterns of Japanese culture is not, however, pessimistic. Influenced by American pragmatism, Benedict placed hope in the exercise of human creativity. Her 1947 speech intensifies the humanism of *Chrysanthemum*, a study that acknowledged the capacities of individuals to rebalance a pattern that events had tragically unbalanced. Extending the moral of the study of Japanese patterns of culture, Benedict's speech affirms the enduring possibility of reversing defeat.

The retiring president proposed a new method for anthropology. Rejecting the heavy social science looming on the horizon at the end of the 1940s, Benedict recommended the great Shakespearean critics as models of method. Critical immersion in texts, she suggested, provides the paradigm for the interpretation of fieldwork data. The anthropologist will succeed only "if he tries to understand the interrelations of discrete bits; if he surrenders himself to his data and uses all the insights of which he is capable" (Mead 1959, 468). Resonant with her phrase *empirical philosophy*, the method Benedict proposed in 1947 gave facts full weight as clues to character. The final element of her proposed method also recalled her first writings, in an emphasis on rhetoric that draws a reader into vicarious experiencing—the only basis for understanding.

The final step is *writing*, the composition of discrete bits into a persuasive portrait. For Benedict, as for Mead, method ended not with inquiry but rather with presentation. In the 1947 speech, Benedict exhorted her colleagues to trust the efficacy of writing for fulfilling the goal of the discipline: altering the perspectives of its audience, from statesman to common man. For Benedict, the achievement of empathy in writing meant the absorption of method into portrait, not the stark presentation of modes of inquiry that postwar anthropology displayed. While she condemned the growing influence of social scientific writing, she did not turn to the novelistic as solution. With biography and philosophy framing her approach, Benedict held to the importance of the ticket stubs, imperial rescripts, and schoolboy tales that compose a culture. Like Mead, she did not fear the art in anthropology. Like Mead, too, she meshed art with the scrupulous observation of details that yield insight into an "alien" life and give that life purpose.

Benedict's farewell speech to the American Anthropological Association communicated a practiced understanding of the discipline. Her emphasis on art, literary criticism, and philosophy pointed to the central role of human beings in anthropology and to her conviction that humanism was the only appropriate framework for an examination of diverse cultures.

Had Benedict lived to follow her own advice, she might well have returned to the study of individual lives as a strategy for inserting human emotion and ethics into anthropology. In the farewell speech, she spoke ardently of the necessity of reconsidering the value of life histories in anthropology:

> The unique value of life histories lies in the fraction of the material which shows what repercussions the experiences of a man's life—either shared or idiosyncratic—have upon him as a human being molded in that environment. (Mead 1959, 469)

Her notion of writing lives had changed, from presenting the particulars of an individual experience to the more general proposition of discovering the conditions under which any self achieves fulfillment. Life histories served an exemplary function in anthropological writing, illustrating possibilities for the *vita activa*. The trajectories of lives exhibited the potential for creativity, for reversing defeat, and for combining the dynamics of social cohesion with the opportunities for individual freedom (see Young 2005). By 1947, Benedict had a clear sense of the purpose of anthropology: to detect and render persuasive the terms under which human beings can achieve harmony between the demands of "individuality" and the environments of "living."

Had she lived, would her writings have also become more hortatory than edificatory? Geertz compares Benedict with the great and furious social critic Jonathan Swift, in the "relentlessness" and "severity" of her prose (Geertz 1988, 105). I argue that it is less her prose style that unites her with Swift than her conviction that writing could alter social conditions by changing the minds of readers. Where Swift is fierce and sarcastic, Benedict wields a subtler and, paradoxically, a more direct weapon in the attack. As she matured in the discipline, Benedict increasingly left parable behind and rested her case on the arrangement of empirical evidence. She seized her readers' attention not with extravagant parody but with straightforward examples and resolute prose. She took on her readers, too, with faith that she could draw them through the "living" she presented into a new perspective on the conditions that formed their own worlds.

Had Benedict lived, her movement into a new anthropology would likely have followed the course she initiated in *Chrysanthemum* and outlined in the speech to her professional association. Too seeped in the significance of facts to relinquish those building blocks to revelation, she would have continued her quest to ground a humanistic anthropology in "reality." She would have continued to depict the lives of diverse others in terms of

the distinct purposes each embraced, trusting that to be the guarantee of tolerance and harmony on a global level. She might have addressed the world as passionately as she addressed the anthropological association and her American readers, but she would have communicated the lesson through *verstehen* and the artful interpretation of data, not by exhorting her audience to agree.

Benedict's notion of humanism drew on a lifetime of inquiry that consistently kept human capacity, fulfillment, and creativity at the center of studies of society and culture. "Humanism" was not abstract or theoretical but a concept that directly addressed the betterment of human lives. The anthropology that Benedict advocated in 1947 remains of critical relevance today, when a Manichaean worldview reigns and understanding is a limited resource.

## NOTES

I presented the first version of this article at the annual meetings of the American Anthropological Association in November 2003 in a session on biography. Comments during that presentation suggested important ways of expanding the ideas in the paper. I delivered a revised version at the ASAO meetings in February 2004 and, acknowledging responses there, drafted a fuller version for the ASAO meetings in February 2005. Comments and criticisms along the way have made the article stronger and better, and I thank all those who participated in the several panels. In particular, I thank Albrecht Funk, Susan Gray, Gerald Sullivan, and Sharon W. Tiffany for cogent critical remarks. Finally, I thank once again the Special Collections of the Vassar College Library for permission to use material from the Ruth Fulton Benedict Papers.

1. Benedict sent a draft of the first essay, "Our Contemporary: Mary Wollstonecraft," to Houghton Mifflin along with a letter describing the projected book. Her papers contain abundant notes on Margaret Fuller. The only hint about Olive Schreiner occurs in the letter of inquiry to the publisher, when she wrote of the need to select "a still living woman."

2. Inasmuch as Benedict did not cite titles in her reading notes, an exact attribution to James is difficult. Much of his work, however, evinces this confidence in man's spirit, will to believe, and capacity to alter his setting.

3. Caffrey makes the connection in another way as well, by applying the concepts "Dionysian" and "Apollonian" to Benedict's descriptions of Wollstonecraft and Fuller (Caffrey 1989, 90).

4. Even Geertz (1988), restoring *Chrysanthemum* to its proper place in the canon, did not discuss her sources; throughout the chapter, he sidestepped questions of how Benedict knew what she knew about Japanese culture.

5. There are seven, closely spaced pages of sources (Kent 1996b).

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# RUTH BENEDICT AND THE STUDY OF THAI CULTURE

Nicola B. Tannenbaum
*Lehigh University*

In this paper, I place *Thai Culture and Behavior* in the context of Benedict's work and life, American anthropology, and Thai studies. The paper begins with a summary of Benedict's study and a critique of her Thai ethnography. This is followed with a discussion of its initial reception when it was first published and its later place in biographies about Benedict. Finally, I turn to my initial question of the role of *Thai Culture and Behavior* in Thai studies. Here I examine the two subsequent anthropological studies of Thailand: Embree's seminal essay, "Thailand—A Loosely Structured Society," the first field-workbased study of central Thailand, and the Cornell-Bennington Bang Chan Project. I then discuss the place of Benedict's work in more recent anthropological analyses of Thailand.

## Ruth Benedict and the Study of Thai Culture

Ruth Benedict's best-known study of a culture from afar is her *Chrysanthemum and the Sword*, first published in 1946. A classic in the field, it remains in print and continues to be cited as an important contribution to the study of Japan (Schachter 2009; Kent 1996; Ryang 2002). Much less well known is Benedict's *Thai Culture and Behavior: An Unpublished War-time Study Dated September, 1943*. The Institute for Intercultural Studies in New York first published this in 1946 in mimeograph form. In 1952, the Cornell University Southeast Asia program published *Thai Culture and Behavior* as its fourth data paper and reprinted the paper in 1955. While *Thai Culture and Behavior* is no longer widely cited, it marks the beginning of anthropological studies of Thailand.

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In this paper, I place *Thai Culture and Behavior* in the context of writings about Benedict's work and life, American anthropology, and Thai studies. The paper begins with a summary of Benedict's study and a critique of her Thai ethnography. This is followed with a discussion of its initial reception when it was first published and its later place in biographies about Benedict. Finally, I turn to my initial question of the role of *Thai Culture and Behavior* in Thai studies. Here I examine the two subsequent anthropological studies of Thailand: Embree's seminal essay, "Thailand—A Loosely Structured Society," the first fieldwork-based study of central Thailand, and the Cornell-Bennington Bang Chan Project. I then discuss the place of Benedict's work in more recent anthropological analyses of Thailand.

## Thai Culture and Behavior

The Thai government declared war on the Allies on January 25, 1942. The Thai government allowed the Japanese forces free access to the country, thereby facilitating their assault on Burma. At that time, the government of the United States knew little about Thailand; consequently, it became a nation worth analyzing at the Office of War Information (OWI). While much of the OWI's work focused on supporting the war effort within the United States, there was a section on overseas intelligence staffed with anthropologists working in various government offices (Mead 1959: 351–54). Benedict was Head of the Basic Analysis Section, replacing anthropologist Geoffrey Gorer in the Bureau of Overseas Intelligence (Caffrey 1989, 318; Mead 1959, 352). It was here that Benedict finished Gorer's report on the Burmese, her study of Thailand ([1946b] 1952), and her work on Japan ([1946a] 1989), among others (Caffrey 1989: 268–71).

Benedict developed her own style of analysis for studying cultures at a distance, which integrated published materials with interviews.[1] Margaret Mead described Benedict's approach:

> Her long experience working with students, laboriously going over and over a student's unorganized notes and half-comprehended impressions, had giver her a basic technique for getting at cultural data through the medium of a second person. Her training in English literature and her intensive reading gave her a disciplined and highly sophisticated approach to published materials. And her penchant for building up a picture from fragmentary data came into play in a new way in bringing these very diverse and uneven source materials together in a significant relationship. (Mead 1974, 59)

Mead's analysis fits well with Benedict's own description of the materials and methods she used in *Thai Culture and Behavior*. The paper is relatively short, being forty-five single-spaced typescript pages in the 1952 Cornell Data paper format.[2] In addition to the foreword, it is divided into two parts and five chapters. Part 1 includes three chapters: the first, on traditional background (thirteen pages); the second, a brief chapter on religion with a section on animism (six pages); and the third, on adult life (six pages). Part 2 consists of two chapters: one on the child (eight pages) and the fifth and final chapter, "Some Thai Characteristics," which includes sections on the enjoyment of life, the cool heart, male dominance, and a summary for a total of eleven pages.

In "Traditional Background," Benedict synthesized and summarized her various sources to provide an overview of Thailand at the time. There is a brief introduction, placing Thailand and its people in historic context and delimiting the subject matter—"this study deals with these people of the [irrigated] rice lands, the Lao-Thai, living in North, Central, and East Thailand" (Benedict [1946b] 1952, 2)—and a summary of Thai history. This is followed by sections on the absolute monarch, the administrative bureaucracy, and villages. Benedict noted two aspects of Thai governance that made it different from both India and China: the Thai *sakdina* system of rights to land and its product based on rank, which differentiates it from the Chinese administrative system, and the rule of declining rank. Increasing generational distance from the king meant a decline in inherited rank, until, at five generations removed, the person became a commoner. This rule distinguished Thai aristocracy from such hereditary systems as caste in India. Benedict balanced her information on the rulers with an account of villagers and villages and their semiautonomous existence, with leaders appointed from within the community, their involvement in local markets, and their relative security with only warfare interfering with community survival. Thai communities were semiautonomous since villagers owed corvée service to lords for up to four months a year. In the final section, Benedict discussed European contact and modernization. This section included a summary of recent history, trade with the West, and use of modern medicine and ended with an account of Thailand's declaration of war against the Allies.

In the second chapter, Benedict discussed Thai religion. Here she identified Thai Buddhism, belonging to the Southern or Hinayana form, as the state religion. Official ceremonies are Brahmanical, but "culturally the two [Brahmanism and Buddhism] are welded into what appears to all Thai who are not historians a consistent and homogenous whole" (Benedict [1946b] 1952, 2:14). She provided a brief statistical summary of the number

of temples, monks, and temple residents. Ordination as a monk is not a lifetime commitment but rather more a *rite of passage* for men, who rarely stay in the order for life. Benedict briefly described an ordination festival and other Brahmanical festivals, such as the King's first plowing ceremony. She separated her discussion of animism from that of the Brahmanical/Buddhist practices. Animist practices are directed toward spirits of the dead, "*phi*," which is a rather narrow view of spirits in Thai life (see below).

