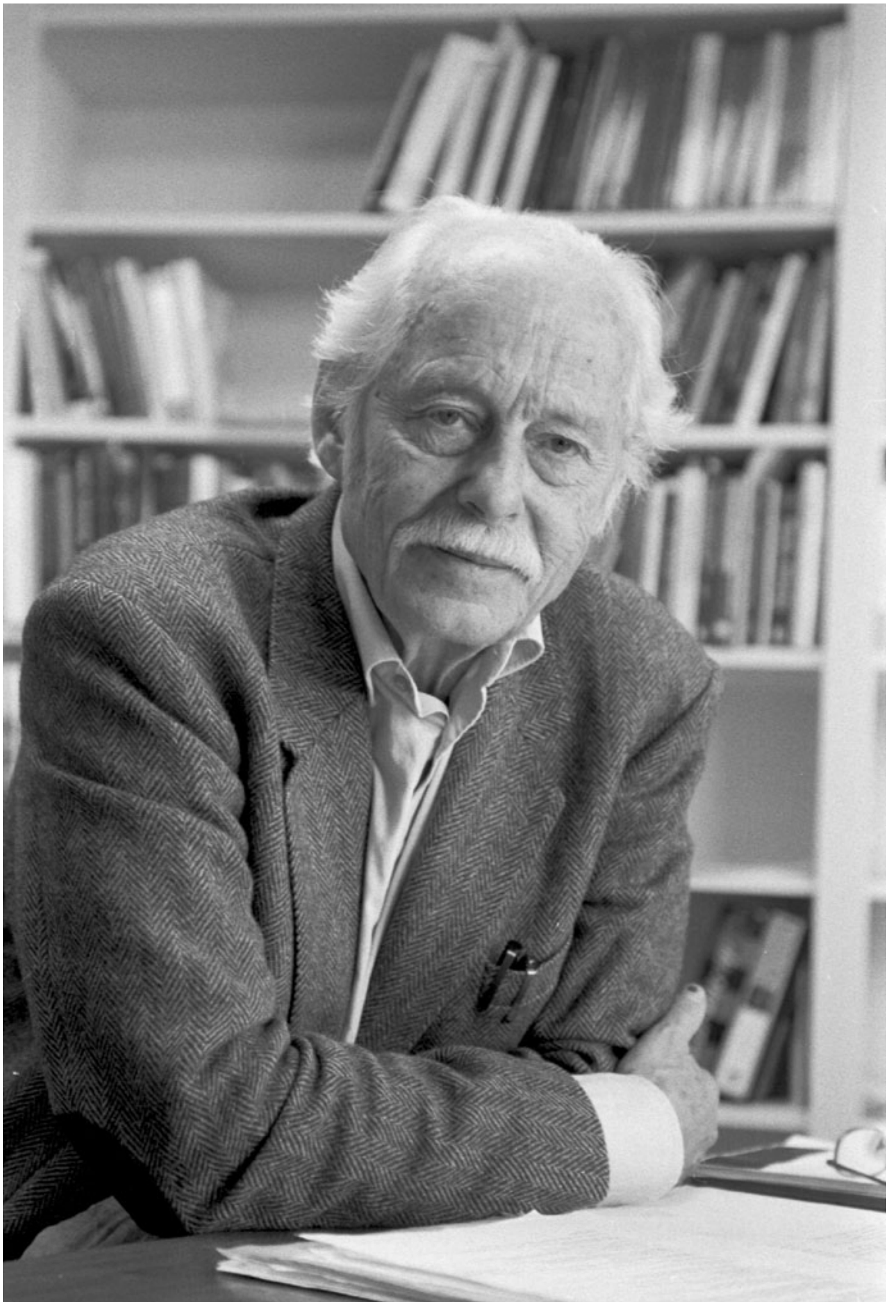

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Overview: Sixty Years in Anthropology

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Abstract

My main efforts in anthropology have sought to unite ethnographic and theoretical work by using empirical findings as provocations to critique received theory and raise unasked questions. I have used generative modeling to identify the empirical processes that, in their aggregate, shape social and cultural forms. Much of my time has been spent in ethnographic field studies in the Middle East, New Guinea, Indonesia, and the Himalayas, as well as Norway. This review traces the reflections and opportunities that connect these activities.

INTRODUCTION

Looking back, it seems as if the events in my life as they unfolded have provided a nearly optimal basis for my own professional development, even though they often appeared, at the time, fortuitous, distracting, and sometimes unpromising. I take this to indicate that my intellectual trajectory in anthropology has been heavily shaped by my responses to experiences, and so it can best be told in a biographical framework in which I give an account of my professional life course.

When I enrolled as an anthropology student at the University of Chicago in 1946, it was the realization of my highest wish. During my boyhood in Norway I had developed an early love of zoology, which had grown into an interest in evolution and human origins. World War II had ended, and I had discovered the existence of a field called anthropology, stimulated by a brief meeting with Conrad Arensberg who passed through Oslo while still in uniform. Soon my father, a geologist and geochemist, was invited as a visiting professor to the University of Chicago just as I completed high school, and I was able to tag along with him and enroll there as a student. Five years of constraint in occupied Europe were over, and a world of unlimited possibilities seemed magically to open up. I joined a cohort of mature and motivated students at Chicago, most of them financed by the GI Bill of Rights and received an intensive and broad training from the outstanding faculty then at the University (among them Robert Braidwood, Fred Eggan, Robert Redfield, Sol Tax, Sherry Washburn, and Lloyd Warner). I had just enough time to complete an M.A. in Anthropology before returning to Norway in 1949.

FINDING A WAY

My special interest had meanwhile moved across anthropology's four fields to end up in social anthropology. But I had a promise from Braidwood that I would be invited to join him on his next archaeological expedition to Iraq as his "bone man." In preparing to switch from archaeology to ethnography while in the Middle East, to test if I could handle the tasks of social anthropological fieldwork, I did a small community study among mountain farmers in southern Norway. I then went to join Braidwood in Iraq in 1951 and stayed on to study Kurds when the archaeologists left.

