14. The Anthropologist's Voice: Margaret Mead and Donald Tuzin

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This chapter is dedicated, as is this book, to the memory of Don Tuzin, a great anthropologist who studied the Arapesh-speaking village of Ilahita in the Sepik region of Papua New Guinea. If I seem, at times, to be a wayward admirer, critiquing as well as giving homage, I hope that this will be taken in the spirit meant—as an essay presented in the absence of a great talk with Don, usually a brief time out from the chatter of professional meetings when, for a few moments, two voices might be heard speaking animatedly, sometimes hesitantly, grasping for the right word, seeking meaning, striving to comprehend a point, a parrying engagement of two individuals who have something to say and much in common—a dialogue. I did fieldwork only a few miles from Ilahita in the Abelam-speaking village of Apangai. This proximity meant that I engaged with Don's work in a terrain where similarities and differences in cultural forms could be, at times, closely compared. I, and the rest of us in this volume, will not have that conversational dialogue again; the text might remain, but the voice disappears, except in fragile memory—much to my great sadness.

The work of Don Tuzin (1976), as well as that of Anthony Forge (1966, 1970), cast a long shadow over my own fieldwork and subsequent writing (Losche 1989, 1997, 2001) about Abelam ethnography, not very far from where both men had carried out research before me. My own sense of the field varied so much from the canonical, magisterial textual accounts such as those of Tuzin (1976) that I faltered in confidence. I embodied, when I went to the village of Apangai in 1976 to do PhD research, a *zeitgeist* of doubt—almost entirely self-doubt. Having read the texts of those who had preceded me—great names of ethnography such as Gregory Bateson (1936) and Margaret Mead (1935, 1938, 1940), as well as Phyllis Kaberry (1941), Anthony Forge (1966, 1970) and Donald Tuzin (1976)—I was rendered almost paralytic with anxiety that the place in which I found myself seemed, as I phenomenologically experienced it, completely different, indeed almost estranged from, in no way commensurate with, the texts I read. So that those reading do not jump to the conclusion that I was entirely witless, I should mention that I was aware, even in my paralysing doubt, that the distance between the field as experienced and the text was great. The memories of those early moments have never left me, because I simply did not realise how remarkably long, arduous, and destructive as well as constructive was the transformation from field to text. The devil is in the

details as we all know, and in many cases it takes months, sometimes years—I now know through my own experience—to transform these experiences into written texts called ethnography.

The authors of these memorable ethnographies about the Sepik are not to blame for my uncertainties, for in the beginning I made a basic methodological error: comparing my experience with these polished, finished works. I learned the error of my ways only painfully, slowly, by my own experience of transforming field experiences to text, realising only then the enormous distance between the one and the other. Once, as I lamented the slow pace of my transformative efforts, a great teacher of mine, Robert Murphy, made the observation that the process of going from fieldwork to text took much effort and time because the process involved a form of ‘forgetting’—enough forgetting (perhaps the word would be better framed as synthesis) to see, as he put it, the forest for the trees, the form in the huge mass of detail. As the field of anthropology developed through the twentieth century, the emphasis put on the brilliance of the analytical framework grew and the conceptual distance between field and ethnography grew proportionately.

The narrative known as ethnology is at the core of anthropological activity, as any undergraduate major can tell you; it is that from which many other interpretations derive. It has also long been known to be fraught with interpretative problems. There are two moments of interpretative angst in anthropology, from both of which I want to distance my concerns—not so much because they were not useful debates in their time but rather because they have tended to involve the repetitive recitation of now banal insights, which, if they were once fresh and useful, are, by now, trite and clichéd. The first was of the sort embodied by the almost mutually contradictory descriptions of a Mexican village in the ethnographic work of Oscar Lewis (1960, 1963) and Robert Redfield (1930) and the far better-known imbroglio of the so-called Mead–Freeman (Freeman 1983) debate. If one has done fieldwork, one quickly tires of the simplifications to which the debates about these opposing pictures of a place, people and time descend. In this first moment, the question was not of the type of interpretation; rather the ethnography was subjected to a pseudo-scientific notion of a transparent reality, which must be found because it is there to be found. Here it is assumed that whatever the descriptive style of the author, somehow core truths of the social should emerge from the reality of the situation, and if one ethnographer challenges another then it is a matter of a truth versus a falsehood, as was belaboured *ad infinitum* in Freeman’s critique of Mead’s work in Samoa. Is ethnography description or interpretation? Patently it is both.

