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OVERVIEW

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FORGING AN APPROACH

The last 50 years has seen anthropology become part of the intellectual baggage of American and western European thought. The media, novelists, literary critics, historians, politicians and political scientists, development economists, and religious leaders speak of culture, cite ethnographic examples, and bolster their arguments by invoking the names of anthropologists. Social workers and educators report their research as based on "the ethnographic method" and "the holistic approach." At a time when so many nonanthropologists think they know what anthropology is, it is disconcerting to realize how little many of us in fact have in common, and the extent to which we have forged our own approaches through questions that have seemed to us worth pursuing.

I see certain consistent assumptions informing whatever I have written as an anthropologist during the last 50 years, whether it has been on contemporary American society, including the bureaucracy of development agencies (11), American Indian reservations (4), the political order of an Australian city, or various aspects of the life of the Tonga-speaking people of the southern Zambian Plateau and the Gwembe Valley. Whether I have written about rituals or residence patterns (7), familial relationships (5), social controls (5, 10), shifts in drinking behavior (13), or the consequences of forced resettlement (9, 12, 40), I have assumed the transiency of social forms and a higher degree of freedom of action than many anthropologists appear to grant to whoever it is they define as "others." With his persistent probing about why I asked certain kinds of questions and formulated my research as I did, Asraf Ghani recently helped to put all this into perspective during three days of intense discussion.

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In the first place my central focus is on how people behave and the relevance of that behavior for their own further actions and the activities of others. I assume that they are pursuing goals and have choices, and that these are limited both by their own skills and by the fact that they compete with others in a social universe where access to power is unequal. People act in an opportunistic fashion, which does not mean that they have no regard for others or that they lack generosity. But they are not bound to a code that tells them what must be done. They act and then explain their actions, to themselves and others, in universalistic terms to justify what was in fact contingent. Since the conditions under which choices are made shift from one moment to the next, all action in a sense is exceptional and can be justified as such. The universalistic terms are what James (26:6) has called "an archive" that constitutes "a lasting base of past reference and future validation." They provide continuity because they are an assurance that a normative order exists. While these assumptions derive in part from my training in anthropology they have been reinforced by the people among whom I have carried out research. It would be instructive to examine the extent to which our anthropological theory reflects the assumptions of the people with whom we have worked. The humans I have known have been skeptical, empirical, and prepared to take what they regarded as acceptable risks. At the same time they have wished to be regarded with respect, and to think of themselves as worthy of respect, and so they have explained both themselves and others in terms that make mutual interaction possible, understandable, and, it is hoped, justifiable.

Despite the hypotheses of linguistic philosophers and the more extreme arguments of cultural relativists, I think it possible to communicate across a barrier of different experiences and to understand about as much of a code as is understood by native users of that code. Indeed, under the probing questions of outsiders, native users may make explicit to themselves for the first time matters that have never previously been publically discussed and in so doing discover how much their interpretations differ. We can communicate across barriers of experience because we share pretty basic desires and fears; the fact that we need contact with others to survive provides common experience of the problems of social interaction. If we can never penetrate completely the thinking of someone from another society, neither can their nearest and dearest. People respond to what they believe others are thinking; it may be fortunate that they do not *know* what the other thinks. Social life depends on face saving and the tacit agreement that understanding is mutual.

Anthropological data come primarily from the observation of living people who are getting on with life and making decisions that have implications for the future. Our discipline is not about some reified entity called culture. We do not have anything like a holistic approach and it would not do us much

good if we had. For one thing, good research is based on a sense of what is important, and this means selectivity. For another, whatever it is we do observe, it is not an integrated culture or an integrated social system. Of course it may be more comforting to talk in terms of cultures rather than people, for while culture can be destroyed—and it is painful to think of lost art, lost music, or lost social conventions—culture cannot be hurt. People can be, and horribly so.