My situation in Kurdistan could not have been more fortunate. I had a network of acquaintances among the local Kurdish workmen who Braidwood had engaged in the excavation. I secured a research permit through the Department of Antiquities and the sponsorship of Baba Ali Shaikh Mahmoud, a Kurdish leader of prominence. He invited me to study the villagers who were his tenants outside Suleimaniyya; I also visited the villagers I had met during the excavations, who were members of the Hamawand tribe. I took the regional situation to be one in which tribal groups formed in the isolated mountain areas of the Zagros and disbanded as they moved down into the Mesopotamian plains and came under the control of state authorities and rich landlords. I chose to look for materials to understand this transformation. This choice liberated me from the conceptual constraint, so prominent in anthropology then and even now, that one must identify a community or place as one's "object of study" and instead challenged me to focus on processes and on the intellectual task of conceptualizing variation. As the guest of Baba Ali I also met his distinguished father Shaikh Mahmoud who in the 1920s had, for a time, been the king of an independent Kurdistan, and I visited their affine, who was the leader of an influential Sufi brotherhood. I returned to my Hamawand acquaintances and spent time with them and visited other localities that attracted my attention. I returned to Oslo with materials that embraced different community compositions and different forms of leadership, as well as glimpses of turbulent local political processes.

These were not the kinds of data, field procedures, or questions that were favored in most anthropology schools at the time.

Scandinavia was still struggling with a descriptive and diffusionist legacy, whereas holism and functionalism dominated English-speaking anthropology. Like many of my Chicago cohort I was influenced by the ideas of Radcliffe-Brown and admired the work of contemporary British social anthropologists, in my case Raymond Firth in particular, so I applied for Norwegian money to spend a year at the London School of Economics while writing up my materials from Kurdistan. There I met the still-unknown Edmund Leach and was captivated.

The book that resulted from my year in London (Barth 1953) set the direction for much of my subsequent research, but the book was rejected when I submitted it for a doctoral degree in Oslo and my prospects seemed bleak. However, some members of the Humanities faculty in Oslo felt I should be given a second chance, and they awarded me a university stipend for five years. I used those years to learn Pashtu (Afghan language) and do a year's fieldwork among Pashtuns in Swat, Pakistan. I thereupon spent two years in Cambridge, where Leach had meanwhile moved, and submitted a dissertation there. One result of this combination of training and contacts was to put me in an exceptional insider/outsider position simultaneously in England, America, and Norway, which gave me much freedom and autonomy to follow my own path through the next 50 years of shifts and fashions in anthropology.

INTERACTION AND POLITICAL BEHAVIOR

I proposed to my teachers in Cambridge two alternative plans for analysis of my field materials from Swat. One approach would focus on the various relations and transactions by which political leaders built the followings and alliances that shaped this basically stateless political system. The other plan proposed to use Gregory Bateson's concept of schismogenesis (the mutually reinforcing effects that the actions of two different persons or roles can have on each other) to analyze the formation of the two types of leaders in the area, namely, "chiefs" and "saints." Meyer Fortes favored the former, safer plan, which became my thesis design. It allowed me to work through the ethnographic details of a dynamic analysis of the formation of political groups and factions in Swat. As in Kurdistan, I had collected materials in Swat from a diversity of villages. Theoretically, I much admired the work of Edmund Leach, who had just completed his *Political Systems of Highland Burma* (Leach 1954), and I looked for further theoretical guidance from Max Weber and his analysis of sources of authority.

Instead of seeing political organization in the traditional anthropological manner as an "institution," based on rules and norms and defined by its function for society, I wanted to describe it as an outcome of the choices and alignments made by its participants: In Swat, that was the local adult males. I had indeed observed people making such choices and discussed them at innumerable occasions during my fieldwork. The central issue, as I saw it, was how best to understand how the patterns come about that make up social organization (Barth 1959a). Do they derive from cultural rules and norms that enjoin the particular forms of behavior that are practiced, or are they the outcome of a more complex play of considerations taken into account by political actors? Society is no doubt a moral system, but the political alignment of the persons within the polity in Swat could be shown as the aggregate result of myriad individual tactical decisions. There existed recognized forms of descent, property, and regional identity, but no man's membership in any particular politically corporate group was ascribed to him. Landowning agnatic brothers or cousins might be allied, or they might choose to oppose each other; tenants could shift their tenancy contracts and political allegiance to other, opposed landowners. Such decisions, I found, were determined by individual judgments of advantage and strategy, not dictated by moral considerations. People getting along in this situation required rationality: They needed to consider the prevailing circumstances and decide on a course of action that was determined not by ascribed statuses and collective moral norms, but by an assessment of what best promised to further each individual's interests, under his particular circumstances.

This perspective strikingly explained the emergence of a basic organizational pattern in the region as a whole of two dispersed political blocs, in constant tension and occasional violent confrontation with each other. Everyone was, of course, aware of the existence of two such blocs, but the pattern had never been purposely instituted as an organization: It was the unsought outcome of strategic coalition-forming between multiple actors. In the library I came across von Neumann & Morgenstern's volume on the *Theory of Games* (von Neumann & Morgenstern 1944) and was intrigued to find that their simple model of a multiperson game of alliance favored the formation of just such a pervasive two-party coalition. Thus I could compare my empirical findings in Swat with the outcomes of a rigorous formal model. Leach encouraged me to write this up in the context of a comparative discussion of segmentary lineage organizations (Barth 1959b) and submit it for the Royal Anthropological Institute's Curl Prize. The committee decided to withhold the prize that year, and I was told confidentially that this was because, although my essay was interesting, I was not sufficiently respectful of Evans-Pritchard in it. But in my anomalous and marginal position of independence I did not need to be too concerned by that, and I continued to pursue my own course. I also felt vindicated in my analysis when, 25 years later, the then-deposed ruler of Swat chose me to write his biography (Barth 1985).

