THE SORROW OF THE LONELY AND THE BURNING OF THE DANCERS

EDWARD L. SCHIEFFELIN

SECOND EDITION

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Preface to the Second Edition

The material for this ethnography was gathered over a period of 26 months of doctoral research undertaken in Papua New Guinea in 1966–68. The first edition was published in 1976 and remained in print for 25 years. Over that time, the discipline of anthropology has gone through many changes and it is worth pausing at the beginning of the second edition to consider the book from the perspective of its time and from the present. What were the questions that inspired it? What kind of context motivated it? What tasks did it set itself to accomplish? and finally, What does it still have to offer?

Origins and Motivations

Before I came to the field of anthropology I studied science and philosophy. The questions that eventually led me to travel to New Guinea came originally from this context. Simply stated they came down to two: The first question arose from a keen interest in epistemology, the inquiry into the nature of human knowledge and the conditions that make knowledge possible. Could a single universal theory of knowledge account for all human knowing, as Kant believed when he proposed his universal categories of the understanding? Or did people of different cultures not only have differing customs, beliefs, and bodies of knowledge, but fundamentally different modes of organizing

their understanding as well—derived, say, from the structure of their societies (as Durkheim believed) or the organization of their languages (as Whorf and Sapir hypothesized)? Or could it be, as Jung's work suggested, that the basic categories of knowledge were not wholly cognitive or even logical at all? These questions were not entirely new to scholars in anthropology, but in the early 1960s, they had not been actively pursued for a long time.

The second question derived from my interest in existential philosophy, with its focus on fundamental human concerns: the problem and meaning of freedom, the life of the spirit, moral responsibility, love, the encounter with Others, with the Gods, and with Death. Surely, I thought, these issues must be shared and basic for all humankind—yet it seemed clear that people in different cultures framed and lived them very differently. These reflections, seen in the light of my first question, gave rise to the second: Was there a connection between different modes of apprehending reality on the level of the organization of thought and the different ways in which people lived out the fundamental concerns of their everyday lives? How did a people's basic mode of knowing affect their fundamental concerns in life—and how, conversely, did their life experience affect their fundamental frameworks of thought?

While these questions were important philosophically in their own right, I found myself much more drawn to what they might mean for actual human beings trying to live real lives in the world. Anthropology, with its close, long-term scrutiny of everyday human life in different cultures, seemed the proper (and perhaps the only) place to pursue them.

I started graduate study in anthropology in the early 1960s. These were restless times in the United States. The Cold War was at its height. America had recently risked a nuclear war with the Soviet Union over the Cuban missile crisis. Not long afterward the president was assassinated. The Johnson administration was now embarking on a serious escalation of the Vietnam War. At the same time, young people were joining the Peace Corps and fanning out across the third world to help the poor—while at home, the Civil Rights movement was resolutely forcing liberal reform. A counter-cultural movement was taking shape

that had a profound antipathy to a way of life that seemed driven mostly by the desire for money and power. This restless confrontation of old-line anticommunist militarism, feisty reborn democratic idealism, and activist moral determination generated an atmosphere of heady excitement in many American universities. Change was in the air and, for the moment, there seemed a real possibility that the actions of ordinary people could bring about a more just and humane society.

At the University of Chicago, anthropology was also undergoing a process of ferment. Structural functionalism, which had reached the zenith of its influence in the 1940s and 50s, was falling out of fashion. In many respects, this was not because it failed to say important things about human life and society, but rather because, broadly speaking, it did not easily accommodate individual agency or historical change. Its proclivity to see human societies as relatively unchanging systems of functionally interrelated parts supported a view of society grounded in a timeless status quo. This could not sit well with the dynamic inspiration of the times. Many anthropologists simply became impatient with it and began to look outside the bounds of traditional social science perspectives for new ideas and directions for the discipline.