I also want to distance the concerns in this chapter from a second, more contemporary moment—that of the interpretative turn in anthropology,

embodied in essays looking at the rhetoric of ethnographic description laid out in volumes such as *Writing Culture* (Clifford and Marcus 1986). This particular phase of rhetorical investigation has a gladiatorial edge, and I am thinking here very much of Vincent Crapanzano's text 'Hermes' Dilemma: The Masking of Subversion in Ethnographic Description' in *Writing Culture* (1986). Here, the analyst seems to cling to some ideal of description that is never spelled out, and the analysis of allegorical moves in texts is seen as somehow undermining some utopian, but undeclared, transparently descriptive model. Identification of the gaps in the texts of other ethnographers seems to be a eureka moment for the rhetorical analyst, as if, using Occam's razor, one could not as easily subject their own text to the same form of destructive interrogation.

Both these moments have limitations imposed by the particular methods used to interpret the texts. In many of these analyses, a particular ethnographic text is lifted from a whole set of texts written by an ethnographer and dissected. Although lip-service might be paid to the fact that most ethnographers realise that many of their descriptions are somewhat provisional, nevertheless, each ethnographic description is simultaneously treated as a stand-alone project. In all of this contestation over the truth value of texts, nuances of interpretation are absent, and in this chapter I try to get at one such. I call it the voice of the anthropologist and suggest that we can look at texts as if they contain voices, which are like one particular tone that can suffuse a painting, adding a certain unifying hue to all of the many colours that make up the painting's palette. In the same way, I suggest that a certain voice can permeate entire sections of material, colouring, with one tone of voice, the many different types of cultural forms that an ethnographic work describes.

The characterisation of individual style has long been recognised, and some anthropologists, such as Clifford Geertz (1988) and Margaret Mead (1935), are well known for their brilliant individual styles. At the same time, this style is often greeted with some ambivalence, and brilliant stylists often attract particular attention, critical as well as admiring—as seen, for example, in critiques of Mead and Geertz (Crapanzano 1986; Lutkehaus 1995; Worsley 1957). The point is that style rests ambivalently in the anthropological endeavour, with its roots in the urge to be a science rather than humanity. There is little training in the writing of ethnography as writing *per se* in anthropological education, while there is much emphasis on the analytic frameworks needed to synthesise the vast quantities of material spewed forth in everyday life (for example, the structural analysis of ritual and myth). The problem with this lacuna is that there seems to be little understanding of the complex relationship between the field experience and the final texts produced by an ethnographer.

In this chapter, I want to complicate the notion of style and suggest that style is not simply a mode of writing across a range of texts that identifies an individual

author but that different moods or tones can occur in different texts of the same author. These moods often vary according to the circumstances and contexts in which the work is produced. I characterise this as a type of voice, and just as the voice of a person varies with context, so too can the mood of different ethnographic texts by the same author. I chose the term voice, as slightly different from style, to 'open up' the notion of textual stylistics, to allow for variability of tone in the texts of the ethnographer and to allow me, at least in this chapter, to move away from the analysis of ethnographic style as necessarily entwined with truth value and often at odds with it, as has often been done before. Here I want to liken the stylistic tone of texts to the voice of the anthropologist, as I remember the voice of Don Tuzin, in dialogue, as perhaps an unfinished conversation. As in conversation, the voice of the anthropologist is not the same in all texts, and it changes according to many circumstances including new knowledge, who is speaking, to whom one is speaking and a variety of other circumstances. Identifying voice allows new characteristics of texts to emerge and avoids the false idealism of some objectivity embedded in the distinction between description versus interpretation, literature versus science, and it will, hopefully, allow space for ethnographic writing to emerge as a genre separate from other written forms, with its own distinct characteristics.

In this chapter, I am seeking a particular kind of voice, one in which the author opens up his or her text and reveals doubt and uncertainty about the nature of the fieldwork that the author has already carried out. I sought out these texts not for the sake of critique itself but rather because such texts are extremely valuable, for they bear traces of that remarkable but usually suppressed process by which fieldwork becomes ethnography. This search is motivated certainly by my initial shock at the extreme distance between the experience of fieldwork—with its chaos, anxiety and uncertainty—and the finished text. I often wondered if there was any way to write so as to simultaneously present a powerful synthetic analysis and reveal traces of the process of transformation. At the same time, I sought these traces in the texts produced by others working in the same field. I have chosen to examine texts by Donald Tuzin (1980, 1997) and Margaret Mead (1935, 1938, 1940). Both were compelling writers, with extensive works about the Arapesh language group of the Sepik region.