ECOLOGY AND ECONOMICS

While I was doing my fieldwork in Swat in 1954, I occasionally met some strange figures in the local bazaars: bearded, long-haired men wearing an assortment of heavy clothes, with poorly cured goatskins wrapped, puttee-like, around their legs and feet. They were Kohistanis, Dardic-speaking mountain people from the high valleys of Upper Swat and the Indus gorge, living in areas that had recently been conquered by the Wali of Swat. When I felt I needed to take a break from Pashtuns around the middle of fieldwork, I decided to go exploring in Kohistan, rather than spend my time resting in a hotel in Peshawar. Some parts of Kohistan had been visited ten years earlier by the Central Asian explorer Sir Aurel Stein; other parts had never even been seen by a Westerner. I spent three weeks doing an ethnographic survey of these areas (Barth 1956) and discovered some striking patterns in the distribution of Pashtuns, the various Kohistani groups, and the pastoralist Gujars who utilized mountain summer pastures in some of the areas.

The distribution of cultures and its connection to the environment was a classic theme of the American cultural anthropology I had read, particularly in the form of the culture area concept as codified by Alfred Kroeber. Julian H. Steward had developed a more explicitly ecologic orientation in connection with his studies of the Shoshone of Nevada in the late 1930s, and the term cultural ecology was coined for such studies. But I was also acquainted with the basics of zoological ecology from my training in Chicago and was struck by the inadequacy of the culture area perspective for the materials I was collecting in Kohistan. I needed concepts that would allow me to analyze the coresidence and competition of populations with distinctive cultures—here, overlapping distributions in some areas and not in others. Pashtun communities in Swat were dominant in the lower regions, south of the Kohistanis (who may historically have once occupied the whole area). Small populations of Pashtuns who crossed this boundary were quickly assimilated as Kohistanis. Gujars, however, migrated both in Pashtun and Swat Kohistani areas and retained their distinctive identity, but they were not found at all in Indus Kohistan.

I adapted the concept of niche from zoological ecology to analyze these patterns and thereby uncovered the role that the presence or absence of other cultural groups, as well as specific features of the natural environment, played together in generating such distributions. Only populations that utilize the same niche in a territory are in real competition and tend to be mutually exclusive, whereas the presence of another population in another niche may sometimes be a positive factor in contributing to a niche for the adaptation of a culturally different population in a shared territory. The analysis I published on this material (Barth 1956) attracted the attention of U.S. colleagues with similar interests and led, in due course, to my first invitation as visiting professor in America, at Columbia University in 1961.

Meanwhile, my work among Middle Eastern tribals led to an offer from UNESCO's Arid Zone Project to make a study of the sedentarization of nomads, again, as I saw it, requiring an ecological analysis. Middle Eastern governments were concerned mainly about the political ramifications of having tribal and mobile populations within their polity and the difficulties of controlling them as well as providing them with education, medical services, and other social facilities that would assimilate them with sedentary society. UNESCO shared the latter commitment but mainly embraced a vision of making the deserts bloom by settling the nomads and enhancing economic development by shifting them to more productive pursuits as farmers. I wished both to influence public policy in these matters and to further our anthropological understanding of nomadism.

Alfred Métraux was is charge of the social sciences in UNESCO at the time and agreed that a major purpose would be to seize this opportunity to do modern fieldwork among Asian pastoral nomads. He accepted my choice to work among the Basseri, a fully nomadic tribe of ~16,000 members in southern Iran who offered the advantage of speaking standard Persian. It proved very difficult to obtain a research permit from the Iranian authorities to study a tribal group, but after long bureaucratic preparations and a three-month delay in Tehran I finally succeeded.

I quickly found that the standard of living among nomads in terms of diet, water, health, hygiene, and level of general satisfaction was significantly higher than that among villagers in the areas through which the nomads migrated. Furthermore, their contribution to the regional product, by having their herds use the seasonal pastures that were largely inaccessible to sedentary stockbreeders and delivering lambskins, wool, and butter through the local markets, was significant and probably greater than their production would have been if they were employed in agriculture. Thus my project became an argument to reevaluate both the economic contributions of pastoral nomadism and the cultural value of a nomadic lifestyle within a modern state. This was not what the authorities in Iran had hoped to hear, and Métraux suggested that when I published my materials I underplay the role that UNESCO had in the project; however, he gave the ideas I presented every support. Fortunately, Iranian scholars also supported me, and I was later able to join anthropological colleagues in Khartoum to argue similarly for the economic and cultural role of nomads in the Sudan. Hopefully, these contributions have had some effects in modifying attitudes and changing public policies in the Middle East.

The task of giving an ecologic and economic analysis of a pastoral nomadic adaptation (Barth 1961) demanded that I construct a relatively complex systems model. Nomadic pastoralism involves a number of management decisions concerning assets of several different kinds, and these will be variously embedded in a social organization that affects practice. One distinctive characteristic of pastoralism is that the key production factor, namely, livestock, comes in consumable form and increases through biological reproduction. Among the Basseri, the basic arrangement was one of private ownership and autonomous household management of flocks, whereas pasture commons were held as collective rights within large tribal units. This was long before the debate about "the tragedy of the commons," but I had to handle those empirical issues. For a pastoral adaptation to be sustainable and prosperous over time, which it clearly was in Southwest Iran, there had to be mechanisms that adjusted the size of the animal population to the carrying capacity of the pastures, or the pastures would be overgrazed and seriously damaged. By which mechanisms could overgrazing be prevented?

Despite the overwhelming valuation of a nomadic pastoral lifestyle among the Basseri, a quick survey of sedentary villages showed considerable numbers of sedentarized Basseri in the area, both as agricultural laborers and as small landowners. Rather than face the daunting task of collecting quantitative economic data on this, and thereby jeopardizing my whole field study within the limited time frame that the Iranian authorities had reluctantly granted, I merely cross-checked it by collecting some family and descent group histories among the Basseri and accounts of their understandings of the scenarios that led to sedentarization. I then constructed my tentative systems model of the processes, encompassing two sets of balances.

