

History, Culture, and the Comparative Method

A West African Puzzle

At the most general level, comparison is not a special method or in any way unique to anthropology.¹ Comparison is implicit in any method of deriving understanding through *explanation*—that is, by determining the sufficient and necessary conditions for the existence or occurrence of any phenomenon or action. To say *why* a thing is so is to indicate particular obtaining conditions, and it follows that where these conditions obtain otherwise, so also must the object of explanation. If it does not, the adequacy of the alleged explanatory conditions, or the description of the *explanandum*, or both, are called into question. Comparison's key role, then, is as a test on explanations, in the manner classically set out in John Stuart Mill's *System of Logic*, the Method of Difference providing a more powerful test than the Method of Agreement.² This is as true in principle for explanations of occurrences in daily life as it is for those sought in science, as true for explanation of sociocultural as of natural phenomena. The more elaborate or systematic explanations that we call theories may be little invoked in fields like history or textual criticism, but the logic of explanation is still present in such seemingly idiographic exercises as the construction of a plausible chain of events in history or the determination of a most likely reading in a classical text.

But the West African puzzle of my chapter title does arise from a theory: namely the systematic explanation that Robin Horton has given for the widespread occurrence, timing, and distribution of conversion to the world religions in Africa over the past two centuries.³ Briefly, it explains this as an adaptive response to changes in the scale of people's social experience. As traditional African religions make cogent sense of living in localized, small-scale communities, so when people move into a wider field of social relations—as through labor migration or more extensive trade networks—they are drawn to more general, transcendental forms of

religion. This theory made much sense of the Yoruba data (see further chapter 7) but failed completely to explain why the trajectory of religious change of the seemingly comparable Akan should be so different. Since the Akan yield nothing to the Yoruba in terms of the kind of factors that Horton's theory specifies as relevant to conversion—they even had earlier direct relations with Europe, a richer export-oriented colonial economy, the earlier development of modern education, and so on—why should their religious development have been markedly so much slower and more uneven?

In 1960, according to the *Population Census of Ghana*, just over 60 percent of all the Akan were reported as being adherents of world religions, the great majority of them Christians. By contrast, already by 1952 well over 80 percent of Yoruba were Christians or Muslims, though the proportions varied considerably by region.⁴ By 1960 the difference between the two peoples had grown to over 30 percent. Only after the mid-1960s did this gap start to close, with the further growth of Christianity among the Akan. Other divergent features of their religious histories appear to correlate with the difference. In fact, what first pointed me in this direction was puzzlement as to why the strains of high colonialism had produced a Christian-prophet movement known as Aladura among the Yoruba, whereas the main Akan response had been to turn to pagan antiwitchcraft shrines.⁵

So we have to look elsewhere than to the factors specified in Horton's microcosm-to-macrocosm theory in order to explain the Akan/Yoruba difference. The explanation I shall eventually propose—that it is to do with the contrasting relationship between religion and political authority in the two societies—will involve us in a critical reappraisal of the strong comparative literature produced by social anthropologists, mostly in the 1960s and 1970s, about the conditions of state formation in West Africa. But to get our intellectual bearings here, it is helpful to go further back and consider the tradition of comparative study from which it arose, and in which *the* comparative method occupied a central position. This was seen as a means toward developing a natural science of society, in opposition to history as the study of unique sequences of events. But it has been far from being a unified enterprise; and I shall argue that it has involved several distinct modes, which differ in how they handle history. This will bring us back to the Akan/Yoruba contrast, where I shall argue that they need to be compared in their histories, not (as with most anthropological comparison) as static social systems.

MODES OF THE COMPARATIVE METHOD

The comparative method, sometimes argued to be *the* method of social anthropology⁶ or treated as if it were one, single thing,⁷ exists in at least five distinct modes:

1. a single, universal, ideal history or natural history of society;

2. a branching, concrete history, on the model of comparative philology;
3. where history is denied or ignored, as comparison is used to derive sociological universals or general laws;
4. where a degree of common history is presumed, as in regional comparative studies;
5. where it is histories, not societies, that are compared.

These modes tend to be products of particular historical moments, but at the same time they have a perennial appeal, since they represent distinct logical options for the analysis of social phenomena.

Mode I: An Ideal, Universal History

Mode I began as a projected natural history of society or *histoire raisonnée*; and it involved the search for a single, logically appropriate (and hence also normative) sequence of stages. The comparative method was to provide the confirming evidence. This mode existed fully fledged by the 1760s and 1770s in the four-stages theory of Smith, Turgot, and Millar.⁸ The presents of backward societies were the equivalent of the pasts of advanced societies, so that comparative evidence from contemporary non-European societies could be used to fill in or corroborate evidence for stages of Europe's past. "It is in [the American Indians'] present condition we are to behold, as in a mirror, the features of our own progenitors."⁹ The nineteenth century produced much fresh data, more complicated stage models, and several special applications (e.g., to marriage types, forms of religion), as well as the authoritative paradigm of comparative anatomy and physiology, worked through most thoroughly in Herbert Spencer's theory of social evolution; but the basic components were the same.

Though social evolution had ceased to be the absolutely paramount form of social thought by 1914, this mode of the comparative method continued to be practiced in anthropology for some time. Indeed one could hardly find better instances of it than in such late works as those by L. T. Hobhouse, G. C. Wheeler, and Morris Ginsberg, or by A. M. Hocart.¹⁰ Neoevolutionism apart, some of its devices continue to find valid heuristic employment within projects of a quite different overall character. For example, the device of using an undeveloped community as a model to reconstruct the baseline form from which a culturally related but developed community has grown has been used by M. G. Smith (contemporary Abuja = pre-Fulani Zaria) and Robin Horton (contemporary Niger Delta fishing villages = New Calabar before the Atlantic trade).¹¹