One way to hurt people is to destroy their familiar environment so that they can no longer achieve what they value, a message driven home by anthropologists who have worked with the organization Cultural Survival. Pain can likewise be inflicted by physical torture, which has become the resort of many contemporary regimes, sometimes tutored by American officials. This is now the background experience of many who once hosted anthropologists, some of whom have been tortured while others have become torturers. In the past, and probably for good reasons, including the protection of human subjects, we downplayed the violence, cruelty, and unhappiness existing in the areas where we worked. One reason may have been the belief that such actions were momentary departures from cultural norms that generated long-term harmony, but whatever the reason, by doing so, we falsified the record.

If anthropology has a rationale beyond the description of a particular moment in the history of a given community, then it lies in trying to understand the range, bitter to sweet, of human experience. Thus there are good theoretical as well as ethical reasons for looking at what is happening now to those who once provided us with hospitality and accepted our request to be taught what they knew. It is not enough to turn anthropology into a critique of its own past or to define it as the study of the structuring of myth, art, or other forms of discourse. Torture, too, is a form of discourse. I am not arguing here against the relevance of studying myth and other forms of art. Kapferer (27), after all, has demonstrated how Sri Lankan and Australian myth and ritual are invoked and transformed to create environments wherein ethnic violence, including murder, becomes acceptable.

If anthropology is more than a game or an academic discipline that provides employment to anthropologists, then at least some of its research effort should be focused on policy environments that lead to the impoverishment both of other countries and of our own people, and create the political chaos that currently has driven 10 million refugees into flight across international borders while an unknown number are displaced and homeless within their own countries. Many people are trying to flee killing poverty across the barriers set up by governments now fearful of their own prosperity and the reaction of their citizens to the stranger. Still others are uprooted because big development projects are financially profitable to a powerful minority, and multinational companies can take advantage of the situation. The political and

economic choices of people everywhere are formulated within an international community based on the interplay of nation states and multinational corporations (16).

At various times and places it has been fashionable to deny that anthropologists may legitimately advise programs that work to bring about change. Such a role, it is claimed, attacks the cultural integrity of a community and may lead to new gross inequities as people become dependent upon relationships with the world economy. Applied anthropology, action anthropology, development anthropology, and practicing anthropology—whatever it is called—has also been characterized as "mere application" that leads to no new theoretical advances. I can understand skepticism about the impact that anthropologists can have on the implementation of programs based on nonanthropological priorities (11). But the second claim, that anthropology is advanced only to the degree that its practitioners remain detached from world events, has never seemed to me to have a reasoned basis. Theory, after all, should inform practice; when theory fails, practice has a chance to inform theory.

Sorting over data in academia does not automatically produce new theory; in fact, it can be a dead end. What counts is quality of thought, and this is likely to be stimulated by new experience, whether through the acquisition of new data or through brainstorming with people who, while pursuing different questions, query whatever the anthropologist says. I was therefore delighted when in 1988 the president of the American Anthropological Association, Roy Rappaport, created a number of panels to examine how anthropologists can help to tackle current issues in both industrial and nonindustrial regions of the world. This action encourages anthropologists to involve themselves in an examination of contemporary life.

One of my own long-term concerns (9, 12, 40) has been with the consequences of forced resettlement. Once resettled, people can no longer rely upon survival strategies worked out in their old environments. These prior habitats may have been destroyed by large-scale irrigation schemes, urban clearance, or warfare; they may have been placed out of bounds by political disorder; or they may now be inaccessible for other reasons. Though some people move as communities, they must deal with the loss of the old setting and the outmoding of skills. How does all this affect their relationships with one another, their trust in political leaders, their explanations of misfortune, and their belief in an orderly universe? Who emerges empowered? How long does it take to reestablish confidence in one's ability to cope with an environment that must be learned afresh? What measures by external agencies help or hinder adjustment? Thayer Scudder, who has collaborated with me in the longitudinal study of the Gwembe people displaced by the creation of Kariba Hydroelectric Dam and Kariba Lake, has looked at resettlement schemes