The moving force was found in annual variations: occasional killing frosts during spring lambing, and remembered episodes of sudden and disastrous contagious animal diseases, both of which irregularly reduced stock population. I also identified the minimal size of herd needed to sustain a normal household: in Iran at the time, this was ~100 adult sheep/goats per household with an optimal flock size of ~200 head for good shepherding.

The picture in South Persia was one of robust growth rates in both human and animal populations. The latter was the result of careful and persistent management efforts to increase household herds; the former was the result of good nutrition and health and was reflected in census materials and family histories.

But the very persistence of a pastoral nomadic adaptation is threatened by this buoyancy: The finite carrying capacity of the pastures can be quickly overshot by the nomads' careful husbandry of the animal population. However, Malthusian constraints on the human population would drive people to consume their capital in animals and eliminate the very basis for their pastoral adaptation.

Private animal ownership among the Basseri thus seemed to be part of a crucial mechanism in their adaptation. The occasional catastrophic animal losses strike differently and somewhat randomly in different herds, leaving some households above the hundred animal limit of economic viability and pushing others below it and into swift consumption of the rest of the animals, and thus the owners' elimination as pastoralists. Basseri do not practice the loaning of stock to unfortunate households, as found among some other pastoral nomads. Consequently, stockless households have no option but to become sedentary, propertyless agricultural workers. At the other end of a scale of wealth, rich herd owners find that herds in excess of 200–400 animals give a much reduced rate of return because hired shepherds provide inferior care to that of owner-herders. For the rich Basseri nomad, it is therefore profitable to transfer excess capital from stock to landed property on which they can collect tenancy fees, in due course often leading to their settlement as land owners. Thus human population growth tends to be drained off by sedentarization of both the most impoverished sectors and the most wealthy of the human population, and thus animal populations will be reduced, well before general human starvation sets in and before pastures are overused.

This model can frame a comparison with other pastoral nomadic regimes and can be tested by more precise demographic and sustainability analyses, as has indeed now been done. My Basseri materials further served to strengthen my own trust in a focus on processes rather than on descriptions of mere patterns and functions. As in my analysis of politics in Swat, it also recognizes that both the sought purposes and the unsought, indeed unseen, consequences of practices must be included in any systematic analysis of culture and society.

THE DEPARTMENT OF ANTHROPOLOGY IN BERGEN, 1961–1972

Shortly after this time, my situation suddenly changed. I was invited to come to the recently established University of Bergen, Norway, and take up the task to create a Department of Social Anthropology there. I was acutely aware of the danger of isolation in a small, provincial university, after having enjoyed the freedom and advantages of being a roving scholar, free to follow my own whims whenever I could have them funded. But I was also attracted by the challenge to create the first department of social anthropology in Norway, indeed in all of Scandinavia. This was a chance that I could not pass up.

My new post forced me to shoulder a multitude of simultaneous tasks. I had to think through the discipline and construct a curriculum that would be as suitable as possible for Norwegian conditions. I needed to do outreach to develop a public awareness of the discipline and academic promotion to recruit students and establish collegiate support within the university. I needed to show that anthropology could also be applied and was useful in social planning. Most basic in the long run, I needed to direct my own research efforts to provide maximal clout and relevance in these roles, and in due course for a sustainable, internationally oriented anthropology.

I regarded it as basic to the nature of anthropology to do cross-cultural and international research, so my task was to create a greater public appreciation of the enormous value and contemporary relevance of active international research. But I also needed to promote empirical work within Norway because showing this form of relevance was no doubt important to the general reception of anthropology there. Taking my brief from the Tavistock Institute in London and their industrial studies of task organization in coal mining in England, I launched a study of task organization in herring fishing with purse seines along the west coast of Norway and also a cooperative study of the role of entrepreneurs in social change in northern Norway (Barth 1963). The latter drew on community studies done by colleagues and allowed me to develop some perspectives on the study of social change that I later found very useful in my work in the Sudan. The herring fishing study provided an opportunity to integrate the work of Erving Goffman (Goffman 1959) into my analysis of social relations and led to work by other scholars on various forms of industrial fishing.

Over the years my small and slowly growing group of colleagues and I tried to adjust our priorities between these and other tasks to ensure anthropologic quality and relevance. Our British colleague Maurice Freedman once rightly remarked that you could "count Norwegian anthropologists on a mutilated left hand," but Norway is a small country and over time we had a big impact. Today, the discipline in Norway is of a size and vigor beyond what one could ever have expected.

An essential precondition for success was to maintain active international collegiate relations. Senior British anthropologists were supportive in accepting invitations to visit and lecture; I tried to raise funds to send junior associates abroad as visiting scholars; and we received a trickle of enthusiastic, supportive students and colleagues from less favored European countries and from the US. I marked a kind of "affirmative action" by going myself for a year to teach at the University of Khartoum in Sudan in 1963–1964, and this we followed up with systematic cooperation and exchange between the two departments over the years and progressively a broader program of scholar-directed cooperation between several disciplines in the universities of Bergen and Khartoum. Meanwhile, I benefited very much from the Wenner-Gren Foundation's program of conferences at Burg Wartenstein, which served to keep me up to date on the thinking of many of the best of my colleagues and directly stimulated my research, as well as the research of several of my Norwegian colleagues.

MODELS OF SOCIAL ORGANIZATION

Professor Raymond Firth was particularly supportive in several ways. As early as the winter of 1963 he invited me to give a series of lectures at the London School of Economics, a series later published as *Models of Social Organization* (Barth 1966). This gave me the opportunity to formulate the theoretical foundations for my work on strategy and choice. I drew on fresh materials from Norway as well as accumulated materials from the lessons I had learned in the Middle East. My basic point was that anthropologists need to study processes, not merely patterns, and construct generative models that can explain how social forms come about, rather than simply summarize and characterize such forms.

