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AS A MATTER OF FACT...

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and

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THE SIN

Jarvie (1964) once suggested that patricide was a major trend in the development of anthropological theory: In "killing off" our "fathers" (by destroying their theories) we progress and develop. The patricidal process incorporates a handy guilt-release and guilt-negating mechanism. As we either systematically or sporadically demolish the theoretical contributions of our ancestors, we simultaneously heap praise upon their ethnographic contributions, and we laud their legacy of first-rate ethnographic data. Our consciences thus assuaged, we return to the grim task of completing the patricide.

There is no such relief or expiation for those who question the data of others. Fratricide is an entirely different business than patricide. We cannot praise someone's theory if we judge his data faulty. Anthropologists build theory out of facts, and when the facts are questionable, the edifice of theory has shaky foundations. Hence, when we question someone's data, we commit a sin with no redeeming features. We allow no way to salvage the image of the attacked (and the attacker); someone, somewhere along the line, must be factually wrong. Being factually wrong differs from being theoretically incorrect. The latter has the saving grace of being a matter of interpretation, while the former has no saving grace whatsoever. Wrong in fact is wrong: wrong in interpretation is a matter of opinion. Or is it?

Perhaps the difference between patricide and fratricide (or sororicide) can account for the relative absence of disagreement about data as opposed to the quite frequent arguments over theory in anthropology. Perhaps, as a species, anthropologists shy away from those situations (i. e., fieldwork in the same locale) which would lead to attacks on members of the same generation, and which might thereby reduce the viability of the population as a whole. We use a fine sense of territoriality to assure reproduction of the species. Our demise must be at the hands of our children, not through our brothers and sisters. We have a tacit understanding to assure our survival through reproduction: you don't haunt my territory and question my data, and I won't invade yours.

Occasionally, though, the unspoken territorial agreement is violated, and the struggle for space begins. The requirements of the species dictate an exceedingly

low population density: anything over that minimal maximum and things can get vicious as competition for available resources (informants, information, the "truth") intensifies. The struggle is cast in a temporal, as well as spatial, framework. Ghosts of fieldworkers past in the same locale come to haunt their spiritual siblings but temporal descendants.

We found ourselves in an incredibly muddled andovercrowded situation in Kilenge. Not only did we have ancestral siblings, but we also had real live siblings as well. Behind us, on the mountain, stood a great ethnographic unknown, yet on the beach, we dwelled in an ethnographic milieu well-represented in the literature. As our field work progressed, and later as we analyzed our data and began the tedious process of publication, we found ourselves contributing to the population of ghosts as we too committed, in a gentle but nonetheless remorseless fashion, the unforgivable sin of fratricide. In the following pages, we will explain our actions to our living brethren, and through explanation seek expiation for our sins.

THE FIELDWORKERS

Since the characters in our story are just as significant as the locale, we depart from standard anthropological procedure and introduce them to the reader before we describe the setting.

Kilenge has seen a steady stream of anthropological fieldworkers in the past two decades. As far as we have been able to ascertain, there was no intensive ethnological investigation carried out in Kilenge prior to 1964. A. B. Lewis did visit the area in 1909, but his stay was brief (a matter of hours) and resulted in one or two achingly brief paragraphs in his diary (see Lewis ms.). The first fieldworker to stay for any length of time was Philip J. C. Dark, an anthropologist working out of Southern Illinois University. Dark's background at the time was in African art. He was accompanied by a colleague, the linguist Joel Maring, on his first visit. The research period was two months.

Dark later returned to Kilenge with his wife for an extended stay in 1966 - 1967, and finally for a short time in 1970. In 1967, he was joined by Adrian Gerbrands, of Leiden University, an ethnographer and student of art with experience in Irian Jaya. Gerbrands returned to Kilenge in 1973 to continue the production of ethnographic films on Kilenge dances, and then went again for a short time in 1978.

David and Dorothy Counts came to Kilenge for a few months in 1976, after their third research sojourn in Kaliai. Their aim was to collect comparative materials, particularly myths, legends, and stories.

The present writers arrived in Kilenge in 1977 as graduate students. We stayed for 10 months, doing general ethnography and several specific projects related to social change. We departed early in 1978. We returned for a 10 week period in 1981-2 to update material on change and to study child-rearing and child-related ceremonies.