throughout the tropics and is now able to predict the response over time to displacement (37, 38, 40). That work has had some impact. Recently the World Bank has issued guidelines to be followed when it agrees to finance major projects that will result in the displacement of large numbers of people. These guidelines require the same attention to the social effects as to the technical problems of construction (3). Many of the same issues are relevant to the work of the Refugee Studies Programme in Queen Elizabeth House, Oxford University, founded by Barbara Harrell-Bond, an anthropologist who has studied at first hand the impact on refugees of relief measures fielded by international organizations and the multitude of voluntary organizations specializing in humanitarian assistance that are now endemic to the so-called Third World (23). We are now at a stage where it is possible to see the commonalities among the experiences of those who flee as refugees, the victims of large-scale forced resettlement carried out by governments in the interest of economic gain, those who find their communities destroyed by urban renewal projects, and the old who are forced from their homes into care-centers by the logic of public health systems (22, 34). It is possible to analyze their experience as a process in time, with reactions appearing in a predictable sequence as "displaced people strive to maintain whatever power they can" (21:14) in an attempt "to recreate a social world which would offer the highest degree of safety and control" (29:241 15).

ACADEMIC STIMULUS

My current work is a culmination of years of exposure to teachers, colleagues, and those who have educated me during the course of field research. I have also had to do a good deal of thinking about the environments within which I have worked.

In the late 1930s when I began graduate work in anthropology at the University of Minnesota, the world was in the midst of a depression and fearful of the outbreak of further war in Europe, where the Spanish Civil War had just ended. Refugees arriving from Nazi Germany were a reminder of how easy it was to become vulnerable to an oppressive political regime. The economic chaos of the depression and growing recognition of the savageries being carried out in Germany and other European countries undermined belief in the inevitability of progress and the superiority of Western civilization.

Like most anthropology students, I had chosen the subject for the love of it, not because I expected that it would lead to a job. (A joke at the time was that one needed a PhD to find a job as a dishwasher.) It is difficult to remember, however, what it was that made us love anthropology, other than the fact that it seemed to provide a powerful critique of the world as we knew it—particularly of the social rules that confined us. But we could also believe that

anthropology had an important role in encouraging tolerance of differences and in combating racial and ethnic prejudices. It was also an exciting time to be a student. The discipline was being transformed as anthropologists experimented with participant observation in a great many different contexts, which gave them a lively awareness that the field dealt with the very stuff of human life. A few anthropologists were doing research on the social life of American cities and towns, providing a critique of accepted views on the nature of American society. A number were looking at the influences of the reservation system on the formation of 20th century American Indian culture. Others, inspired by Freud and the neo-Freudians, were engrossed with questions of the interplay between personality and culture.

Students in that era had an advantage over those entering the field today because anthropologists were few and the theoretical issues that divided them were matters of common discussion. The number of monographs and journal articles appearing annually was small enough so that we could be expected at least to look at them. This still left time for us to read widely in other fields. At Minnesota (1935–1940) and Radcliffe (1940–1941, 1943–1944), David Mandelbaum and Clyde Kluckhohn encouraged me to read in the psychoanalytic and social psychology literature since I then planned to work on personality and culture, using life histories as a data base. I discovered Moreno, later known for the psycho-drama but then examining social dynamics through methods that led on into sociometry. That work was a predecessor of both small group dynamics and network analysis.

Equally important for my own development was the field theory of Kurt Lewin, who conceived of human behavior as a response to current forces operating within a given environ
Coser (14:15) has suggested that Lewin was strongly influenced by his own experiences as a refugee, since refugees "are compelled to cultivate a detached form of knowledge that will help them along paths not marked by traditional signposts. They are structurally compelled to use analytical reasoning, where for the insiders custom and habit are sufficient guides to conduct." Nevertheless, such an approach was appealing to a midwesterner who shared the typical American belief in the importance of the individual and the possibility of exercising choice in a world that was not predetermined. Trying to think in Lewin's terms, however, encouraged skepticism about current theories of "culture." These either dealt with culture as a thing in itself (whether it was seen as an integrated whole or a compound of shreds and patches) or, following Sapir, reduced culture to the realm of the individual. Skepticism about the utility of "culture" as an analytic concept was not lessened when I worked as Clyde Kluckhohn's research assistant while he and William Kelly wrote their critique of culture theories (28) for a volume edited by Ralph Linton entitled *The Science of Man in the World Crisis*. If culture

was problematic, then so was Herskovits's concept of acculturation (24) and Malinowski's views on culture change (31): It was never clear what interacted with what.

