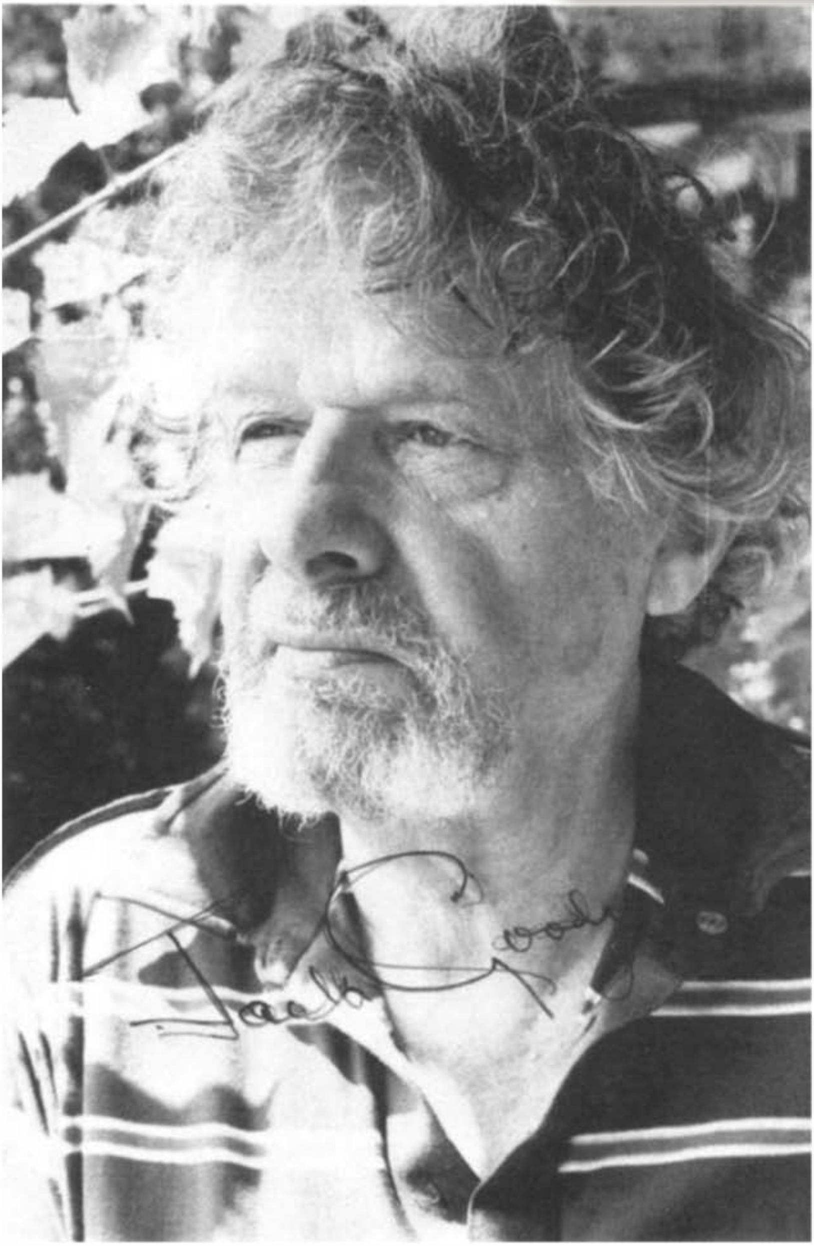
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# TOWARDS A ROOM WITH A VIEW: A Personal Account of Contributions to Local Knowledge, Theory, and Research in Fieldwork and Comparative Studies

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When the editors asked me to make a personal contribution to the *Annual Review of Anthropology,* I accepted mainly because my colleagues, Fortes and Leach, had already done so. I write here more to mark our joint achievement at what became the Department of Social Anthropology at Cambridge, and to review my own particular contribution, than to offer new insights or revelations about anthropologists.

Fortes arrived in Cambridge in 1950 as Professor, I returned there as a graduate student at the end of the same year, and Leach came in 1954. Our relationships were never entirely harmonious—what relationships are? But we collaborated from the beginning in a way that was helpful to each other and to others.[1] That collaboration began with seminars every Friday of term, which

[1]In her interesting piece on Meyer Fortes in the *American Ethologist,* Susan Drucker-Brown (2) describes her introduction to Cambridge and to the postgraduate seminar where Leach and Goody were always being rude to one another. That we argued vigorously is true, but such was the intellectual culture to which we were accustomed. We saw this not as rudeness but as frankness. We generally saw eye to eye on the way the Department should go, except that Fortes did not approve of the proposal to merge with Social and Political Sciences, which is where I thought the most favorable environment for social anthropology lay. However we remained close friends thoughout our time in Cambridge.



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all those attached to the Department felt an obligation to attend—on the Oxford model, which in turn was based on the practice at the London School of Economics. There was literally a continuing clash and exchange of ideas. The seminar crystalized into the *Cambridge Papers in Social Anthropology,* to the first of which, *The Developmental Cycle in Domestic Groups* (1958), we each contributed. For that volume the central idea came from Fortes; but we circulated editorial responsiblity and each edited subsequent collections on a particular topic with a long theoretical introduction. We founded, too, a series, *Cambridge Studies in Social Anthropology,* which published some 70 volumes before changing its title—many by our staff and students, but including also works by authors from Europe, the Americas, and elsewhere.

Our backgrounds were very different. Fortes came from a family of Jewish migrants to South Africa, Leach from a lineage of mill-owners with interests in the Argentine, myself from a more modest background in the Home Counties. I had originally gone up to Cambridge on a scholarship to read for a degree in English literature and became interested in, among other things, aspects of the sociology of literature. After my first year at the University war broke out, and for the next six and a half years I was in the army. In the Near East one was brought face to face not only with a wide variety of humanity—Greeks, Turks, Egyptians, Palestinians, Jews—but also with the remnants of ancient civilizations. Directly I had an opportunity I began to read works such as Childe's *What Happened in History*? That opportunity came sooner than I anticipated, since I was captured by the German army at Tobruk in June 1942 and spent the next two and a half years in and out of prison camps in Italy and Germany. There I was also able to read fairly widely, following up various interests, but the situation in which I found myself also turned my attention to the variety of human behavior. Spending time with Italian peasants in the Abruzzi and with men of various nationalities, origins, and personalities in camp made me think about social relations in a more sociological and psychological frame. On my release I was particularly attracted to the work being done at the Tavistock Institute of Human Relations in London, which was concerned with returning prisoners of war.