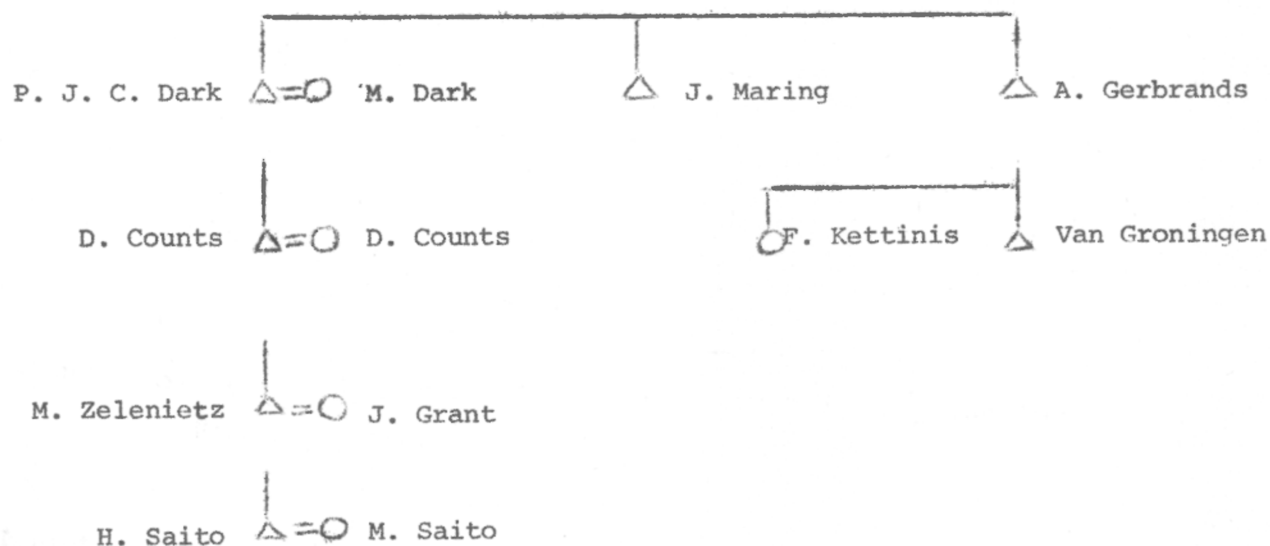
The cast is not complete. About the time we arrived in 1977, another field-worker came on the scene. Frances Kettenis was an M. A. student from Leiden, and had come under the auspices of the Education Research Unit of the Department of Education in Port Moresby to study mission education and traditional counting systems. Ms. Kettenis was in the area for six months. She was joined by another M. A. student from Leiden, Derk Van Groningen in August 1977. Van Groningen was in Kilenge for intervals from August 1977 to February 1978.

Finally, we have Hisafumi and Machiko Saito, who were in the area for 2½ months in 1981-2. Saito was a visiting doctoral student at the University of Papua New Guinea, and studied business groups in Kilenge.

To put this all into quasi-genealogical terms, and to get the relationships straight, we have created Figure 1.

> Dark and Maring came to Kilenge. When he returned, Dark was accompanied by his wife, and later joined by his friend and colleague, Gerbrands. Dark's student Counts, along with his wife, went to Kaliai several times. Eventually, Counts and Counts saw the light, and went to Kilenge. Counts sent his student, Zelenietz, to Kilenge, and Zelenietz brought his spouse, Grant, Gerbrands sent his students Kettenis and Van Groningen to Kilenge. Zelenietz and Grant returned to Kilenge, and Zelenietz brought his student, Saito, who brought his wife.

Figure 1



THE SETTING

Lest readers fear that a tiny village of 50 people was continually overrun by a horde of anthropologists in hot pursuit of data, .please be reassured that there are currently over 1000 Kilenge residents in a number of social units on the north-west tip of the island of New Britain. Let the readers also be reassured that even the seemingly straight-forward questions of "who and where are the Kilenge?" and "how many Kilenge villages are there?" are problematic and subject to debate in the literature.