Of those at the forefront of this movement, I found the work of Clifford Geertz, Claude Levi-Strauss, Gregory Bateson, and Victor Turner particularly inspirational. These scholars (variously inspired by Oxford Language Philosophy, literary criticism, Saussurian linguistics, Durkheimian sociology, cybernetics, and philosophy of mind) were exploring ways of understanding human culture that emphasized the importance of symbolically constituted systems of meaning as much as structures of social relations. They were writing about the ways in which human worlds were constructed as domains of knowledge, as cultural systems structured by systems of symbols. The exploration of how human social experience is woven within systems of symbolic meaning seemed to open a new and powerful way to investigate fundamental questions about how people mutually constructed the religious, moral, and existential worlds in which they lived, and how, in turn, their worlds lived in them.

I was drawn to this philosophically inspired, broadly humanistic approach, because it seemed to offer the best avenue to

understanding other cultures at the level of indigenous knowledge and social experience. It led naturally to the formulation of epistemological kinds of questions on the one hand, and existential ones on the other—to an understanding of the ways in which people constructed their life worlds, and what it was like to live in them. The new thinking enabled philosophical questions to engage with ethnographic ones at the level of the social construction of reality. My philosophical interests slowly refocused into an anthropological research agenda. If human social experience was symbolically constituted, I wanted to explore the process by which this came about and how it worked in an ethnographic context. This was the avenue to understanding culture at the level of the constitution of social experience as well as the structures of social relations.

I proposed to explore the ways a people's manner of thought and belief affected the way they experienced their particular human condition and also how that condition and the conduct it demanded shaped them, the kinds of experience they had, and the kinds of people that they were. To do this, as a matter of research, I wanted to be sure that I was not unwittingly rediscovering Western patterns of thought and practice—whether due to projection from myself (which I hoped I could discipline), or because, due to historical (usually colonial) circumstances, they had already been imposed from outside upon the people with whom I was working. For this reason, it seemed important to locate a people who had relatively little experience of Western (colonial) activity.

New Guinea

The history of anthropology provides a number of examples of ethnographic regions, which, for a time, are particularly productive of interesting insights and theoretical advances in the discipline. From the late 1930s through the 1950s this "hotspot" was Africa. In the early 1960s, however, the area of interest was beginning to shift to Melanesia.

This development was largely an outcome of the re-opening of New Guinea following the Second World War. After the Japanese defeat in 1946, the Australian colonial authorities resumed

their efforts to complete the exploration and consolidation of the territories of Papua and New Guinea under a UN mandate. By the early 50s, government patrols were fanning out into the last areas of unvisited mountains and 0tropical forests. They were soon followed by pioneering anthropologists seeking uncharted ethnographic territory. Significant publications of new findings based on their research began to appear by the early 1960s.[1] These, together with the discussions already in the air, prompted intense debate and re-thinking of a number of traditional anthropological problems (cf. Barnes 1962). The new findings from New Guinea evoked a sense of new horizons to discover and there was great excitement among young anthropologists preparing for the field. Many of the newly opened areas had had very little contact with the outside world. If such people as I sought were to be found, I thought, this was likely to be the place.

The region I decided upon for my research was part of an enormous area of dense tropical forest called the Great Papuan Plateau on the southern borders of the Southern Highlands District—about 6 degrees below the equator. This area had been first contacted by government explorers in 1935 and 36 (Hides 1936, Champion 1937, Schieffelin and Crittenden 1991) but in the subsequent thirty years had been briefly visited by only eight government patrols. It had been declared pacified only two years before I arrived. I settled in among a people on the northern foothills of Mt. Bosavi who called themselves the Kaluli. My home was a one-and-a-half-room thatched house built by the people of clan Bonɔ, about forty yards from their main longhouse at Sululib, three day's walk from the nearest government station.