When I returned to Cambridge in 1946 I completed a degree in English, then switched to the Faculty of Archaeology and Anthropology. After some years of inactivity in prison camp I was unwilling to think of spending more time in the rarified atmosphere of the University so, having completed a Diploma, I took a job in adult education of a more basic kind. Even when I decided to return to the social sciences it was not with the aim of spending my life working on "other cultures".

I went to West Africa not primarily because I wanted to become an Africanist but because I had become interested in the comparative study of human society more generally and intended to return with a perspective that would enable me better to look at European culture, possibly at those Italian peasants who had fascinated me when I was hiding among them, more likely the inhabitants of Britain itself with whose future I was deeply concerned in the aftermath of war. It is difficult to reconstruct the situation that faced many of my generation at that period. If I may put the matter a little more dramatically than an Englishman should, I saw post-war Britain under the Labor government of 1945 as the national outcome of many lives like my own: We had grown up between the two wars, with an interval of only 21 years from one terrifying destruction to the next, and lived our adolescence under the shadow of continental fascism, with its devastating oppression and annihiliation of man by man both in ideology and in practice. This period began with the Japanese attacks on China, the colonial wars of Italy, the civil war in Spain, and the expansion of Germany, against the background of widespread suppression and maltreatment in those countries. There followed for my generation some six-and-a-half years of life under arms, during which time all one could look forward to was post-war reconstruction, through the national government and through the United Nations.

That reconstruction obviously involved the dissolution of earlier empires, the whole process of decolonization that began with India in 1947 and that was envisaged, at least under Labor rule and to some extent by Conservative politicians as well, as gradually extending to the rest of the colonial territories. In a sense the deconstruction of the empire was part and parcel of the reconstruction of Britain. And although we were not fully aware of the consequences of decolonization, it was a heady prospect, indeed a heady actuality, too, for the major part of the process was over within some ten years, by 1960, the Year of Africa. The Gold Coast, where I chose to work, led the way and became the independent nation of Ghana in 1957 under the premiership of Kwame Nkrumah. It was this general background set against five years' residence in the country that led me to write about matters that touched on political theory and development.[2] Anthropologists had often tried to avoid administrators and problems of development. That seemed a churlish way of treating the new Africa that was coming into being so rapidly.

That new Africa was also demanding a history. Again earlier anthropologists had often steered away from historical considerations, partly because of the often rash speculations of their predecessors about the past, partly because of an alternative methodological approach associated with fieldwork, and partly for the practical reason that archives and records were officially closed for a 50-year period. The situation had changed in many respects. Particularly in France many anthropologists began to explore the historical dimension, and elsewhere, too, historians became interested in anthropological research

[2]For example, I wrote articles on "Consensus and dissent in Ghana" (1968), "Rice-Burning and the Green Revolution" (1980), as well as on general impediments to development in Africa as compared to Asia. Much else exists in the unpublished form of talks.

and approaches. The "structural-functional" paradigm was ready to be loosened in a number of ways.

My own choice of Ghana was largely personal. At Cambridge I was friendly with Joe Reindorf, later Attorney General, who started a PhD in history, finished a law degree at the same time, and then returned to Ghana to work with the firm of a relative, Victor Owusu. Owusu was closely identified with the Ashanti opposition to the Convention People's Party, although Joe's own sympathies lay elsewhere.[3] Since I had a family I did not want to work too far from Europe; in those days of boat travel, distance mattered. For this reason when I followed my colleagues to Oxford, after the spell in adult education, I was asked to talk to Meyer Fortes, the West Africanist, my first encounter with whom I have described elsewhere (17a).

In his review of social anthropology in West Africa, Keith Hart speaks of the structural-functional Cambridge school of Fortes and Goody (17). From the standpoint of ethnography and fieldwork, such a characterization is no doubt correct. But as he goes on to point out, the intellectual preoccupations in my research had other roots. Few students in the 1930s could avoid having to make some kind of resolution of their interests in two major figures, Marx and Freud. My contact with Marxism had by then been longstanding, but there were other influences. Through social anthropology I encountered Durkheim, through Harvard students of social relations I encountered Weber, as well as Parsons (with whom I later worked for a year when he was at Cambridge). But equally important to me in the early phases was Freud, as well as the interests I had developed in studying literature and history.

In literature I was strongly influenced by the "New Criticism" associated with F. R. Leavis and other contributers to *Scrutiny,* regarding not only the creative arts themselves but also their whole grounding in society. One further current that affected many of my generation was the logico-positivism of the Vienna circle and the subsequent transformations that appeared in the Anglo-Saxon world. Of particular importance for me was the work of the contributors to the *International Encyclopaedia of Unified Science* edited by 0. Neurath, R. Carnap, and C.W. Morris from Chicago (1938- ), which included works by E. Nagel, L. Hogben, and many others.

Two other factors of a different kind played a part in defining my fields of interest. Over six years of war had left their mark: Life in a prison camp led me to wonder how people got on or came into conflict with one another; life in the army encouraged an interest in the military side of "other cultures". Evans-Pritchard's clinical analysis of feud in his lectures in Cambridge in 1946-1947 helped to place those years of experience in a wider context, and

[3] I had later the pleasure of acting briefly as his clerk in the reknowned Al-Hajji Baba case in Kumasi in 1957, where the Government tried to expel him from Ghana as an alien.

the threat of nuclear warfare continued to keep "conflict studies" in the center of one's vision. If Marx drew my attention to modes of production, the "New Criticism" and logical positivism to modes of communication and of thought, so did the times lead me to consider modes of destruction.

Largely at Fortes' prompting I went to work in a community in Northern Ghana. These were the LoDagaa (or Lobi as the British then called them, or the Dagari or Dagara as they were known to the French and now generally to themselves), a study of whom had been listed as a priority in Raymond Firth's report to the Colonial Social Science Research Council.[4] The LoDagaa inhabited an area that lay astride the Black Volta, which served as the boundary between the anglophone colony of the Gold Coast (later Ghana) and the francophone colony of Haute Volta (later Burkino Faso). I was thus required to become familiar with the early French writing about the region as well as with French anthropology more generally. That encounter enabled me to indulge my enduring attraction to France and at the same time to modify, in the course of time and in marginal ways, some of my Anglo-Saxon attitudes.