Dark (1973: 50; 1974: 13) seems to consider all "Kilenge-speaking" people to be Kilenge. He further divides the population into the Kilenge, or Natai, the coastal dwellers (more or less), and the Lolo, or Nalolo, the bush-dwellers (more or less). He labels the language they speak as Kilenge[1]. Dark's coast/bush distinction more or less holds up, with some reservations (mainly regarding Lolo settlements that have moved to the beach in the historical past). The boundary between the Kilenge and the Lolo is more than a mere geographical division: while they share the same basic language and culture, the Kilenge and the Lolo differentiate themselves on the basis of dialect, cultural peculiarities, settlement, location, and marriage patterns. If we can accept the conclusion that the Kilenge are the beach dwellers and the Lolo are the bush dwellers (at least in the historical past, much evidence (see Zelenietz 1980) suggests that within the last 200 years the Kilenge moved down off the mountain onto the beach), then just where are the Kilenge villages and how many are there?

The Kilenge currently live in a cluster of villages about 4 km. along the coast of northwest New Britain (at 5° 29' South, 148°22' East) facing the islands of Sakar and Umboi. In the very recent past (until WWII) Kilenge settlements were more dispersed, but a series of natural and manmade disasters reduced both the population and the number of settlements.

> Before the Pax Germanica, the Kilenge lived in semi-autonomous hamlets, each formed around its naulum, or men's house. Hamlet clusters were named; the names have continued as village names, resulting in confusion regarding the precise number of villages present in Kilenge. The government administers the area in terms of four villages (from southwest to northeast): Potne, Ongaia, Ulumaienge, and Saumoi-Waremo. Dark (1973; 1977) notes five villages: Portne, Kurvok, Ongaia, Ulumaiinge, and Waremo. For distributions of pigs and ceremonial foods, the Kilenge count either five or six villages/village sections: Portne, Kurvok,[5] Ongaia, Ulumaienge, Varemo and Saumoi. In daily conversation and action people refer to and act in terms of

[1]We found that both the Kilenge and the Lolo referred to their language as Maleu.

> three villages: Portne, Ongaia and Kilenge proper. Communal work for the government or mission is divided on the basis of these three villages, and each village acts as an independent unit with independent responsibilities in this work. When one village is the site and sponsor of a performance of a ceremonial cycle (narogo), the hosts speak of inviting people from the other two villages. Traditional activities requiring pan-village cooperation, such as major pig hunts or fishing expeditions, pulling new canoe hulls from the bush to the ocean, or major ceremonial events, also occur in terms of three villages. (Zelenietz 1980: 22-23)

So we see that the term itself, Kilenge, can confuse rather than clarify the situation. It can be used in a geographical sense to refer to:

(a) the whole Kilenge/Lolo Census Division (as Dark does, but this is not a common usage)

(b) the coastal area defined above (common usage).

The term can also be used in a population sense to mean:

(a) all Maleu speakers in the Kilenge-Lolo Census division (uncommon);

(b) all speakers of a particular dialect of Male'u who live in the cluster of villages called Kilenge (common);

(c) all residents of the village known as Kilenge (uncommon).

The referents of the term are patently a matter of interpretation, but we should keep in mind that the Kilenge seem to know what they mean by the term when they use it. Our general practice has been to follow local usage, specifying the parameters of that usage so that readers can follow our meaning.

Non-debatable (or shall we say agreed-upon) features of the Kilenge setting allow the following thumb-nail description:

> The Kilenge are swidden horticulturalists who make their gardens on the footslopes of the extinct (dormant?) volcanoe, Mt. Talave. They raise domesticated pigs, important for ceremonies, and also hunt and fish to supplement their diet of taro, yam and other tubers and root starches (sweet potato, manioc, etc.).

That is about as far as we can go in quick ethnographic description without entering contested ground.

THE PROBLEMS

It seems that most of the articles which we have written on the Kilenge fall within the fratricidal category, in that they dispute previous findings or offer alternative explanations. In order to keep this paper brief, however, we will focus only on three of the topics: artistic production, migration, and social organization.

1. Artistic Production

Before we went to Kilenge we read Dark's Kilenge Art and Life and saw one of Gerbrand's dance films, so we were prepared to find a people for whom art was an integral part of life. It isn't that Dark or Gerbrands ever said that art played the central role in culture, but we almost expected it from the importance granted art in their work. Instead, we found people leading rather mundane lives without bothering to be very aesthetic. There was little carving, little painting. Artists had little importance in society, unless they were also good carpenters. (Grant and Zelenietz 1978).