Fieldwork

Anthropologists sometimes compare their first fieldwork experience to a rite of passage, a period of enforced isolation

[1] Influential works produced around this time would include: Berndt 1962, 1964, Glasse 1959, 1962, Langness 1964, Meggett 1959, Lawrence and Meggett 1965, Reod 1952, 1953, 1965, Reay 1959, Sahlins 1963, Salisburg 1962, and Watson 1964. A more complete account of these pioneering ethnographers may be found in Hays 1992.

and life-changing experience that affects one for the rest of his or her career. My fieldwork in 1966–68 had something of that quality. There were hardships and discomforts, but being in the tropical forest, getting to know the people there, and learning about their lives was a tremendous life-opening adventure. It is sometimes difficult in the case of any given fieldworker to assess the effect that living in the field has on him or her or on their thought. For me it was profound. I was forced to re-consider a great deal of what I thought I had learned about social anthropology. A good deal of what this book became, as a piece of ethnographic research and writing, derives from the quality of that field experience.

Two months after arriving in the field and hard at work with the difficulties of the language, I was invited to attend a major Kaluli ceremony, the Gisaro. I was quite unprepared for the impact of this performance—its beauty, its grandeur, its passion, and its violence. I was deeply affected by it. Something profound and important to the Kaluli people was being worked out here, and I realized that it was fundamentally important to understand what it was. I began to think of Gisaro as a kind of reference point around which to weave the various directions of my research.

As I became more familiar with people's lives—began to understand how they felt about things and why they reacted to circumstances in the way that they did—I became increasingly perplexed as to how I could possibly capture an account of their life experience in the categories and constructs of the social anthropology of the time. The life I saw in the longhouse, in the forest, and in the grand ceremonies, the life I found myself immersed in every day, had a special dynamism and vitality. How was I to convey these people's voices or communicate their substance and energy as anthropological analysis?

After some months I made a decision to set my anthropological training aside for a while and "let the ethnographic material unfold in its own way" without preconceptions. This idea, which probably comes to every new fieldworker at some time or other, was of course, by present-day standards, naïve. Ethnography does not "unfold itself" in some transparent way—nor can ethnographers, in the end, entirely escape their cultural backgrounds or intellectual traditions. Nevertheless, I found that

the disciplined attempt to temporarily ignore the received body of anthropological theory served its purpose. It allowed me to be more open to my own intuitions of where the interesting aspects of Kaluli culture lay and gave me the intellectual space to consider new ways of thinking about it.

It quickly became clear that in order to accomplish what I wanted to do, the passionate and performative dimensions of Kaluli life would have to be brought into central focus and given some kind of theoretical encompassment. The effort was to see them as constitutive of a particular human condition and experience while holding them in relation to the structures of social relations that gave Kaluli society its shape.

I thought I might be able to achieve this by focusing investigation not so much on structures of social relations as on forms of social process. This meant that, besides the usual data on kinship and social organization, I began collecting material on the "narrative" aspects of behavior. That is, I began to look at social processes as possessed of a sequenced, dramatic shape that had its own internal structure and strategy, and that generated its own expectations, tensions, and resolutions quite apart from the social relations that were played out through them. It became increasingly clear to me that a good deal of people's knowledge, including that of the structure of the social and spirit world, was located in their unselfconscious proclivities to handle situations, to present themselves, and to achieve resolutions, in certain particular ways. Moreover, focusing on Kaluli culture through its processual aspects made it inevitable that I pay attention to the trajectories of people's feelings and emotions as well as their actions. Nearly all social processes entailed some emotional orchestration, buildup, tension, and release of emotional energy, in the very way they worked. All these issues would have to be encompassed not only in the kind of material I gathered in the field, and the way I theorized it, but also in the manner in which the ethnography was written.

Writing

I have always believed that ethnography fails in an important respect if it does not evoke for the imaginations of its readers

the image of a living people and a feeling for the way of life they experience. Given the way I wanted to understand the material, such evocation became also a theoretical necessity. It wasn't just that the description of Kaluli life should (as a matter of principle) retain their humanity, account for what their lives meant to them, what moved them, and why they felt and acted as they did. Given my focus on experiential reality and social processes, I *had* to organize my ethnography around making sense of these issues. The dynamic surfaces of the people's ordinary lives became, in a sense, as important as their depths.