Mode II: A Branching, Concrete History

Mode II emerged in the early nineteenth century, its paradigm being comparative philology. The achievement of William Jones and Franz Bopp was to explain the affinities between Greek, Sanskrit, and other languages in terms of their descent

from a putative common ancestor, Indo-European, and to work out rules governing the phonological shifts that lay between them. The essential point of comparison here was to reconstruct a particular *ur*-form, the actual histories of the languages being paths of divergence from it. Compared with Mode I, Mode II dealt with several actual histories rather than with one ideal or normative history, and its focus was the point of origin or departure rather than the path of development from it. Moreover, whereas Mode I depended on a unity grounded *in nature* ("the psychic unity of mankind"), Mode II pointed to a limited and *cultural* unity, that of the Indo-European (or Ural-Altaic, or Semitic, . . .) stock. A linguistic version of Mode I was found in a theory like Alexander von Humboldt's, which held that *all* languages, by virtue of their common human nature, pass through the same sequence of developmental stages. Even in modern anthropology there are instances of Mode II, such as Luc de Heusch's study of Bantu mythologies.¹² Besides Claude Lévi-Strauss, the major influence on de Heusch was the work of Georges Dumézil on the transformations of mythical archetypes in Indo-European cultures.¹³ Both Dumézil and de Heusch, characteristically, are more concerned to demonstrate the existence of an *ur*-form that serves to bring out the resemblances between diverse myths than to map the historical path of that model's transformations.

COMPARISON: FOR OR AGAINST HISTORY?

All use of the comparative method in the nineteenth century, and especially in its dominant Mode I, was informed by two profound inclinations. The first was to reduce a vast and perplexing variety by postulating an underlying unity of some kind: in the terms of Mill's *Logic* the Method of Agreement got vastly more attention than the Method of Difference. Consequently, the manifest variations or differences are less often explained than set aside or treated as superficial: comparative analysis thus pointed away from history.

Second, there was the impulse to make sense of things in terms of how they had come to be, in terms of origins, sources, or paths. That led Auguste Comte to regard the comparative method (Mode I) as a *méthode historique*, which for him also had the appeal of providing scientific grounds for divining the path into the future. But this is a historical approach only in a very particular sense: in the sense of dealing with time and change but not in the sense of dealing with the unique totalities or conjunctures, the action and the contingencies, out of which concrete instances of social change are formed. Spencer went further and expressly set the project for a science of society in opposition to any notion of a humanistic history, in terms of a series of antinomies: process vs. events, structure vs. individuals, necessity vs. contingency, and so forth.¹⁴

It was the legal historian Frederick Maitland who saw that such ahistoricism was self-defeating and succinctly stated comparative anthropology's dilemma:

“by and by anthropology will have the choice of being history or being nothing.”¹⁵ Rarely has a clear statement been so often misunderstood by being read out of context.¹⁶ Maitland was *not* telling a functionalist anthropology that it should study social change. His essay “The Body Politic” was directed at the whole organicist metaphor in which the comparative method (Mode I) sought laws of development, taking Spencer as the great exemplar. His point was that processes of change must be seen in terms of contingencies and specific conditions, not as the working out of immanent laws of organic development. The great irony was that, whereas Maitland wanted the time perspective without organicism, what British anthropology eventually produced after the structural-functionalist revolution was a form of organicism without the time perspective.

The fundamental methodological issues here were posed most sharply in Germany, where a strong attraction to evolutionary and organicist models of society coexisted with the greatest contemporary school of historical scholarship and an antipathy to Anglo-French universalism and utilitarianism in such fields as law and economics. The famous *Methodenstreit* concerned the antithesis of history and science, of *Geist* and *Natur* as objects of study, and of the placement of any so-called social sciences in this academic scheme. Sociology was precisely what Max Weber called his attempt to transcend the distinction, to meet scientific standards in the definition and analysis of historical problems without denying the meaningful character of their subject matter.¹⁷ But for all Weber’s vast influence on the history of sociology, anthropology was shaped instead by the rather different response to this dilemma proposed by Franz Boas, the main conduit by which German historical idealism was transmitted to American anthropology. Boas polarized the historical method, concerned with the development of unique cultural wholes, and the comparative method.¹⁸ The latter sought to establish synchronic links between discrete variables expressed in the terms of a general theory.

This distinction between history and comparison was already implicit in what has come to be called Galton’s Problem.¹⁹ At the first presentation of Edward Tylor’s famous essay in Mode I comparative method on the evolution of systems of marriage and descent, Francis Galton drew attention to a major difficulty with its research design.²⁰ How could one tell whether the adhesions or correlations between variables were independent cases of the postulated causal relationships between traits, thus serving to confirm the theory, or were the result of societies’ borrowing traits at some particular stage in their history? The problem indicates the tension that must exist between the search for a theory specifying causal relations that hold irrespective of time and place, and the evident fact that social variables may be rather loosely fitting and combine in unique configurations (cultures) under contingent circumstances (history). In that sense, as Boas saw, both culture and history presented refractory materials for the comparative method.

Mode III: History Ignored

Mode III applies when the comparative method is detached entirely from considerations of time and change. This was decisively achieved only after the structural-functionalist revolution, but some of the groundwork had already been laid. Even when, as with Mode I, the ultimate object of the comparative method was to construct a natural history of society, the temporal sequence was essentially something added to the correlations from outside. The sequence itself usually followed from some naturally ordered feature such as population size or density, degree of social differentiation, or level of technology. The comparative method was to determine the corresponding sequence of religious beliefs, kinship systems, ethical values, and so on; and obviously it could continue to be used apart from any social-evolutionary project.

Moreover, there is an ambiguity in the very notion of explaining a thing by reference to its source or origins. This may be interpreted phylogenetically, in which case an institutional history (as with a language's descent from an *ur*-form) is required; or ontogenetically, in which case the genesis in an individual of an instance of the thing is required. These two interpretations can be combined, as in Freud's theory of religion, where a historical myth about its supposed origins is taken up in an account of the origin of individual neuroses that reach out to religious solutions. We find the same thing in James Frazer. For besides the evolutionary progression from magic to science, he also seeks explanation by looking for a link between some need or habit of thought inherent in human individuals and some type of magicoreligious action. The intellectual tedium of *The Golden Bough* is largely due to the fact that the vast range of comparative materials is used to provide repeated illustration of such linkages between source and effect according to Mill's Method of Agreement.

