

## INTRODUCTION: ON FOUR ANTHROPOLOGISTS AND THEIR HISTORIES

Virginia Yans<sup>1</sup>  
*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

---

*Pacific Studies*, Vol. 32, Nos. 2/3—June/Sept. 2009

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]looped 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.<sup>2</sup> 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.<sup>3</sup> 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).<sup>4</sup> 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.<sup>5</sup>

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 accommodationists 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 accommodationist 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 accommodationist 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”<sup>6</sup> 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 accommodationist, it is useful to ask of both historically inclined empiricists and postmodernists a question: What do they want to know about the past?<sup>9</sup> (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.<sup>7</sup>

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.<sup>8</sup> 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).<sup>9</sup>

### **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.



### ACKNOWLEDGMENTS

I wish to express my appreciation to the staff of the Reference and Reader Service Section of the Manuscript Division of the Library of Congress, and particularly to the late Mary Wolfskill, who passed her last years generously and intelligently assisting me and other scholars engaged with the Mead Papers. Mary Wolfskill's contribution presents itself in the published work of many researchers. I would also like to thank Gerald Sullivan and Sharon W. Tiffany for their exceptional collegiality and intellectual support during the writing of this Introduction. Funds from the National Science Foundation and Rutgers University supported the research for this essay.

### 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.

### REFERENCES

- Banner, Lois W.  
 2003 *Intertwined lives: Margaret Mead, Ruth Benedict and their circle*. New York: Alfred A. Knopf.

Bateson, Gregory

- 1936 *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*. Cambridge: Cambridge University Press.
- 1937 An old temple and a new myth. In *Traditional Balinese culture*, ed. Jane Belo, 111–36. New York: Columbia University Press. Originally published in *Djawa* 17 (5–6): 291–307.
- 1979 *Mind and nature: A necessary unity*. New York: E.P. Dutton.

Bateson, Mary Catherine

- 2005 Using and abusing the works of the ancestors: Margaret Mead. In *Reflections on Pacific ethnography in the Margaret Mead centennial, 2001*, ed. Sharon W. Tiffany. Special Issue, *Pacific Studies* 28 (3–4): 162–75.

Benedict, Ruth

- 1934 *Patterns of culture*. Boston: Houghton Mifflin.
- 1939 The natural history of war. Unpublished Manuscript. In *Writings of Ruth Benedict: An anthropologist at work*, ed. Margaret Mead, 369–82. London: Secker & Warburg.
- 1943a *Background for a basic plan for Burma*. Manuscript dated September 13, 1943. Ruth Fulton Benedict Papers. Special Collections. Vassar College Library, Poughkeepsie, New York.
- 1943b *Thai culture and behavior: An unpublished war-time study dated September [1952] 1943*. Data Paper no. 4. Cornell University: Department of Far Eastern Studies, Southeast Asia Program, February 1952. Third printing, May 1963.
- 1946 *The chrysanthemum and the sword: Patterns of Japanese culture*. Boston: Houghton Mifflin.

Boon, James A.

- 1985 Mead's meditations: Some semiotics from the Sepik, by way of Bateson, on to Bali. In *Semiotic meditation: Sociocultural and psychological perspectives*, ed. Elizabeth M. Mertz and Richard J. Parmentier, 333–57. Orlando, Florida: Elsevier.
- 1999 *Verging on extra-vagance: Anthropology, history, religion, literature, arts ... showbiz*. Princeton, NJ: Princeton University Press.

Caffrey, Margaret M.

- 1989 *Ruth Benedict: Stranger in this land*. Austin: University of Texas Press.

Caffrey, Margaret M., and Patricia A. Francis, eds.

- 2006 *To cherish the life of the world: Selected letters of Margaret Mead*. New York: Basic Books.

Caldararo, Niccolo

- 2004 War, Mead and nature of criticism in anthropology. *Anthropological Quarterly* 77 (Spring): 311–22.

Côté, James E.

- 1994 *Adolescent storm and stress: An evaluation of the Mead/Freeman controversy*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Crook, Tony

- 2007 A temperamental *ménage à trois*: Mead, Bateson and Fortune and the trafficking between social and analytical relations in ethnography. In *Anthropological knowledge, secrecy and Bolivip, Papua New Guinea: Exchanging skin*, 121–154. Oxford: Oxford University Press, British Academy Postdoctoral Fellowship Monograph Series.

Darnell, Regna

- 2006 Reflections on departmental traditions and social cohesion in American anthropology. *Histories of Anthropology Annual* 2:212–41.