Fortes directed my attention to the LoDagaa partly because he had carried out research among the "patrilineal" Tallensi and the "matrilineal" Ashanti and was interested in those societies of which the Yakö of Nigeria, studied by Daryll Forde, was the paradigmatic case, in which named unilineal descent groups of a patrilineal and a matrilineal kind existed side by side—that is to say, everyone was a member of one of each set. I became involved in the topic of descent of necessity, for it was the subject of lively discussion among graduate students. I also took up other topics on which people were working at the time, such as the developmental cycle of domestic groups (8), the maintenance of social control in acephalous communities (7), the role of the mother's brother (9), and the general theme of incest itself (6). These topics drew me into comparative analyses that I later pursued with Esther Goody using material on the societies of northern Ghana for the purposes of controlled comparison (10, 11). Another general question that struck me was how communities defined themselves not only in opposition to one another but often in relation to particular activities, so that among the LoDagaa one's identification was with those to the east in one context and with those to the west in another. That is to say, in the Gold Coast at that time there were no "real" Lobi and Dagari, just a series of communities who defined their similarities and differences by means of these roughly directional terms—

[4]The Colonial Social Science Research Council, working under the inspired secretaryship of Sally Chilver, was responsible for administering the grants that enabled Lloyd Fallers, Jim and Paula Bohannan, Al and Grace Harris, and Bob Armstrong to work, first at Oxford, then in Africa. At the end of the war the Council promoted surveys of research needs in the social sciences in West Africa (undertaken by Raymond Firth), in East Africa (hy Isaac Schapera), and in Sarawak (by Edmund Leach).

Lo(bi) = west, matrilineal; and Dagaa(ri, ra, ti) east, patrilineal (to simplify a more complex situation).

I was interested in both the subtle differences in the importance of descent groups among and for the LoDagaa and in their systems of kinship, family and marriage at the domestic level. Indeed I attempted, in the spirit of the times, to look at the total range of the community's activities. When I came back after one year, between field trips, I wrote a general account for the Bachelor of Letters degree at Oxford, which was later published as *The Social Organisation of the LoWiili* (1956). I then returned to Africa to live in another area of the LoDagaa country where more emphasis was given to the matrilineal clans. Here was a case of "double descent" along the lines of the Yakö in that movable property (cattle, money, grain) was visualized as belonging to the matrilineal clan (or *belo,* species) and was inherited between, first, maternal siblings and then by sister's sons—that is, between uterine kinsfolk; whereas immovables (houses and land) were transmitted within the patrilineal clans, between, first, maternal siblings and then to sons—that is, between agnates. My major interest in this situation lay in the problematic of Malinowski and Fortes, relating to the different nature of interpersonal ties in patrilineal and matrilineal societies, depending upon the systems of authority, transmission, and organization. That is, my interest lay in an attempt to link the social and personality systems in Parsons's terms, or, in more general terms, the approaches of Marx and Freud.

My doctoral thesis, a much-revised version of which was published as *Death, Property, and the Ancestors* (Stanford, 1962), was intended to be an analysis of the religion of the LoDagaa. Having already written a general account of the social organization, on my second return I completed a historical and ethnographic outline of the region in order to supply a context for my more detailed studies.[5] The concentration on religion resulted partly from the fact that Fortes had written two major studies on the political and kinship systems of the Tallensi, which were sufficiently like those of the

[5] *An Ethnography of the Northern Territories of the Gold Coast, West of the White Volta* (1954). I have subsequently published, often in obscure places, a number of contributions to the ethnographic, historical, and linguistic study of the region. I would like to acknowledge the collaboration of a number of scholars working in the lively Institute of Africa Studies at the University of Ghana, Legon, at that time under the direction of Thomas Hodgkin—especially Kwame Arhin, Ivor Wilks, and Nehemiah Levtzion. At the same time I would pay tribute to my long-standing collaborators among the LoDagaa, especially S. W. D. K. Gandah, and among the Gonja, especially the late J. A. Braimah, who became paramount chief of his kingdom and wrote extensively about its history; with Braimah I edited *Salaga: The Struggle for Power* (1967). I have said little about these historical papers in this account, but they were important in providing a diachronic view of the region as well as in trying to allow for the influences, both stabilizing and disruptive, that colonial governments had on the peoples among whom earlier anthropologists were working, whether in Africa, the Americas, or Russia.

LoDagaa to make me direct my attention elsewhere. Like Evans-Pritchard, Fortes left the monograph on religion to the end, as was the usual progression in analysing field material at the time, *The Work of the Gods* following on from *We the Tikopia*. I chose to take another direction. My material on the religious life was rich, partly because I had attended so many funerals and sacrifices, partly because of an earlier interest in myth and ritual, partly because I knew people who would discuss these matters, and partly because the Bagre ceremony was being performed in the valley below my house during my first year. Not only could I attend the public aspects of this initiation, but I had the good fortune to meet a man who had become marginal to the society itself. He offered to recite his version of the Bagre so that I could write it down, and he then took the trouble to explain many of its details. It was unheard of at the time to recite before someone who was not a member of the association, and at first I was reluctant to publish. But local friends thought it would be better to do so as a way of recording the richness of their culture.

My first aim was to present a general analysis of LoDagaa religion. Having previously sketched out the social organization, I was obviously concerned with those aspects linked to relatively permanent roles and groupings (as with ancestors and descent groups, earth shrines and parishes). But I was also interested in the turnover displayed by cults such as those associated with medicine shrines, which migrated from one group to the next over a wide range of territory. Such cults were marked by a rise and fall, by birth and obsolescence, by the recognition of the God who failed as well as by the innovative capacities of those seeking new and different solutions to old and persistent problems. A quest was involved, one that was intellectual and problem-solving as well as emotional and theological. The structure of meaning to the actors had first to be elucidated, including the overt symbolism of ritual acts.

Although the aim of the thesis was to present a study of the whole domain of ritual and religion, I never got further than an analysis of funerals (and of the ancestors), the topic on which I had begun, taking it to represent the point of transition and of transmission between this world and the next. On this one aspect there seemed already too much to say, especially if one tried to take into account what previous writers in a variety of disciplines had contributed. In any case the interest the subject held for me ranged outside the sphere of religion narrowly conceived.

Among the problems with which I was concerned in discussing LoDagaa funerals were those I related to the work of Marx and Freud. There were other influences. The French school, including the work of Van Gennep and especially Hertz, was most relevant; some have seen this trend as dominant. Others have taken the analysis of the role of kin groups based on matrilineal and patrilineal descent to lie at the core. Typical of this first view is the comment of Jacques Lombard: "De même, J. Goodie (sic), reprenant certains thèmes de R-B (Radcliffe-Brown), a expliqué la fonction de rites mortuaires en les liant à la structure sociale et plus particulièrement au statut du défunt et de ses proches" (19). Lombard contrasts the work of Middleton, Turner, and myself with that of Douglas, Lienhardt, and Beidelman who wanted to "désociologise le religieux" following the studies of Evans-Pritchard and the volume edited by Forde entitled *African Worlds*.