Mead's style has often been commented upon (for example, Geertz 1988; Lutkehaus 1995; Worsley 1957), including by Tuzin himself (Tuzin and Schwartz 1980), in a somewhat awkward obituary piece attempting to characterise her work on the Sepik as a whole, and I am aware that Mead's celebrity/notoriety as well as her voluminous output can swamp other materials. Tuzin's work on the Arapesh is itself voluminous, however, and the juxtaposition of these two very different writers illuminates characteristics of what I call the 'voice' of the text. In this case, I want to compare Mead's much less known and cited work on the

Arapesh in 'The Mountain Arapesh' (1938, 1940) with her description of the Arapesh published in *Sex and Temperament in Three Primitive Societies* (1935). Donald Tuzin's tone in *The Voice of the Tambaran* (1980) is compared with that of *The Cassowary's Revenge* (1997).

Mead described *Sex and Temperament* as 'her most misunderstood book' (Mead 1950:Preface) and, like *Coming of Age in Samoa* (Mead 1961), it has been subject to scrutiny, criticism and doubt (Lutkehaus 1995). Critics have suggested that her description of three Sepik groups was 'subjective' and exaggerated certain features of the three—the Arapesh, the Tchambuli and the Mundugamor—specifically to illustrate elegantly her thesis that what she called 'sexual temperament', as differentiated from sex roles, varied in spectacularly different ways in the small but linguistically diverse region. In *Sex and Temperament*, Mead describes Arapesh culture in global, holistic terms, as the following quote from the beginning of Chapter 2, 'A Cooperative Society', demonstrates:

Arapesh life is organised about this central plot of the way men and women, physiologically different and possessed of different potencies, unite in a common adventure that is primarily maternal, cherishing and oriented away from the self towards the needs of the next generation. It is a culture in which men and women do different things for the same reasons. (Mead 1935:15)

And this is how she describes their political organisation: 'There are no political units. Clusters of villages are grouped into localities and each locality and its inhabitants have names' (Mead 1935:16).

Mead—once again glossing the Arapesh as a culture—goes on to suggest that one factor in the lack of political organisation is the Arapesh temperament: 'The whole emphasis of their economic lives is that of participation in activities others have initiated, and only rarely and shyly does anyone tentatively suggest a plan of his own...This emphasis is one factor in the lack of political organization' (1935:22).

As the above quote illustrates, Mead's voice is a very authoritative one in this work. Her descriptions of landscape are vivid and her portraits of individuals compelling. There is also a decisive clarity to her description and analysis; indeed, one of the strengths of the work is that the description itself embodies her interpretation so that every environmental, social and cultural feature is described in a way that adds to the cogency of her overarching theme about variations in sex and temperament. So vivid is her picture of place and people that the descriptive passages add to the convincing nature of her premises. This method is familiar in literature, especially fiction. This brilliant, unforgettable description is achieved, however, not only *via* precise descriptions of individuals

and the environment but also by broad glosses of large amounts of complex materials into succinct sentences, which by their very nature can only be broad generalities such as 'a lack of political organization'.

Sex and Temperament is not the only text in which Mead describes the Arapesh. Her more voluminous 'The Mountain Arapesh' was published in 1938, three years after *Sex and Temperament in Three Primitive Societies*. What is striking about 'The Mountain Arapesh' text in contrast with *Sex and Temperament* is the scrupulosity of Mead's statements and the care she takes in revealing the limitations of her research, text and methods. The volume opens with a section titled 'Method of Presentation' in which she sets out the limiting conditions of her text: 'The arrangement of any monograph is in itself a statement of method, but may be influenced by a large number of conditions extraneous to the author's methodological intentions' (Mead 1938:148).