A generative model seeks to identify a set of factors that, through specified operations, will produce a determined output. It is this output that can usefully be compared with our descriptions of the empirical forms of a society to test our understanding of the systems and the empirical processes that sustain them. A generative model is thus of a different order from the structural-functional models that were favored at the time and even now. The structural-functional model seeks to generalize the overt form of a society and show its coherence with people's own cultural representations. A generative model tries to identify sets of regular events, processes, that lead toward the emergence of such an observed form in a local or regional social system.

I gave special attention to the concept of transactions, so as to analyze the construction of those social relations that are inherently built on confrontation and strategy and cooperation. Transaction, I argued, is "the process which results where the parties in the course of their interactions systematically try to assure that the value gained for them is greater or equal to the value lost" (Barth 1966, p. 4). Not all social relations are constructed in this way, but many certainly are. The point is, where people shape their decisions and actions in a relationship by a transactional accounting, systematic and cumulative effects ensue that will determine many features of that relationship. Thus, the substantive content of behaviors and actions that make up such a relationship is significantly constrained by the transactional process; the distribution of prestations in the relationship is systematized as a result; and thus the character of the relationship is to that extent determined. The same will not be true in regard to nontransactional relations. Impression management between parties to transactional relations is further circumscribed by the mutual vigilence that the parties must practice, with identifiable consequences for the behavioral outcomes. Finally, transactions bring about what we may regard as the main work of integration in culture because they make actors establish commensurability between the forms of value over which they consummate transactions.

These ideas allowed me to uncover some determined connections in any social organization. They provided a way to model relationships between the microlevel of interpersonal relations and the macrolevel of social systems. They also encouraged me to observe the details of human relations with a systematic purpose. This hinged on the importance of observing the behavior of actors at the moment of action, rather than accumulating elicited informants' accounts of patterns and customs; and on developing a series of subsidiary concepts to facilitate the analysis (such as opportunity situation; strategy and game; assets and convertability; and sought and unsought consequences). These ideas were of interest to many of my cohort and received considerable attention for some years. I pursued them in areas such as the theory of economic spheres (Barth 1967a), the nature of entrepreneurship (Barth 1963), and the study of social change (Barth 1967b).

In retrospect, the viewpoint of *Models* has sometimes been identified and dismissed as "methodological individualism." On the contrary, my argument was not based on any such philosophical position. My general point was certainly not to declare collective facts nonexistent or derivative, but to acknowledge that whole persons are involved in social relations; that they often resist and confront each other, as well as cooperate in accordance with institutional roles; and that they thereby will converge on characteristically limited and constrained patterns of interaction in concrete situations, which can be explained by my analysis. This position is later clarified in Giddens' somewhat heavy concept of "the duality of structure" (Giddens 1984).

In the late 1960s, interests in these issues were increasingly submerged in our discipline by new fashions such as Marxist macrotheory, and a shift to the study of meaning. To the extent that attention to strategy endured, it tended, especially in the other social sciences, to focus on the theory of rational choice. It is always important for an anthropologist to stay in touch with the major topics that engage colleagues, so I felt that, if I wanted to be where my colleagues were, I needed to refocus rather than continue to plug my particular views on the study of strategy and choice. But the exegesis of Marxism I found uninspiring, and the debate on rationality somewhat retrograde, because it was framed with little reference to some issues that I felt I had already recognized: most importantly, the dependence of rationality on local and culturally constituted knowledge and premises.

I therefore decided to take up issues of meaning. I had been disappointed by the limited purchase that my idea, that transactional relations produced cultural integration, had on our understanding of how value positions arise. I hoped that perhaps a deeper understanding of the construction and communication of meaning might reveal new wellsprings for the emergence of values. I decided to study ritual, where I could observe behavior and learn something about the representation of meaning.

I also received a special bonus from this new departure. I have always enjoyed fieldwork in new places and believe that novelty in the field revitalizes an anthropologist's theoretical imagination. Up to this point, I had worked in a diversity of Muslim areas but had not been among people with a tribal religion. To study ritual, I could choose to go to an entirely new place. I chose a very pristine population in New Guinea, hoping that it would give me rich and vibrant materials on ritual.

ETHNIC GROUPS AND BOUNDARIES

But first, another matter developed. The Wenner-Gren Foundation encouraged me to arrange a symposium with anthropological colleagues in Scandinavia in 1967. For a topic, I scanned various materials from my fieldwork that seemed to present puzzles with general and theoretical potential. I remembered a friend in Kurdistan who claimed to be equally a Kurd, an Arab, and a Turkoman—i.e., a member of all three main ethnic groups in the region—and exploited this to his own advantage. I also recalled my good fortune when in 1960 I went to Baluchistan to supplement the data of Robert Pehrson, who had died in the field and whose materials on the Baluch I was struggling to write up and publish (Pehrson 1966). It turned out that the Baluch he had known were bilingual in Baluch and Pashto, so I could speak with everyone when I visited his main field site; their bilingualism, they explained, came about because they were Pashtun by descent but assimilated as a branch of the Marri Baluch tribe. And in Kohistan, Pashtuns turned into Kohistanis; whereas further down the Swat valley, no man would ever give up his claim to being a Pashtun. Inspired by Leach's monograph on the Kachin

(Leach 1954), I assumed that some of my Scandinavian colleagues had data with which we could modify conventional anthropological assumptions about the congruence of our categories of tribe = culture = society. So our symposium topic became ethnicity. I circulated a brief discussion of issues and concepts with my invitation to the symposium, and we ended up with seven ethnographic cases, in addition to my introduction, in our resulting publication (Barth 1969).

Our findings were fairly unambiguous. We documented situations where people changed their ethnic identities under pressure, or as a result of ecologic change, or where they clung to them in minority situations by careful impression management, or used impression management to deny patent cultural differences that might have been given ethnic significance. We argued that in view of such situations, the construction of ethnic identities was understood most readily by looking to the ethnic boundaries and modes of boundary marking.