The contrast is too stark. It seemed hardly possible to deal with a *rite de passage* without building upon Van Gennep's pioneering work, nor did it seem useful to discuss "symbolic" meanings without recourse to the classic but equally simple techniques developed by Radcliffe-Brown and Srinivas—that is, without being concerned with "action". While I dealt with the way social groups, including descent groups, emerged and participated so clearly in the funerals, I was also specifically interested in (*a*) differences in funeral practices that were not connected with sociological variables; and (*b*) meaning to the actors of the acts, verbal and gestural, in which they were engaged, as a counterpoise to the interpretations of the anthropologist. Indeed my hesitation to use a variety of hardy anthropological concepts, such as ritual, religion, the sacred, and the profane—except as vague sign-posts—was precisely because they were not based upon, nor did they reflect, indigenous categories, which were more complex and more shaded than such constructs allowed.

Above all, my central thrust was aimed in another direction, more closely linked to much of my later work. In looking at these aspects of religious action and belief, I reviewed earlier studies of ancestor worship, of funerals, and in particular of patterns of grief and mourning. How did the behavior of individuals in these situations correspond to the wider contexts of their lives? The question raised issues considered by Malinowski and Fortes when they examined matrilineal systems with the aim of specifying the typical patterns of tension and cathexis, of ties and cleavages, in interpersonal relations. It was a theme pursued by Malinowski when he argued that the "Oedipus complex" was not universal but was linked to a particular range of social institutions.

The central theme of the enterprise was parallel to one that developed in the study of witchcraft, in particular of witchcraft accusations. Evans-Pritchard's book *Magic, Witchcraft and Oracles among the Azande* (1937) had attempted to map out the logic of African witchcraft in opposition to the view of Levy-Bruhl that "the primitive mind" was illogical or alogical, unable to perceive contradiction, a theme that I pursued in connection with a very different line of research. What Evans-Pritchard did not do was look at witchcraft in terms of the relationships of accuser, witch, and victim. Starting with the work of Nadel, these relationships came to comprise an important topic in the two post-war decades, in Europe as well as Africa, as the work of

Favret and others has shown. Gluckman extended the analysis to the realm of gossip, and my own work on ancestor worship raised problems of a similar kind. Who were the ancestors seen as most likely to demand sacrifices from the living, and were these relationships linked to the control they had exercised and to the benefits received? The tensions that arose between those who gave and those who received were in turn connected with the incidence of grief, with the particular situations of the dead and the living, and with the guilt about a deceased parent for whom one may not have done enough or whom one had left on his or her own in old age. That connection was often made at the less explicit levels of the mind, and the general link with Freud is obvious.

I was concerned to elaborate and differentiate the relevant kinds of tension that marked relations between the generations. It was not simply a question of "splitting the Oedipus complex" (between roles) as Malinowski had argued for the matrilineal Trobriands, where mother's brother counterpoised father, but of splitting up the Oedipus complex, the intergenerational relationship of conflict, into its analytic components, some of which were concerned with sexual jealousy, others with the process of socialization, and others with authority more generally. This is where Marx was relevent. For an important element in preindustrial societies of this kind was the tension arising out of inheritance, the tension between the holder of property and the heirs. My field data came from the two adjacent communities I have mentioned, in one of which all male property was transmitted between fathers and sons while in the other immovables (land and houses) passed in that way and movables between a man and his sister's sons (in both cases after full sibling in the same farming group). In the latter case, that of a fully fledged double-descent system, these tensions were particularly in evidence; they were quite explicit and the splitting quite formal.

My enquiry aimed to examine differences in the funeral and ancestral ceremonies of the two adjacent commmunities and to try to explain at least some of these by reference to the differences in the transmission of property. Not all differences were so explained. I had no monocausal answer. Others I considered to be the result of intellectual exploration, yet others perhaps as the result of cultural drift. But certain central differences I did see related to social relationships, and these I discussed in a chapter entitled "The Merry Bells: Inter-generational Transmission and Its Conflicts," which was the prelude to a general analysis of property and inheritance, before I engaged upon an enquiry into the institutions of the LoDagaa themselves. Those 50 pages are for me among the most critical I have written. Beginning with Freud and Fromm, I went on to refer to Engels's discussion of production and reproduction in *The Origins of the Family* (1884). I attempted here, and later in *Production and Reproduction* (1977), to see kinship and the economy as distinct but related, and to do so at a more formal analytical level than those who had concentrated upon them separately. I saw that a key lay in the area of the intergenerational transmission of goods, including the instruments of livelihood, especially in the basic means of production at the domestic level in precapitalist (or non-industrial) societies, as well as in the authority relations associated with their management.

By attempting to deconstruct intergenerational relations in this way, tensions based on sexual rights and domestic authority could be given separate consideration from those based on different forms of property rights, each in turn being linked to the processes of production and reproduction. In addition I needed to distinguish the transmission of office (succession) from that of property (inheritance) since these might well diverge. The latter gave rise to the kinds of tension brought out in the speech of Henry the Fourth to his son, Prince Hal (the future Henry V), when the latter finds the king sleeping in his room and, thinking him dead, picks up the crown. The waking king reprimands his son:

> Thy wish was father, Harry, to that thought.
> I stay too long by thee, I weary thee.
> Dost thou hunger for mine empty chair
> That thou wilt needs invest thee with my honours
> Before thy hour be ripe?. . .
> Thou hast stol'n that which, after some few hours,
> Were thine without offence;. . .
> Thy life did manifest thou lov'dst me not,
> And thou wilt have me die assur'd of it. . .
> Then get thee gone, and dig my grave thyself;
> And bid the merry bells ring to thine ear
> That thou art crowned, not that I am dead.
>
> *(Henry IV, Pt. 2, iv, 2)*

The notion of the "merry bells" was intrinsic to what I called, for short, the Prince Hal complex, which emerges in anticipatory acts of wish-fulfillment as well as in the mixed reactions to death itself, inducing feelings of guilt and appeasement.

Putting together these relationships, which featured conflict as well as cooperation, I presented a schematic table (Table 1).