Later, I found new problems with the treatment of cultures as things in themselves when I found anthropological formulations of culture to be similar to arguments used in South Africa to justify the color bar with its denial of civil rights to Africans and to entrench European domination. This was evidence of the power exercised by conceptual frameworks, for the belief in cultures as enduring entities was a common heritage from Herder and other German romantics of the early 19th century. That same belief had empowered the ethnic nationalisms of Europe. Wilson Wallis at Minnesota earlier had aroused my interest in the intellectual ancestry of the discipline. He stressed the influence of Locke, Hume, Ferguson, and other moral philosophers of the 17th and 18th centuries upon the development of a discipline that examined the interplay between the givens of biology and the freedom to create displayed by human inventiveness (44).

In the early 1940s Kluckhohn was already involved with Talcott Parsons and Henry Murray in the interchanges that led to the creation of the Social Relations group at Harvard after World War II. By that time I was gone, but it was Talcott Parsons who introduced me to Max Weber and the vocabulary of social action (35). His structural-functionalism was stimulating but too rigid for the direction in which I was developing. Durkheim's work, especially his concern for a collective morality, was more difficult to come to terms with, and I have found generations of American students responding in much the same fashion. It was Balzac's novels that gave me an inkling of the massive questioning of order, including the categories of time and space, in revolutionary France, which provided the background to Durkheim's concerns. Since then, the struggle of new African governments to create a national consciousness and give their regimes legitimacy has made the Durkheimian legacy more relevant.

THE IMPACT OF FIELD WORK

My first field work was with Native Americans, the Pomo of northern California in the summers of 1939, 1940, and 1941. My supervisors were Burt and Ethel Aginsky, who had founded the New York University Field Laboratory for Research in the Social Sciences. Field schools were a characteristic of the period, but the Aginskys may have been unique in recruiting students from sociology, psychology, and literature as well as anthropology and encouraging us to examine the interplay among townspeople, ranchers, migrant agricultural workers, and Pomo. It was a dynamic situation with strong overtones of caste. The older Pomo told of being hunted

down for internment at Round Valley and returning to find their lands preempted by whites. Since then their lives had been dominated by the multiracial society, and that domination was strongly reflected in the life histories of the Pomo women with whom I spent much of my time during those summers (8).

Dissertation research at Neah Bay, Washington followed (1941–1942). During the course of that year, the town doubled in size as construction workers arrived to build a naval base and soldiers came to guard the seafront against a possible Japanese invasion. My field data inevitably reflected a period in the history of the town that all knew to be ephemeral, though it would have consequences for the future. This experience convinced me of the importance of grounding ethnographic description in the historical moment, whatever one does in analysis. Dates are as important in ethnography as they are in history.

It was obvious that people within the town were interacting on some basis of understanding although they came from different regions and shared no common past. It was problematic that they shared the same culture. The Makah also disagreed about much that had happened in the past, and I began to doubt that there had ever been a homogeneous entity that could be called Makah culture. History, as they handled it, provided convincing demonstration of the validity of Malinowski's dictum that myth should be seen as a social charter reflecting the interests of its tellers. People disagreed about historical events in the same way that they disagreed about relative social status. They said there had been such a thing as a Makah culture but the details of that culture were in dispute. They did agree that to be a Makah had little to do with either language or culture. I came to suspect that disputes might be at least as important as consensus in producing social cohesion (4).

Many Makah themselves linked their existence as a people to their continued control of their land. They had never been subject to forcible exile, although their children might have been taken away to boarding schools, and they had been able to retain some semblance of economic independence. Their confidence in themselves and their refusal to be subordinated to white Americans was a revelation after experience among California Indians, who had known exile and thoroughgoing disruption. Also revealing was the Makah fear that the government would use the war as an excuse to remove them. They insisted that they could remain a viable community only on their own terrain, where those who chose to live elsewhere could later find a refuge in adversity. They were prepared to use the courts to fight off efforts to displace them.