Dobrin, Lisa, and Ira Bashkow

- 2006 “Pigs for Dance Songs”: Reo Fortune’s empathetic ethnography of the Arapesh roads. In *Histories of anthropology annual*, vol. 2, ed. Regna Darnell and Frederick W. Gleach, 123–54. Lincoln: University of Nebraska Press.

Freeman, Derek

- 1983 *Margaret Mead and Samoa: The making and unmaking of an anthropological myth*. Cambridge, MA: Harvard University Press.

Fortune, Reo

- 1939 Arapesh warfare. *American Anthropologist* 41:22–41.

Geertz, Clifford

- 1983 Blurred genres: The refiguration of social thought. In *Local knowledge: Further essays in interpretive anthropology*, ed. Clifford Geertz, 19–35. New York: Basic Books.

Gilkeson, John S.

- 2009 Clyde Kluckhohn and the new anthropology: From culture and personality to the scientific study of values. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 251–72.

Guddemi, Phillip V.

- 2009 The ecology of the anthropological mind: Gregory Bateson’s influence on three late twentieth-century Pacific scholars. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 392–417.

Handler, Richard, ed.

- 2004 *Significant others: Interpersonal and professional commitments in anthropology*. History of Anthropology, Vol. 10. Madison: University of Wisconsin Press.

- Howard, Jane  
1984 *Margaret Mead: A life*. New York: Simon & Schuster.
- Janiewski, Dolores, and Lois Banner, eds.  
2004 *Reading Benedict/reading Mead: Feminism, race, and imperial visions*. Baltimore: Johns Hopkins University Press.
- Jarvie, Ian  
1988 Comment on "Rhetoric and the authority of ethnography: Post-modernism and the social reproduction of texts," by P. Steven Sangren. *Current Anthropology* 29 (3): 427–29.
- Kennedy, Elizabeth Lapovsky  
1955 In pursuit of connection: Reflections on collaborative fieldwork. *American Anthropologist* 97 (1): 20–33.
- Lett, James  
1997 *Science, reason, and anthropology: The principles of rationale inquiry*. Lanham, MD: Rowman & Littlefield.
- Lipset, David  
1980 *Gregory Bateson: The legacy of a scientist*. Englewood Cliffs, NJ: Prentice-Hall.  
2003 Rereading *Sex and temperament*: Margaret Mead's Sepik triptych and its ethnographic critics. *Anthropological Quarterly* 76 (Fall): 693–713.  
2005 Author and hero—Rereading Gregory Bateson: The legacy of a scientist. *Anthropological Quarterly* 78 (Fall): 899–914.
- Lohmann, Roger Ivar  
2004 Sex and sensibility: Margaret Mead's descriptive and rhetorical ethnography. *Reviews in Anthropology* 33:111–30.  
2009 Dreams of fortune: Reo Fortune's psychological theory of cultural ambivalence. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 273–98.
- Lutkehaus, Nancy C.  
2004 Margaret Mead: Anthropology's liminal figure. In *Reading Benedict/reading Mead: Feminism, race, and imperial visions*, ed. Dolores Janiewski and Lois Banner, 193–204. Baltimore: Johns Hopkins University Press.  
2008 *Margaret Mead: The making of an American icon*. Princeton, NJ: Princeton University Press.
- Mead, Margaret  
1935 *Sex and temperament in three primitive societies*. New York: William Morrow.  
1949 *Male and female: A study of the sexes in a changing world*. New York: William Morrow.  
1972 *Blackberry winter: My earlier years*. New York: Simon and Schuster.

Mead, Margaret, and Gregory Bateson

- 1942 *Balinese character: A photographic analysis*. Vol. 2. W. G. Valentine, ed. New York: New York Academy of Sciences.
- 1951 *Trance and dance in Bali*. New York: New York University Film Library.

Molloy, Maureen

- 2008 *On creating a usable culture: Margaret Mead and the emergence of American cosmopolitanism*. Honolulu: University of Hawai'i Press.
- 2009 "More like fighting than like waiting": Mead, method, and the proper object of knowledge in anthropology. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 325–47.

Novick, Peter

- 1988 *That noble dream: The "objectivity question" and the American historical profession*. New York: Cambridge University Press.

Orans, Martin

- 1996 *Not even wrong: Margaret Mead, Derek Freeman, and the Samoans*. Novato, CA: Chandler and Sharp.

Roscoe, Paul

- 1995 Familiar partners? The Mountain Arapesh and the Westermarck effect. *Journal of Anthropological Research* 51(4): 347–62.
- 2003 Commentary: A response to di Leonardo. *American Anthropologist* 105 (4): 882–83.

