DEEP-DOWN TIME: POLITICAL TRADITION IN CENTRAL AFRICA[1]

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I

Around 1850 the peoples of central Africa from Duala to the Kunene River and from the Atlantic to the Great Lakes shared a common view of the universe and a common political ideology.[2] This included assumptions about roles, statuses, symbols, values, and indeed the very notion of legitimate authority.[3] Among the plethora of symbols connected with these views were the leopard or the lion, the sun, the anvil, and the drum, symbolizing respectively the leader as predator, protector, forger of society, and the voice of all. Obviously, in each case the common political ideology was expressed in slightly different views, reflecting the impact of differential historical processes on different peoples. But the common core persisted. The gigantic extent of this phenomenon, encompassing an area equal to two-thirds of the continental United States, baffles the mind. How did it come about? Such a common tradition certainly did not arise independently in each of the hundreds of political communities that existed then. However absorbent and stable this mental political constellation was, it must have taken shape over a profound time depth. How and as a result of what did this happen? Is it even possible to answer such queries in a part of the world that did not generate written records until a few centuries ago or less?

This paper addresses this question: how can one trace the social construction of such a common constellation over great time depths and over great regional scale? All the peoples involved are agriculturalists and the political repertory with which we are concerned could not easily exist in its known form outside sedentary societies. The time depth involved reaches back all the way to the initial settlement of this vast area by farmers, because the common ideology is expressed in a terminology which goes right back to the ancestral language spoken by these first settlers. For all languages spoken in this area belong to a single language family, Western Bantu.[4] The approximate date of the arrival of farmers and the dispersal of the Western Bantu languages may well be as early as 2000 BC. However, the major patterns of settlement and the later technology of production were firmly in place only around the middle of

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the first millennium AD.[5] Thus, this inquiry may cover over 1400 years, and perhaps, with regard to certain aspects, even longer, back to that period of common speech some 3500 years ago.

Sources relevant to this question can be divided into two classes: direct evidence flowing from sources contemporary to the situations studied, and indirect evidence—data from recent times that give evidence about past events and/or situations. Direct evidence is scarce. There is very little archeology in the area as yet and, except for the coastal area, written data appear only in the nineteenth century and often late in the century at that. Documents reach back to the sixteenth century on the coast of northern Angola, and after 1600 a sprinkle of information appears elsewhere along the littoral. We must therefore rely on indirect sources. Of this evidence there is an abundance of oral tradition and linguistic and ethnographic data, most recorded in writing after 1880.

Because of the importance of indirect evidence, we must first establish an ethnographic baseline, which will represent the earliest time for which written data are available that give us information about language, society, and culture. That time happens to occur on the eve of, or even during, the actual colonial conquest in most of the area, because this is when the first written data appear in most cases, not because they refer to any "traditional period"—there is no such traditional period. Important changes, about which we have some knowledge, swept the whole area long before the conquest. The impact of the intercontinental slave trade is only one of these. This baseline does not disclose anything traditional or unchanging. All the more reason to pay attention to earlier direct evidence where it exists.

In this case there exists another possible ethnographic proto-baseline referring to the time when the single ancestral language was spoken. As we will see below, it is possible to recover the original linguistic form of a word and its associated meaning (etymology). Even though the evidence remains indirect, it is extremely reliable. As it happens, the terminology for the common political ideology, among others, can thus be recovered and can discern what this ideology was at the beginning of the Western Bantu expansion. We are thus in a position to know the situation at a starting point on an ethnographic baseline of *ca.* 2000 BC and at the point of arrival, *ca.* AD 1900.

We cannot postulate that the logically simplest or most elegant way to proceed from start to arrival necessarily reflects the actual unfolding of past history. If we did this we would exclude not only historical accident, but the dynamics of complex phenomena of borrowing, invention, and convergence. Moreover, we must fear circular reasoning. We establish etymology by conclusions derived from comparison of recent data. Our conclusion as to the original meaning of the form studied thus involves recourse to logically most common denominators, although this may not be correct in all cases of borrowing and convergence. Therefore, this proto-baseline, the starting point, is not evidence of the same order as the baseline at the point of arrival. I first discuss issues relating to the establishment of the recent ethnographic baseline, and then discuss appropriate methodologies for the use of oral traditions and linguistic data, after which I turn to the development of research designs utilizing the various kinds of indirect evidence considered in this paper, including the insights derived from the setting up of the proto-baseline.