Problems of succession to high office played a minimal part in LoDagaa life, for there was little or nothing that fell into that category. The last row of the table referred instead to enquiries among the Gonja of northern Ghana where I had been working with Esther Goody and where I was especially interested in one particular aspect of the political system. Instead of direct intergenerational succession, such as we find among modern monarchies,

**Table 1** The Transfer of Rights Between Roles

| Type of exclusive right | Authorized transfer (prescribed *propter mortem*) | Role relationships | Unauthorized transfer | Role relationships |
|---|---|---|---|---|
| property | inheritance | holder-heir | theft | holder-thief |
| sexual | levirate, etc | husband-levir, etc | adultery, incest, abduction, etc | cuckold-adulterer, etc |
| roles and office | succession | incumbent-successor | usurpation | ruler-rebel |

each vacancy entailed a lateral shift of power between different segments of the ruling estate. While this rotational movement was sometimes phrased in terms of fraternal succession, the "brothers" were very distant "relatives," the kinship term referring to the members of other segments of the ruling group. The important rule was that, in an expression from Mampong in Asante, the elephant never sleeps in the same place twice. No segment should hold office for longer than the reign of one incumbent (12).

Rotational and next-of-kin systems resulted in different lines of tension and alliance. When the prince was no longer the next heir to his father, he had more to gain by keeping him on the throne than by his disappearance. The contrast lay not only in the implications or consequences of rotational as against direct succession, but also in the possible predisposing factors ("causal" factors) that pertained to the systems of political power. In rotational systems, power is distributed, for instance, among the members of a mass dynasty each of whom retains an interest in the holding (or holders) of high office. The confinement of those eligible for high office to close rather than to distant kin means a narrow rather than a mass dynasty. The different ways power is distributed may relate to, among other things, the control of the means of destruction (or the means of coercion, as Ernest Gellner has suggested) on which most early states and many modern ones are ultimately based. In West Africa a narrow dynasty tended to arise where the means of coercion were controlled by a professional group, as often with firearms when they reached the simpler societies, partly because of the investment and technical skills required when arms were imported. Whereas a mass dynasty tended to be associated with the equality of status and opportunity associated with a ruling class of armed horsemen. The ruling estate in Gonja constituted such a mass dynasty comprising roughly 20% of the population; power was rotated between the owners of the means of coercion, that is, between the chiefs with their horses and armed followers.

Interest in the nature of the ownership of the means of destruction ran parallel to interest in the nature and the ownership of the means of production and the way that this effected and was affected by the intergenerational distribution of resources. At the political level many anthropologists wrote of feudal and tributary systems in Africa as if they were similar to those of Europe or Asia (12a). The comparison seemed to me weak—not so much because of the distribution of power as such but because of the differences in productive systems (including craft production) within which power was exercised. The major states of Eurasia were characterized by the intensive cultivation of "advanced" agriculture typified by the use of the plough or irrigation while a more extensive hoe cultivation marked Africa (and other areas such as New Guinea), usually involving shifting, slash-and-burn farming. The extensive system of agriculture did not prevent the rise of the state in Africa, but that state had usually to be based on taxes on trade (or exports) and on raiding neighbors for slaves rather than on the internal accumulation of surplus through primary agricultural production, since any such surplus was usually limited in size, in transportability, and in exchange potential. Booty production and taxes on trade obviously affected the external relations between peoples, giving rise to the characteristic distribution of states separated by bands of "acephalous" peoples. Internally, too, that difference affected the nature of hierarchy in that under extensive systems the control of land as a factor of production gave no overwhelming advantage to the rulers. The differential transmission of resources was also affected. Under a more "advanced" agriculture, land was a scarce good and its control differentially distributed within the hierarchy and between particular families. In such a situation different strata would have their own strategies for passing down property and managing the estate, partly in order to preserve their hierarchical position. Indeed different families would have their own strategies of marriage, management, and heirship. In *Production and Reproduction* (1977), I tried to sketch out some of the general implications of this difference for kinship relations—for example, in encouraging in-marriage rather than out-marriage. If there are few resources to protect, then the marriage of chiefs to commoners may be a sound political strategy, extending the cross-cutting ties that ensue from unions between status groups. Such marriages frequently took place in Gonja, where the political benefits were overtly recognized.

This interest led me to ask why, for example, one found formal adoption in Eurasia but not in Africa, although fostering was common in parts of that continent, as Esther Goody discusses in her work on Gonja and West Africa generally (4, 5). One function of the widespread adoption that existed earlier in Eurasia was to provide an heir for the heirless, and often a person to continue the ancestral cult after death. These considerations became relevant when the transmission of property, especially property in land that differentiated positions in the hierarchy, became of importance. Similar considerations applied to the distribution of monogamy and polygyny as well as of other institutions such as filiacentric unions (the incoming son-in-law), for one aspect of all these practices was their role as mechanisms of heirship and continuity, of transmitting relatively scarce resources between generations, as well as of passing down the management of those resources. While none of these features (nor indeed any features of human society) are monolithically tied to any other variable, they do constitute a cluster of features that I called the "women's property complex". Under such hierarchical systems efforts were made to maintain the position of daughters as well as of sons, especially by means of endowment and/or inheritance. Such modes of transmission were significantly linked to advanced forms of agriculture under which dowry as distinct from bridewealth prevailed, for it was then that the maintenance of the position of daughters became an issue.

This distinction was discussed in a volume of Cambridge Papers, *Bridewealth and Dowry* (1973), written with S. J. Tambiah. This work entailed a very broad comparison between Africa and the major societies of Asia and Europe. My interest in European kinship had been partly fuelled by the examination of inheritance systems developed in the book on LoDagaa funerals. But I was also struck by the fact that in Western Europe, unlike Rome, India, and China (but like Islam and Judaism), adoption was not practiced after the 5th century AD until the 20th century, or the mid-19th in parts of the United States. It was this situation that constituted one of the starting points for *The Development of Family and Marriage in Europe* (1983). The absence of adoption and certain other mechanisms of heirship in Europe seemed related to the fact that it was the Church that effectively established itself as the heir when family continuity failed (and claimed a substantial share in other situations). One priestly writer of 5th-century France described the adopted as "children of perjury." They cheated God of the goods that He had first given to mankind and that should be returned to Him at death through his church. Some allowance was made for the children of one's own "blood", but others were improper recipients of the goods rightly belonging to the Church and which should be used for God's purposes. In this way Christian doctrine and the demands of an ecclesia were consistent with the realignment of strategies of heirship at the familial level, and by this and other means the Church rapidly became owner of a third of the available farming land of Europe.