She suggests that there were three such limits. The first was that this was part of a joint expedition with Reo Fortune, her former husband. What follows from this is that she will not be including material on issues such as men's initiation or language, both of which were covered by Fortune. A second limitation was the previous publication of *Sex and Temperament*, and she suggests she will be leaving materials covered there out of the Arapesh monograph (Mead 1938:149). Most interesting is Mead's caveat in her regional discussion about the influence her subsequent fieldwork among the Tchambuli and the Mundugamor had on her comparisons: 'The intra area comparisons are drawn, not from those cultures where they would be most illuminating but from those for which some details are at present available. This is a very important distinction in all comparative work, often ignored by advocates of the historical method' (1938:149).

Mead characterises the disproportionate amounts of material she has about the Arapesh compared with the Tchambuli and Mundugamor as a 'disharmony' and suggests that because of this, 'I have frequently overstepped the strict methodological requirements of the historical method' (1938:149).

In her introduction, Mead once again clarifies the necessary limitations of her discussion:

In cultures in which there is some correspondence between political and cultural boundaries and a degree of interchange within the area, permitting some degree of standardization, it is possible to write about their characteristics as if the observer stood outside and looked down upon a well-defined social system. Among the Arapesh, as among so many New Guinea peoples, this condition does not obtain. The individual Arapesh does not see his culture as a whole, nor does he distinguish the customs of his linguistic group from those of the adjacent

language group, if indeed he clearly notes his linguistic boundaries. Each local community, sometimes only a hamlet, sometimes several hamlets, occasionally three or four villages, presents an aggregation of widely diffused traits peculiar to it. From this narrow vantage ground each individual sees the behavior of members of neighboring communities as becoming steadily more diversified from his own as the distance increases between the communities involved. The ethnographer...who gathers every object found in an area and records the series of non-material traits observed among a people who speak the same language, and publishes these results as the 'culture' of the people who speak such and such a language, is doing great violence to the actual conditions. (Mead 1938:151)

Mead's text here—in contrast with *Sex and Temperament*—has a very careful voice, one that expresses doubt, hesitation and awareness of how her circumstances and methods affected her knowledge. Furthermore, she takes great pains to alert the reader to these limitations. Throughout this text comes a most refreshing sense of a person who is somewhat unsure of her ground, with as many questions as answers, trying to present answers but hedging these with questions. This exploratory and somewhat hesitant text is one that summons far more those doubts, hesitations and uncertainties that characterise the 'field' and those processes that are involved in the transformation to text.

In many of her other texts—both before and after this great work of Sepik ethnography—Mead seems to ignore her painstaking caveats about the difficulties of making generalisations about the area. Many of them fashion materials that, as she cautions against in *The Mountain Arapesh*, do seem to stand outside and look down on the system. Indeed, Mead herself had done this three years before *The Mountain Arapesh*, in *Sex and Temperament*, where, as we have seen, she glossed Arapesh culture with seemingly no concern for those important characteristics that make it so very difficult to gloss Arapesh culture.

We would be in a large company if we, like so many others, critiqued Mead for these apparent contradictions between one kind of text and the other, both based on the same field materials. There is no doubt, however, that Mead was quite aware of the very great differences in her two portraits. As Nancy Lutkehaus (1995:189–90) points out, Mead was one of the most self-aware of writers, one who paid careful attention to the differences that she felt were required in writing texts for different audiences. Mead herself would undoubtedly have pointed out that one text, *Sex and Temperament*, published in the United States by William Morrow, a major publisher of fiction and non-fiction for a large and so-called general audience, was a different proposition than writing a text published by the American Museum of Natural History, a small press that published monographs by scientific staff of the museum aimed

at a small, professional audience. What strikes me is how easy it still is to find *Sex and Temperament* in libraries and bookstores while *The Mountain Arapesh* has almost disappeared, even from academic libraries. Perhaps what ought to give us pause for thought is that, even in academic circles, Mead's *The Mountain Arapesh* is seldom referred to, analysed or quoted, while *Sex and Temperament* is often cited and critiqued. It has become part of her infamous celebrity-like profile, her signature and style, while her other texts about the Arapesh, which would challenge this view, are largely ignored. My point here is that not only authors create texts that establish the author's authority and command via cultural glosses and the suppression of all sorts of uncertainties; academia also institutionally privileges, especially via citation and critique, canonical texts. It was not Margaret Mead who turned *Sex and Temperament* into a canonical text but rather the volume's reception and institutionalisation.