We are here only a very short step from Mode III, which was described by Alfred Radcliffe-Brown as a means "to pass from the particular to the general . . . to arrive at the universal, at characteristics which can be found in different forms in many societies."²¹ This is the Method of Agreement exclusively and *à l'outrance*. In the next generation this universalist ambition was sustained above all by Meyer Fortes. In *Oedipus and Job in West African Religion* Fortes acknowledged the lead of Frazer in the great project "to bring home to us the unity behind the diversity of human customs," and where he refers to the beliefs of other West African peoples it is only to point out the similarities, not to use the differences to get a better explanatory purchase on the specifics of the Tallensi situation.²² Again, in "Pietas in Ancestor Worship" he gives far more attention to parallel cases that fit his theory of ancestor worship as a ritualization of lineage authority,²³ and even to extensions of it to such spheres as the "pietas" displayed by Russian cosmonauts and Cambridge college fellows, than to problematic countercases such as the Tiv that might sharpen up the explanation.²⁴

The conditions for finding generalizations applying to “all human societies, past, present and future”²⁵ were better met when Lévi-Strauss displaced the subject matter of anthropology upward, from social relations to cultural forms such as myths, and explanation was sought in terms of laws of the mind, not of society. Rodney Needham’s book *Exemplars*, written very consciously as comparativist, shows the clear outcome of Mode III. Though Needham considers a historical sequence of writers, neither their pastness, nor the temporal relations between them, nor their historically specific circumstances are of essential concern to him; for through comparison he is looking for “fundamental inclinations of the psyche” or “natural proclivities of thought and imagination.”²⁶ Such things point to “cerebral vectors” as where explanation must ultimately lie; and at that point the natural science of society teeters on the edge of physiology.

Mode IV: Regional Comparative Studies

The trajectory of Mode III, from Radcliffe-Brown to Needham, was not, however, the most typical development of the comparative method in social anthropology from the 1950s onward. This was Mode IV, where more limited comparisons are essayed, usually dealing with particular social institutions and within a particular ethnographic area. For Africanist anthropology, it arrived in the classic volumes *African Political Systems* (ed. Fortes and Evans-Pritchard, 1940) and *African Systems of Kinship and Marriage* (ed. Radcliffe-Brown and Forde, 1950), albeit prefaced with Mode III manifestos from Radcliffe-Brown. This mode of the comparative method did not simply make use of ethnographies but, more than any of the preceding ones, really arose out of ethnography and remained close to it. Consequently much more use is made of Mill’s Method of Difference, in two principal ways: explanation and exploration.

Two essays by S.F. Nadel—who had a better idea of what he was about theoretically than any of his contemporaries²⁷—indicate the difference in emphasis. Explanation is predominant in the tightly organized argument of “Witchcraft in Four African Societies”²⁸ (1952): two pairs of closely related societies, a single definite question about each (presence or absence of witchcraft beliefs? female or male witches?), and a clear guiding hypothesis (that witchcraft beliefs answer to frustrations and anxieties arising from the pattern of social relations). His essay on Nuba religion, on the other hand, is more exploratory, seeking to clarify a rather diffuse difference between the religions of two further Nuba peoples, one of which has a more anxious, ritually obsessive outlook, and the other, a more serene and submissive attitude toward the gods.²⁹ No definite explanatory hypotheses are evident here, beyond an assumption that one should look for “more significant, because more far-reaching, causal relations connecting religion with acts of an altogether different order, that is with conditions which are functionally autonomous and hence represent ‘independent’ variables.”³⁰ So Nadel proceeds to look at a number

of variables, most of which are germane to his interest in social psychology: the regulation of adolescence, the jural status of wives, sexual morality, attitudes to homosexuality, and so forth. Thus ethnography reaches, through the comparative method, to further and better ethnography.

In the early 1950s, a time when the surge of new ethnography studies encouraged several reviews of the comparative method,³¹ Isaac Schapera strongly urged its methodological advantages, in contrast with sweeping cross-cultural studies such as those based on the *Human Relations Area Files*. At the very least, where social-structural relations were being investigated, comparison within an ethnographic region enabled culture to be held much more securely constant. Its further potential was that it allowed variations genuinely to be analyzed as variants or transformations of locally given basic forms. This remained a productive seam, as was shown in such fine studies as those of Adam Kuper on Southern Bantu marriage systems,³² or Richard Fardon on social organization in the Benue Valley region of Nigeria and Cameroon.³³ One original aim of Mode IV was to avoid being bothered by culture through setting up situations where it could be set aside as a constant, yet the regional focus eventually pointed the way back to historical questions, and hence reintroduced the problem of culture. Mode IV could also converge with Mode II, as with the work of de Heusch.

REGIONAL COMPARISON WITHIN BRITISH SOCIAL ANTHROPOLOGY

Before turning to issues that bear directly on my initial puzzle, it is necessary to examine two closely related features of British social anthropology as practiced in the 1940s and 1950s: holistic presentism and sociological reductionism. These infused most exercises in Mode IV comparative method without being strictly entailed by it, and together they utterly inhibited an adequate analysis of the role of culture in social transformation. Holistic presentism followed from the practical rejection of historical explanation by the founders of structural functionalism. Where the histories of preliterate societies were judged to be unknowable, conjectural history, using Mode I's comparative method, was worthless, and so apparent history or oral tradition made better sense when interpreted as a charter for present social arrangements. Thus, all social phenomena had to be explained in terms of other social phenomena with which they cohered in whole systems or else in terms of the external conditions of such systems. With this doctrine, social anthropology acquired a wonderful self-sufficiency as a discipline, since ethnographic fieldwork, if sufficiently thorough, could provide all the material needed for explanation.

What holistic presentism did *not* provide was guidance as to what explains things. In principle, it might be environment, race, technology, cultural values, . . .

But after Radcliffe-Brown, it was social structure: social anthropology for a while became more sociological than sociology. Now, while this still left open many questions about the relations between such social institutions as politics, law, kinship, the economic division of labor, and the like, it did propose a definite answer, or rather two somewhat inconsistent answers, to the interpretation of culture. The core message was: culture does not matter much in social analysis.