Those were different times from now: One could publish with a minor Norwegian academic press and yet be ensured the attention of international scholars. The work was widely read, first by anthropological colleagues and then in other disciplines, and it ended up among the top 100 on the social science citation index for a number of years. What caught the most attention was its counterintuitive propositions: Contrary to the common-sense reifications of people's own discourses, and the rhetoric of ethnic activists as well as anthropology textbooks, ethnic identity is determined not by massive facts of shared culture and shared history, but instead in each case by a more limited set of criteria. It can also be deeply affected if it is subject to the manipulations of political entrepreneurs.

RITUAL IN NEW GUINEA

Although the results of the ethnicity symposium were in publication, I went to New Guinea for a year's fieldwork. I chose to work with the Baktaman, a recently contacted group in the western mountains of Papua, aiming to investigate the meanings of their rituals in the context of their lives and, in due course, to try to understand the nature of their ritual codes. I did assume that their rituals were full of meaning, but I was skeptical of the two dominant anthropological approaches to the analysis of such meanings: One, the highly intellectualized and abstract theory of Lévi-Strauss, modeled on structural analysis in linguistics and emphasizing myth; the other, the loosely theorized "thick description" of Clifford Geertz, which sought to arrive at a cultural interpretation of meaning by piling up data on a mass of local cultural institutions in the hope that an inherent systematic would gradually emerge. My hope, on the contrary, was to reason from the specific to the general: by close familiarity with the performers and the local circumstances in a New Guinea tribe to come to learn something of what their rituals were actually saying to participants, and only then to work backward toward the features that could make the rites capable of codifying and communicating such particular messages.

The Baktaman were a community of 183 members, separated from neighboring groups by a high level of intercommunity violence until contacted and pacified by the first Australian patrol four years before my arrival. Nonliterate and isolated, I assumed that their rites would have the home-grown character of springing only from the facts of their own local life, which were then in principle observable also by me while I was among them. I faced a number of vexing problems of field methodology and language, but I had the extraordinary good luck to be associated with Nulapeng. Nulapeng was a very intelligent young man from a previously contacted community in the region, who had lived for a while as a child in fosterage among the Baktaman and was thus uniquely knowledgeable in both the Baktaman Faiwol dialect and in Neo-Melanesian/Pidgin. Indeed, after our association he became government translator at the local patrol station.

My intention was to make my presence among the Baktaman as noninvasive as possible. I lived a very frugal life and tried simply to participate by joining and observing activities as they unfolded. I was particularly concerned to avoid introducing new topics and issues that did not arise spontaneously among people and to ask only the kinds of questions I heard them asking, so as to partake within the discourse that they themselves were sustaining, without introducing extraneous ideas. I wanted to know what thoughts and ideas they were having and communicating, especially in their ritual codes.

Their rituals were heavily concentrated on male initiations in seven stages, spread over most of the life course, and on the frequent minor sacrifices to ancestral relics that these initiations authorized. The emotionally dominant theme was secrecy and fear induced by hardships during the initiations. Women may know nothing of the rituals, and idle talk about even the simplest features of the rites, even with fellow novices, was forbidden and dangerous.

I observed an initiation to the fourth degree and was later allowed to join the set of novices—men in their thirties—to the sixth degree initiation. This served to legitimize my being shown the details of the intervening fifth degree by the head initiator Kimebnok, in preparation to becoming a novice in the sixth degree. Nulapeng told me about the events of the three lowest degrees of initiation, which he had gone through; after successfully going through the sixth degree and having proven myself fully respectful and reliable on the matter of observing taboos and not telling secrets, Kimebnok led me before the most sacred shrine and whispered the content of the final, seventh degree to me. By this extraordinary generosity on the part of the Baktaman, I managed to press a whole life of male initiations into my one year in the field (Barth 1975).

I sometimes felt a certain disappointment that Baktaman rituals were not grander, by New Guinea standards; but indeed I was fortunate that they were not. Had they been richer and more complicated, I could never have learned to master them as I did. I also sometimes longed to meet "the Wise Man of Dogon" (Griaule 1965) to have the meanings of the rites interpreted for me. But even in the highly irregular verbal sessions describing the rites, I was never supplied with exegeses, nor did I hear interpretations being discussed between them. Indeed, I do not think that any tradition of exegesis existed among them, and even the corpus of myths was remarkably small and simple (in contrast to what has been found in some culturally related groups).

What did I learn of concrete meanings by attending these rituals? I slowly became increasingly captivated by the acts and images and learned to sense their reference, their subtlety, and their sophistication. They were about sacrifices to the ancestors (represented by relics) and prayers for taro; about the growth of taro, and of children; and about features of animal species and natural elements and the significance of colors. Clearly, Baktaman rites communicated by metaphor and contrivance and used concrete symbols to call forth meanings during the ritual events. I followed Turner (1973) in exploring the fans of connotation of the key symbols that made them templates and concepts for abstract, but persistently nonverbal, ideas. The ritual acts adapted these connotations, and their metaphorical import, by sometimes adding colors to ancestral bones or body paint to novices; associating fur and foliage to vegetative growth; manipulating fat and water; and thereby creating harmonies and extensions within such broad fans of connotations.

The secrecy added levels of complexity. For example, lower-grade novices were told "secrets" that would later turn out to be hoaxes, such as warnings of pollution in a game that, in later initiations, would prove to be sacred and suitable for sacrifices; they would claim features to be insignificant that were later revealed to be highly significant. Paradoxical significance was sometimes stressed, as when water—a removing and cleansing agent—was mystically used in the form of dew as an agent of growth and fertility. Some deep paradoxes were left unresolved, as central puzzles of existence. Through a series of secrets, like veils behind veils, I sensed that the whole initiation sequence contained a meta-message of the ambiguity of any and every claim and led to the final embracement of mystery as the most valid way of knowing these awesome truths.