The work on which I have recently been engaged, entitled *The Oriental, the Ancient and the Primitive* (1990), pursues this analysis among the major states of Asia, including the ancient and modern Near East. While one can differentiate Christian Europe from the rest of Eurasia on the basis of what the Church took and how it did so, certain main features of kinship systems in the major states of the Old World are sufficiently similar in a structural sense to cast serious doubt not only upon the way many anthropologists and sociologists have treated this aspect of "the uniqueness of the West," but also upon the way this same theme has been pursued by a number of European historians of the family (and especially by English ones). This difference with regard to the influence of an established ecclesia with monastic institutions is less clear under Buddhism, which may be why Japan, Tibet, and Sri Lanka share certain structural resemblances. It is true that the marriage age for both men and women was later in Europe and that in-living life-cycle servants were more common, but these features of domestic life in the West do not seem sufficient to account in a significant way for any predisposition it had towards the development of capitalism, as has sometimes been suggested or assumed. Indeed as we look at the rapid development of industrial capitalism in East Asia (and elsewhere in Asia) today, and at the increasing contrast with Africa, the uniqueness of the West seems less significant than the uniqueness of Eurasia. To this "uniqueness" Asia has contributed a great deal and has never been the stagnant oriental society (though all societies have been this at particular periods) of which European writers have written.

In this recent volume I tried to modify the notion that an unbridgeable gulf exists between the kinship systems of premodern Europe and Asia—a constant theme of European historians. But anthropologists, too, have been overly ready to compare Chinese and South Indian "kinship systems" with those of the Australian aborigines, or have singled out features in Ancient Egypt and Arabia to compare with those of Africa. Such comparisons take a restricted view of what constitutes a "system" of kinship and marriage. My thrust has been to look at the entire domestic domain, especially in relation to the economy, and to argue that neither Oriental nor Ancient systems were "primitive" in this sense but resembled the preindustrial societies of Europe in many significant respects.

This work on kinship inevitably raised matters that verge on demography, and it has often been historical demographers, such as the members of the Cambridge Group, who have discussed crucial issues about comparative family structures. Demographic themes have played a consistent part in West African ethnography; intensive census data had to be collected in any examination of the cycle of domestic groups discussed by Fortes (see 8), but both he and others such as Oppong made more systematic attempts to integrate their findings with wider demographic concerns. I sought a way of assessing the number of individuals who, under different demographic conditions, would be left without male or female heirs, a question that was of direct importance in the resort to certain strategies of heirship. There was also the possibility that the different kinship regimes of Africa and Eurasia might have varying effects on the number and especially the sex of children in a family. N. Addo and I collected a large sample of completed families in Ghana and found no evidence of any discrimination in favor of one sex, but rather a strategy of maximum numbers (12). This evidence was compared with other large samples of sibling groups where these were available, but we found less difference than anticipated (14, 15). However, the results were consistent with the fact that in Africa, as distinct from Asia, there was no evidence of any discrimination by sex in, for example, the survival of children. Other enquiries showed that people went as far to hospital for a daughter as they did for a son, and it was generally recognized that in a bridewealth system any domestic group needed equal numbers of both sexes to get the best results from marriage transactions.

I began this account by explaining how I was led to work on the ritual and religion of the LoDagaa partly because of my involvement with the Bagre association during whose rites a long myth is recited. This work became an important strand of my research from that day to this. I had been interested in myth and other genres, both in oral and written cultures, mainly because I had read some English medieval literature, to which I was directed partly by the interpretative notes to T. S. Eliot's "The Waste Land". In prisoner-of-war camp in Germany I had been fortunate to find a copy of Frazer's *The Golden Bough* in the library as well as E. K. Chambers's study of *The Medieval Stage* and other similar works much influenced by earlier anthropology. One of the first series of lectures I delivered in Cambridge in the early 1950s was on the subject of myth, before it had become so fashionable a topic with the publication of Lèvi-Strauss's major works. As the result of these interests I was never inclined simply to treat myths and legends in the Malinowskian fashion as "charters" of social situations, as many of his pupils tended to do (see, for example, 3 and 18). Just as in my ethnological work in the region I saw an element of "history" in legend and genealogy, so too I saw some intellectual quest in myth, with the important level of meaning located in the actor domain rather than purely in the deep structure posited by those who tended to see the surface meaning as "absurd" and fantastic. I wanted to understand the explicit as well as the implicit meaning to the actors, and hence I was interested in the words as words, in the text as text, or rather in the utterance as utterance.

The advent of the transistorized tape recorder made this possible to an altogether different extent than before. Of course, electronic recording had previously been available and was used in ethnographic research, mainly for recording music and song. That was the case for the important work of Milman Parry and A. B. Lord in Yugoslavia in the 1930s, when Parry set out to study some of the characteristic features of "oral" epic in order to determine the status of that great interstitial figure, Homer. Lord subsequently published a volume called *The Singer of Tales* (1960), which has rightly been the source of much subsequent inspiration, research, and debate.

But Parry and Lord were working with café singers in Europe, with secular song in a region where electricity was available and where the church maintained its hold over religious action and belief. It was a very different story in field situations where the recitals were ritual, often secret, and made only in formal conditions, and where the possibilities for mechanical recording were minimal. The change in technology had important effects on the study of oral discourse in the simpler societies, probably as important as many broad "theoretical" shifts, especially in the sphere of pragmatics. Its influence has been of a cumulative kind. For the first time it has enabled anthropologists to record long recitations like the Bagre in situ, in the actual conditions of performance—in other words, to create a reasonably accurate text from a ritual utterance, one that can be checked back against the tape. It has also enabled them to make repeated recordings over time and so to examine questions of continuity and change in these standard oral forms. In earlier times only one version was usually available, and even that was often a summary of a recitation written down from an "informant's" recollection of the original performance. Not only did the summary concentrate on the retellable, narrative elements, but the impression was also left that there existed a single authorized version. Any differences tended to be attributed to the mistakes of observers, or alternatively to structural factors. The new technology made it possible to examine the elements of individual intellectual exploration, of creative innovation, as Barth has done for the Ok of New Guinea (1) using comparative evidence, or as I have begun to do for the Bagre—more programmatically, perhaps, than in actuality. Of course such explorations occur within a set of constraints, but which constraits will be relevant in any specific case is difficult or impossible to predict.

The first version (published as *The Myth of the Bagre* in 1972) I wrote down from dictation; the process took a full ten days. Since then I have worked with my friend, S. W. D. K. Gandah, with whose help we have recorded, transcribed, translated, and annotated another four versions of the Black Bagre and ten of the White, a long and arduous program of work. One of these versions was published in 1981 as *Une Recitation du Bagré,* with French and English translations. Having originally thought the recitation highly standardized, we were suprised to find the range of variation. Together with Colin Duly, an attempt was made to analyse this range by means of the computer, the results of which were set out in an unpublished report for the Social Science Research Council (13). Our sample was small but nevertheless produced a far greater body of data than for any other long oral recitation to date. It enabled us to compare the performance of different speakers on the same occasion and of the same speaker on different occasions. The former varied more widely than the latter, indicating that no one learned precisely anyone else's version but always introduced variants of his own. The result was that suprisingly different recitations were to be found in nearby communities, though they were all recognized by the actors to be the same "Bagre." The dictated version was longer, richer, and more sophisticated linguistically and thematically than those we recorded electronically, owing something to the personal accomplishments of the speaker, but also to the nature of dictation, the pauses in which enabled him to elaborate to a greater extent than was possible in the rapid recitativo of thc actual ritual performance. The dictated version was also significantly more theocentric in that greater attention was paid to the part played by the High God, although he is given much more prominence throughout than would appear from ordinary social interaction.