On the one hand, culture is a kind of clutter, which has a certain obscuring tendency and so needs to be cleared away if valid comparisons are to be made. Because there might be "the same kind of political structures . . . in societies of totally different culture," comparison should be "on an abstract plane where social processes are stripped of their cultural idiom and reduced to functional terms."³⁴ Over thirty years later I.M. Lewis, in *Ecstatic Religion*, was to propose just the same thing. He urged "the crucial importance of distinguishing between the unique cultural forms of particular institutions and their actual social significance in any society." Only if anthropologists did this would they be able to "develop useful typologies which cut across cultural *forms* and which facilitate meaningful comparison." Thus would anthropology be able to storm "the last bastion of the unique," religion.³⁵

Alternatively, instead of varying randomly, culture was argued to covary exactly, as a dependent variable, with the forms of social structure. If the Tallensi have a cult of their ancestors, it is not (as Frazer would have argued) because of a fear of the dead, "but because their social structure demands it."³⁶ And it was precisely with those forms of religion—ancestor worship and witchcraft/sorcery beliefs—that seem *in fact* to reflect social structure most closely that the comparative analysis of religion was attempted to best effect.³⁷ As R.E. Bradbury put it in a fine study of the Edo cult of the dead, where "relations with the objects of worship derive very directly from the typical experiences of individuals in their relations with certain categories of deceased persons . . . severe limits are set upon the imaginative capacities of the religious thinker."³⁸

Bradbury clearly recognized that this need not be true of all forms of religion, but any great exploration of cultural autonomy was long impeded by a strong methodological resistance from social anthropologists. John Middleton and E.H. Winter, for example, counterposed two ways to explain the content of witchcraft beliefs: cultural analysis and sociology. Only by sociological analysis, they argued, can we develop explanations that subsume the facts from more than one society; and cultural explanations are in any case untestable.³⁹ But why should cultural explanations be less testable in principle than sociological ones? The contention that phenomenon *A* in society *X* is due to its being Muslim (a cultural fact) can indeed be supported by showing that it is also present in other Muslim societies *Y* and *Z* (the Method of Agreement), especially if it is absent from otherwise comparable but non-Muslim societies *P*, *Q*, and *R* (the Method of Difference). But there

has to be a theoretical interest in finding explanations of this kind; and mostly, in the comparative analysis of the 1950s and 1960s, this interest was excluded by satisfaction at the power of social-structural determinism to explain at least some of the empirical variation of African religions.

But what eventually became clear is that substantial amounts of variation were left unexplained by sociological facts, and that these often pointed to culture. Two examples suffice to make the point. Max Gluckman's extended comparison of domestic systems among two centralized Bantu societies, Zulu and Lozi, is a good example of synchronic, social-structural Mode IV comparative method.⁴⁰ He explained the presence or absence of the house-property complex and related phenomena in terms of the presence or absence of strong agnatic lineages. Compelled thus to extend the range of comparison, he found his explanation supported by the same correlations among a cluster of peoples in northeastern Africa. Things started to get untidy when a group of patrilineal peoples in West Africa (Tallensi, Fon, Igbo) provided a negative case—no house-property complex—but this he was able to handle in a theoretically plausible but also ad hoc way, by adducing an extra negative condition—their having brother-to-brother succession. But what is then the import of a case like the Yoruba, another patrilineal West African people, who manage to combine some major features of the house-property complex typical of the Zulu with other features more characteristic of the Lozi, who lack it? Clearly it does not simply invalidate Gluckman's detailed explanation of the Zulu/Lozi differences, but it does negate any idea of a necessary link between the two variables. There is just greater free play between social-structural variables than Radcliffe-Brown's program supposed, so that where variables do cohere, it is within a complex of other conditions that is the product of a particular local history. Any necessity of things is the rolling product of determinations accumulating over time—a subject matter that social anthropology, for a time, forswore to touch.

Mary Douglas's classic paper "Lele Economy Compared with the Bushong" brings us more expressly to a similar conclusion.⁴¹ She asked why the Bushong of the Congo were so much more productive than their neighbors, the Lele, despite identical levels of technology and a similar natural environment. Her convincing answer was that it was due to different patterns of labor use, which in turn depended on contrasting evaluations of what activities were appropriate to particular age/sex categories. In other words, the key factor was specific cultural patterns, which existed in the present simply as a precipitate of the past and were themselves only to be explained historically. By the same stroke, we are forced to take culture/ideas/religion seriously and to open social explanation to history. Holistic presentism excludes historical explanations but not explanation by reference to culture, the form in which the past exists in the present. There were good grounds for this suspicion of historical explanations. But only its companion dogma, sociological reductionism, really closed the door, by refusing to acknowledge the effectiveness

of any constituent of present reality besides the social relations themselves or, for those of an even harder turn of mind, their ecological and physical conditions of existence.

HISTORY REVIVED IN ANTHROPOLOGY?

For all that these were *idées maitresses* of British social anthropology at its acme, they never won universal assent. Opposition to them was most sharply expressed by E. E. Evans-Pritchard. Yet this opposition was both shifting and equivocal, and did not lead to a resolution of the problems of an ahistorical anthropology. This was because Evans-Pritchard was unable to transcend Radcliffe-Brown's terms of debate, which themselves had been set in the Montesquieu/Spencer/Durkheim tradition of science vs. history. He merely opted for the other alternative. Despite his own remarkable analysis of structural transformation (*The Sanusi of Cyrenaica*, 1949), Evans-Pritchard's case for anthropology as history had less to do with the treatment of time or change than with insistence on the uniqueness of what the anthropologist studies: individual cultures. History acknowledged this uniqueness, which a natural science of society would deny.⁴² The kind of explanation he wanted had nothing to do with causes: it was "exact description which bears its own interpretation."⁴³ He was consequently unenthusiastic about the comparative method, that handmaid of science. Some comparative religion he was prepared to countenance, but insofar as it goes beyond hermeneutics, he allowed it only the very weak causal aspirations of relational study.⁴⁴

For the anthropologists who most seriously reengaged with history did *not* take their cue from the later writings of Evans-Pritchard. The new concern arose among those who had done fieldwork in some of the larger-scale African societies, typically societies that had acquired some depth of literary tradition and whose ethnic traditions had the most direct relevance to the newly emergent states: peoples like the Yoruba, Akan, Tswana, Somali, Akan, Ganda, and Hausa-Fulani. Methodological essays⁴⁵ led on to monographs⁴⁶ and to collaborative volumes.⁴⁷ Though the forms of the anthropological engagement with history varied considerably, its typical position was in fact diametrically opposed to Evans-Pritchard's. Rather than adopt the supposed traits of history (idiographic, hermeneutic, etc.) in order to be adequate to its subject matter, anthropology should preserve its own disciplinary identity, which was as some sort of science, and bring it to bear on questions of change. Anthropologists might interact closely with historians, but they would do distinctive things with historical data.