The problem was to find a way to theorize social process. I formulated the notion of "cultural scenario" to denote a culturally constituted (but not necessarily conscious) procedure or "way of doing things" that was customarily followed (in this case by Kaluli) to define the issues in problematic situations and carry them to resolution. Once the initial moves were made, the process unfolded, guided by an underlying strategic cultural logic to reach a culturally acceptable completion or outcome. The idea had something in common with what Bourdieu was later to characterize as the "conductorless orchestration" of practices (Bourdieu 1977). It also implied an ethnographic perspective close to social constructionism in which social structures are approached from the perspective of the strategic processes that bring them about, through which they are reproduced, and through which they (and their effects) are emergent.

I decided to present this ethnography, then, as much through a discussion of socially generated practices and performances, and the qualities and social orchestration of feelings and emotions, as through its social and political structures and strategies. This itself presented certain difficulties. To communicate the quality and tone of Kaluli feeling in situations, it was not enough to describe Kaluli life analytically; I would have to try to *evoke* it for my readers.[2] In part I accomplished this through quoting directly from my informants and through various poetic strategies

[2] The use of evocation is potentially a tricky business in the discourse of a discipline that aspires to rigor. It entails moving away from objectification, toward empathy and participation. It is a potentially problematic attempt to move the reader into imaginative participation with the Other's experience—at least so

in the writing itself. But I also tried to ground my account in the realities of my own field experiences, bringing my own presence in the field into the story in those places where I felt my personal experience would further the purposes of ethnography. I also felt it was important to convey the fact that ethnographic research is neither a matter of strict scientific objectivity, nor of personal confession, but of disciplined observation, dialogue, and critically scrutinized reflection. At the same time, I wanted to convey something of the excitement, adventure, and fascination of being in the field.

I decided to organize the writing of the ethnography around the Gisaro ceremony. This was in no small part because I found this ceremony so remarkable in itself. But it was also because Gisaro seemed to exemplify and entail, on both symbolic and social interactional levels, the major social processes that made Kaluli society and social experience what it was. It was also a cultural performance that Kaluli themselves found splendid and moving. As such, it was an excellent point of reference and departure for a discussion leading to the heart of the issues of social process in which I was interested. This focus on a dramatic and spectacular element of Kaluli culture may seem old-fashioned to some anthropologists today. But, it has to be said that by Western standards, the Kaluli are an expressively passionate people, and this has had real consequences both for their everyday lives and over their historical past. Moreover, passion is integral to those social processes that it is the purpose of this ethnography to encompass.

Since Then

Any anthropologist looking back at something he or she published almost 30 years ago will view it critically both from the perspective of his or her subsequent research and from the hindsight of a discipline whose theory has moved on. One looks to discover the mistakes made, the (now) obvious areas left unexamined,

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far as it could be grasped by the ethnographer. Difficult, but, I thought, not completely insurmountable, and, to make sense of Kaluli sentiment and feeling, essential.

and the lacunae that appear when the discipline has advanced and new interests and agendas become central to the field that didn't exist (or were muted) before.

Looking at this volume now, perhaps the most obvious and important omission, from the vantage point of the present, is the absence of the voices and perspectives of women. This is also apparent in the decidedly (if unconsciously) male voice of the text. The rise of feminist theory and the understanding of the importance of gendered perspectives in anthropology did not fully dawn in anthropology until several years after this fieldwork was completed and this book was in press. Like many male anthropologists at the time, I did not work very much with female informants (and indeed it was awkward and difficult for a man to do so at the time). Nonetheless, this has left a gap and an imbalance in the ethnography that I am now unable to amend.