I have used the work on the Bagre in a number of ways, but chiefly to draw out some features that contrasted with written literary genres. That contrast has turned on the role of verbatim memorizing, which I find charateristic of societies with writing, the emerging division between creator and performer (both artists in their own way), and the tighter structure of written works. As a consequence I would see both the work of Homer in Greece and the Rig Veda in India as the products of early literate societies rather than purely oral cultures.

These studies of the effects of the introduction of writing on oral genres were published in *The Interface between the Written and the Oral* (1987). This work was part of a series of studies that began with a fortunate collaboration with Ian Watt, Professor of English at Stanford University, on "the consequences of literacy," the title of the article we published in 1963 (16). Watt and I had attended the same College at Cambridge where we were influenced by the work of Q. D. Leavis and others on the relationship among changes in the production of reading material, the nature of the audience, and the content and form of literary works (see, for example, 31 and 33). We had also been through a long period of warfare, and especially of imprisonment, where books were scarce or absent, that led us to pursue an interest in the influence of modes of communication on human societies, especially the introduction of writing. Watt provided much of the initial inspiration, but my later engagement was greater, for the project made possible an historical or even "evolutionary" approach to some of the problems of differentiating "simple" and "complex" societies, which were the staple of anthropological discussions and certainly of anthropological categories. Differences in cognitive processes could be looked at in a developmental way, which made me less willing to accept in its entirety the elusive relativisim of much anthropological discourse.

In our essay we attempted a broad assessment of the contribution that writing, and specifically alphabetic writing, had made to human cultures. Acknowledging the work of Havelock and of members of the Toronto School, we looked at the intellectual contributions of the Greeks against the background of their invention of a fully alphabetic script, especially in regard to ideas of "myth" and history, concepts of time and space, notions of democracy, categories of intellectual activity, and the notions of logic and contradiction that Levy-Bruhl had found absent from "primitive" societies. While accepting that Evans-Pritchard was correct to refute this proposition, we argued that "logic" in the limited philosophical sense, as well as the refinement of proof and contradiction, were critically dependent upon the use of writing.

Subsequently this research was continued in an edited volume, entitled *Literacy in Traditional Society* (1968), which examined instances of the impact of writing on mainly tribal societies. In the course of this later work I came to qualify the weight placed upon alphabetic writing in Greece, partly because the introduction of the alphabet itself was less clear cut than formerly suggested by Indo-European (as distinct from Semitic) scholars, and partly because a number of the features that had been seen as consequential on the introduction of that script appeared as embryonic in earlier forms of writing itself. The results of these enquiries into the cognitive effects of early writing systems appeared in the *Domestication of the Savage Mind* (1977) in which attention was drawn to the role of lists and tables in early written cultures as ways of organizing information in non-speech-like ways. That work led to a profitable collaboration in the field of "literacy" studies with psychologists Michael Cole and David Olsen. The former invited me to cooperate in his research into the uses and cognitive implications of the indigenous Vai script in Liberia, a test case in such investigations because learning this script took place separately from school education. We were fortunate to come across a body of writing by A. Sonie that amply illustrated the powerful role which writing could play in reorganizing information for the purposes of recall, logical presentation, and accountability.

The research proceeded in two directions. The *Interface* (1987) volume gathered together studies on genres and similar topics. Previous to that, in *The Logic of the Writing and the Organisation of Society* (1986), I had suggested ways the introduction of writing had influenced the domains of religion, the economy, politics, and law. At the same time I had tried to elucidate certain anthropological and sociological problems surrounding the broad difference between "primitive" and "advanced," simpler and more complex societies. One of the major pressures behind the studies on writing had been to get away from the simplistic dichotomies of much social science, in order to isolate some of the mechanisms behind sociocultural changes; one such set of mechanisms consisted of the mode of communication. But whereas scholars had paid much attention to the effects of language on social life, the forms and implications of writing had received comparatively little notice. That was a situation I tried to remedy.

The tape recorder has changed not only the analysis of myth and the procedures of the anthropologist. It has also produced an emphasis on discourse, on letting the actor speak for himself. Before the appearance of sound movies, ethnographic films had to invent a spoken script, a text. Now, as in the Granada television series *Disappearing Worlds,* the natives could speak for themselves. But the result was in some ways an impediment to research. Not only was there an overabundance of material, not only did investigators think less before they turned on the machine, but there was even a tendency, which remains strong, to consider a presentation of the actor's picture as the be-all and end-all of analytic endeavor. That is *verstehen* carried to an illogical extreme. Of course there is a lot to be said for recording as much as we can about "disappearing worlds" before they finally disappear. But this level of ethnography, of reportage ethnology, does not require any great expertise apart from a journalistic ability to get people to talk into the mike. And it is no substitute for theoretically oriented research or even for the analysis of research materials within the framework of a scholarly tradition. Understanding the meaning to the actor may be the ultimate goal for some literary studies; it hardly exhausts all levels of enquiry.

The remaining sector of my work relates both to the contrasting sociocultural situations in Africa and Eurasia and to the implications of writing. In reading analyses of cooking in Africa, I was struck by how they differed from my own experience, in terms both of the traditional patterns and of what was emerging in interaction with Europe and the rest of the world (*Cooking, Cuisine and Class,* 1982). Quantity apart, I found little evidence of different forms of cooking even within most of these African states where a hierarchy existed, no development of an *haute cuisine*. But as we have seen, different kinds of hierarchy were involved in Africa than in Eurasia where cultural activities and values were by and large held in common. Partly because of frequent intermarriage between the different groups in the hierarchy, there were few tendencies for the emergence of subcultures, except in those ethnically distinct and endogamous ruling classes found in some of the interlacustrine kingdoms of East Africa. Partly because of the nature of the hierarchy, partly because of the productive system, partly because of the absence, except under Islam, of writing, there was less tendency to elaborate a distinctive high cuisine of the sort we find in many of the major societies in Eurasia, with their considerable literatures on cooking and eating, and their institutionalization of the differentiated production and consumption of food.