Schachter, Judith [Modell]

- 1983 *Ruth Benedict: Patterns of a life*. Philadelphia: University of Pennsylvania Press.
- 2009 Writing lives: Ruth Benedict's journey from biographical studies to anthropology. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 348–66.

Shankman, Paul

- 2009a Derek Freeman and Margaret Mead: What did he know, and when did he know it? In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 202–21.
- 2009b *The trashing of Margaret Mead: Anatomy of an anthropological controversy*. Madison: University of Wisconsin Press, in press.

Stagoll, Brian

- 2004 Gregory Bateson (1904–1980): A reappraisal. *Australian and New Zealand Journal of Psychiatry* 2005 (39): 1036–45.

- Stocking, George W., Jr.
- 1986 Essays on culture and personality. In *Malinowski, Rivers, Benedict and others: Essays on culture and personality*, ed. George W. Stocking, Jr., 3–12. History of Anthropology Series, Vol. 4. Madison: University of Wisconsin Press.
  - 2004 A.I. Hallowell's Boasian evolutionism: Human ir/rationality in cross-cultural, evolutionary, and personal context. In *Significant others: Interpersonal and professional commitments in anthropology*, ed. Richard Handler, 196–260. History of Anthropology Series, Vol. 10. Madison: University of Wisconsin Press.
- Sullivan, Gerald
- 1999 *Margaret Mead, Gregory Bateson and Highland Bali: Fieldwork photographs of Bayung Gede, 1936–39*. Chicago: University of Chicago Press.
  - 2004a Of feys and culture planners: Margaret Mead and purposive activity as value. In *Reading Benedict/reading Mead: Feminism, race, and imperial visions*, ed. Dolores Janiewski and Lois Banner, 101–14. Baltimore: Johns Hopkins University Press.
  - 2004b A fourfold humanity: Margaret Mead and psychological types. *Journal of the History of the Behavioral Sciences* 40 (2): 183–206.
  - 2008 Three Boasian women: Margaret Mead, Ruth Benedict, and Ruth Landes. *Reviews in Anthropology* 37:201–30.
  - 2009 Of external habits and maternal attitudes: Margaret Mead, gestalt psychology, and the reproduction of character. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 222–50.
- Sullivan, Gerald, and Sharon W. Tiffany
- 2009 Guest editors' note. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 131–39.
- Tannenbaum, Nicola B.
- 2009 Ruth Benedict and the study of Thai culture. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 367–91.
- Thomas, Caroline
- 2009 Rediscovering Reo: Reflections on the life and anthropological career of Reo Franklin Fortune. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 299–324.
- Tiffany, Sharon W.
- 2001 Imagining the South Seas: Thoughts on the sexual politics of paradise in Samoa. *Pacific Studies* 24 (3–4): 19–49.
  - 2005a Contesting the erotic zone: Margaret Mead's fieldwork photographs of Samoa. In *Reflections on Pacific ethnography in the Margaret Mead centennial, 2001*, ed. Sharon W. Tiffany. Special issue, *Pacific Studies* 28 (3–4): 19–45.

———, ed.

- 2005b *Reflections on Pacific ethnography in the Margaret Mead centennial, 2001*. Special issue, *Pacific Studies* 28 (3–4).

- 2009 Narrative, voice, and genre in Margaret Mead's *Coming of age in Samoa*. In *Gang of four: Gregory Bateson, Ruth Benedict, Reo Fortune, and Margaret Mead in multiple contexts*, ed. Gerald Sullivan and Sharon Tiffany, special issue, *Pacific Studies* 32 (2–3): 163–201.

Williams, Elgin

- 1947 Anthropology and the common man. *American Anthropologist* 49 (1): 84–90.

Yans, Virginia

- 1986 Science democracy and ethics: Mobilizing culture and personality for World War II. In *Malinowski, Rivers, Benedict and others: Essays on Culture and Personality*, ed. George W. Stocking, Jr., 184–217. *History of Anthropology Series*, Vol. 4. Madison: University of Wisconsin Press.
- 2004 On the political anatomy of Mead-bashing; or, re-thinking Margaret Mead. In *Reading Benedict/reading Mead: Feminism, race, and imperial visions*, ed. Dolores Janiewski and Lois Banner, 229–48. Baltimore: Johns Hopkins University Press.

Young, Virginia Heyer

- 2005 *Ruth Benedict: Beyond relativity, beyond pattern*. Lincoln: University of Nebraska Press.