Nor was it only Mead who ignored her own caveats about the Sepik area; so too did most subsequent anthropology of the area. By 1941, Phyllis Kaberry's early descriptions of the area around Maprik (a subdistrict headquarters of the Sepik region) glossed the area as home to the Abelam tribe with little caveat (Kaberry 1941). By the 1960s and 1970s, ethnographies such as those produced by Forge (1970) and Tuzin (1976) would routinely gloss certain areas as Arapesh and Abelam with little attention to variations within, or boundaries between, these groupings. I am not so much criticising this move as pointing out that it is the common ground from which texts arise. Indeed this crucial glossing of an area into 'cultures' defined according to linguistic criteria was, and remains, a crucial step in allowing communication between anthropologists—that is, the transformation of the inchoate into objects or units for comparison, study and analysis. That this involves seldom examined and intuitive transformations from field experience to text is an outcome, I suggest, of the peculiar conditions of ethnographic practice in the twentieth century, at least in the Sepik region. Ethnographers had to try to make sense of quantities of material gathered from particular, small populations against a backdrop of the relatively few comparative studies of the region that appeared sporadically and irregularly, in different languages and frameworks, and with almost no systematicity. Indeed, any apparent systematicity only grew out of the articulation of units, such as Abelam and Arapesh, which allowed anthropologists to speak to one another. Much of Donald Tuzin's work, like most ethnography of the Sepik, including my own, thus ignored Mead's caveat against standing outside, and looking down on, the system—but for very compelling reasons!

Thus, by the 1970s, when Don Tuzin began producing his major ethnographic texts on the Ilahita Arapesh (1976), the practice of glossing one's field experience into a unit was already well established. Tuzin's ethnographic writing glossed his experience, which was based in one uniquely large village of some 1500 people

called *Ilahita*, as a text about the 'Ilahita Arapesh' or 'Arapesh', and most of his writing stands outside and surveys this unit, little troubled by the doubts about method that Mead had voiced in her *The Mountain Arapesh* material. Tuzin was a writer who could equal Mead in creating a compelling text, although his 'voice' is very different than either her careful, almost uncertain voice in *The Mountain Arapesh* or her more seamless texts such as *Sex and Temperament*. Tuzin's textual voice is often magisterial, authoritative and very dramatic, creating a sense that we are in a grand narrative of great historical moment. With one exception, his texts seldom introduce caveats, unanswered questions or doubt. These absences, as well as the magisterial tone, create a canonical text about a cultural system. Here as elsewhere, Tuzin writes with a compelling narrative urgency that, as much more commonly in fiction, leads the reader to want to find out what happens next.

In *The Voice of the Tambaran* (Tuzin 1980), his second full-length ethnographic work, which describes the *Ilahita Arapesh* initiation structure, Tuzin begins each chapter with a myth—a device that operates as a code or key to understanding aspects of the initiation system discussed in the chapter. The opening chapter begins with the famous myth of Nambweapa'w. It takes up eight pages, and Tuzin provides two alternative endings. He does not explain the reason for including these alternative endings, nor does he specify who provided the particular version included in the book. In a footnote, he refers to 'The story teller', as if the storyteller were a single person, but this is not elaborated on (Tuzin 1980:7). Having conducted fieldwork in a nearby but different language, and having gathered a similar though truncated story, I have methodological questions that are seldom answered in Tuzin's book. How many people tell this exact version of the story? How widespread is the knowledge of this particular story? Is this a gloss of many versions or one of the longest versions?

Following the myth of Nambweapa'w, Tuzin continues to create a seamless cultural gloss and to make broad generalisations about the *Ilahita Arapesh*:

Nambweapa'w crystallizes for the Arapesh a sense of themselves in relation to (and in mythic priority over) a larger humanity. The Tambaran, largely by virtue of its participation in the same array of existential themes, likewise serves as a summary symbol of Arapesh cultural identity. When the two images are brought together under ritual auspices, the complex wedding of male and female, past and present, creation and destruction—all raised to mythic proportions—yields a transcendent meaning of supreme cultural value. (Tuzin 1980:10–11)