My most recent book, *The Culture of Flowers* (in press), pursues what is on one level a similar theme. Visiting Bali and carrying out some limited fieldwork with Esther Goody in Gujerat, India, I was amazed at the use of cultivated flowers, especially for worship, for which they were deliberately produced and purchased. This "non-utilitarian" form of agriculture was virtually absent from Africa, where the usual form of offering to a shrine is blood sacrifice. A partial explanation of the difference is not hard to find. It concerns the lack of stress in Africa on intensive agriculture, although tobacco, onions, and rice were cultivated by such methods. When I looked at forms of ritual, at pictorial representations, or at the imagery of poetic forms, I found many references to trees, leaves, and roots, but little or nothing on flowers, wild or domesticated, except along the coastal regions of East Africa where influences from India and the Near East had played an obvious part. My suggested reasons for this state of affairs were several, and partly connected with the nature of agriculture. As a comparison I was led to look in greater depth at the major uses of flowers in Europe and Asia. But initially I concentrated upon the history of flower use since the Bronze Age, its development in the Near East, and especially Ancient Egypt, its culmination in Rome, and then the rapid decline under Christianity, where the use of flowers was connected with pagan worship, with luxury, as well as with a certain conception of God and a Biblical reluctance to represent His creatures. The effects of this ambivalence on the history of Europe were radical. After the collapse of Rome, that continent saw a dramatic decline in the use of flowers, which had hitherto so often decorated the altar, the priest, and the sacrificial animal itself. This decline was paralleled by the virtual disappearance of three-dimensional statuary. Europe also experienced a decline in botanical knowledge which, beyond a certain threshold of folk usage, was partially dependent upon the representation of plants, whether as separate images or in manuscripts. The relative backwardness of medieval Europe with respect to Asia is a significant comment on the effects on systems of knowledge, not only of the barbarian invasions but of Christianity itself.

Europe saw a gradual renaissance in the growth, the use, and the representation of flowers in graphic and literary works from the 12th century onwards. The repertoire greatly expanded with the expansion of Europe, with the demand of the aristocrats and bourgeoisie, and with the supply of new plant varieties by traders, missionaries, administrators and later by specialist plant hunters. Before flowers could play a role in cemeteries and in worship they had to overcome deeply established "puritanical" beliefs that tended to differentiate Protestant from Catholic regions. The continuing differences (and similarities) in the use of flowers in Europe and America, the expansion of their use with the expanding economy, and the world market in cut flowers made possible by the airplane (and before that by the train and truck) are the subjects of several chapters. That urban market was linked to the growth of the formal, written "Language of Flowers" that sprang up in Paris in the early part of the 19th century and soon became accepted as the rebirth of a vanished code of eastern origin. It spread throughout Europe, and the efforts made to adapt the language to the United States provide an interesting example of the domestication of the foreign mind.

I pursue the themes of specialist production, of the stratification of use, and of a perpetual ambivalence in the culture of flowers in India, China, and Japan, partly based on observations of the markets and rituals, of the houses and temples, in those countries. At the end I return to the theme of the ambivalence about representation in Africa itself, this time in respect not of God's creatures but of the High God himself, and often of other divinities as well. I argue that the reluctance to portray the Creator God displays a concept of divinity at odds with the characterization of African religion as animism and fetishism. The intellectual and cognitive problems of African thought appear closer to those of Western man than is often supposed, especially when we can point to precise reasons for some of the difference. At this level, too, it is necessary to reconsider assumptions about the uniqueness of the West.

I have tried to give somc idea of the themes that have been important in my research. One constant regret has been the failure of sociocultural anthropology to become more cumulative. Part of this failure has to do with the methods favored over the past half century. Many anthropologists seem imprisoned by their rejection of any method other than fieldwork, being unprepared to see how their enquiries relate in a systematic way to those of others. Local knowledge is essential, but for many purposes it is a beginning rather than an end. We have come the full cycle since the 19th-century addiction to a version of the comparative method. In my view it is essential to integrate and test our observations and conclusions with material gathered by other scholars in this and related fields, using whatever methods we can, imperfect as these will always be. In these resources I include the Human Relations Area Files, which I have tried to use for this purpose. The rejection of techniques other than fieldwork and speculation tends to lead to a withdrawal into deep descriptions of "my people," combined on the one hand with unsystematic global statements and, on the other, with the search for new gods; as with medicine shrines, the old gods become obsolescent as they fail to meet impossible demands. Thus there are shifts—for example, from structuralism to post-modernism—on a basis analogous to the "global exchange" ("global replace") function in computer software rather than on the modification and development of existing programs.

I have said little on my work in changing social systems and in the history of languages and ethnography of Africa, even though these took up much of my time and energies in the 1960s when I was frequently in Ghana. That gap is partly because I have not completed as much in these areas as I wished. Nevertheless the bright hopes for the future and the harsher realities of the present have formed a constant theme of my other research. As with other colleagues, that research involved entering some of the no-go areas of earlier scholars and making use of the work of various fields, especially history.

This breach in disciplinary walls has its dangers, and it could be argued that the so-called crisis in social anthropology (though crises exist all the time and in all fields) has resulted in part from discarding the boundaries accepted by earlier workers in the field, including their tighter paradigms. There seems to me much truth in this claim. Anthropology has become diffuse in a number of ways, which means there is room for a range of approaches. For me the way ahead is not the way back but consists in supplementing the analysis of sociocultural "facts" or situations with a problem-driven approach. That means looking for evidence where it exists rather than confining oneself to arbitrary boundaries, useful as these have been for specific problems or topics.

I have drawn much satisfaction from working with scholars in other fields, with historians (especially those associated with *Past and Present, Annales E. S. C.,* and the Cambridge Group), with psychologists, and with those in the neighboring fields of literature and oriental studies. An anthropology that does not or cannot communicate with other disciplines stands in danger of placing an excessive value on its own conclusions. It is only too easy to draw back to the safety of the community one has studied (or even one's individual "informant" in some American traditions), about which one may know more than most of one's colleagues; or perhaps to seek the shelter of a hermeneutic approach to which only the writer holds the key. But knowledge exists in communication not only within arbitrarily defined fields (for that is essentially what we are dealing with) but also outside them. Fields such as cognitive anthropology seem sometimes to aim not to widen knowledge and provide a link with other students of cognitive processes but to protect and separate off a distinctive anthropological patch. Yes, we say, we anthropologists are also interested, but in our own way and on our own terms. That has always seemed to me the way to ruin. There is no anthropological truth, enlightenment, or even insight that is not related to the work of scholars in other fields.

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