The third and final chapter in this section concerned adult life. After a brief description of a Thai household and its members, Benedict discussed what constitutes adulthood: for men, it is ordination as a monk; for women, it is giving birth to a child and her postpartum lying by the fire. Marriage usually follows after a man spends a period of time as a monk, and Benedict provided a description of an upper-class wedding drawing on Chandruang's (1938) account. However, she recognized the differences in life goals for the upper class and the peasantry. Princes and civil servants wanted to improve their status positions and were dependent on royal favors that could be withdrawn; thus, they suffered from status anxiety. Peasants, whose social positions rarely changed, did not. Rather, they could enjoy recognition in their communities, based on their reputations as good farmers, clever poets, singers in village competitions, and knowledgeable men. Men flew kites competitively and enjoyed gambling:

> The Thai certainly do not conceive of life as a round of duties and responsibilities. They accept work and make it as gay as possible; when it is done they are free to take their leisure. They have no cultural inventions of self-castigation and many of self-indulgence and merriment. (Benedict [1946b] 1952, 24)

This description provided the background for Benedict's second, more analytical section, where she discussed the child and what she described as "Some Thai Characteristics." The discussion of childhood focused on early independence training and the general lack of gender distinctions in child-rearing practices. Benedict emphasized the importance of early childhood experiences for the formation of adult character. Thus, she reported information on child care and nursing and how both parents and older siblings enjoy the child. Children are carried straddled on the hip, and this position "never admits of the baby's passive relaxation to every movement of its carrier's body, as shawl carrying" (Benedict [1946b] 1952, 27). Nursing is on demand, and this "lays the basis for his life-time lack of food anxiety" (Benedict [1946b] 1952, 28).[3] Children's names are not sex specific, and there is little gender differentiation in child care. Social distinctions are

based on age and generation rather than gender, and this generational chasm is "unbridgeable, even in fantasy" (Benedict [1946b] 1952, 29). I interpret Benedict's words as an indirect reference to the Oedipus complex and as a critique of the universalization of psychoanalytic concepts.

Benedict saw a connection between childhood independence training and Thai Buddhist practices. Thailand is often characterized as a Buddhist nation, and Thai behavior is explained in Buddhist terms.[4] However, Benedict does not make this claim on abstract doctrinal grounds, but rather it follows from "their [the Thai's] selection among Buddhist teachings . . . that what a person is depends on himself alone" (Benedict [1946b] 1952, 28). This is a sophisticated recognition that Thai Buddhism is localized; particular aspects of Buddhist practices that fit with Thai culture are selected and emphasized.

The difference between hierarchical but caring relationships within the household and the more egalitarian but insecure relationships outside the household is made clear around three or four years of age, when a child starts playing with peers. These relationships are fun but unreliable. Benedict used the Thai term to characterize these as "play friends," *len puen*, and contrasts them with "die friend," friends who would die for you. These latter are rare, while most friends are those who will eat with you—if you are paying—and desert you if it is in their interest to do so. Since these behaviors are learned at an early age in play groups, there is little hostility when these behaviors emerge:

> Everybody knows the rules and what to expect, and the dolt—the one who got cheated—always draws a laugh. That is, laughter is directed toward the cheated, not condemnation—even moral condemnation—toward the cheater. The later has a "cool heart," sangfroid, which is one of the most admired Thai assets. (Benedict [1946b] 1952, 30)

In the final chapter, Benedict drew on the grounding of Thai behavior in childhood and child-rearing practices to discuss "Some Thai Characteristics," as she titled her last chapter. Benedict's choice of topics in this chapter acknowledged that her study was limited by the materials at hand and that informant interviews could not provide complete information. She focused on three aspects of Thai character: the enjoyment of life, the cool heart, and male dominance.

Thai enjoy life: social interaction, festivals, and religious ceremonies are all to be enjoyed. Anger is discouraged since it is seen as the prime disturber of the good life. However, according to Benedict, "this didactic counseling against anger goes with and not against the Thai grain, Siamese

by nature, are a quiet people" (Benedict [1946b] 1952, 37). They tend to be nonviolent, even when drinking (Benedict [1946b] 1952: 37–38).

While Benedict had earlier described the localization of Buddhist practices to fit Thai character in a neutral tone, here she stressed how Thai practices contradict the canonical forms of Buddhism:

> Central in Gautama's [Buddha's] teaching was the doctrine that sorrow attends existence and that only from the extinction of desire can come cessation of sorrow. But the Thai have an indestructible conviction that existence is good. (Benedict [1946b] 1952, 34)

Further, "Like the Four Noble Truths about suffering and the extinction of desire, the Five Great Commandments of the Buddha have been culturally interpreted" (Benedict [1946b] 1952, 34).[5] The first precept is to not destroy life. Benedict commented that "the Thai have had to exercise their facility in rationalizing this unpleasant rule for they have remained a nation of fishermen eating fish daily" (Benedict [1946b] 1952: 34–35). Similarly, concerning the second precept to not take what does not belong to you, Benedict noted that "there is plenty of theft in Thailand" (Benedict [1946b] 1952, 35). Rather than emphasizing the precepts, Thai highlight merit making. Merit making does not mean doing good in general or getting along with one's neighbors:

> [T]hose merit-making acts which feature in everyone's calculations are, rather, giving food to the monks each morning, being a monk, plastering a few square inches of gold leaf on a Buddha . . . and innumerable other observances. (Benedict [1946b] 1952, 36)

This enjoyment of life is complemented by the idea of the cool heart, which means not being overly concerned about responsibility or trouble. Benedict illustrated this coolness with stories from Reginald LeMay's (1930) collection of folktales, where the morals are often expressed as it is better to go along with what other people believe since "even if you speak the truth, no advantage will come of it either to the speaker or the listeners if you are speaking against their convictions" (Benedict [1946b] 1952, 34). Having a cool heart is to go along with this situation and/or to use it to your advantage or to help people. Benedict summed up her discussion as follows:

> In situations where hierarchical status is well established, the Thai have clear and unresented patterns of behavior; where they are not, the virtues of the "cool heart" are the code provided. One is

> "cool," too, in hierarchical relations—using flattery, allowing the superior to win the game, etc. When not placed in a clear hierarchical position—one lives by one's wits and counts it as virtue to be as inventive as possible. (Benedict [1946b] 1952, 40)

Finally, Benedict discussed gender relationships in Thailand. She concluded that men are dominant, based on her analyses of folktales, proverbs, didactic stories, and games:

> The man's attitude toward the relations of the sexes is given symbolic elaboration in the national game of kite flying—which is played exclusively by men. . . . It is a "courtship" of a female kite and a male kite. The female kite is a four-sided diamond shape and goes up with a lilting motion . . . the man who flies the female kite stays in one part of the field. And his kite is not allowed to cruise. Presently another kite-flyer from another end of the field sends up his male kite. This is a much heavier kite, perhaps six times as big, in the shape of a five pointed star. It ascends higher than the female kite and cruises towards the female to "capture" it. . . . The game well symbolizes the relation of men and women. Men are not doubtful of their masculinity—which is here symbolized in the kite's size, shape, and activity . . . the object of the game is to keep a "wife" within their orbit and both male and female "flying" . . . but attacking her too closely—perhaps it would be fair to say dominating her, or possessing her, in the European sense—would mean, in the kite game, falling to the ground and being defeated. (Benedict [1946b] 1952, 43)

Benedict concluded that

> [the] psychic security which makes possible Thai cheerfulness, easy conviviality, and non-violence is grounded in a long and remarkable permissive infancy during which no disciplines are imposed either in feeding or sleep routines or in toilet training, and no attention at all paid to infantile erections or to the child's playing with his genitals. (Benedict [1946b] 1952, 44)

Mead (1974) suggested in her biography that Benedict was not interested in psychoanalytical approaches to explaining cultural behaviors. Yet Benedict's emphasis on early childhood experiences, especially on feeding

and toilet training, placed her Thai analysis firmly in psychological approaches, in which early childhood experiences shape the psychological and, hence, behavioral aspects of cultures. Indeed, Benedict's emphasis on nursing and toilet training paralleled Mead's ([1935] 1963: 40–60) discussion in *Sex and Temperament in Three Primitive Societies* (see Sullivan 2009).

## Benedict's Ethnography and Analysis

I have quoted extensively from Benedict because her own words illustrate the material she used and the analysis she made of those materials. Drawing on folklore and games, Benedict took the themes she had discovered and expanded on them to develop conclusions about cultural patterns of behavior.

Benedict relied on previously published works. Acknowledging E. Young's *The Kingdom of the Yellow Robe* (1898), Graham's *Siam* (1924), LeMay's *Siamese Tales Old and New* (1930), Landon's *Siam in Transition* (1939), Virginia Thompson's *Thailand: The New Siam* (1939), and Chandruang's *My Boyhood in Siam* (1938), Benedict described the latter as "entirely different in character" from other works cited (Benedict [1946b] 1952, ii). The bibliography of *Thai Culture and Behavior* lists thirty-four sources. In addition, Benedict and her assistants interviewed Thai living in the United States.[6] These interviews provided information on child rearing and relationships between men and women (Benedict [1946b] 1952, iii).

Benedict's view of Thai society was limited by sources available to her, some of which have since been discredited, most notably Anna Leonowens's *The English Governess in the Siamese Court* (1874). Other texts were used uncritically, such as Earnest Young's (1898) translations of the texts used for the top-knot cutting ceremony to describe expectations and experiences of young children. One of Benedict's informants cautioned her about relying on Young's accounts since the informant found Young's discussion of kite flying inaccurate (Meesook 1943). Chandruang's (1938) autobiography is an interesting and useful account of growing up in Siam in the 1920s, during the latter part of the absolute monarchy.[7] The author's father was a provincial governor. Consequently, Chandruang's own experiences are of growing up in an elite setting and should not be generalized to the majority of the population. Nonetheless, these criticisms are after the fact, and Benedict used what was available.

The topic of animism constitutes the ethnographically weakest part of Benedict's discussion. While spirits are important, Benedict emphasized the centrality of ancestral spirits and spirits of the dead. Thai do not, in general, propitiate spirits of the dead or make offerings to ancestral spirits.

Most spirits of the dead are dangerous, especially those that result from accidental or violent deaths and deaths in childbirth.[8] The exception to this is the founder of a community who, after death, often becomes the guardian spirit of the community (Tannenbaum and Kammerer 2003). Rather than spirits of the dead, the important spirits are cadastral spirits, that is, spirit owners of house sites as well as those associated with fields and natural objects (L. Hanks 1972; Sharp and Hanks 1978; Tambiah 1970; Tannenbaum 1995; Textor 1960). Benedict's discussion of the spirits of the dead comes from an informant who was Sino-Thai; his account reflects Chinese rather than Thai cultural practices (Prathoomratha n.d.).[9]

Benedict's view also reflected the meager information about spirits in the literature. Earlier writers about Thailand did not have a clear understanding of the role of spirits and their relationship with Buddhist and other practices. Benedict's informants may have either downplayed the role of spirits and animist practices, fearing that they might be considered superstitious, or perhaps they practiced a reformed and rationalized Buddhism that did not include animist practices.

The analytical distinction between Buddhist and animist practices remains important and largely unquestioned in the anthropology of Thailand. Benedict's distinction is a reflection of the sources she used. The contrast between canonical Theravada Buddhist ideology and Thai behavior is relatively standard in earlier travelers' accounts; nonetheless, one could make similar negative comparisons between the Christian Ten Commandments and the behavior of Christians.

## Reception of *Thai Culture and Behavior*

Benedict's monograph did not receive much attention when it was first published, nor did it receive much discussion in historians' biographies of Benedict.

### *Early Reactions*

The Institute for Intercultural Studies mimeograph version of *Thai Culture and Behavior* apparently circulated within Benedict's anthropological circle. Alfred Kroeber (1948: 589–91) discussed it in his revised introductory text in the section on "empirical descriptions of national characters" in chapter 15 on cultural psychology. Kroeber compared and contrasted Gorer's (1943) study of Burmese personality with Benedict's Thai study.[10] Thai and Burmese cultures are similar in that women are in charge of the family budget and men are seen as more patient. However, they differ significantly in modal personalities: "the Burmese are relatively touchy, proud,

theatrical, and violent; the Siamese relaxed, amiable, easy in their dignity, and serene" (Kroeber 1948, 590).

Benedict had made a similar comparison in her personality and culture course that she taught in 1946–1947 (Young 2005: 236–39). Benedict saw basic similarities in cultural traits and aspects of culture that had diffused into both areas, but Burmese and Thai interpretations are totally different. For the Burmese, there is a high level of violence: men drink until insensible, and violence is associated with drinking; gambling is heavy and could result in violence; and men are insecure. For the Thai (Siamese), there is little violence: drinking makes men amiable, men gamble but not until they lose everything, and, unlike the Burmese, Thai men are secure and responsible (Young 2005: 236–37). Although she does not say so, Benedict also drew on Gorer's study of Burmese personality, which she is reported to have finished (Caffrey 1989, 269; Young 2005, 103).