On the positive side, although many of the ideas and approaches I experimented with in this book are no longer as new as they once were, many of them have subsequently become central to the concerns and practices of mainstream anthropology and continue to inspire research and writing to the present. The interest in the social construction of emotion (Abu-Lughod [1987], Levy [1973], M. Rosaldo [1980, 1984], R. Rosaldo [1980, 1984], Lutz [1988], Lutz and White [1986], Kleinman and Good [1985]), the links between the poetics of song, memory, and place (Feld [1982], Feld and Basso [1996], Weiner [1991], Gell [1995]), the investigation of the performativity and practice of ritual and social life (Geertz [1980], Kapferer [1983], V. Turner [1982, 1988], Schieffelin [1985]), the interest in scenarios (or schemas) (Shore [1995], V. Turner [1974], Ortner [1978, 1989]), plus the reflexive self-placement of the ethnographer in the ethnographic narrative (Marcus and Fischer [1986], Jackson [1989], Clifford and Marcus [1986]), all these have become more or less mainstream agendas and practices.[3] Although it would be absurd to claim these trends found their original inspiration in this book, they represent

[3] This names but a few of the well-known authors pioneering these trends. The full bibliography is, of course, much more extensive.

developments in the field in which this book has from the beginning played an important part.

Since the early l970s the Kaluli have received ethnographic attention from other workers, resulting, notably, in important and innovative books published by Steven Feld (1982) on Kaluli music and poetics, and by Bambi Schieffelin (1991) on child language socialization. At the same time a considerable amount of high quality ethnographic work has been undertaken among adjacent peoples of the Papuan Plateau and the Strickland Bosavi Plain. Immediately north of the Kaluli area Ernst (1984) worked among the Onabasulu people. Northwest of them, Kelly (1977, 1993) and Dwyer (1990) worked with the Etoro (Etolo). To the West Sørum (1980) has studied the Bedamini, Knauft (1985) the Gebusi, and Shaw (1990) the Samo.[4] To the east, Wiener (1988) has worked with the Foe, while Kurita has published on the Faso (Fasu). To the south, Brunois (2001) has recently completed a disseration on the Kasua. This body of literature, focusing variously on issues of social structure and demography, conflict, initiation rites, ideology, and human ecology, provides a rich variety of different subjects, styles, and perspectives. *The Sorrow of the Lonely and the Burning of the Dancers* continues to be one of a very few ethnographic studies that approach a people's way of life from the perspective of their fundamental cultural scenarios.[5]

In the final analysis, no ethnography of a people can ever be wholly comprehensive. This is not only because no ethnographer could ever exhaust the material in any culture in a lifetime of fieldwork. It is also because no ethnographer can ever fully escape the perspectives of the culture and historical epoch in which they work. But, in addition, ethnography itself is the study of a moving target from a moving vehicle. Over time the people anthropologists study change, the perspective of anthropologists themselves change with further research, and the

[4] This paragraph cites only the major ethnographic volumes by these authors. It does not include the large number of articles in journals and edited works.
[5] Among the modest number of important ethnographic and theoretical writings that have advanced this approach, the most notable are Ortner (1978, 1989), V Turner (1974, 1982) and, most recently, Shore (1995).

discipline itself moves on. And after a few decades, ethnographic accounts begin to transform into historical ones.

Despite this, what all ethnographers strive for is that their accounts of the people they work with remain fundamentally fair and accurate representations of the way their lives were at the point of the field encounter as understood with the best use of the intellectual tools then available. My own subsequent research among the Kaluli together with that of colleagues Bambi Schieffelin and Steven Feld show that this book holds up well as an account of the Kaluli people in the late 1960s. Inevitably there are awkward places where the text hasn't aged gracefully, but apart for some minor corrections and clarifications, I have left the original very much as it was.

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Kaluli Pronunciation

a	as *a* in f*a*ther
e	as *e* in b*e*t
o	as o*a* in bo*a*t
ɔ	as o*u* in o*u*ght
i	as *i* in mach*i*ne
u	as *u* in fl*u*te
æ	as *a* in h*a*t
š	as *sh* in *sh*oe
ɨ	as *i* in s*i*ster