How vulnerable we all are to arbitrary action was demonstrated in 1942 when, as one response to American hysteria after Pearl Harbor, the US government removed all Japanese and Japanese-Americans from the west

coast into so-called "war-relocation camps." There was no pretense of fair trial. Instead people were stigmatized, segregated, and denied their civil rights. I had the chance to join a research team in Poston, one of the camps, where I worked under Alexander Leighton, a psychiatrist with anthropological training, and Edward Spicer, a social anthropologist trained by Radcliffe-Brown. In studying the dynamics of camp life, the former emphasized the forces playing upon individuals, while the latter described the creation of social roles that were part of an interactive system. Again we were looking at a situation that had only just come into existence, that changed before our eyes, and that we hoped would not last (30).

Poston displayed many of the characteristics now being reported from camps set up for refugees in Hong Kong (2, 29), on the Thai-border (20, 25, 29, 36, 41), in Sudan (23), and in Zambia (21). In describing camp life observers invoke Goffman's formulation of the total institution (19). Staff and inmates become progressively more polarized. Each stereotypes the other. Homogenization is the general aim of the procedures in the interest of administrative control; but factionalism is the order of the day, since those who once held authority try to maintain some kind of status and control over their own actions. Irrespective of the cultural traditions of those who administer or those who are administered, the social order and culture of the camp are created as people respond to what are basically the same kinds of forces operating within a similar environment.

Recently anthropologists like myself who carried out research in "war-relocation" camps have been criticized on the grounds that we provided legitimation for the internment and the losses suffered by those interned. I can only say that we regarded the internment as a gross violation of civil rights. But I thought then as I think now that witnesses were needed and that anthropologists had skills suited to that task.

By the time the war ended, I was eager for further field work, this time outside the United States. It was exhilarating to arrive in Northern Rhodesia at the Rhodes-Livingstone Institute in 1946 to discover scholars who shared my approach and interests. The Institute had been more or less on hold during the war years but was now ready to start new research. The proposal submitted by Max Gluckman to the Colonial Social Science Research Council called for the investigation of how involvement in the market economy affected rural African communities that were either exporting labor or growing cash crops. We were asked to look at people who were moving about, making choices, adjusting to changing circumstances.

By then labor migration had long been seen as relevant to the research of anthropologists working in southern Africa. Such studies had been carried out at least as early as the 1930s when two things became apparent: that people were moving between rural areas and employment centers in response to

economic forces acting throughout the region, and that rural areas were being transformed by the absence of men and the dependence on cash. Research had also begun in the receiving areas where new urban societies were being born. Godfrey Wilson, the first director of the Rhodes-Livingstone Institute, worked in Broken Hill (now Kabwe) at the end of the 1930s (46). He reported that a significant number of African workers were now permanently resident in town and dependent on wage labor for a livelihood. They and their families needed adequate housing and other services. The government of Northern Rhodesia was unwilling to concede that the towns were generating new communities, new needs, and new aspirations and that the Africans in towns should be treated as town residents. This conflicted with the official policy, which maintained that Africans were "tribesmen" who should be encouraged to retain their own cultures and also continue to support themselves in the "tribal" areas.

Gluckman, who had replaced Wilson as director, was a student of Radcliffe-Brown; but as a reader of Marx and Freud, he saw conflict as intrinsic to society and social organization as dependent upon economic factors. As a South African he was well aware of the color bar and the power relationships that maintained it, but he also observed that cooperation existed across the color bar that could not be explained simply in terms of raw power or by the role of ideology (17, 18). From his observation of the ceremony of the opening of a bridge in Zululand in the 1930s, he had developed what was later described by Van Velsen (43:xxv) as "a situational approach." In this, as in Lewin's field theory, actors make choices within a field where multiple forces are operating, and their behavior shifts as they encounter different situations. Mitchell (33:70) has summed up the approach succinctly in his recent reassessment of his work in the 1950s on labor migration and urbanism, carried out at the Rhodes-Livingstone Institute: "*behaving* . . . is *situational* and not an immutable characteristic of actors."