Much more was known about Burma because of the British conquest and colonization of Burma. By contrast, little was known about Thailand or its culture, history, and social relations; because Thailand was never directly colonized, there were no colonial scholars. Benedict was somewhat cautious in her statements: "'in Siam there is a low rate of criminality, no record of fiestas culminating in violent brawls and no concern with criminality'" (quoted in Young 2005, 236). These contrasts between Thai and Burmese character no longer ring true as more information about Thailand has become available. Thailand, like Burma, had its history of rebellions at both elite and local levels—some of them in reaction to the centralization of power that paralleled those in colonial countries (Chatthip 1984; Keyes 1977; Tanabe 1984). And there is a similar high level of violence associated with drinking and festivals.

Benedict's discussion of male dominance was republished in *The Study of Culture at a Distance* (Mead and Metraux 1953; Benedict 1953b: 382–86). However, Benedict's discussion received little attention in reviews of the volume (Cahnman 1954; DuBois 1954; Sebeok 1954; Sirjarmaki 1954; Vidich 1954; Wallace 1954).

After Benedict's *Thai Culture and Behavior* was published as a Cornell Southeast Asia Program data paper in 1952, it was reviewed in the *Far Eastern Quarterly.* However, the reviewer simply quoted the preface to the data paper itself (P. H. C. 1954). In 1955–1956, the Human Area Relations Files coded *Thai Culture and Behavior* for inclusion in the Thai Culture File (AO1) in its fourth installment. In 2000, only the Central Thai files (AO7) had been converted to electronic form. Benedict's study was not included since its scope was broader than central Thailand, and the rest of the Thai files have yet to be converted.

## Thai Culture and Behavior in Benedict's Biographies

Benedict has been the subject of a number of biographies, starting with Mead's *An Anthropologist at Work* (1959) and, most recently, Virginia Young's *Ruth Benedict* (2005). Benedict's wartime studies have not received much attention, and, because Benedict lost interest in Thailand after World War II, her study of Thailand has received even less scholarly attention.

Mead gave it a brief sentence in her collection of Benedict's work, stating that after her work on Romania, Benedict worked on Thailand (Mead 1959, 353). Mead's (1974: 58–59) biography of Benedict also mentions that the study of Thailand came after her Romanian work. Here Mead discussed Benedict's methods for studying culture at a distance.

Margaret Caffrey's biography devotes a chapter to the war years and discusses Benedict's Thai study in this context. Research in the Office of War Information provided the opportunity for Benedict to show that anthropological analyses could have useful policy implications (Caffrey 1989, 318). The Thai study is described in some detail because it was the first that Benedict had completed; Caffrey (1989, 320) discusses Benedict's other unpublished studies in a single paragraph.

Caffrey states that the objective of Benedict's report was to provide "background material to plan a program of psychological warfare on the Thai as allies of the Japanese, and for reconstruction after the war" (Caffrey 1989, 319). Caffrey (1989, 319) then provides a two-paragraph summary of the work, arguing that Benedict had moved beyond characterizing cultures in single overarching patterns. Caffrey's interpretation fits with Mead's (1974, 59) comment about methods for describing "national character in complex, highly literate cultures," which suggests that Benedict and others working on national character recognized cultural variations. Alternatively, Benedict's ([1946b] 1952: 34–44) label of these themes as "some Thai characteristics" suggests that she did not have enough information to synthesize an overarching pattern.

Virginia Young (2005: 103–13) considers Benedict's wartime studies of Thailand, Romania, Holland, and Japan as a whole, analyzing them together for insights into Benedict's work. Young (2005, 105) argues, *contra* Caffrey, that Benedict continued to find a characteristic pattern, albeit a more complex one, that reflected the culture's history as well as class-based difference. Benedict's analysis of Thailand reflected the complexities of Thailand's history and the class differences.

Judith Modell's biography of Benedict has two rather admiring paragraphs about *Thai Culture and Behavior.* Modell suggests that the "result resembled her best anthropological writings" and that Benedict was "charmed by the Siamese much as she had been by the Zunis" (1984, 270).

Modell's comment that Benedict showed her "usual awe at a distinctive, unfamiliar culture—especially one that showed such remarkable consistency" (1984, 270), suggests agreement with Young's analyses of Benedict's work.

Finally, Benedict's discussion of gender in Thailand is briefly mentioned in Lois Banner's (2003: 410, 421–22) book on Mead's and Benedict's circle.

From these biographers' perspectives, *Thai Culture and Behavior* is not one of Benedict's major works but simply one of the studies she wrote for the Office of War Information. Young is the only historian to take Benedict's monograph seriously, showing how it fits with the rest of her wartime studies. There is little controversy about *Thai Culture and Behavior*, except whether this study preceded or followed Benedict's report on Romania (Caffrey 1989, 319, 393 n. 33; Mead 1974: 58–59; Young 2005, 193). Benedict did not pursue the study of Thailand after the war. Rather, she finished her analysis of the Japanese, published as *The Chrysanthemum and the Sword* (Benedict [1946a] 1989), and later turned her attention to Eastern Europe (Benedict 1949, 1953a).

## Ruth Benedict's Legacy in Thai Studies

Benedict's essay marked the beginning of formal anthropological studies of Thailand. In this section, I discuss its place in the early academic analyses of Thailand, focusing on two subsequent studies: Embree's (1950) "Thailand—A Loosely Structured Social System" and the Cornell-Bennington Bang Chan studies that began in 1952 (Keyes 1992, 2). I then turn to the place of Benedict's work in the current anthropology of Thailand.

### *Embree's Loose Structure*

Wanting to trace Benedict's influence on later academic writings about Thailand, I read John F. Embree's essay, which is second in the time line of academic writings about Thailand.[11] He cited Benedict's essay as "the only anthropological analysis of Thai cultural materials" and listed other more impressionistic sources, such as travelers' reports, missionary accounts, and publications by people associated with the Thai government (Embree 1950, 3 n. 1]). Embree characterized Thai society as "loosely structured," as compared to rural Japan, where he previously had done fieldwork. Some of the contrast is a consequence of the differences between rural fieldwork

in Japan and urban Bangkok immediately after World War III. This characterization of Thailand as a loosely structured society became the defining problem for Thai anthropology. What did "loose structure" mean? And how was it to be explained (Dieter-Evers 1969)?

There was considerable bibliographic overlap between Embree and Benedict. For sources about Thailand published before 1943—the date of Benedict's study—both authors shared 75 percent (eight of twelve citations). This is not surprising, given the relative dearth of published materials about Thailand. Embree made a larger comparative argument about how Thai social organization differed from that of Japan and the rest of Southeast Asia, and he drew on many topics to show that Thai social structure was loosely organized. In the process, Embree (1950) discussed lack of discipline (pp. 4–5), Thai individualism and freedom from family obligations (pp. 5–7), the "looseness" indicated by improvisational versus memorized poetry games (pp. 7–8), unreliability (p. 8), the cultural admiration for successful liars (pp. 8–9), minding one's own business (p. 9), the importance of fun (pp. 12–13), and the high value on the cool heart (p. 7). These topics and associated citations to the literature parallel Benedict's *Thai Culture and Behavior*, but Embree does not, in fact, cite Benedict beyond the introductory footnote.

It is possible to use Benedict's citations to lead to many of the quotes that Embree used to support his own argument: diplomacy—Benedict (p. 10), Embree (p. 8); lying and Reginald LeMay's (1930) folktales—Benedict (pp. 35, 39), Embree (pp. 8–9); improvisational poetry—Benedict (p. 22), Embree (p. 7); fun—Benedict (pp. 34–38), Embree (p. 12); and cool-heartedness—Benedict (pp. 38–40), Embree (p. 9), although in the latter, Embree quoted a different source, while Benedict relied on LeMay's (1930) folktales.

I am intrigued by the similarities in content and sources, and, while Embree does not cite Benedict's work for anything substantive, I find these parallels suggestive. The two works have different origins and intentions. Benedict's work was primarily descriptive, attempting to characterize Thai social life, primarily as an aid for the Allies so that they could better understand and predict how the Thai were likely to behave in a war situation. Embree, on the other hand, sought to explain the pattern of behavior he perceived. While Benedict looked for the explanation in child-rearing practices and the ways in which these shape character, Embree saw the connection between social organization and individual behavior. However, both authors were concerned with Thai character and the characterization of Thai outlooks on life rather than analyzing any particular cultural domain.

Benedict's wartime study and Embree's postwar essay provided the academic foundation for Thai studies. However, Embree's focus on loosely structured social systems rather than Benedict's interest in national character defined the central concern in post–World War II studies of Thailand. Embree's analysis is important now as part of the history of anthropology of Thailand; his theoretical argument is no longer taken seriously, and no one characterizes Thailand as a loosely structured society.

*Benedict and the Anthropology of Bang Chan*

Benedict's work was cited in early publications of the Cornell Bang Chan project and then fell into relative obscurity. The Cornell-Bennington Bang Chan project, established in 1952, was part of a larger comparative research program on the problems of change and modernization among tribal and peasant societies (Leighton 1952, 9).[12] The three senior researchers for Thailand were Lauriston Sharp, Lucien Hanks, and Jane Hanks. While Sharp did not publish much on Thailand (Sharp and Hanks 1978), Jane Hanks and Lucien Hanks published extensively on Bang Chan.[13] They were interested in both psychological anthropology and national character; Jane Hanks had studied with Ruth Benedict; Lucien Hanks, Jane Hanks's husband, trained as a psychologist.

The Bang Chan project began when Benedict's health was failing; Jane Hanks reported conversations with Benedict about the Bang Chan project, and, while they were aware of Benedict's wartime study, they did not rely on it (J. Hanks, pers. comm., January 26, 2005). While citations to Benedict's monograph appeared in some of the earlier works about Bang Chan, most were citations that acknowledged *Thai Culture and Behavior* but omitted substantive discussion of its contents or issues.

The most extensive references to Benedict's work in the literature on Bang Chan occurred in Herbert Phillips's (1966) *Thai Peasant Personality*. Analytically, he focused on Embree's loose structure argument; nonetheless, Phillips' topical concern with culture and personality made Benedict's work relevant. Because he was part of the Bang Chan project, Phillips had access to the observations and analyses of other researchers to provide the context and interpretative material; thus, he could focus on a single topic. And, unlike Benedict, he was able to do ethnographic fieldwork. Phillips cited her discussion of patterns of respect in describing Thai family life and social organization and later, briefly, in his section on observations of Thai behavior drawn for the literature on Thailand (Phillips 1966:33, 39–95). Phillips characterized Benedict's study as

> [w]ritten in the style of a 'national character' study, the essay is essentially a psychological analysis of semi-ethnographic materials;

> that is, Benedict attempted to identify and analyze whatever aspects of the psychological functioning of the Thai she could discover *reflected* in their social institutions and cultural products. (Phillips 1966, 48, emphasis in original)

Although Phillips rarely cited Benedict, his work is in many ways a systematic investigation and confirmation of the themes and characteristics that Benedict discussed. In his analysis of the naturalistic observation of Thai personality, Phillips (1966: 39–95) discussed, for example, the pleasures of social contact, the importance of social play and the place of fun (*sanuk*) within it, politeness as a social cosmetic, and the dynamics of loose structure. These topics parallel Benedict's discussions of politeness, enjoyment of life, and cool-heartedness. In the sentence completion test that Phillips developed from naturalistic observations, he addressed a constellation of issues concerning authority, dependency, relationships with others, and aggression (Phillips 1966: 143–99). In his analysis of the sentences relating to authority, Phillips (1966, 155) stated,

> The data clearly confirm the generally recognized willingness of villagers to respond positively and undefiantly to authority figures. Their response is accompanied by feelings of esteem, admiration, and often diffidence to authority figures. However, their behavioral and emotional responses to authority are not absolute: when the authority is wrong, they are most likely to ignore him. They do this, however, without in any way challenging the prerogatives of his authority or pointing to his error.

His findings recall Benedict's discussion of hierarchy and authority, summarized earlier in this paper.

The differences between Benedict's and Phillips's analyses lie in methodology and fieldwork as opposed to interviews and analyses of literature, but also in Phillip's self-conscious use of the sentence completion test to develop a more valid and reliable measure of personality. His work straddles the interest in culture and personality and the concern for an explicit methodology that could strengthen the scientific rigor of anthropological analyses.

### *Benedict after Bang Chan*

Ruth Benedict initiated discussion of Thai gender and family roles. Although these topics are still discussed in the anthropology of Thailand, here, too,

her work is seldom cited. Benedict characterized Thai society as male dominated. The subsequent essay that focused on gender in Thailand was that of Lucien Hanks and Jane Hanks (1963), who described the relations between men and women as equal. Their essay is descriptive, based on their fieldwork and experiences in Thailand, and there are no citations to any other literature about gender or Thailand. Because Hanks and Hanks drew on their fieldwork rather than on the sorts of literature that Benedict used, their conclusion about gender equality contradicted Benedict's conclusion about male dominance. Nonetheless, both Lucien and Jane Hanks seemed unaware that Benedict had written about Thai gender. This supports Jane Hanks's statement that they did not pay much attention to the Benedict piece (J. Hanks, pers. comm., 26 January 2005).