2. Migration

When we began to gather employment histories in Kilenge, we had no clear idea of what we would do with them. We used the interviews to familiarize ourselves with the men of the village. Eventually, though, we developed a paper on wage labor migration in which we suggested that migration served as a kind of rite de passage for young men on the road to adulthood (Grant and Zelenietz 1980). Van Groningen, who was in Kilenge for part of the same period as us, suggests that wage labor migration has an adverse impact on economic development (1979), whereas we argue quite the opposite. While he fears that it removes manpower and skills from the village, we conclude that it reduces intergenerational conflict within the village and limits pressure on land resources; hence it does not interfere with horticultural productivity.

3. Social Organization

On the question of social organization, we find ourselves in disagreement with all of the other Kilenge ethnographers, save the Saitos. Where others see the Kilenge as patrilineal (sometimes with moieties, patrisibs or totemic-like groups), we describe a society with cognatic descent, ramages, and patrilocal residence. None of the previous ethnographers have taken social organization as their focus of concern, and all describe Kilenge social structure only in passing on the way to other topics.

The central question which we must consider in this entire discussion, is why do differences in interpretation occur? Why does Dark see a society where artistic production has a pivotal function while we find a dearth of art production? Why does Van Groningen suggest that Lae is increasing in importance as a migration destination, while we focus on the special role accorded the Gazelle Peninsula in migration ideology? How is it that we see flexibility in a social organization which others perceive as rigidly defined? We believe that there are a number of reasons for such radically divergent views.

a. Different informants

In Kilenge, as in other societies, particular informants convey particular images of their own society. Each individual has, in his own mind, a model of society which approximates "reality" from his perspective. We found this out within weeks of arriving in Kilenge. Our initial discovery of this manipulation of the "truth" left us distraught: how could we know whom to believe when everyone told us a different story? We had come to live with a group of would-be heros who had to be the central figure in any story told. But we soon learned that by asking for enough versions of any story we could eventually build a composite which we believed to be instructive (if not necessarily accurate). Asking for many perspectives on any issue became a firm policy with us. In all major research problems that we investigated, we tried as hard as possible to interview all adult males in the village so as to leave no stone unturned, no version unheard.

This problem of information manipulation by informants affected other researchers as well. One rather naive ethnographer stood up in a village meeting and announced that people had been lying during interviews and that sort of behavior was not acceptable. It is not surprising that villagers were insulted and henceforth reluctant to interact with the ethnographer. In Kilenge you do not publicly call a man a liar unless you are openly fighting or quarelling with him. People avoid blatantly contradicting each other whenever possible. For example, one man told us that the Kilenge always inherited from their fathers; six other men who were present said not a word until thesspeaker left. Then they told us, "Don't believe that liar, we inherit from our mother too." Our analysis of land use patterns confirmed the denial, but to have told the man so to his face would have been a breach of etiquette.

From an analysis of Dark's work, and from the reminisences of villagers, we conclude that Dark's access to information was strictly controlled because he worked in Kilenge at a time when a very influential man ruled the area. Aisapo, the paramount luluai, kept tight reins on the Kilenge, and it was he who determined access to Dark. Aisapo's desire to keep Dark (and his wealth) to himself kept Dark from forming close relationships with most Kilenge villagers. Dark had a house outside the village, not far from Aisapo's house, and the artists were brought to him there. Dark's image of Kilenge society and Kilenge art obviously reflects Kilenge images of themselves in a very different world. Aisapo bent the rules of Kilenge land tenure, inheritance and descent to suit his own desires (see Zelenietz 1980). In the post-Aisapo period, the Kilenge are trying to unravel his work and reshape their social structure.

b. Different interests and Backgrounds

It is a truism to say that the ethnographer often finds what he expects to find, but it is a point well worth remembering.

Sometimes, one has to look hard to find it. When we, as field-fresh raw graduate students at our first international conference, allowed that artistic production in Kilenge had certainly declined since Dark had done his research (Grant and Zelenietz 1978), we were shocked to hear Dark admit that it probably hadn't. He told us that art production had declined long before his arrival, and that he had "stimulated" artistic activity. We simply found the relapsed patient.