I thus found rituals to be constructed to embody metaphorical meanings and experienced a growing appreciation of these codes of expression and the corpus of knowledge about nature and life processes that was contained and transmitted in the codes of Baktaman ritual. I was captivated by the access that this analysis seemed to give me to Baktaman thought and experience and felt I could resonate with them. I remain a pluralist in not denying the possible presence of other meaning-bearing codes and structures in rituals, but I found the exploration of metaphor in concrete imagery to be a compelling way to gain access to meaning in these acts.

CULTURAL PLURALISM IN SOUTHERN ARABIA

Some years after New Guinea I sought a new fieldwork opportunity. Considering the extraordinary fluency in Arabic of my wife and colleague, Unni Wikan, it seemed a good idea for us to make use of this resource. Southern Arabia had recently opened up to anthropologists, and in 1974 we went to Oman. We ended up in the town of Sohar, the reputed birthplace of Sinbad the Sailor. I decided to pursue the theme of ethnicity there (Barth 1983).

Sohar was an old trading town of 20,000 inhabitants. It contained a diversity of ethnic groups: Arabs (~50% of the population), Baluch, Persians, Zidgali, Indian merchants, and formerly also Khoja (associated with Hyderabad, India) and Jews (from South India). Sohar was very much part of the cosmopolitan world of Indian Ocean trade, although essentially closed to Westerners till 1970. I proceeded to investigate the cultural differences associated with these ethnic groups.

As materials accumulated, the picture became increasingly complex. Several schools of Islam were represented: Sunnis, Shiahs, and Ibazhis. The Baluch and Zidgalis were all Sunni, the Persians mainly Shiah but some also Sunni, the Arabs variously Sunni, Shiah, and Ibazhi. I further discovered that the deepest cultural differences, in terms of lifestyles and values, might be between townspeople of all the different ethnicities, as against the bedouin, who were ethnically regarded as Arabs. I recognized that such statements of greater and lesser cultural difference were too impressionistic and unquantifiable, although very much part of Sohari stereotypes. After the demise of the concept of "culture traits," anthropologists were left without any method to make cultural comparisons objective. As we had concluded in *Ethnic Groups and Boundaries*, we are speaking of "the social organization of culture difference" and not a specification and summary of cultures in terms of their content. But it then dawned on me that bedouin versus townsman, and the various congregations, were of course also a question of "the social organization of culture difference."

If so, should we not also regard the professions in the town as social identities likewise associated with culture differences? There were the merchants in their snug shops fighting their cut-throat battles with competitors, the hard-working date and lime cultivators on the irrigated lands, the full-time fishermen, the soldiers and gendarmes in the fort, the day laborers. Each occupation required its distinctive knowledge and skills and entailed a way of life, and thus indeed a distinctive culture. Some occupations were heavily favored by one, or two, ethnic groups, but others were more widely and freely practiced; therefore, the different categories were not linked directly to ethnicity. There were also ex-slaves (manumission was as recent as 20 years earlier) hailing originally from East Africa and still entailing a special and ambiguous social status, as well as some distinctive cultural features. And indeed, men and women lived such different and segregated lives; they were also culturally distinctive (as was a small category of male transvestites).

Two striking features were clearly important. On the one hand, none of the social categories emerged as political groups. Government was highly centralized, with the Governor of Sohar representing the absolute Sultan of Oman and exercising an active administration. In cases of conflict, people turned to the governor's court for a resolution and did not seek to mobilize partisan social support for their individual causes. This was associated with a special code of honor between men: to be always tolerant and perfect in their manners and not to seek to dictate or judge others. Conflicts between them were solved by the administration. Conflict with the administration could be solved only by migration; so groups disappeared and other groups joined over time as residents of the town. A striking feature was that each of the ethnic groups (except the tiny Zidgali minority group) was an offshoot of a larger population extending in different directions as parts of different plural states in the region. Although ethnic groups and congregations clearly felt affinities to their comembers elsewhere, they were not prepared to let themselves be mobilized in the local or regional conflicts that existed elsewhere. The centralized and authoritarian local political organization seemed to lay the foundation for the peaceful coexistence of these groups in Sohar.

I thus found a dynamic range of cultural variation beyond ethnicity in the town of Sohar, linked to other features of social organization and identity. Mapping this out with some care seemed necessary and interesting, and I tried to identify boundary crossing of persons and of cultural features; but a focus on ethnicity did not lead me to any conceptual resolution. More seminal to my own thinking was the fact that these materials forced me to acknowledge the lack of closure I had found on levels higher than Sohar and to abandon the simple part/whole scheme that we tend to use when discussing local and regional distributions. Clearly, some situations can be described only in terms of a much more complex schema, where parts of a local system connect in diverse and criss-crossing ways on higher levels. Stimulated by my direct field experience of the very small-scale and closed world of the Baktaman, this gave me the impetus to problematize the dimension of scale in our comparative analyses of social systems (Barth 1978).

VARIATION AND REPRODUCTION IN CULTURE

During my fieldwork among the Baktaman, I became aware of variations between their rituals and those of the neighboring communities but had no opportunity to explore these matters further. Then suddenly in 1982, I was offered the opportunity to revisit the area. Copper and gold had been discovered in what became the Ok Tedi mines, about—seven to eight long days' march west of the Baktaman settlement. The newly independent authorities of Papua New Guinea invited me back as a consultant on labor relations and cultural policies vis-à-vis the locals. This also gave me a chance to revisit the Baktaman and to survey local communities in a larger area. Meanwhile a handful of ethnographic studies had been published on some neighboring groups. Moreover, rumor of my strange residence and participation among the Baktaman 14 years earlier had spread widely in the native population, and I was now welcomed as an insider to shrines that would formerly have been closed to me. This gave me a chance to explore the theme of local variation in some depth (Barth 1987).