In the 1980s, the question of Thai gender relations became a hot topic, as it did elsewhere in anthropology. The debate about Thai gender concerned male dominance and the place of Buddhism as a cause and explanation (Keyes 1984; Kirsch 1982, 1984; J. Van Esterik 1982; P. Van Esterik 1982a, 1982b). Thomas Kirsch (1982, 21) cited both Benedict's discussion of familial threats to reject children (1982, 20) and her reference to a Chinese source that women dominate business. Later works, however, do not cite *Thai Culture and Behavior* except, occasionally, to acknowledge that Benedict initially addressed this topic (Muecke 1992; Tannenbaum 1999).

Life cycle ceremonies that mark important rites of passage are also connected to gender roles, and Benedict discussed what makes a person adult. For men, it is their ordination as a monk, however temporally; for women, it is the "lying-by-the–fire" after giving birth, common throughout Southeast Asia. While the practice is relatively widespread in Thailand, it appears to be limited to the central Thai (Siamese) as a rite of passage to adult womanhood. Again, there are brief discussions of lying-by-the-fire where it is relevant (Attagara 1968, 105; Ayabe 1973; J. Hanks 1963; Keyes 1984) but with no references to Benedict. These ethnographic observations confirm Benedict's account of the importance of lying-by-the-fire. Nonetheless, these more recent authors do not cite Benedict's observations about this practice in her *Thai Culture and Behavior*.

For anthropologists of Thailand, Benedict's monograph remains an obscure source, no longer available in the Human Relations Area Files and long out of print as a Cornell Data paper. Keyes's (1978) review essay on the ethnography of Thailand devotes a paragraph to *Thai Culture and Behavior* in his section on ethnography before World War II (1978, 5). Later, he described it as "the first attempt to identify fundamental premises upon which present-day Thai social life are based" and goes on to say that

"it has remained suggestive for subsequent students of Thai society and culture" while noting that Embree's essay has been more influential (Keyes 1978, 21). Keyes (1978, 38, 41, 43) annotated those sources he judged most significant and included both Benedict and Embree, among others.

## Conclusions: Ruth Benedict and the Study of Thai Culture

When I started this paper, I expected to find many citations to Benedict's work and ended up somewhat surprised at the few references to it. John F. Embree rather than Ruth Benedict served to define the topics and issues of concern for the anthropological study of Thailand. While the Cornell Southeast Asia Program published *Thai Culture and Behavior*, neither it nor Benedict's analytical perspective played much of a role in the study of Bang Chan. Yet when I reread Benedict's work, much of its content was familiar to me, based on what I had read about and seen in Thailand.

Some of Benedict's topics, such as the importance of fun (*sanuk*), have fallen out of favor in contemporary discussions of Thai character and attitudes toward life. It did appear in Phillips's (1966) analysis of Bang Chan as well as some other works.[14] The most extensive discussion of fun occurs in Mulder's (1978) analysis of Thai values and interactions, a work that strongly resembles the earlier national character studies. Tom Kirsch was interested in exploring what happened to "*sanuk*" in the analyses of Thailand but passed away before he could do so.

Other elements of Benedict's analysis made sense because she accurately presented the ethnographic reality of central Thai life. This is remarkable, given the limits of her sources. Benedict's work is not cited to support modern ethnographic observations, a consequence of its relative obscurity, its "old-fashioned" analytical style, and the lack of fieldwork-based research.

The relationships among culture and personality, initially sketched by Benedict for Thailand, drew on the complex connections among ethos, values, child-rearing practices, personality, and cultural structures. Anthropologists writing about Thailand continue to reflect this theoretical perspective, perhaps unknowingly, as it is reflected and refracted through the works on Bang Chan and taught by anthropologists who did their first fieldwork there. Because Benedict synthesized much of the early literature, I suspect that her work became general knowledge for those working in Thailand and, as such, not needing citations.

The analysis of Thailand as a Buddhist nation has its roots in the early works that Benedict drew on for her analysis. The analytic split between

practices that can be justified in canonical Buddhist terms and other practices, often characterized as animist, became a defining issue in the analyses of religion in Thailand. While it is true that most Thai are Theravada Buddhists, world religions are always transformed and localized. Buddhist practices in Thailand reflect local political, social, and cultural practices, something that Benedict recognized in her discussion of childhood independence training and the selection among Buddhist teachings that fit with it (Benedict [1946b] 1952, 28). Nonetheless, an uncritical and canonical view of Buddhism and its role in Thai social life persists in most academic writings about Thailand. Benedict, to some degree the Bang Chan researchers, and more recent analysts continue to accept the Thai elite perspective on Thai society, beliefs, and religious practices. Anderson's (1978) critique of these practices remains relevant.

I first found Ruth Benedict's study when I was a graduate student working in the Human Relations Area Files at the University of Iowa. This was 1975, and I was just beginning to study Thailand. I enjoyed reading Benedict's essay for its historical significance, both as the earliest anthropological study of Thailand as well as its connection to Benedict's work and life and the history of American anthropology. Benedict's *Thai Culture and Behavior* deserves its place in both histories.

## NOTES

I thank Dr. Mary Catherine Bateson, president of the Institute for Intercultural Studies, Inc., for permission to cite from the Ruth Fulton Benedict Papers at the Vassar College Library, Special Collections. I also thank Dean Rogers, special collections assistant at the Vassar College Library, for helping me with my first excursion into archival research. I thank Gerald Sullivan for luring me into this project and Sharon W. Tiffany for her editorial suggestions.

1. For further discussion of Benedict's methods, see Schachter (this volume).

2. All page references to "Thai Culture and Behavior: An Unpublished War-time Study Dated September, 1943," are to the Cornell Data Paper, reprinted in 1952.

3. See Sullivan (this volume) for a discussion of the role of child nursing and the formation of personality.

4. This is standard in travel as well as academic literatures. See Keyes (1984, 1987), Kirsch (1982, 1984), and P. Van Esterik (1982a, 1982b). For a critique of this approach, see Tannenbaum (1999).

5. These are actually not commandments in the Christian sense of the Ten Commandments. They were characterized in these terms by the authors Benedict referenced and, I suspect, by her informants seeking a common terminology.

6. Benedict does not specify how many Thai informants she or her colleagues interviewed. Benedict's office was in Washington, D.C., as was the Thai embassy, and I am guessing that some of the Thai were from the embassy. At least one informant, Amporn Meesok, was from the Cambridge, Massachusetts, area.

7. The absolute monarchy ended in 1932.

8. For discussions of these spirits, see Kirsch (1973, 14), Spiro (1967, 51 n. 19), and Condominas (1977: 97–118).

9. I drew the conclusion that Prathoomratha was Sino-Thai, because, according to Benedict ([1946b] 1952, 2), "His soccer team had gone down the [Malay] peninsula as representing the Chinese in Thailand [in soccer]." Since the only dated interview is 1943, I am guessing that the interviews with Prathoomratha were around that time.

10. Gorer's *Burmese Personality* (1943) is even more obscure than Benedict's ([1946b] 1952) study of Thailand. It seems to have disappeared completely from the scholarly horizon. Lucien Hanks (1949, n. 2) mentions Gorer's work in a footnote in his 1949 publication on Burmese personality, stating, "I have been unable to restudy his insights, not having access to a copy of his 'Burmese Personality' since I first read it in 1944 prior to embarking for Burma." Kroeber's access to Gorer's report also suggests that the OWI studies themselves were available to anthropologists of the time such as Kroeber, as well as to people working in the Office of Strategic Services, as Lucien Hanks did.

11. Embree ([1950] 1969, 3) worked in the American embassy in Thailand. It is not clear for how long; his first footnote simply states that "in 1947 the author was United States cultural officer in Bangkok and later in Saigon, French Indochina." His essay is based on his impressions during this time. Embree died in December 1950 in an automobile accident. At the time, he was a professor at Yale and director of the Yale Southeast Area Studies Program.

12. The other areas were India, the American Southwest, and Peru. The project in Vicos, Peru, is the best known.

13. For a bibliography of their work, see *Crossroads* (1992: 46–64).

14. A search of the Thai file (AO7) in the electronic Human Relations Area Files turned up eleven of the twenty-seven total sources that used the word "fun" or two different standard transliterations of the Thai terms *sanuk* or *sanug*. Nine of the eleven sources were published before 1980. The Thai file, updated in 2000, was accessed May 30, 2005.

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# THE ECOLOGY OF THE ANTHROPOLOGICAL MIND: GREGORY BATESON'S INFLUENCE ON THREE LATE TWENTIETH-CENTURY PACIFIC SCHOLARS[1]

Phillip V. Guddemi
*Union Institute and University*

The influence of Gregory Bateson's path-breaking ethnography, *Naven*, is well acknowledged in anthropology, as well as his collaborative work with Margaret Mead on Bali. Bateson's later work, however, departed from anthropology, as conventionally conceived, to focus on issues of communication, psychiatry, animal ethology, cybernetics, and epistemology. These ideas have been influential for relatively few anthropologists. This essay focuses on the influence of Bateson's later ideas on three anthropologists, all of them Oceanists: Roger Keesing, Robert I. Levy, and Roy Rappaport. These scholars shared an exposure to Bateson's ideas prior to their popularization in collected essays published in *Steps to an Ecology of Mind* (1972). This essay pays particular attention to how Batesonian epistemology informed the work of these anthropologists.

## Introduction

Gregory Bateson will always have a special place in anthropology for his innovative research on the Sepik region of Papua New Guinea and on Bali, at times in collaboration with his then-wife, Margaret Mead. His classic book *Naven*, subtitled *A Survey of the Problems Suggested by a Composite Picture of the Culture of a New Guinea Tribe Drawn from Three Points of View* ([1936] 1958), was not only a pioneering work of New Guinea ethnography, but also a unique experiment in explanation and understanding, a creative synthesis of social dynamics, ethos, and cultural patterning.

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Its profound influence on regional ethnography and on anthropological theory continues. Bateson and Mead's (1942) work on Bali, typified by the photographic book *Balinese Character*, harnessed unparalleled new techniques in photographic and film documentation to the development of theory about the cultural shaping of individual psychology. But after *Naven* and *Balinese Character*, Bateson changed from a purely anthropological thinker to a more interdisciplinary theorist. He did not pursue a typical anthropological disciplinary career in the postwar years, but worked instead on a unique range of issues: schizophrenia, animal and human communication, learning theory, and cybernetics. A collection of Bateson's essays on these and other topics was published in 1972 as *Steps to an Ecology of Mind* (hereafter, *Steps*). This book catalyzed much interest and substantially widened Bateson's audience. His biographer, David Lipset, characterized Bateson's persona in the last decade of his life (he died in 1980) as that of a "man of knowledge" (Lipset 1980: 279–302).

In this essay I examine the work of three anthropologists—as it happens, all Pacific scholars—who were students of Bateson, directly or indirectly, while he was pursuing his eclectic postwar researches, but prior to the publication of this work in *Steps*. My interest here concerns how these three scholars—Roger Keesing, Robert Levy, and Roy Rappaport—were able to use Bateson's post-*Naven*, interdisciplinary (or transdisciplinary) ideas to enrich their own theory and practice as they themselves remained within anthropology.[2]

These anthropologists applied Bateson's ideas, which would eventually be published in *Steps to an Ecology of Mind*, at a time when much of his postwar work was to be found in articles often secreted in obscure or specialized journals. The Batesonian ideas that interested Keesing, Levy, and Rappaport were not only those of the ethnographic Bateson of *Naven* and *Balinese Character*. They were also the cybernetic and communicational ideas that Bateson had developed over a period of three decades, beginning in the 1940s.