This characterisation of Tuzin's technique of broad glossing is not intended as critique, for indeed I have done the same myself and not as well as he did. Indeed, his text is an extremely good example of the type of ethnographic text

exciting to other anthropologists, as was Mead's *Sex and Temperament*, because of an elegant aesthetic, compelling narrative structure and bold synthesis. It might not address questions that would be raised in postmodern discussions of ethnographic texts (Clifford and Marcus 1986), but *Writing Culture* was published six years after *The Voice of the Tambaran* (Tuzin 1980). My point is that Tuzin's is an outstanding and inspiring example of an ethnographic text of its time, as brilliant in its style as was Mead's, and like that volume it ignored, for purposes of the aesthetics of exposition, issues that would obscure what the author considered his most important points. Like *Sex and Temperament*, Tuzin's texts create a powerful synthesis from the motley assembly of field experience, deliberately putting aside some of the fascinating questions raised by Mead in *The Mountain Arapesh*. Many of Tuzin's texts on the Arapesh reflect this seamless quality, where the singular eye of the anthropologist sweeps up irregularities, limitations and variations into summary statements. There are many who would claim that such synthesising activity is the most important aim of ethnography. It was only in the 1980s that some of the concerns voiced by Mead in the 1930s began again to be aired in anthropology. As Lutkehaus (1995:190) has pointed out, Mead's concern with writing led her to observations about narrative styles and audience reception that predate recent critiques of ethnographic texts.

Like Mead before him, Tuzin, in one of his ethnographies, has a voice strikingly different than that in the others. This is his final book-length ethnography of the Ilahita Arapesh: *The Cassowary's Revenge* (1997). This volume, like his earlier Ilahita Arapesh work, is also a compulsively readable narrative with an extremely dramatic tone. The Preface introduces the drama:

This book is about something that happened in Ilahita, a village in the interior lowland of northeastern New Guinea. The year was 1984. The event was a murder, not a senseless anonymous killing, but a conspiratorial act carried out by men with a purpose. The assailants had known their victim all their lives and most of the time regarded him with profound respect, possibly even love.

The shocking truth is the killing was a parricide. (Tuzin 1997:ix)

In the book, Freud and the Old Testament are summoned as interlocutors, and Greek models, such as Oedipus, hover over this story of the destruction of the men's cult by Ilahita villagers, adding to the drama and sense of the historical importance of the events being described.

Tuzin, however, enters this ethnography as a character, as he finds his own, very personal fate has become entwined with the story of the destruction of the

cult of the *Tambaran* in Ilahita. In his Preface, Tuzin not only opens up for the reader a gap created by his own involvement but also frames this involvement as necessitating a structure of multiple narratives:

The third narrative is awkward to tell, for it is about my return to Ilahita after an eventful absence of thirteen years and my disturbing, uncanny involvement with the recent collapse of the men's cult. Swept into the current of events and fantasies, this return encounter rewrote the significance of my original visit, merging it into a stream of prophecy to which the ethnographer was now a reluctant, hapless contributor... The telling of what happened in Ilahita requires then, three different arguments, three different voices, three different domains. (Tuzin 1997:x)

The Cassowary's Revenge makes for compelling reading particularly because of the shock with which Tuzin recounts how his earlier fieldwork had been incorporated, very prominently, into the history that led to the downfall of the men's cult. He confesses to his own stake in the men's cult during 13 years' absence from the village, during which his own professional activities were heavily invested in describing and analysing it, with his volume *The Voice of the Tambaran* entirely given over to it: 'The intervening thirteen years of writing, lecturing and nostalgizing added up to an emotional investment in all that the villagers were seeking to destroy' (Tuzin 1997:5).

Much of Chapter 1, titled 'Going Home', describes the circumstances of this unwitting involvement in the demise of Ilahita's *Tambaran* cult. This is a complex story in which Tuzin's own cancer (Hodgkin's disease)—diagnosed while preparing his PhD subsequent to his first period of fieldwork (Lipset and Roscoe, this volume)—became known to villagers through letters, and along with the later death of a close informant also from cancer became entwined with the demise of the *Tambaran*. In describing these events, Tuzin broaches the complexities of ethnographic fieldwork in terms much more familiar to contemporary anthropologists than Mead's earlier angst over the limitations of her observations, trying to conceptualise his particular difficulties about the ethnographer's involvement in a particular place and to make important points regarding the nature of the ethnographic enterprise:

In all the recent attacks on ethnographic objectivity, no one as far as I know has remarked on the difference between going to the field for the first time and returning there after a lengthy absence...Both situations may involve observer bias, but returning to the field entails a larger, more complex hazard because it means tampering with a relationship that already exists laden with emotion, personal history and moral ambiguity. (Tuzin 1997:5)