A full account of the contrasting styles of history and anthropology is not needed here. Suffice it to note briefly three characteristic ways in which anthropologists tackled historical questions. First, potted histories or ethnographies were used to test general theories of social processes, or else theoretical models were

used to shed light on historical questions, as with Lloyd's conflict model applied to Yoruba kingdoms.⁴⁸ Second, anthropologists opposed the supposed historian's interest in unique sequences of events to their own search for structural regularities or sociological time.⁴⁹ Here the most telling voice was M. G. Smith's, since few anthropologists of West Africa had collected more extensive oral-historical data or pursued such a consistent theoretical project. From Zazzau (1960) to Daura (1978) his (parahistorical, as he called it) objective had been to establish relations of logical necessity holding through time.⁵⁰ He did not shrink from the implication that the elements to be so temporally ordered required abstraction from the total historical process and that causal analysis, qua determination of the conditions of concrete historical reality, were not what he was about. Finally, of course, there was again the comparative method. Thus Mode IV, though still marked by some ahistorical tendencies that ran back a long way, was brought directly to bear on historical questions. How it fared in relation to substantive issues in the history of West African societies, I now turn to consider.

ANTHROPOLOGY AND STATE FORMATION IN WEST AFRICA

From the 1950s to the 1980s the dominant theme that brought anthropologists and historians together was state formation. In the age of African nationalism, it was of pressing interest to West Africans themselves, and the local schools of academic historiography made states and elites their central topic.⁵¹ Social anthropology was able to respond effectively because it had, in its own tradition, already addressed some cognate issues. Fortes and Evans-Pritchard in *African Political Systems* provided the conceptual groundwork for later collections on precolonial states.⁵² Behind the presentist ethnography of structural functionalism there still lurked the evolutionary schemes, based on Mode I comparative method, of Lewis Henry Morgan and Henry Maine. The dichotomy between segmentary lineage systems and states that was used as a static typology in *African Political Systems* was turned again into a description of process in the studies of political centralization. This was thus conceived as a process in which the decline of lineages was the essential condition of the state's advance.

Discussion about the factors making for political centralization in precolonial West Africa faces a considerable initial difficulty. How do we distinguish a state of greater from one of lesser centralization? The most appropriate sort of evidence would seem to be historically concrete actions indicative of the center's political capacity: a king able to tax his subjects heavily, to raise armies and use them to ends determined by himself, to effect his will in distant provinces and to maintain public works, to place his own nominees in influential positions and to remove them at will. By such criteria Asante and Dahomey are reasonably considered centralized

kingdoms. Elsewhere, adequate historical evidence of *actions* being lacking, inferences are often made from *institutions*, typically observed in the twentieth century but presumed traditional, which are considered appropriate indicators or proxies of relative centralization. The typology contained within *African Political Systems* gives clear guidance as to what those institutions might be. The baseline for development would be a segmentary state, in which titled offices belong to lineages and the king is selected by nonroyal titleholders from the segments of a royal lineage.⁵³ Of the four forest kingdoms that were at the center of discussion—Asante, Benin, Dahomey, and Oyo (Yoruba)—the Yoruba approximate most closely to this model, and, in true Mode I fashion, their present condition was taken as analogous to the condition from which the others are presumed to have developed.⁵⁴

There are several difficulties with this procedure. The first is logical. The argument becomes circular when the same thing—for example, the decline of descent groups—is *both* used to define centralization *and* treated as a factor of change itself.⁵⁵ In a diachronic analysis, of course, the same institution may at one moment be treated as a cause or condition, at another as an effect. But here the comparisons are essentially static, between one generalized societal description and another, as in the manner of Gluckman's Zulu/Lozi comparison, albeit with the aim of isolating historically significant variables; so circularity is hard to avoid. Hence the perennial appeal of technological determinism, which offers a way to break the circle. Thus the argument, first put forward by Peter Morton-Williams to explain Asante/Oyo differences, and elaborated by Jack Goody for West Africa more widely, that military technology (specifically the horse vs. the gun) was the main factor determining the allegedly greater centralization of the forest kingdoms.⁵⁶ Behind military technology lay geography (savannah vs. forest), since rulers near the coast would get the guns, which they could store and use to take power from lineage chiefs into their own hands. But empirically, as Robin Law showed,⁵⁷ Goody's hierarchy of causes—geography, technology, sociology—just doesn't work. To give just one example, the introduction of guns into nineteenth-century Yorubaland, one of the later areas to receive them, served to accentuate political fragmentation and conflict, both between states and within them, rather than to create a greater centralization of power.

But centralization cannot be *plausibly* defined in this question-begging way, with a theory of the process built into the definition. The case of Asante shows this most clearly. P.C. Lloyd expressed a once common view when he grouped the Asante *with* most of the Yoruba in one category (open representative government), while Benin and Dahomey were placed in another (government by political association). In the first of these, chiefs are selected as lineage representatives, an arrangement that expresses the coherence and importance of the lineages in society at large. The latter category, indicated by nonlineage titles, close succession in the royal house, and so forth, is more centralized. The same linking of Asante and

Yoruba occurs in Fortes's essay "Strangers," where he argues that in both societies, because lineages are the building blocks of the community, strangers can become members of the community only through assimilation to it.⁵⁸ Mostly by the 1970s, however, the Asante and the Yoruba were being placed in contrasting categories, the Asante alongside Dahomey and Benin as relatively centralized states (gun-using, with nonlinearage titled offices), in contrast to less centralized states (cavalry-using, with lineage titles), such as Oyo, Gonja, or Mossi.⁵⁹ The shift in the classification of Asante resulted from Ivor Wilks's demonstration of the bureaucratic aspects of the Asante state in the late eighteenth and nineteenth centuries,⁶⁰ which had eluded the attention of an anthropology too rooted in the colonial period.