## Bateson's Later Cybernetic and Communicational Epistemology

The event that started Bateson gestating these new ideas was his participation (along with Margaret Mead) in a pioneering set of interdisciplinary conferences sponsored by the Macy Foundation in New York from 1942 to 1953. The Macy Conferences, the transcripts of which have been collected and republished (Pias 2003), were instrumental in the development of systems approaches in a number of fields, from computer science to

neurobiology to the social sciences. At the sixth conference in 1950, the new term "cybernetics" was applied to these approaches. Although cybernetics tends to be associated today with the radical expansion of computer technology, the Macy Conferences, from their very beginning in 1942, included psychologists and social scientists, such as Lawrence Kubie and Lawrence Frank, as well as the anthropologists Mead and Bateson. By 1946 scholars from biology and philosophy were also included (Lipset 1980, 179). Of course, Bateson himself was the son of a prominent biologist and evolutionist, and Bateson's awareness of the problems of biological explanation formed a backdrop to his adoption of the new cybernetic ideas. These included seeing social and biological systems as composed of circular, feedback processes that led to self-regulation or self-amplification, as Bateson had already described with respect to his theory of *schismogenesis* in *Naven* (Bateson 1958: 171–72).[3]

In 1948 Bateson, who had failed to be rehired for a visiting professorship at Harvard, moved to San Francisco to teach at the University of California Medical School. In 1951 he affiliated with Stanford University, while spending most of his time at the Veterans' Administration Hospital in Palo Alto with the title (held from 1949) of "Ethnologist" on his door. The term "applied anthropologist" may seem odd to affix to the relentlessly theoretical Gregory Bateson, who was also often skeptical of well-meaning activism. Yet Bateson can be counted as one of the pioneers of the extension of anthropology away from its usual academic home ground. Much of Bateson's efforts were to go toward the study of schizophrenia, concerning which he developed his famous "double bind" theory (Bateson 1972: 201–78; Lipset 1980: 206–19).

More influential to anthropologists than the "double bind" would be Bateson's postwar contributions to communications theory. Some of this was worked out during 1948–1951, when Bateson collaborated with the Swiss psychoanalyst, Jurgen Ruesch, on possible cybernetic foundations for psychoanalytic theory (Lipset 1980: 184–9). This work would eventually be published as *Communication: The Social Matrix of Psychology* (Ruesch and Bateson 1951). As part of this project, Bateson developed a new way of thinking about nonlinguistic forms of human communication, an interest later taken up by the semiotics movement in anthropology. Bateson saw nonlinguistic communication as related (in a hierarchical manner) to conventional linguistic communication, which in an analogy to computers of his day he referred to as "digital" communication. By contrast, nonlinguistic ("analogic") human communication is a metacommunication about the relationship between the communicators, rather than the overt subject of

discussion. Bateson noted that such communication is similar to animal communication, which is also about relationship.[4] For example, Bateson developed a special interest in the study of play as a particularly instructive example of a metacommunicative (mostly nonverbal) frame in both animal and human communication (Bateson 1972: 177–93; see also Lipset [1980: 191–7]). Bateson supported this approach with evidence ranging from otter behavior (Lipset 1980, 192) to children's play and adult humor.

Bateson elaborated on this multileveled theory of communication by introducing Bertrand Russell's theory of logical types. For Russell this was a theory, propounded in *Principia Mathematica* (Whitehead and Russell 1913), of how to avoid certain logical paradoxes in which a class could be taken as a member of itself (Bateson 1972, 202; see also Lipset 1980, 189). Bateson, however, unafraid of paradoxes, creatively applied Russell's concept of logical types to derive a hierarchy of levels of learning and metacommunication in humans and nonhuman animals. Bateson later developed his theory of levels well beyond Russell in his opus, *Mind and Nature*, showing that the hierarchies it entails are levels not so much of classes but of contexts (Bateson 1979: 127–42).

Another theoretical thread Bateson initially developed in the 1940s and continued thereafter involved a twist on contemporary behaviorist models of learning. Bateson was interested in the ability to learn to learn, which he called "deutero-learning" (Bateson 1972: 159–76). Much of what anthropologists consider as culture—or what psychoanalysts view as transference—involves learning on this second-order level. Deutero-learning, sometimes denominated as "Learning II," involves generalizing from repeated behavioral sequences of adaptation. Eventually, Bateson postulated the theoretical possibility of a third level of learning, in which it would be possible to move from one second-level understanding to another. Such "Learning III" might only be possible for Zen masters and the like (Bateson 1972: 279–308).

Roy Rappaport later described Bateson's multileveled learning theory in the following way:

> The learning of individual facts or tasks could be an example of first-order learning. Second-order learning would involve the learning of how to learn such facts or tasks or, at times, the learning of particular contexts, such as (but not limited to) cultural contexts, in which such facts or tasks fit into a larger pattern. Third-level learning would be the ability to learn and shift between these larger contexts. Bateson, however, thought this last form of learning to be, in fact, rare. (Rappaport 1999: 304–7)

After spending much of the 1950s and 1960s developing cybernetic and communicational frameworks for schizophrenia and animal behavior, Bateson recognized that his disparate intellectual enterprises had important points of convergence. He felt that he had been developing a "new epistemology," as he termed it—one that took account of the wider webs of complex causality characteristic of ecosystems and social systems, and that thereby constituted a critique of narrow "conscious purpose" focused linearly on instrumental goals (Bateson 1972: 440–93). Bateson saw the realms of the aesthetic and of religion as potentially corrective of this linear narrowness of vision. He also recommended, in this regard, the fostering of what Martin Buber ([1923] 1970) termed "I-Thou" relationships, and of a more sympathetic involvement with the natural world (Bateson 1972: 446–7).

## Three Anthropological Disciples of the Later Bateson

At this point in Bateson's career, a selection of his key papers was collected and published as *Steps to an Ecology of Mind* in 1972. This book influenced a number of movements and disciplines, but its effects upon publication are not my interest here. The three anthropologists under consideration—Roger Keesing, Robert Levy, and Roy Rappaport—share the distinction of having been influenced by Bateson's later ideas without the convenience of having seen those ideas collected in a more accessible published form in *Steps*. For Keesing and Levy, Bateson's influence involved an initial personal exposure to Bateson as a teacher.

These three scholars were very different in theoretical approach and subdiscipline. Keesing did fieldwork among the Kwaio people of the Solomon Islands. His initial interests were kinship theory and ethnoscience, although he later worked on religion and on issues of colonialism and ethnographic authority. Levy was a Freudian who conducted psychological tests and modeled his fieldwork on psychiatric interviewing practice; he studied in Tahiti and, later, Nepal. Rappaport was perhaps the preeminent ecological anthropologist of his generation, whose first book, *Pigs for the Ancestors* (1968), was a methodological tour de force, integrating hard-science ecological anthropology and a unique social anthropological perspective focusing on ritual cycles among the Maring of the New Guinea Highlands. His later work, culminating in his posthumous opus, *Ritual and Religion in the Making of Humanity* (Rappaport 1999), developed a careful and philosophically precise argument regarding the nature and evolutionary import of ritual.

## Roger Keesing: Bateson's Communicational Ideas within a Mainstream Anthropology

Roger Keesing was the son of an influential New Zealand anthropologist, Felix Keesing. The younger Keesing was an undergraduate at Stanford in the 1950s, when Bateson's affiliation with Stanford coincided with his work on schizophrenia at the Palo Alto Veteran's Hospital. This began a friendship that lasted several decades. Several letters between the two testify to their close relationship, albeit a relationship with significant ups and downs. The subjects of this correspondence include kinship theory and Keesing's impending fieldwork among the Kwaio (Roger Keesing to Bateson, letter dated November 13, 1961 [University of California, Santa Cruz, Gregory Bateson Papers, Folder 759, document 759-5]). Keesing, who in the late 1960s and early 1970s taught at UC Santa Cruz, seems also to have been involved in lobbying Bateson to take up a position there, which Bateson eventually did, on a "soft money" basis, in 1972 (Roger Keesing to Bateson, letter dated October 29, 1968 [University of California, Santa Cruz, Gregory Bateson Papers, Folder 759, document 759-13a]).

In a 1972 article, "Paradigms Lost: The New Ethnography and the New Linguistics," Keesing suggested that cognitive anthropology should take into account not only the insights of transformational linguistics but, complementary to these, Bateson's concerns about the algorithms of the unconscious, which were not coded like the logics of language. Keesing quoted Warren McCulloch, Bateson's cybernetic mentor (1965, 395): "'man, like the beasts, lives in a world of relations, rather than in a world of classes, or propositions'" (quoted in Keesing 1972, 317). Keesing (1972, 320) argued that we need a less simplistic conception of the mind than Lévi-Strauss, for example, offered. He placed his bets, as he put it, with the "integrative framework of systems theory and cybernetics," (1972, 326), and even cited Rappaport's work (including *Pigs for the Ancestors*) as an example of this new approach.

Decades later, Keesing (1991) dedicated his paper, "Experiments in Thinking about Ritual," to Gregory Bateson as "teacher and friend." Now neglecting Rappaport's growing corpus on the topic of ritual, Keesing engaged in a number of thought experiments to define ritual's domain. Keesing's paper explored Bateson's analysis of communicative frames, derived from Bateson's study of play (Bateson 1972: 177–93). Keesing (1991, 65) defined ritual as a type of stylized, serious, scripted play, which can be recognized by its frame, rather than its content. Keesing (1991, 66) also noted that for Bateson, "ritual is not about 'things'—birth, rebirth, cosmic re-creation or what have you—but *relationships*, formal patterns that have substantive referents at different levels."

Reanalyzing Victor Turner's (1967) classic view of Ndembu ritual multivocality, Keesing (1991, 66) claimed "Bateson would have wanted to add that the iteration of a relational pattern on multiple levels is a major source of its power, both to 'mean' and to transform." In this vein Keesing asked whether the ritual frame, like the play frame, has the potential (or "power") to transform consciousness—a question that brings to his mind Bateson's encounter with a seemingly schizophrenic otter at the San Francisco zoo, cured of its withdrawal by the evocation of play (Keesing 1991, 67). Keesing (1991, 68) therefore proposed that rituals "work" as they do "because of the way participants think and participate while they are in the ritual frame," rather than "primarily because of the covert symbolic structures embedded in them." But at this point Keesing moved on from Bateson to Austin and Derrida, having marshaled Bateson's ideas into supporting his own struggle against the "symbolic anthropology" of the day.

In *Kwaio Religion*, Keesing's (1982) evocation of the later Bateson similarly follows upon an examination of Turner's concept of the multivocality of ritual symbols. The Bateson paper Keesing chose to foreground is "Style, Grace, and Information in Primitive Art" (Bateson 1972: 128–52).

> Bateson suggests that art—and, I would add, ritual and metaphor—depends on an integrative/aesthetic capacity to perceive *patterns* and *relationships*. These relationships are *by their nature* inexpressible in language, except by indirection. (Keesing 1982: 181–2, italics in original)

Any translation of ritual symbols thus inevitably distorts, whether this be anthropological interpretation or native exegesis. In a footnote, Keesing (1982, 182) followed Bateson (1972, 137) in quoting Isadora Duncan, "'If I could tell you what it meant, there would be no point in dancing it'"[5] Keesing (1982, 183) related this theme to a dilemma then being discussed in the ethnography of Melanesian rituals. This was the problem of how to assess indigenous exegesis (particularly when such exegesis is not explicit), and in such cases how or whether to discern covert meanings in rituals. This problem was central to the work of a number of anthropologists who had worked in the region, such as Gilbert Lewis, Alfred Gell, and Ron Brunton. Keesing noted in particular Lewis's distinction between the logic of iconicity in ritual versus "what can be conveyed in words." Initiation in particular changes these iconic and discursive logics of ritual as initiates proceed through new revelations (Barth 1975; Poole 1982). Indeed, Melanesians (in Keesing's view) tend to see ritual as action rather than communication. Ironically, this discussion is encompassed in "Symbolism

in Kwaio Ritual," a chapter whose title upheld precisely the conventional "symbolic anthropology" of the day that Keesing wished to subvert.

In addition to his prolix writing for his academic peers, Keesing was noted for a series of iterations of a challenging textbook for undergraduates. The first version was officially a collaboration between Keesing and his father (Keesing and Keesing 1971); Keesing (1976, 1981) then wrote versions under his own name; and it was a mark of the esteem in which Keesing was held by his colleagues that a posthumous edition was edited after his death by Andrew Strathern (Keesing and Strathern 1998). In these textbooks, undergraduates were exposed to the ideas of Bateson on play, art, and frame analysis that Roger Keesing was using in his professional work. Keesing was also fond of Bateson's humorous discussion of the croquet game in *Alice in Wonderland*, which Keesing used to illustrate why it is impossible that any science of humans and other organisms will ever fulfill the traditional natural science ambition of being able to predict their behavior. The most emphasized aspects of Bateson's work, throughout the various versions of the textbook, had to do with his ideas of communication about relationship and algorithms of the unconscious, ideas that Keesing used as part of his critique of the symbolic anthropology of the day (1976: 167, 169–70, 200, 424–5).

Keesing was profoundly affected by Bateson's person and ideas and considered him a friend and a strong intellectual influence (Keesing 1994, 311). Keesing was, at the same time, a mainstream anthropologist, not only conversant with trends in the discipline but often on the leading edge of them. Thus Keesing tended to use Bateson's ideas as part of an eclectic toolkit within established disciplinary frames and ways of thinking—that is, a Batesonian content subsumed into a conventional anthropological form.