But this wasn't our first eye-opening experience. In the field, we had met Derk Van Groningen. When he arrived, he told us that he had come to study migration. We gently suggested to him that since we had already gathered considerable data on the subject, he might find it productive to either change his topic or change his fieldsight. The ensuing discussion showed us that our data did not fit with his theory. However, Van Groningen felt he had expended too much time and effort in study and in the preparation of his model to consider a change of project, even in light of data which differed from his theoretical projections. Small wonder, then, at the radically different published views of Kilenge migration. (Grant and Zelenietz 1980; Van Groningen 1979).

Dark found unilineal descent and patrimoieties. He had previously worked in Africa and was undoubtedly familiar with unilineal descent models and totemic groupings. That is what he describes for Kilenge. The subsequent Dutch M. A. students supported his findings.

We went to Kilenge expecting to find unilineal descent. Our first two months of field notes make frequent reference to "clans." However, we found ourselves getting increasingly frustrated in trying to make sense of group identity and membership in terms of a unilineal descent paradigm. But it was only in delineating the Kinship terminology system that it finally dawned on us that we weren't dealing with unilineal descent at all. Perhaps we came to that conclusion in part because each of us had previously studied cognatic systems: the Inuit of the Canadian Arctic and the Kibbutz of Israel. At any rate, our further inquiries confirmed our hypothesis, and we henceforth challenged other interpretations. (see Zelenietz and Grant ms.)

c. Time perspective

In part the differences which appear in different ethnographic interpretations reflect the problem of what time frame is being discussed. Obviously, as anthropologists we are interested in attempting to recreate the past as well as describe the present. But that tends to be confusing and often conjectural. Dark

falls into the trap in some of his work: In three sketches of Kilenge Social Organization, (1969, 1973, 1977), he only once mentions that the system he describes is not really to be found today (1969: 81). We also liberally illustrate our points with references to the past. It is in these attempts to recreate traditional practices and ideals that differences of interpretation reflect most the ethnographers' preconceptions. In another sense, the passage of time does not offer an adequate explanation for the divergent accounts of Kilenge social organization. Subjectively, we feel that less than a decade is an inadequate amount of time to account for a total change in principles of social organization and people's behaviour based on those principles. This is not to say that people do not change the way they organize themselves: they can do so, deliberately or inadvertantly. We have no way of predicting what an ethnographer will find in Kilenge 20 or 50 years from now, or even that he or she will find Kilenge.

It is conceivable, though, that unilineal descent will come to Kilenge. In 1977, no one used the Tokpisin expression "klan" (clan) to refer to social units. By 1982, it had entered local parlance, albeit sparingly. Discussions with people who used the term showed us that they had no idea at all that it referred to unilineal descent groupings. It is not hard to picture a government official entering the area, hearing the term, and assuming that the Kilenge have, in truth, clans. After all, the old patrol reports speak of patrilineal descent. Furthermore, government agents may find it more convenient to deal with the relatively simple structures implied by "klan" than to explore the ramifications of cognatic descent: unilineal descent groups make land dealings so much easier, and the government has been known to impose such structure on cognatically-organized people, either through ignorance or for convenience (Mitio 1981).

The Kilenge are not a people who believe that the past, and all that is inherited from it, is sacrosanct. They have seen enough in the last eight decades to take to heart the lesson that change will happen, and come what may, they can not stay the same. There are too many paths to follow, too many decisions to make, for their world to remain static. We wish the best of luck to those who will follow us there, and hope that they can demonstrate that at least part of our work was accurate, as accurate as ethnography, the product of ever-changing minds and circumstances, can be.

THE POSTSCRIPT

Hatchet jobs, no matter how well-intentioned or gently done, are never pleasant.

The personally damning and damaging incidents that transpire in anthropology are passed through the generations as anecdotes, oral traditions never committed to paper where they can take on a life of their own. But in the process of oral transmission, context can become lost, motivation obscured. In violating a cardinal (unspoken) principle in anthropology, in writing down anecdotes, we have no wish to malign anyone. What we seek is shared understanding of the whys and wherefores of theoretical, empirical, and methodological differences.

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