Tuzin not only narrates the story of his role in the events he describes, he also conceptualises and comments on the processes involved in the rethinking necessary when the unexpected causes one to reframe earlier work, producing a meta-theory about the processes of long-term fieldwork in an area:

Rediscovering, restarting, revisiting, reconsidering, folding back—these are the convolutions of symbolic process, and therefore these must be the apparatus of understanding. Ethnographers return to the field and modify the meanings of previous facts in the light of intervening events; cultures return to their mythic charter and unwittingly edit it to accord with historical experience; individuals return to places of precious memory, thinking to capture some fragment of the experience—never to succeed. (Tuzin 1997:xi)

The lodestone that Tuzin suggests for conceptualising the problems of his own experiences is memory—a recent and powerful entry to the arsenal of analytic frameworks used to conceptualise historical change at an institutional and individual level. He suggests that memory will be the touchstone that unites his three narratives—personal and collective—and that will provide the framework for analysing the problems made evident in his earlier understandings of Ilahita, made evident only on his return after 13 years:

Always one must go back, searching for what things of the past ‘really’ meant as a clue to present realities. Going is never the same as going back. Going is mundane, going back is mythic, because it confronts the self in memory. Indeed, in a curious way that poets understand, it is through memory that past and present are finally reconciled, finally realized, and the future is made possible. Individuals relate to themselves in this way, and so do cultures. (Tuzin 1997:xi)

Here, in a most poetic voice, Tuzin is suggesting a methodological solution to the problem of the very disparate pictures that emerge when one does long-term research in an area.

In *The Cassowary’s Revenge*, Tuzin returns to the story of Nambweapa’w, the cassowary myth that occupied such a central role as a charter for the *Tambaran* in his earlier volume. He acknowledges some of the difficulties (not explored in the earlier text) he had with conceptualising the role of the Nambweapa’w story in *The Voice of the Tambaran*: ‘The truth was that I did not know what to say that would do justice to the story’s intuitive importance. Additional years and events have clarified and, in curious ways that this book tries to explain, realized and fulfilled those intuitions’ (Tuzin 1997:71).

Three chapters of *The Cassowary’s Revenge* are devoted to the Cassowary myth, and in these Tuzin is far more meticulous in outlining the questions of variation

that the myth, as deployed in *The Voice of the Tambaran*, had raised for me. I suggest that the involvement of his personal narrative in the events described in *The Cassowary's Revenge* also sensitised him to give greater attention to issues of irregularity, variation and uncertainty in a variety of the cultural phenomena found in Ilahita.

The point of this chapter has been to suggest that, rather than lifting particular texts out of an entire body of work and arguing the validity of a particular ethnographic point vis-a-vis the facts of a particular field site, we look across the entire body of work of an ethnographer, giving attention to a variety of circumstances surrounding the production of different kinds of texts, which, as I have tried to demonstrate here, vary in mood, tone and voice. These tones cast particular cultural phenomena into remarkably different kinds of forms, from the nature of the social units under discussion and how broadly the anthropologist can generalise from a very small sample, which troubled Margaret Mead, to the variations in myth that Tuzin took as a central issue in *The Cassowary's Revenge*.

In the introduction to this chapter, I mentioned two modes of analysis from which I distanced the concerns of this chapter: the critique that privileges de-contextualised social fact, and the interpretative turn, which ignores the issue of the truth value of social facts but critiques the rhetorical narrative modes by which the author of a text constitutes him or herself as an 'authority'. Both these forms of critique, I have suggested here, ignore the complexities of both fieldwork and ethnography, including the many 'moments' of this arduous and often lifetime venture. As Donald Tuzin suggested, going back is quite different from going. I suggest that in 'going back' to the texts of anthropologists who have gone before us we listen to the questions they raised in their own 'going back'. The insights that both Mead and Tuzin gained from their own revisions are some of the most valuable of their contributions to the ethnography of this area, and they provide glimpses of that arduous process by which the 'field' becomes the 'text'. This chapter has examined two authors who produced texts that bear the marks of uncertainty and hesitation—characteristics that, under a variety of compelling professional forces, are often lost. It is intended as homage to those works, with all their caveats and hesitations, for they bear the trace of the anthropologists' voices, which I imagine I still hear.

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