## Robert I. Levy: Bateson's Ideas within Psychological Anthropology

Robert I. Levy, like Roger Keesing, applied Batesonian ideas within an established disciplinary framework, in his case psychological anthropology. A trained medical doctor and psychoanalyst, Levy worked in the Langley Porter Neuropsychiatric Institute in San Francisco during the 1950s and early 1960s. He subsequently conducted anthropological studies in French Polynesia and Nepal. Bateson mentioned, by way of recommending Levy for his Nepal research, an acquaintance of about ten years "during which he has from time to time sat in on my classes and contributed importantly to discussions" (National Science Foundation, Proposal Rating Sheet for Robert I. Levy, Proposal P2 S1655A, n.d. [University of California, Santa Cruz, Gregory Bateson Papers, folder 848, document 848-5a]). Levy himself remarked that

> [T]he most important, transformative, and longest-lasting influence on me was Gregory Bateson, whom I first met when he was working near San Francisco on schizophrenia, work which lead [*sic*] to elaborations of his theory of learning (of great anthropological usefulness) and the double-bind theory of schizophrenia. Bateson's work, particularly the theoretical papers eventually collected in *Steps to an Ecology of Mind*, introduced me to the revolutionary shift in models of behavior initiated by cybernetics and communication theory, which allowed behavior/ mind/ thought to be understood (in part) as located and learned in a structured field of dynamic and mutually constructive relations in which individuals were nodes. He provided an entrée into the developments of late-twentieth century thought (including the French thought of recent decades, which traverses much of the same new ground from a different entrance place) and a partial corrective to the (still flourishing) mechanistic, intrapsychic, and "culture-personality" models which were residues of nineteenth-century ways of understanding. (Levy 1994: 188–9)

Levy cited Bateson frequently in his classic ethnography, *Tahitians: Mind and Experience in the Society Islands* (Levy 1973). Levy was not yet a professional anthropologist when, in 1960, Douglas L. Oliver of Harvard invited him to join a research team, along with other anthropologists and an archeologist, to conduct a multisited study of Tahitian (i.e., Society Islander) culture and behavior. Levy's particular role was to study the private and personal world of behavior among Tahitians. To do this he compared two communities, a traditional community he named Piri, and an enclave in the capital Papeete called Rotu.

Levy's field research methodology was based on traditional psychoanalytic categories, thus the Batesonian influence on his work is more evident in how he wrote up the research. For example, a chapter in *Tahitians* entitled "The Question of Maintenance" (a term taken from the psychiatric theorist, Jerome Bruner) used Bateson's authority to overcome disciplinary distinctions between individual and culture, in order to show how cultural ideas and institutions feed back upon the "internal" psychological structures of individuals. In particular, Levy analyzed certain traditional Tahitian institutions as sending "messages" to individuals in their psychological development. These institutions included local styles of *mahu* (homosexuality) and adoption that contrasted with their Western counterparts. Such institutions may be "good to think," in Lévi-Strauss's (1963, 89) sense, as illustrations of how culturally specific forms can affect the development of psychosexual

and other personality styles. To show how the message of cultural forms can be incorporated into the emerging development of the individual, Levy used Bateson's idea of mind outside the skin. For Bateson, "mind" is an entity comprised by ideas traveling in circuits and thereby forming cybernetic systems (Bateson 1972, 459; cited in Levy 1973, 471). These systems are not bounded by our traditional philosophical divisions between self or individual and society, culture, or environment.

Levy speculated about the village mahu (a male homosexual of a very public Tahitian style). Villagers held the stereotype that, in a manner "arranged by God," there tended to be just one such mahu per village. Levy saw the village mahu role as "part of men's minds," in the sense that non-mahu men define themselves by contrast with the mahu role. I suspect that Levy implicitly followed some of the homeostatic models of family dynamics that arose from Bateson's work with schizophrenics. For example, the "identified patient" can assume a role that enables other family members to define themselves as unlike the member receiving medicalized attention. It is important to note that Levy in no way intended to pathologize the Tahitian mahu; nor did he apply an American concept of normality, i.e., the medicalized equivalent of the proven grace of Calvinism, to Tahitian social and psychological ideas.

There is, to my mind, a subtle Batesonianism in the larger analytical structure of Levy's *Tahitians* that arises from Bateson's ideas about the proper use of abstractions. Bateson was very careful to avoid what he often referred to, following Whitehead, as misplaced concreteness; thus Bateson preferred vague or vernacular formulations when he felt his concepts to be imprecise. In a 1940 article reflecting on the intellectual history of *Naven* and entitled "Experiments in Thinking about Observed Ethnological Material," Bateson (1972, 84) noted this as a "trick of thought and speech, which I have found useful." Levy similarly presents his material in *Tahitians*:

> I have sliced up behavior, or rather abstractions at varying degrees from behavior . . . into gross categories—"bodies," "souls," "feelings," "thinking"—purposively naïve categories which are natural for me. Within these gross categories there are finer ones which take some account of native categories. (Levy 1973, 94)

The reference to "slicing up" behavior, and then the careful emendation of this to note that it is really abstractions, and not behavior, which are under discussion, are both marks of a faithful and attentive student of Bateson.

Outside of *Tahitians*, Batesonian ideas appeared in a 1984 article for *Ethos* entitled, "Mead, Freeman, and Samoa: The Problem of Seeing

Things as They Are." Levy expressed the hope, inspired by Bateson's hierarchy of learning, that the "clash of two systems of certainty" can enable individuals to learn about "the constructed features not only of the other system, but of all such systems" (Levy 1984, 89). This would be "third-level learning," which at first may bring all systems into question, prompting an "epistemological and ontological crisis" (Levy 1984: 89–90).

The Newars of Nepal, whom Levy (1990) studied after his Tahitian work, found themselves in a nexus of cultural contact that elicited for them a sense of crisis about the nature of reality. This led these Newars to a kind of critical analysis and creative insight, which Levy argued to be of the same nature as that which ought to result from anthropological participant observation and analysis. For Levy, anthropologists and sophisticated Himalayans alike were scaling, as it were, the higher Batesonian levels of learning (Levy 1984, 2001). Bateson himself may have felt third-level learning to be rare and on a par with Zen enlightenment, but for Levy, Learning III could indeed be the result of culture contact, not only for anthropologists, but also for the people they studied and learned from.

## Roy Rappaport: His Earlier Cybernetics and Systems Theory Perspective

It is hard to determine to what extent Levy's studies with Bateson in the 1950s and early 1960s were influential in the thinking of Levy's cousin, Roy Rappaport, who, at the beginning of this period, was engaged in the hotel business. According to Rappaport's own account (1994: 166–7), Levy, along with the psychoanalyst, Erik Erikson, (who was also a friend of Bateson and Mead) were instrumental in bringing Rappaport to anthropology as a field of study. However, according to the same account, Rappaport's initial meeting with Bateson in Hawai'i did not occur until 1968, well after the completion of *Pigs for the Ancestors*. This is surprising, given the centrality of cybernetics to that work.[6] Evidently Rappaport, unlike Keesing and Levy, did not have the privilege of formally being Bateson's student. In fact, the 1968 edition of *Pigs for the Ancestors* contains only one citation of Bateson (Rappaport 1968, 207), specifically the 1936 edition of *Naven*, rather than the second edition of 1958 that incorporated cybernetic ideas. Bateson is mentioned in the company of a number of other theorists of ritual, such as Sigmund Freud, Max Gluckman, and Theodor Reik; however, in the ensuing discussion it is only the theories of Gluckman and Freud who are given specific attention.

The cybernetic citations in *Pigs for the Ancestors* demonstrate that what Rappaport sought at the time was a basic, mechanistic cybernetics or

systems theory. For example, Rappaport cited the article by Powers, Clark, and McFarland, "A General Feedback Theory of Human Behavior" (1960), which presented an abstract model of an organism as a "feedback control system," modified to include memory (1960, 71). However, the system it proposed is rather different from that in *Pigs for the Ancestors*, precisely in that Powers and his coauthors defined a goal-seeking system that has perceptions and attempts to make these perceptions match (or relate to) goals—in other words, a living organism. In *Pigs for the Ancestors*, by contrast, we see a pioneering effort to model an ecosystem in which humans are the ecological dominants, but not the conscious regulators. Rappaport described a system that does not control itself by using a centralized model or planning function and that, therefore, is not an organism writ large of the kind modeled by Powers et al. Nevertheless, Rappaport purported to find cybernetic regulators—specifically, "homeostats" and "transducers"—operating to ensure perpetuation of a system composed of both goal-seeking humans and nonhuman organisms. Much to the dismay of later critics (e.g., Gillison 2001; Sahlins 1976), the self-regulation of the system was, according to Rappaport, achieved by unconscious system operations that subsume most of the conscious activities of Maring ritualists and warriors. System regulation became, uncomfortably for many anthropologists, a partially unintended consequence of the social and ecological life of a human group. In a 1979 article, "On Cognized Models" (written for his collection of essays, *Ecology, Meaning, and Religion*), Rappaport explicitly made this point. Human views of the world are to be seen as "part of populations' distinctive means for maintaining themselves in their environments" (1979, 98), rather than as being complete models of those environments. At the same time, these human models of the world are richer and more meaningful than they would need to be if their only role were to regulate adjustment to environment. Furthermore, such regulation is accomplished, not merely by conscious human relations with the environment, but also by the ways these interact with environmental and social processes, whose ecological consequences are not always fully perceived.

Rappaport's model in *Pigs for the Ancestors* was exhaustively criticized at the time, though perhaps not as thoroughly understood, for reasons Bateson would have found familiar. One of the signal attractions of the cybernetic perspective to Bateson was the hope that it would enable the study of systems to be immunized from the traditional criticisms of teleology and purpose, which in classical (precybernetic) natural science were considered inimical to scientific method. The criticism of Rappaport's cybernetic approach as being "neofunctionalist," or simply "functionalist" (e.g., Sahlins 1976: 87–8), demonstrated that such traditional fears of

teleology were not universally assuaged by the new systems perspectives.[7] Anthropology proved resistant to the systems view, partly because of its methodological challenges, but also because of common misapprehensions of the cybernetic model, such as the idea that systems approaches could not deal with change. Rappaport responded to his critics on this and other issues in fifteen added chapters appended to the enlarged 1984 edition of *Pigs for the Ancestors* (1984: 299–444).

## Second-Order Rappaport: Bateson's Ideas in the Study of Ritual

By the time Rappaport published this response to critics, however, he had entered what I term his "second-order" phase, drawing upon the difference between first-order and second-order cybernetics—a distinction that became popular in the cybernetics movement by the early 1980s. Second-order cyberneticists include the observer in the description of what is observed. It should be noted that the similarity with some postmodern approaches in anthropology and elsewhere is no accident, though the two currents of intellectual influence are parallel rather than convergent. This approach contrasted with mechanistic models that characterized much of the initial work in cybernetics, with its systems engineering focus.

Rappaport's systems view, in my opinion, developed a new richness, perhaps of a second-order type, after he met Bateson in 1968. Rappaport's major project after this time focused on the development of a comprehensive theory of ritual in human culture and evolution. I would argue that this later project has more "Batesonianism" in it than did Rappaport's earlier so-called "neofunctionalism." Perhaps the later Rappaport is also less "materialist" than the earlier, albeit the "materialism" of the earlier Rappaport has, in my view, been exaggerated. In fact, as I discuss below, the analysis of systems dynamics in Rappaport's later work is phrased in Bateson's later, in some ways "idealist," terms—in contrast to the more "materialistic" phrasings characteristic both of early cybernetics and Rappaport's earlier work.[8] Rappaport's posthumously published 461-page magnum opus, *Ritual and Religion in the Making of Humanity* (1999; hereafter, *Ritual and Religion*), presented a final synthesis of his earlier arguments published in *Ecology, Meaning, and Religion* (Rappaport 1979). The following discussion considers only those features of Rappaport's argument that owe the most obvious debts to Bateson.

Rappaport's later work asked the question why ritual is a universal feature of human culture. Both philosophical and theoretical, Rappaport's work used ethnographic examples primarily for illustration. The first two chapters of *Ritual and Religion* are devoted to the definition and

clarification of terms and development of a theory of ritual form. As part of such a theory, Rappaport cited Bateson among those scholars who broadened the domain of communication to include not only "*saying*," but also the kinds of "*doing*," which are more "efficacious" in the realm of information rather than (only) that of energy (Rappaport 1999, 51). Such communication can be self-referential. In these matters Rappaport, like the later Bateson (1979, 94), found the thinking of the nineteenth-century philosopher and semiotician, Charles Sanders Peirce, to be a useful framework for analysis.