THE BEGINNINGS OF THE MANCHESTER SCHOOL

Those of us who worked with the Rhodes-Livingstone Institute in the 1940s and 1950s were fortunate in having contracts that gave us contact with the people of a region through several years. This led to the emphasis upon time as an important methodological tool. Later, in the 1960s, when travel became easier and repeated visits to the same region became feasible, the recognition of the importance of extended observation led to the advocacy of long-term field research and longitudinal studies (15, 39). It was observation over a period of several years that made it possible for Van Velsen (43) to use the extended case method and Turner (42) to develop what he termed "processual analysis." It is instructive, however, that each method also focused upon

conflict, for it was conflict that connected the various episodes through which a set of actors assessed and reassessed their commitments. Conflict likewise made possible the examination of "the variations in actual behaviour, the confusion and conflicting events which occur within the same frame of values (43:xxv)." Barnes, also affiliated with the Rhodes-Livingstone Institute in the late 1940s, went on to develop the concept of social networks, where the emphasis lay upon the multiplicity of relationships maintained at any one time with individuals who might or might not otherwise be associated (1). Network analysis was vital to much later urban research (32, 33).

Spurred by Gluckman's enthusiasm and love of field data, we engaged in close-grained analysis of case material and saw ourselves as moving towards a more dynamic view of social action and one better adapted to deal with complex situations than the structuralism of some of our colleagues.

In the analysis of the African material, we recognized that we were looking at people caught up in a colonial system whose influence was pervasive. This was a basic datum. We certainly indicated that in both rural and urban areas people resented the economic and political domination and the gross inequities of the system. At the time, however, we let much that they said go unreported because it was dangerous to them, and sometimes to the continuation of our research, to do otherwise. Nor did writing a polemic about the inequities of the colonial system then seem a contribution to ethnography. That we failed to provide descriptions of the working of the colonial administration adequate to the needs of later readers is due to our assumption that since administrative practices were everywhere similar, readers who knew anything about Africa could fill in the details. Today, of course, readers cannot. This is one of the dilemmas of the ethnographer: Just how much needs to be spelled out for readers distant in time and space? Those who described urban situations were more likely to include administrators and employers as prominent actors in the social dynamics of the rapidly evolving society, and they also included the trade unions and political parties that were organizing African demands for better living and a role in government. Their studies now seem less dated than the monographs based on work in rural areas.

At the Institute and later at Manchester University I contributed to the general pool of ideas my own concerns about how people define themselves, a refusal to see individuals as overpowered by roles, a view of disputes as having integrative functions, a desire to discover what kept conflict from getting out of hand, and the concept of cross-cutting ties as basic to the maintenance of community. I do not claim that these were unique contributions, for when people are working on close collegial terms a certain degree of homogeneity develops. Where I differed from some of the others was in a skepticism about the value of reifying a method or approach under a label or of promoting a particular approach as useful for examining all questions. I

thought we needed a more eclectic toolkit, which may be a characteristic feminine attitude. The contention that social arrangements are contingent was also a reflection of the way Pomo, Makah, and Tonga women explained the social environment in which they lived. What they thought had to be taken seriously, since they were actors in their own right. Information from males ought not to be privileged over their testimony.

FORCED MIGRATION AS A RESEARCH FOCUS

In the 1940s I carried out research among the Plateau Tonga who lived near the railway line in what is now Zambia. They had already lost a good deal of land to European settlers and were afraid they might again be forced out. On the other hand, many were themselves immigrants drawn by the opportunities for cash cropping provided by the proximity to the railway line and markets. Many of them, or their parents, were immigrants from Tonga-speaking villages in Gwembe Valley to the east, where the majority of the population lived in large villages near the Zambezi River. Nevertheless, Plateau villagers spoke of the Gwembe Valley as the epitome of backwardness, where people followed ancient custom. This they contrasted to their own access to education and adoption of European styles. Gwembe men, moreover, were forced to become labor migrants by the lack of economic opportunities in their home region. Forced mobility and a clinging to old custom were both considered undesirable, since each conflicted with fundamental values. Plateau villagers stressed the right of people to make their own decisions. They did not regard custom as sacrosanct and were prepared to jettison whatever prevented them from experimenting with new possibilities. The right of mobility was cherished; to be restricted was to be treated like a slave. Before I left in 1950 a few of the villagers in my sample had moved to a completely unfamiliar area 150 miles away where they could get land that would allow them to expand farming operations. The attempt of colonial administrators to restrict settlement in the towns, which were seen as providing amenities and opportunities lacking in rural areas, caused resentment against the system. Migration leading to settlement in urban areas or distant agricultural regions was an experience men and women were willing to risk, whatever their emotional ties with home.