The materials I obtained uncovered striking differences between the rituals of otherwise similar neighboring groups. As compared with the Baktaman rites, I found important themes and symbols missing, unfamiliar images emphasized, and diverse jarring constellations and inversions present in different rituals. How was this diversity generated, and what could it tell us of cultural processes? Presumably the cults are all historically related, and they represent indigenous thought about cosmology. Might I perform an analysis of them, like what modern science studies do for Western knowledge, to understand their construction and change?

To do so required a framing in the social organization of initiations and a critical exploration of what the indigenous criteria of validity are, as well as a close attention to the performative aspects of the rituals. Because we are dealing with nonliterate traditions that are manifested and transmitted only by being enacted, and their reproduction is under heavy constraints from secrecy, such materials might be suitable to reveal some elementary features of cultural reproduction. The senior shrine keeper in each of these communities is in charge of the production of the ritual performance of each initiation, lasting 3–10 days. Each performance takes place when a group of novices requires it. The Baktaman have no calendar and no idea of a year, but each step seems to be performed about once every ten years, when its content must be remembered and reproduced. The form and content of each initiation are claimed to have been instituted by one's own ancestors and are regarded as specific and unique for each community of descendants.

Some paradoxes are embedded in this framework. There can be no prototype, original performance of the traditional ritual: Only the last previous performance, or perhaps the last two, will be remembered at all by anyone. There should thus be nothing to prevent incremental changes in the form and content of the rite. Place yourself in the position of the initiator: After ten years' secrecy his task is to stage a breathtaking, powerful performance, which transforms the novices and satisfies a handful of seniors. This can hardly be achieved by rote memory and reproduction; it requires an inspired production by the master of ceremonies, and thus requires creativity, although within certain limits of conventional expectation. Here lies the source of incremental change and proliferation of forms as between local traditions, separated from each other by secrecy and distrust. I tried to work out a possible genealogy for the variants I found. More importantly, I tried to theorize on the character of incremental changes, given the contexts and the codes. One could expect the marginal changes to show certain forms of cultural creativity, such as (*a*) conceptual clarifications, (*b*) enrichment of idioms, and (*c*) harmonization between the connotations of key symbols. I found examples of each in the comparative data from different communities and so felt that my theoretical construction was vindicated.

I had now found a perspective and a material that enabled me to analyze the construction and change of meaning in a human tradition of knowledge. Could this be used on other materials and generalized?

TRADITIONS OF KNOWLEDGE: BALI, BHUTAN

My wife and I felt we had both done our share of physically strenuous fieldwork and decided that Bali would be our next field site, where we went in the 1980s. Like many others, we had much admired Geertz's (1973) essay on "Person, Time, and Conduct in Bali." Having discovered that some traditional Balinese communities, who have been Muslims since at least the seventeenth century, also live on the island, I saw an opportunity for a clearly focused comparison that might allow me to understand better the nature of cultural integration. Geertz had vigorously argued that Balinese naming customs, ideas of time and history, and forms of public demeanor cohered in a "cultural triangle of forces," implying the integration of some characteristic features of Balinese personhood. Now if a community of such persons started to employ Muslim names, followed a Muslim calendar, and subscribed to the Prophet's historical revelation, then what else might have to change and where would the tensions arise? The answer should reveal something about the nature of cultural integration.

As so often in our discipline, my research plan collapsed under the pressure of detailed ethnography (see also Wikan 1990). I quickly discovered that Balinese village life showed too great a variation for me to use one account as a foil for my analysis of the Muslim community. This also made me rethink my far-too-schematic initial comparative design, which had indeed ignored my own previous insights from New Guinea. I did not want to draw the contrast between Bali-Hindu and Bali-Muslim institutions that had made the latter population modify their lives: For my analysis I would want to understand the processes through which the diversity of Bali was reproduced and changed. A tall order, indeed. But it made me face the fact that a great civilization like Bali's is a cornucopia of diversity and creativity. New Guinea had proved people to be more creative and more diverse than I had previously recognized. In Bali I was faced with a complexity of an entirely different order, and that complexity certainly could not justify my making my analytical apparatus simpler. We have failed as anthropologists to address the importunate fact that at least the old civilizations of Asia are above all characterized by their own internal diversity. This is where we must seek the key to their character, not by smoothing our depictions down to a generalized structure or stereotype.

My study of Bali (Barth 1993) therefore became more broadly comparative, within the northern province of Buleleng. I found that an initial focus on traditions could serve to sort out some tentative connections in this disordered picture. I thought of the great cultural streams that had flowed into the island of Bali: Malayo-Polynesian, Megalithic, Indian, Chinese, Islamic, and Western. Not only did each bring cultural items, like the old diffusionists depicted the process, but also they came embedded in social organizations, political structures, cosmologies, and moralities and they made their impacts on Bali in ways that were shaped by these organizations and facts. Cultural integration is always a work in progress, and a local community or region will make up a disordered system of partial connections that it is our task to unravel by modeling the processes that are at work, here and now.

A focus on knowledge (Barth 2002) helps us to do just that by identifying the ideas that are reproduced and created and the transactions in knowledge that embed ideas in social relations. I was even tempted to make an even broader comparison, between how knowledge is differently constituted when in the hands of South Asian guru figures as contrasted to the initiators I had known in New Guinea (Barth 1990). These ideas I have carried with me and pursued in my current work in Bhutan, which seems to confront me with even greater complexity than did Bali: ancient literatures and arcane monastic institutions, illiterate peasantries of diverse linguistic groups sustaining artistic and philosophical traditions, etc.

CONCLUSION

The task is endless and ever self-transforming. For most of my lifetime I have seen it as a social science version of the naturalist's old task, of watching and wondering. We need to see our empirical work as an obligation, to acquit as best we can by critically using the variety of methods and concepts available to us at any one time, not by only performing the operations that are most fashionable or refined. But we equally need to regard every new empirical finding as a provocation, to rethink our assumptions and redesign our models. Pursued in this fashion, social anthropology promises to be as exciting in the future as it has been in the past.

DISCLOSURE STATEMENT

The author is not aware of any biases that might be perceived as affecting the objectivity of this review.

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