Wilks's work forced a reconsideration not just of Asante but of the whole comparative framework derived from *African Political Systems*. What had seemed to justify its model of centralization was that, linked to the declining significance of lineages, there should be a number of other socially significant features, including such commonsense criteria of centralization as royal executive capacity. All this now collapsed in the light of Wilks's account of Asante. Certainly the late eighteenth-century Kwadwoan revolution in government involved the establishment of new bureaucratic offices, which were detached from the matrilineages to which titles of general community leadership belonged.⁶¹ But the matrilineages remained of great consequence in social life, access to land, and other local functions, and so continued to be represented in the colonial ethnographies after the structures of the expansionist Asante state had fallen away.⁶² The advance of the state did *not* entail the withering away of the lineage.

Does the model fare better with the other kingdoms? Dahomey, like Asante, combined a powerful royal system of control with the continued existence of corporate, landholding (patri)lineages, which M. J. Herskovits called "the pivot of Dahomean social organization."⁶³ At first, the Yoruba would not have seemed to present much of a problem, since the ethnographic consensus represented their town structures as federations of lineages⁶⁴ that failed in diverse ways to become centralized; but this consensus came to be severely questioned.⁶⁵ Great difficulties arise from the regional variety of Yorubaland, but in general it can be said that the importance of lineage as *the* basis of social organization was much exaggerated. Yoruba title systems do not reflect lineage structure alone; in many instances residential groupings, such as quarters, are as important a principle of cooperation as descent; the ancestral cult known as *egungun* shows a much less decisive recognition of the importance of corporate descent than do the ancestor cults in Dahomey or Asante. It is curious that corporate lineage, this supposed token of the baseline of political development, should have been more strongly emphasized in accounts of the northwestern Yoruba, none of whose communities in its present form predates the wholesale upheavals of the last century, than in the centuries-old communities of the southeastern forests, such as Ondo and Ilesha. Indeed Lloyd ends

by admitting of Ibadan—the new military master of Yorubaland in the nineteenth century, which chronically failed to create a stable political center—that its strongly corporate lineages were “a product of the development of the political structure in response to new opportunities in the sphere of trade and war.”⁶⁶ It is true that at Ilesha the importance of nonlineage titles and nonlineal social units (such as the quarters) grew as an aspect of the town’s successful expansion; but they did not bring (and cannot be used as an indicator of) any marked centralization.

The last case to be considered is Benin. Its contrast with the Yoruba now looks less sharp, though the evidence of its relative centralization is not to be denied: the periodically impressive executive outreach of its kings, the extraordinary role of the palace associations in the integration of the kingdom, the stem dynasty. But is there a plausible trajectory of development? Bradbury argued, from the evidence of the ritual opposition between the *Oba* (from a dynasty of Ife—i.e., Yoruba—descent) and the kingmaker *Uzama* chiefs (representing the elders of Benin), for a convergence of Yoruba and Edo (Benin) political cultures. Divergences from the Yoruba pattern were put down to *ur*-Edo cultural elements (e.g., primogeniture, lineages shallow, nonlandholding). But should we equate what seems distinctively Edo to a twentieth-century ethnographer with a putative *ur*-Edo baseline? Some things count against this: some less centralized peoples of the Edo-speaking periphery in fact have landholding patrilineages more like the Yoruba than Benin;⁶⁷ and the *Uzama* titles at Benin, those supposed tokens of *ur*-Edo culture, are widespread as very ancient titles among the forest Yoruba.⁶⁸ In sum, it strongly looks as if at least some of those institutions distinctive of Benin were the product of its political development rather than drawn from an *ur*-Edo baseline. If, moreover, the reduction in the significance of lineages in the heartland of the Benin kingdom was part of this process, it seems that Benin’s development squares with the lineages-to-state model suggested by *African Political Systems* better than Asante or Dahomey do.

So what can we conclude from this debate about the conditions of political centralization in precolonial West Africa? First, the notion of centralization is thoroughly confused. In fact, at least three distinct criteria seem to be involved. (1) All forms of political development appear to have entailed some concentration, an essentially spatial process by which the population and disposable resources of a region come to be concentrated at a power center. Such concentration appears to be a necessary but not a sufficient condition of (2) the sort of power transfer between institutions within the emergent center that the lineages-to-state concept of centralization is mostly about. Then there is (3) a growth in the capacity of a state executive to extract and direct to its own ends the labor and resources of its subjects. Criteria (2) and (3) need not coincide. Take, for example, the nineteenth-century jihadist state of Masina, on the middle Niger.⁶⁹ This was a cavalry-based state with a mass dynasty, and so belongs to Goody’s group of the less centralized,

by criterion (2). But its Fulani Muslim ruling estate maintained their cavalry by extremely heavy taxation of the Bambara subject population; the land was expected to produce at least double the subsistence of its cultivators, a very high level of exploitation by West African standards. Masina was highly centralized, by criterion (3). Process (3) is surely the one that, from a general viewpoint, has the greatest historical significance. It refers to an increase in societal capacity, achieved through rulers' finding ways of controlling their subjects more, making more continuous calls on their labor and resources, and thus being able to take political and military initiatives not open to the rulers of less geared-up societies. The large armies they raised, the vast size of their palace establishments, the accumulation of resources at their annual Customs (as these ritual occasions are styled), the road networks they maintained—all indicate that states like Dahomey and Asante took a definite developmental step. The question then is: How did they manage to do it?

It is evident that neither anthropologists nor historians have produced very satisfactory answers. The leading anthropological idea, that the advance of the state is linked to the decline of the lineage, has proved to have very little in it; process (2) is only contingently connected to process (3). Technological determinism, military or otherwise, is hardly more helpful; and even the presence of trade routes (for there is plenty of trade without rulers in West Africa)⁷⁰ does not sufficiently explain why states come into being or, in a handful of cases, succeeded in gearing themselves up to higher levels of societal performance. Can we do no more than agree with Law when he writes, after a careful review of the comparative literature, that "ultimately, perhaps, explanations are to be sought in specific historical circumstances [for the failure of Oyo to become more centralized]"?⁷¹ Law raises questions here about the specific factors of change and about the value of the comparative method in general.