Why, then, is forced migration such a traumatic experience? This question has dominated much of my research over the past 30 years, a period during which Thayer Scudder and I have been examining the impacts on Gwembe District and its people of the building of Kariba Dam, a project that flooded much of the middle Zambezi valley and forced the resettlement of many of its people.

Gwembe villagers had moved in numbers to the Plateau in the 1920s and 1930s, and more would have gone if land had been available. Some had settled in cities to the south. Yet in 1956 people were adamant that being *forced* to move was something they could not face (6, 9). Like the Makah, they argued that a particular place gave meaning to their own identity.

We saw in the aftermath of the resettlement that the stress had been severe even though villages were moved as units and an attempt was made to keep neighborhoods together. Because people had to face an unfamiliar physical environment at a time when they had also lost confidence in themselves, their political leaders, and their divinities, they became immediately disoriented and experienced great insecurity. Political refugees who have chosen flight against great odds to reach sanctuary are reported to feel empowered by the knowledge that they have won through and demonstrated their own fortitude and intelligence (29). None of this was apparent in Gwembe, where people saw resettlement as a failure that demonstrated their loss of control over their destinies. They began to recover only after they had mapped their new regions and when opportunities associated with the opening of the Kariba Lake fisheries and the control of tsetse fly restored their view of themselves as independent actors.

Since 1956 we have returned many times to Gwembe. I have also worked with students who have carried out research among refugee and other migrant populations elsewhere in the world (34) and read widely in the literature on forced migration and its consequences. I have concluded that humans are in fact migratory animals in the sense that they treasure possibilities for mobility but at the same time see any attempt to force mobility upon them as an infringement upon their personal space and upon their sense of integrity. Also important to the sense of self is the existence of a place of refuge, a home community. When forcible removal involves also the destruction or consequent inaccessibility of that refuge, people suffer immensely, even those who left voluntarily but with the expectation that they could always return. Those who have worked with refugees report that even those long resettled report grief over the loss of home.

Since so many parts of Africa, Asia, and Latin America are now in turmoil and people are being forced away from their home territories, this sense of loss must now be endemic, permeating contemporary definitions of self and the construction of reality. If anthropologists continue to define themselves as ethnographers, they are going to have to take cognizance of this. This century is characterized by displacement. Many who now make decisions have been formed under the stress of displacement. We need to understand the phenomenon's causes and effects. A start has been made, with the development of such centers as the Refugee Studies Programme at Oxford (involved in

comparative work on uprooting and resettlement) and the creation of various more specialized centers in the United States and Canada.

A SUMMING UP

Although an anthropologist's life involves a great many activities besides ethnographic research and writing—teaching and service to professional and other organizations being common to most of us—our most meaningful experiences are probably associated with field research, just as the experience of enlightenment is most commonly associated with those rare moments when disparate bits and pieces of information fall into a pattern. For those of us who have been engaged in long-term studies, it is field work—and the mulling over the field data—that provides a good deal of our own personal sense of continuity. We live between two worlds and feel somewhat detached from both, but each gains meaning through its contrast with the other. But if field research becomes a process over time, those who pursue it have to develop methods that can make accumulated data useful in the testing of hypotheses—something more than private memorabilia. This necessity lay behind the formation of the Linkages group in 1987, which initially brought together Douglas White, Lilian Brudener White, Scarlet Epstein, Conrad Kottak, Nancy Gonzalez, Michael Burton, Thayer Scudder, and myself. We are using the Gwembe material accumulated over the past 39 years (I first visited the region in 1949, and the most recent material was added by Jonathan Habarad, who worked in Gwembe in 1987–1988) as an experimental base for longitudinal analysis. We aim to systematize it in such a way that it will be susceptible to reworking by other anthropologists who want to test the findings or use the data to pursue their own questions (45).

It is good to know that something more may come of our many months of association with Gwembe men and women.

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