Indeed, Rappaport considered some of the most important messages, which are entailed by the performance of ritual, to be self-referential messages. These can include messages to do with the relationship of individuals to social groups—for example, messages about an individual's status in a group. However, there is also the category of messages about the self that are received by the self. Rappaport saw these communications as constituting part of a "private system" of the psychological self, which has its own informational cybernetics. Such communications may not be easily translatable into discursive or logical terms, and may instead be characterized by what psychoanalysts called primary process, for example, the compressed metaphorical messages in dreams (see also Bateson 1972: 138–42). Although some psychoanalytic conceptions of "primary process" theorized it in terms of somewhat chaotic emotional drives, rather than communication or messages, Rappaport followed Bateson (1972) in foregrounding its communicational characteristics, as well as its emotional salience. However, for Rappaport, the self-referential in ritual is always within the context of a larger "canonical" and (more or less) invariant form. This too should be examined as communicational or informational action, rather than energetic or material substance.

Rappaport (1999, 109) set forth, "in possible disagreement with Bateson," an analytic opposition between mere information and meaning, implying that, in some discussions of information, Bateson may have conflated the two. In particular, and very importantly for ritual, the repetition of an identical sequence carries less "information," in one important technical meaning of that concept, than does a novel sequence. At least, this is so according to Anthony F.C. Wallace's (1966) anthropological interpretation of information theory, based on the work of the pioneering cyberneticist, W. Ross Ashby (1956; see Rappaport 1999, 285). Nevertheless, a repeated ritual could carry a greater sense of meaningfulness than the novel but trivial events of daily life. In spite of this apparent divergence from Bateson's use of the concept of "information," Rappaport used Batesonian (1951) communication theory in a discussion of the metamessage involved in the use of specific linguistic codes (Rappaport 1999, 127).

Rappaport spent considerable time developing the possible implications of an almost offhand speculation by Bateson in his introduction to *Steps to an Ecology of Mind* (1972: xxii–xxv). Bateson examined the Biblical and Iatmul origin myths to compare what is considered significant in them; he noted a distinction between the origin of matter, which is treated as relatively trivial, and the origin of order. Rappaport, who wished to establish the role of ritual in developing a meaningful unity of form and substance, developed Bateson's brief speculation into a full ethnographically illustrated discussion (1999: 155–64). Rappaport followed Bateson in noting the cross-cultural salience of the form–substance distinction—a distinction that, for Bateson, may have arisen from "an unconscious deduction from the subject–predicate relationship in... language" (Bateson 1972, xxv; Rappaport 1999, 165). Rappaport saw, arising from this projection into the universe of the structure of language, the transformation by myth and ritual of "the conventional into the natural" (1999, 167). This transformation is accomplished by re-creating through performance the primordial union of form and substance.

Rappaport next examined the vexing question of time. In the first instance, time seems to be what sociologist Emile Durkheim, following Aristotle, termed one of "the categories of understanding" that serve as a "framework of the intelligence" ([1915] 1961: 21–2; cited in Rappaport 1999, 171). Such frameworks of the intelligence were an important part of what could be called the Batesonian unconscious (Rappaport 1999, 173)—i.e., those assumptions (from nature, culture, or nature modified by culture) that form the "*how*" of our awareness, rather than the "*what*" of it. For Bateson, adaptation, or even ordinary perception, required that these assumptions generally not impede upon our consciousness, and, in fact, they could be almost inaccessible to it. They may exist as a "higher" logical type from that of our normal awareness. Rappaport followed this consideration of time as a category of perception with an ethnographically illustrated discussion, more Durkheimian than Batesonian, of the social ordering of time mediated through ritual.

In chapter 6, Rappaport returned to human universals of ritual tempos and transitions. For example, rites of passage can be considered digital transitions from one defined state to another, yet even computers must mediate their transitions through some analog process, however short in nanoseconds this may be. "The transition from 0 to 1 taking place *in the ignored interval* is not a digital but an *analogic* process" (Rappaport 1999, 217). Similarly, ritual "digitally" enables transitions of individuals from one marked social state to another, but within these transitional phases ritual brings them into a "time out of time," which is often characterized by a

social unison transcending the individual. This transcendence can be epitomized by dance, an activity characterized by a particular relationship to time and appealing to the right, or integrative, hemisphere of the brain, rather than the left, or discursive one (Rappaport 1999, 129). Rappaport did not directly cite Bateson as the authority on the left–right brain hemisphere contrast, a popular and, today, questionable opposition that was often in fact mentioned by Bateson but which does not derive from Bateson's work.

Rappaport then turned his attention to symbolism as anthropology usually understands it. Like Keesing, Rappaport examined Victor Turner's famous example of the multivocality of the Ndembu people's *mudyi* tree (Turner 1967). Rappaport similarly delineated the symbolic dimensions of his own work with the Maring of New Guinea.

"The Idea of the Sacred" (Chapter 9 of *Ritual and Religion*) considers a topic of great interest to both Rappaport and Bateson. Yet, certain key Rappaportian concepts about the sacred—those of ultimate sacred postulates and of unquestionableness, for example—were not influential for, nor influenced by, Bateson's own later work on the sacred (e.g., Bateson 1991: 245–313).

Nevertheless, drawing from earlier work by Bateson, Rappaport saw the "cultural truths" of particular sacred orders as belonging to the category of "truths whose validity is a function of their acceptance" (or "belief": Bateson 1951: 212–27, cited in Rappaport 1999, 304). Such truths included those of deutero-learning, or second-order learning, a concept Rappaport felt should be prominent in anthropological theory, in the place of similar (but in Rappaport's opinion, inferior) concepts, such as Pierre Bourdieu's notion of *habitus* (Rappaport, personal communication to the author, 1986; Rappaport 1999, 304). Here one notes that deutero-learning as second-order learning, "sinks" the patterns learned from repeated experience into increasingly *unconscious* levels of the mind. Much of shared culture is a function of shared deutero-learnings among the coparticipants in that culture. Bateson's (1958, 119) earlier term, "ethos," can be seen as referring to deutero-learnings, or "deutero-truths," shared by members of a society (Rappaport 1999, 306).

Even though more culturally relativist symbolic anthropologists of the day might have grounded ritual or the sacred on the deutero-truths or ethoses of a particular society, Rappaport did not do this. The truths of sanctity may be culturally variable, but nonetheless their kind of "truth" is established, not by symbolic meanings, but by the action of ritual. Rappaport mentioned in *Ritual and Religion* a personal communication with Robert Levy on the difference between isolated societies with little

cultural contact, which may rely more heavily on deutero-learning "to establish the public understandings that underlie social life" (Rappaport 1999, 307), and those with either greater internal differentiation or exposure to other social orders, which have to reckon with the fact that their own understandings are not universally shared.[9] Perhaps the latter societies have more need of the Rappaportian mechanisms of ultimate sacred postulates and ritual establishment of truths than do the former. However, Rappaport argued that in all societies the truths of sanctity in fact limit those of experience and take precedence over them (1999: 310–1); yet, if deutero-learning were enough, the sacred would not have been necessary, as it seems to be in all human societies to date.

The sacred, nevertheless, is used to sanctify particular social orders, for example, by placing "in God we trust" on the currency or by using sacred ritual to crown the king. The sacred also sets up ideas of cosmic order or "logos." Holiness is partly established as the sacred, which is for Rappaport its discursive or "logical" component (i.e., expressible in language, however "meaningless" that language might be to logical positivists). However, holiness also contains the numinous or religious experience that is experienced inarticulately (Rappaport 1999, 371). Rappaport discussed both William James in this connection, as well as Bateson's work, inspired by conversations with Aldous Huxley, on "grace." Bateson defined "grace" as the integration of

> [T]he multiple levels of which one extreme is called consciousness and the other the unconscious. For the attainment of grace, the reasons of the heart must be integrated with the reasons of the reason. (Bateson 1972, 129; quoted in Rappaport 1999, 383)

Art and the aesthetic are integral to the quest for grace, particularly for Bateson. The so-called "inarticulate" numinous nevertheless provides higher levels of meaning that dissolve distinctions. Rappaport contrasted Freud's and Marx's treatment of religion as an illusion with James's (1890) distrust of rational thought as too often an instrument of self-serving rationalization. More profoundly, Bateson saw religious (as well as aesthetic) phenomena as part of a corrective for purposive consciousness. For Bateson, the partial viewpoint integral to "conscious purpose" tends to cut through the integrative, systemic circuits of any larger whole, denominated as "mind." Bateson condemned this kind of partial view as pathogenic (1972: 144–6; cited in Rappaport 1999, 401).

From these heights of the numinous, Rappaport abruptly climbed down to the valleys of adaptive theory, for which, of course, Bateson's corpus

remained foundational. As mentioned earlier, Rappaport's systems theoretical perspective antedated his first meeting with Bateson and most of his published work. However, as I mentioned above, the systems theoretical discussion in the later Rappaport (beginning as early as 1977) had been recast in an "idealist" rather than "materialist" cybernetic idiom. Or more precisely, since cybernetics sees itself as transcending the idealist/materialist dichotomy, an idiom using informational rather than mechanistic language. Specifically, Rappaport uses Batesonian formulations that adaptive (i.e., cybernetic) systems "operate to maintain the truth value of certain propositions about themselves in the face of perturbations threatening to falsify them" (Rappaport 1999, 410). Rappaport attempted to recast ideas of self-organization and self-regulation in this light and cited the Batesonian criterion of flexibility. For Rappaport, flexibility is not the same as variability; rather, flexibility is "a product of versatility and orderliness" (1999, 418), or, perhaps better, versatility under a particular order. Rappaport also discussed the economics of flexibility in terms of the sequence of adaptive processes outlined in Bateson's article, "The Role of Somatic Change in Evolution" (1972: 346–63).

Rappaport explicitly analogized the adaptive responses of social systems to those of organisms, something which Bateson, in the article cited above, only does implicitly. Quickly mobilized, early responses to systemic perturbation are "energetically and behaviorally expensive, but easily and quickly reversible following the cessation of stress" (Rappaport 1999, 420). This is as true for social as for somatic adaptation. Later responses, which to be adaptive should be responses to repeated stress of the same kind, will be structural ones that are far less reversible, but that are less energetically or behaviorally expensive in confronting each instance of perturbation. These latter responses are increasingly "hard-wired," and they in fact can lead to a reduction of the long-term flexibility of the system. Rappaport's (1999) analysis of these matters in his chapter, "Religion in Adaptation," (and elsewhere) is exemplary and deserves to be far more widely read by systems thinkers and cyberneticians (and even land use planners). He extended this analysis to the conservatism of adaptation and the relation of general-purpose systems (e.g., organisms and societies as wholes) to special-purpose systems (e.g., organs and institutions). These entailed a hierarchical structure to adaptation. What Rappaport called the "ultimate sacred postulates" of ritual tend to be "empty" in terms of specifying the specific social adaptations of the societies for which they are sacred; the less "meaningful" they are in this ordinary language sense, the more adaptive they may be. In fact, "if a postulate is to be taken to be unquestionable it is important that no one understand it" (Rappaport 1999, 428)—a quality characteristic, for example, of what Catholic theologians call "mysteries."

Sacred propositions that uphold authorities are not always or only wielded by them. Thus, Rappaport (1999: 429–37) proposed a "cybernetics of the holy" by which dissent could emerge within the framework of the sacred propositions of a particular society. Such phenomena as prophecy and millenarian movements could enter here. However, it is also common for societies, particularly those whose inequalities of power make bottom-up corrections implausible, to exhibit what Rappaport considered maladaptations. These include the privileging of subsystems above the whole (e.g., "the business of America is business") or the oversanctification of particular low-level regulations of behavior (such as the Catholic prohibition against birth control, amusingly characterized by Rappaport 1999, 440 as entailing "very specific low-order rules concerning non-immaculate nonconception"). The emergence of writing allowed for the sanctification of texts such as the Bible. The maladaptation consequent upon this is a loss of adaptiveness fostered by the political and social conservatism known as fundamentalism. For Rappaport, fundamentalism exposes the sacred to dubiety and discredits the sacred by linking it too closely to the transient conventions of social life (1999, 445). The use of power to coerce belief can, like fundamentalism, lead to a discrediting of the sacred, but those who are led by this to alienate themselves from power's corruption of the sacred may themselves suffer a painful "alienation from the deepest parts of the self" (Rappaport 1999, 448). The secular privileging of fact, combined with the dissolving force of money, yields a deeply unsatisfying society which, according to Rappaport, is likely thereby to degrade the ecosystems upon which it depends. In fact, Rappaport concluded with what could be termed a "Deep Green" manifesto for founding the science of the future on holistic and ecosystemic ideas.