RELIGION, IDEOLOGY, AND THE STATE

One large area of thought and activity has by and large been neglected by both historians and anthropologists in their attempts to explain the differences between these societies: religion. For the social anthropologists this neglect is hardly surprising in the light of the sociological reductionism that has been the main key to their interpretation of religion. Even Goody's essay "Polity and Ritual: The Opposition of Horse and Earth,"⁷² which is almost the only treatment of religion in relation to the debate about political centralization, seems to assume that, while religion may have some real effect on the behavior of individuals, it has no significant role in the creation of social forms.⁷³ Historians too have tended in practice to deny significant historical effects to religion. The two major studies of the forest kingdoms, Wilks on Asante and Law on Oyo, have little to say about it. It is only with Islam that a role for religion as a force of social change seems readily

conceded, as with Marion Johnson's argument that "Islam, military force and taxation are connected in a complex way" in Masina's development.⁷⁴

Available evidence suggests that it was not utterly different for the pagan religions, that they too served as models for as well as models of social relations.⁷⁵ For all that they have been little investigated, it is clear that religious innovation and controversy were integral aspects of the growth of state power in eighteenth-century West Africa. In Asante, one king met effective resistance from his chiefs when he contemplated adopting Islam;⁷⁶ and in Dahomey, the great king Agaja (1708–32) introduced the Ifa cult of divination from the Yoruba.⁷⁷ The central occasion for the exercise of the state's mastery over the resources and activities of its subjects was the great annual religious celebration known as the Customs, preparation for which structured the activities of the entire population of the kingdom for a large portion of the year.⁷⁸ In Asante a further key role in this regulation of subjects' activities was the *Adaduanan*, or forty-two-day calendrical cycle, which T. C. McCaskie calls a veritable *Grundnorm* of Asante life, a rooting of social activity in a cosmic pattern.⁷⁹

Here at last we can start to return to the initial puzzle: Akan resistance to adoption of the world religions, compared with Yoruba openness to them. For what that comparison of twentieth-century patterns of action showed is that even after the British had dismantled the structures of Asante state control, the Asante long retained a lively sense that the integrity of their society depended on the sanctions of the traditional religion. A most significant instance of this was the petition of Christian clergy, both missionary and local Akan, to the *Asantehene* (king of Asante) in 1944, arguing that Akan Christians were loyal subjects of their chiefs, even though their religious scruples prevented them from treating Thursdays, sacred to the Earth goddess, as rest days within the *Adaduanan* cycle.⁸⁰ It is utterly inconceivable that Yoruba Christians should have had to make such a protestation as late as the 1940s. The same Akan attitude was evident in other ways too: for example, in the much greater and longer-enduring sense that chiefship and church membership were flatly incompatible, or the much commoner practice of requiring Christian converts to withdraw from the town to live in a quarter outside it, usually known as Salem.⁸¹

So my argument about religious change in twentieth-century West Africa leads back to an argument about political change, and the role of religion in it, in eighteenth-century West African kingdoms. Common to both is an insistence on an aspect of religious change that finds little cognizance in Horton's theory of African conversion from which I began. That is essentially cognitive or cool: change is explained as occurring insofar as people's new experience renders their old explanatory frameworks inadequate. The present argument, however, is that religious change is a much more affective or hot process, because religion (in addition to having explanatory functions, as Horton says) also serves to define the

membership of social groups and to underpin authority in them—and especially so in highly geared-up societies such as Asante. It is important, when we recognize that religion serves as an ideology, to stress that it *must* be more than that. There is a danger here of another form of sociological determinism, the quasi-Marxist functionalism that embodies a teleology: the ruling class needs an ideology to justify its position, and so religion must somehow be on hand to provide one. Such strategies often paper over crucial gaps in the explanation. So Wilks tells that in late eighteenth-century Asante, “government *had* to be extended in range . . . in scope . . . and in proficiency” (my italics).⁸² He tells us *how* it was but not how it *could* be so. The need does not suffice to produce the effect; the crucial cultural conditions had to be met. If it *had* to be ideology to work the trick because purely material conditions fall short—and we must never forget that in West Africa the most segmentary peoples and the most centralized states share the same technological, ecological, and physical conditions of existence—there had to be some independent strength in the religious ideas drawn upon. Religion had this power because it was already the shared idiom in which both chiefs and people confronted the pains and anxieties of the human situation. *Asantehenes* really feared witchcraft; kings of Dahomey, two of whom died of smallpox, respected its cult for all that they disliked it.⁸³ If this was the bottom line of their reality, on what else could rulers better seek to build structures of higher obligation and control—and themselves remain constrained by its premises?

REINTRODUCING HISTORY

It is now clear that some of the failure of the comparative method to explain West African centralization is to be attributed to the neglect of culture or ideology as a causal agency. But the problem goes deeper. The comparative method, as employed by social anthropologists on these questions, is really a combination of two modes: it is Mode I in its view of the process of centralization and Mode IV in its views as to causes. Now, both these modes proceed by abstracting their data from history, even when their aim is an idealized, general history or the determination of real historical causes. For what they compare are either societies described in a detemporalized ethnographic present or a cross-section from a society in history, a frozen moment taken as some kind of whole; and they aim to establish relationships between variables that hold apart from historical time. But it is inconsistent with a realistic concept of what society is, and human experience within it, thus to base comparison on a procedure that eliminates change, incompleteness, and potentiality, memories, and intentions—in a word, historicity. It is small gain to rectify the omission of culture from explanations by introducing it as yet another factor in a *presentist* scheme of comparison. For it is then reified rather than viewed as the hinge between the past of which it is the precipitate and the future that it aims to prefigure.

Mode V: Histories Compared

What we need is a Mode V of the comparative method, where it is histories, or societies in change, rather than just societies that are compared, following the path blazed by the great historical sociologists or comparative historians such as Frederick Maitland, Max Weber, and Marc Bloch. Other modes of the comparative method (except Mode II) pose questions about general sociological categories, aggregates of things taken from their several historical contexts. Underlying this is the antithesis of science and history, and the assumption that if the data are to be treated scientifically, their historicity must be purged from them. Mode V differs in several related ways. Its aim is to explain historical particulars through applying to them general statements, which are theories or models, rather than to move from particulars to empirical generalizations or laws. This is to hold to the general logic of scientific explanation as to the use of comparison but to refuse to distort the data by dehistoricizing them, that is by taking them out of their placement in a time sequence. For their place in a time sequence is an *essential* feature of social facts, constituted as these all are by individual actions. Natural science, whose example has done so much to inspire the comparative method, need have no concern with historicity, since it deals with entities that have fixed properties and hence highly determinate relationships with one another. Social anthropology's attempts to develop such generalizations about the variables of its own subject matter have been extremely disappointing. Virtually all its generalizations turn out to be no more than tendencies, or true only by definition, or holding only under particular historical or cultural conditions, or able to state only the very minimal conditions under which social facts exist. Between its variables there is much more free play: we are forced to conclude that the linkage of variables in particular cases often results less from their inherent properties than from *how they have come to be combined*, through human action in a succession of contexts. By comparing histories or societies in change, Mode V offers a path to the explanation of social phenomena without misrepresenting the general way in which they are brought about.

A FINAL CONTRAST BETWEEN YORUBA AND AKAN

A final recourse to the Yoruba/Akan comparison illustrates the role of culture as the pivot of social change. I have argued that the relative reluctance of the Akan, especially Asante, to embrace the world religions had much to do with their sense that the integrity of their society depended on sanctions bound up with the old religion. Asante society was not religiously static; but the world religions could not be subjected to local chiefly control as other imported cults were. The Asante knew their political community as founded by human agreement, though also given spiritual sanction by the Golden Stool. McCaskie brilliantly conveys

the Asante sense of the fragility of their achievement, speaking of their “abiding fear that without unremitting application and effort, the fragile defensible space called culture would simply be overwhelmed or reclaimed by an irruptive and anarchic nature.”⁸⁴

The Yoruba perception of themselves and their situation was rather different. Again it is instructive to look back from their response to the world religions. Though conversion brought both conflict and persecution, the fact remains that the Yoruba were much more open to religious change. It was, of course, a less highly geared-up society, which could allow more religious toleration—already within Yoruba paganism there was both cultic diversity and religious choice—and whose rulers were perhaps less able to stop religious novelty. Whereas, after *Asantehene* Osei Kwame’s flirtation with Islam in the late eighteenth century, the Asante authorities quarantined Islam and effectively prevented its further spread, in the Oyo empire at the same time Islam made such strides that Muslims became a key component in its overthrow; and Islam grew steadily in Oyo’s successor states in the nineteenth century. A highly distinctive feature of Yoruba traditional religion was its oracular cult called Ifa, whose priest-diviners (*babalawo*) had great prestige as religious professionals. Through consultation with Ifa an individual might be directed to the worship of a particular deity. But most remarkably, *babalawo* could, and on occasion did, advise clients to become Muslims or Christians.⁸⁵ Why could Ifa, a key element in the traditional religion, thus sponsor major religious change?

The many Yoruba kingdoms never enjoyed political unity or a common ethnic name till the twentieth century; but they recognized their affinity through the claim of all their kings to descent from Oduduwa, a god who had reigned at Ile-Ife. Ife had been the first great kingdom of the West African forest (*fl.* 1100–1450),⁸⁶ and even after it had declined to a town of modest political importance, the Yoruba always looked to it not only as a supreme cultic center but as the very site of the creation of the human race.⁸⁷ Ife’s sacred prestige in later centuries was especially conveyed in the cult of Ifa. This was not just because Ife is especially prestigious as a center for the training of *babalawo* but because of the way that Ife is represented in Ifa.

Ifa comprises a vast number of poems (*ese*), organized under the 256 figures (*odu*) that the *babalawo* may cast with his apparatus.⁸⁸ The *babalawo* then recites the *ese* appropriate to the figure cast, one of which will give the key to the client’s problem. Each *ese* takes the form of a mythical precedent, in which such-and-such a diviner or diviners (named by praise names that often encapsulate the problem) is consulted by some archetypal figure, who does or does not do what Ifa advises, usually to make a specified sacrifice; and the outcome is told, usually in the form of an extended myth, parable, or fable; finally the precedent is applied to the case in hand. Ifa is, therefore, a vast corpus of coded messages about the past.

More important than the fragments of specific historical information it may contain is its overall vision of the meaning of the past.

The forty-two-day calendrical cycle or *Adaduanan* of the Akan bears a certain comparison with Ifa—as central elements of their respective cultures and so, as we have seen, factors in the reception of the world religions. Both are to do with the ordering of social life through the assertion that things do, and should, repeat themselves. The *Adaduanan*, with its lucky and unlucky days, is much more to do with the mastery of time as such, structuring human activity in short cycles, well expressive of the perpetual anxiety of that hard-won and humanly constructed political order. Ifa, by contrast, is less concerned with time than with the past, specifically with the Glory that was Ife. For the mythical precedents that prefigure all possible later contingencies are stated or presumed to have taken place in Ife, “un état autrefois florissant, et dont la capitale fut une ville sainte . . . une patrie mystique,” as a *babalawo* working in Dahomey in the 1930s put it.⁸⁹ Ifa presents to its adherents the highly refracted image of a past great civilization, and it is *here* that for the Yoruba the essential order lies, an ideal order. It is moreover a divinely given, not a humanly constructed order; for Ife (unlike Kumasi, Abomey, or Benin) was also the site of a cosmogony. While the *Adaduanan* actually creates definite patterns of activity, Ifa does not stipulate but rather sanctions, in the name of this past, those actions that the client is deeply disposed to take. The flexibility and openness to change of Yoruba society is thus conditioned by the belief that the ultimate order is eternally guaranteed by how things began. What our comparison most importantly teaches is that culture is less a reflection of society than a reflexion on history.