*Ritual and Religion in the Making of Humanity* is, in a sense, a work of constructivism. Ritual practice itself constructs human ideas of social and cosmic order and holism. In a discussion of Heraclitus' concept of *logos*, Rappaport (1999, 368) showed how "the liturgies of a range of societies" construct versions of cosmic order particular to those societies.[10] But this version of socially or culturally particular *logoi* contrasts with Bateson, who tended, in my opinion, to take as almost axiomatic the existence of a cosmic order that is beyond our abilities, individually or socially, to construct fully. Bateson was as emphatic as any postmodernist or constructivist in emphasizing how we create or invent the realities that we perceive and by which we act and think. Yet the *logos* represented by Heraclitus' fragment, "Listening not to me but to the Logos the wise agree that all things are One" (Kirk 1954, 65; quoted in Rappaport 1999, 459) is, I think, that of Bateson. Rappaport, too, seemed to strain toward such a larger conception

of cosmic holism in his own thought; yet, the kind of holism that ritual most often could be shown to create was of necessity a more culture-bound, limited variety. Rappaport's ultimate vision for what Stephen Toulmin (1982) called a "postmodern" science was one that might transcend this opposition. Rappaport envisioned a science that used the ecosystem concept and similar ones—not merely to illustrate how humans construct the worlds in which they live—but also to help humans explore and adapt to a world in which their constructions will always be inadequate to the larger systems in which they are inextricably embedded. Knowledge, as Rappaport liked to say, will never replace respect in human relations with ecological systems.

## A Wounded Holism and Concluding Remarks

It is in this sense of worldview or, if one prefers, epistemology, that Rappaport—particularly in his later work—was the closest of our three anthropologists to Bateson. Rappaport is unique among these three thinkers in working not only with Bateson's communicational theories, but also with some of his lesser known refinements of cybernetics and systems theory. Bateson and the later Rappaport share in the deepest sense a wounded holism, one that is at once the result of their apprehension of possible ecological disaster and the cause of their ability to perceive the prospects for such disaster more clearly than others. Lambek (2001, 247) aptly notes, "Like Bateson, Rappaport appears to have been characterized more by his originality than his location within a paradigm." I sense, indeed, that this was the only kind of disciple whom Bateson would ever accept—since to think for oneself rather than in a paradigm was for Bateson both a personal imperative and one he wished for others as well. However, I do see their original paradigms as having a close family relationship, although it is quite possible to accept or use one without the other, since they are by no means necessary entailments one of the other.

The other two Pacific scholars discussed here, Levy and Keesing, were also more original than the common run of anthropologists. Nevertheless, it seems to me that Keesing's work, and to some extent also Levy's, remained more within the grain of the anthropology of their time than did Rappaport's later studies of ritual.

All three anthropologists had the benefit of personal contact with Bateson as a teacher and, in many ways, as a friend. Their acquaintance with his work antedates its wide dissemination, first achieved in the collection in *Steps to an Ecology of Mind*. In fact, Bateson at one point considered Robert Levy as the person who should write the book's introduction

(Bateson to Robert I. Levy, letter dated August 28, 1970 [University of California, Santa Cruz, Gregory Bateson Papers, folder 848, document 848-3]). Trends in the discipline since that time have moved away from Bateson's ideas, although the pendulum may be swinging back today.

A consideration of the profound effect that Bateson's work had on these three late twentieth-century anthropologists should not only keep alive their memory, but also demonstrate that Bateson's later work and teaching could and did have significant relevance to some of the best thinkers within the discipline of which he had been such a significant figure in the prewar period.

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## NOTES

1. This article began as a paper delivered February 2, 2005, at the symposium, *Gang of Four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in Multiple Contexts*, during the Association for the Social Anthropology in Oceania (ASAO) annual meetings in Kauaʻi, Hawaiʻi.

2. I am not suggesting that these three were the only anthropologists of their era influenced by Bateson. Michael Lieber, an Oceanist as well as a student of the Caribbean, has used Batesonian perspectives since the 1980s. Like Rappaport, Lieber studied the intersection of culture and ecology, but Lieber's ecological perspectives may be in some ways closer to Bateson than was Rappaport's early systems theoretically influenced work. Lieber (1994: 19–34) also relied on Bateson for theories of cybernetics and communication. Lieber's fellow Micronesianist, Vern Carroll, was also influenced by Bateson and compiled the first bibliography of Bateson's works included in the 1972 printing of *Steps to an Ecology of Mind*. One should also mention Roy Wagner as a thinker pervasively formed by his encounter with Bateson's ideas. Though more recent trends in American anthropology have moved away from Bateson's later concerns, his work is foundational in parts of continental Europe, notably Norway (see the work of Thomas Hylland Eriksen 1993). It should also be noted that, in his most recent book, *Apologies to Thucydides*, Marshall Sahlins (2004), of all people, uses Bateson as a theoretical muse. However, it

is *Naven*'s *schismogenesis*, not cybernetics or the ecology of mind, so pilloried in *Culture and Practical Reason* (Sahlins 1976), which the latter-day Sahlins finds useful for his purposes.

3. *Schismogenesis* refers to a kind of self-amplifying social behavior. *Symmetrical schismogenesis* is a recursion of competitive interactions similar to an arms race, while *complementary schismogenesis* is a recursion of interactions that reinforce complementary roles while driving them to further and further extremes (Bateson 1958: 175–6). Since this concept was developed within Bateson's "anthropological period," it has been adopted by more anthropologists than Bateson's later epistemological ideas, discussed in this article.

4. See, for example, Bateson's (1951; 1972: 9–20) fictionalized dialogue about dance, or his research with dolphins, first published in 1966 and reprinted in *Steps* (1972: 364–78.)

5. Bateson, in his turn, expressed his debt to Anthony Forge (presumably a personal communication) for this quote.

6. Mary Catherine Bateson speculated that Margaret Mead may have "directed Rappaport to Bateson's work while he [Rappaport] was at Columbia, as she did with others" (Mary Catherine Bateson, e-mail message to the author, May 17, 2006).

7. Sahlins attributed the origins of the term neofunctionalism to human ecologists themselves (1976, 87). Sahlins' criticism of Rappaport's so-called neofunctionalism implies that Rappaport reduced the complexity of culture to its ecologically regulatory functions, but Rappaport, as mentioned, specifically noted that culture elaborates itself far beyond its role in regulating the adaptation of groups to environments. Rappaport's influences from the ecological anthropology movement of the 1960s, particularly as this expressed itself at Columbia University, were probably more responsible for the supposedly reductive "functionalism" in his approach than Bateson, whose influence was far greater on the "idealistic" examination of ritual across culture to which Rappaport later turned. Unfortunately, untangling these strands of influence more fully is beyond the scope of this essay.

8. Cybernetic epistemology is neither materialist nor idealist in the traditional sense, but the epistemology of the mature Bateson, who tended to describe cybernetic systems in terms of information, difference, ideas, and "*mind*," can still be contrasted to other versions of systems theory, which addressed or emphasized the more mechanistic aspects of systemic self-regulation. Of course, Bateson did build the "idealistic" aspects of his epistemology upon a careful reanalysis of the "mechanistic" ones, which formed an irreducible base upon which his conceptual structure was built. Bateson's efforts to include and explain the "materialistic" cybernetic base of the systems to which his theories of mind applied led some to conclude falsely that he was primarily concerned with systems that worked upon the analogy of a thermostat, or of a simplified model of a living organism.

9. Levy himself expounded upon this point in Rappaport's schema of ritual in a fascinating essay, "The Life and Death of Ritual," published in the posthumous festschrift for Rappaport entitled, *Ecology and the Sacred* (Levy 2001).

10. It should be emphasized again here that the ritual-engendered *logoi* described by Rappaport are social, and not psychological, phenomena. Although they differ from one ritual order to the next (these may, but do not have to, correspond to societies or cultures), they do not depend on the deutero-truths characteristic of any particular culture, but instead they derive, in a manner unique, as far as I know, to Rappaport's corpus, from a kind of general performativity of ritual itself.

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## CONTRIBUTOR BIOGRAPHIES

**John S. Gilkeson** teaches history and American studies at Arizona State University. He is the author of *Middle-Class Providence, 1820–1940* (Princeton University Press, 1986) and *Anthropologists and the Rediscovery of America, 1886–1965* (Cambridge University Press, forthcoming). His research interests include the history of the social sciences, the history of American studies, and culture history.

**Phillip Guddemi** is Adjunct Professor at Union Institute and University and Managing Editor of the journal *Cybernetics and Human Knowing*. He took four courses from Gregory Bateson as an undergraduate at the University of California, Santa Cruz. He completed his Ph.D. in Anthropology (University of Michigan), based on fieldwork in the East Sepik Province of Papua New Guinea. Roy Rappaport was his dissertation advisor. He is currently working on a cybernetic theory of power.

**Roger Lohmann** is Associate Professor of Anthropology at Trent University and Editor-in-Chief of *Reviews in Anthropology*. His research interests include the mutual influence of sensory perception and imagining in dreaming, religion, and enculturation in Melanesia. His most recent publications include "Gendering Religious Objects," a theme issue of *Material Religion* (2007).

**Maureen Molloy** is Professor of Women's Studies in the Department of Anthropology at The University of Auckland. She has recently published a book on Margaret Mead titled *On Creating a Usable Culture: Margaret Mead and the Emergence of American Cosmopolitanism* (University of Hawai'i Press, 2008).

**Judith Schachter** is Professor of Anthropology and History at Carnegie Mellon University. Recent research interests focus on kinship and family (*A Sealed and Secret Kinship*, Berghahn, 2002). She is co-editor of the special issue of *Pacific Studies*, "Changing Interpretations of Fosterage and Adoption in Pacific Island Societies" (2008; 31 [3–4]). She also continues to publish in the history of anthropology, particularly work on Ruth Benedict.

**Paul Shankman** is Professor of Anthropology at the University of Colorado–Boulder. He has conducted fieldwork in Samoa periodically

since 1966 and has written extensively on the Mead-Freeman controversy. His most recent publications on the controversy are "Virginity and Veracity: Rereading Historical Sources in the Mead-Freeman Controversy," *Ethnohistory* 53 [3]: 479–505 (2006), and *The Trashing of Margaret Mead: Anatomy of an Anthropological Controversy* (University of Wisconsin Press, 2009).

**Gerald Sullivan**, Professor of Anthropology and Archaeology at Collin College, has been working with the Margaret Mead Papers since 1993. He is the author of *Margaret Mead, Gregory Bateson and Highland Bali: Fieldwork Photographs of Bayung Gedé 1936–1939* (University of Chicago Press, 1999). He is currently working on another book, *The Making of Balinese Character*, exploring the sources of Mead and Bateson's scientific project and their approach to the unity and diversity of our single species.

**Nicola B. Tannenbaum** is Professor of Anthropology at Lehigh University. She is interested in the history of the anthropology of Thailand. Her most recent publication is "The Changing Nature of Shan Political Ritual and Identity in Maehongson, Northwestern Thailand" (*Contemporary Buddhism* 10 [1]: 171–184).

**Caroline Thomas** has B.A. and M.A. degrees from the University of Auckland, and is currently completing her Ph.D. thesis in Anthropology at the University of Waikato, Hamilton, New Zealand. Her doctoral research concerns the professional career and writings of Reo Fortune, a topic she has pursued in archives scattered through New Zealand, Australia, the United States, and Britain.

**Sharon W. Tiffany**, Professor Emerita of Anthropology and Women's Studies at the University of Wisconsin–Whitewater, conducted her initial ethnographic fieldwork in Western and American Samoa. She currently divides her research time between the Margaret Mead Archives at the Library of Congress and fieldwork in a Zapotec village in the Valley of Oaxaca, Mexico. She has published extensively on Samoan social organization and issues of gender and representation.

**Virginia Yans** is Board of Governors Distinguished Service Professor, Department of History, Rutgers the State University of New Jersey. She produced and wrote a PBS Special documentary film, *Margaret Mead: An Observer Observed* (1997). The author of several articles on Margaret Mead, she is currently writing a personal and intellectual biography that examines Mead's collaborations with other social scientists.

## CONTRIBUTORS

**John S. Gilkeson**, Arizona State University. E-mail: john.gilkeson@asu.edu.

**Phillip Guddemi**, Union Institute and University. E-mail: pguddemi@well.com.

**Roger Lohmann**, Trent University. E-mail: rogerlohmann@trentu.ca.

**Maureen Molloy**, The University of Auckland. E-mail: ma.molloy@auckland.ac.nz.

**Judith Schachter**, Carnegie Mellon University. E-mail: jm1e@andrew.cmu.edu.

**Paul Shankman**, University of Colorado–Boulder. E-mail: paul.shankman@colorado.edu.

**Gerald Sullivan**, Collin College. E-mail: gsullivan@ccccd.edu.

**Nicola B. Tannenbaum**, Lehigh University. E-mail: nt01@Lehigh.edu.

**Caroline Thomas**, University of Waikato, Hamilton New Zealand. E-mail: C.Thomas@xtra.co.nz.

**Sharon W. Tiffany**, Professor Emerita of Anthropology and Women's Studies at the University of Wisconsin–Whitewater. E-mail: swtiffany@gmail.com.

**Virginia Yans**, Rutgers the State University of New Jersey. E-mail: virginiayans@earthlink.net.