II

The Ethnographic Baseline

Establishing an ethnographic baseline initially seems easy and straightforward, since there are numerous anthropological monographs. Indeed, anthropologists and historians accept without questions that we know a lot about the peoples of central Africa in "traditional"—that is, precolonial—times precisely because of this abundance of ethnographic information. In fact, these sources pose serious problems for a reconstruction of society in central Africa, *ca.* 1850 (our ethnographic baseline) for two reasons: First, writings by anthropologists constitute only a small portion of the documentation available for that period. By and large, most ethnographic information prior to 1930 comes from administrators, missionaries, other specialist colonial personnel, and travelers. Second, there are major difficulties with the ethnographic genre itself as practiced by anthropologists and others who were influenced by anthropological method. Despite these problems inherent in ethnographic data(discussed below), rules of evidence can be applied to ethnographic monographs in an attempt to evaluate critically the quality of the information. Judicious extrapolation from the best quality evidence should permit a reconstruction of the way of life of a particular society at a given period in the recent past.

There have been few professional anthropologists in central Africa. Between *ca.* 1870 and 1980 only some sixty-five of them were active in the rainforests, an area as large as the settled and farmed portions of west Africa. Moreover, most anthropologists worked there after 1930—their testimony is late. Murdock makes it clear that most of our ethnographic information stems from administrators, missionaries, magistrates, military personnel, physicians, business men, and travelers.[6] Anthropologists are only one voice in this chorus. So are historians, whose first major contributions date from the 1950s and later. In addition, these deal merely with the published record, the tip of the iceberg. Beyond this, and reflected in the published record, is a huge amount of administrative archives. Officials produced numerous reports on local political systems. In the Belgian Congo this occurred in over a hundred *territoires* at a rate of several per year. For the small area of the Ngiri, Mumbanza mwaBamwele used about 215 local reports written before 1932, not counting political registers for each administrative district. Those were kept up to date year after year. Then he used the Catholic mission archives, one of which included a three-hundred file set with ethnographic data.[7] The first professional anthropologist for this area appeared only in 1969.[8]

Anthropologists are specialists. They are believed to gather ethnographic information in a more reliable way than others. Often this is true, but that should not dissuade anyone from applying the rules of evidence to anthropological monographs, just as they would for other documents. The claim for reliability has to be documented and this implies access to the notebooks of anthropologists, but in most cases these are not available or have been destroyed. In practice the *record* of evidence gathered by non-professionals may therefore be more abundant than that of professionals. Nevertheless, professionals remain crucial because they were usually the models that the amateurs followed. A large portion of the literature is in fact influenced by anthropologists. The ethnographic monograph was a literary genre that moulded perceptions of observations by every foreigner. The genre is so important to our purpose that we must discuss it in more detail.

The unit studied was the "tribe." The area it occupied was shown on ethnographic maps. The maps themselves were pieced together from administrative data.[9] Such data were the product of administrative policies. They represented justification for the creation of administrative units at various levels in colonies of indirect rule and were intelligence information in other colonies. The administration, and to a lesser extent the missions, created tribes. Thus all of the major groups in Equateur province (Belgian Congo) bear names that came into usage late and referred to populations that had no sense of ethnic identity on that scale before the colonial period. Therefore ethnographic maps such as those used by Murdock are a colonial mirage. They cannot be valid for any earlier situation.

The genre used the "ethnographic present," an ambiguous tense that represented by implicit convention the situation in traditional times. Thus the observations were stripped of obvious colonial imports from taxes to court houses, trucks, bicycles, etc. Ironically, British social anthropologists in particular practiced this. As functionalists they stressed the total integration of all institutions. When it came to social change forecasting the future, they were meticulous, but they were blind to the past. They forgot that the most radical change in the recent past had been the colonial conquest. Many did not know, and some still do not know, that colonial rule was imposed by violent conquest. In many parts of central Africa the turmoil of conquest was prolonged and dramatic: settlements and crops were burned, war, epidemics, and famine raged, populations declined precipitously. An extreme case perhaps is that of the northwestern Tsayi (western Congo) whose 148 villages before the military campaign to subdue them were reduced to three and who lost nine-tenths of their population between 1911 and 1920.[10] Central Africa may have lost half of its population and certainly more than one-third during the conquest. Indeed this was recognized in the Belgian Congo in 1920.[11] The socio-cultural life of the survivors was obviously profoundly altered as a result of conquest.

Another convention of the genre was that data gathered in a single locale (participant observation was often limited to a single village) or a limited area could be considered valid for the whole tribal area. Tribal customs were deemed to be uniform over that area and different from all adjacent areas. To state this assumption is to state its absurdity. Thus the genre sidestepped all problems of sampling. It was almost as fuzzy about space as it was about time.

The conventions of participant observation and of generalization from concrete observation make it difficult to assess actual observation. Very few direct records of what informants said have survived. Not many more of what was actually seen on a certain date in a certain place survive. Testimony to some concrete situation occurs often merely as illustration[12] or as literary device. Usually norms, such as "the eldest brother succeeds," are recorded and not what actually occurred. On both these scores administrative reports may be better because they often deal with specific successions, specific lawsuits, and specific statements. But they are records of colonial intervention; they justify why official *A* appointed *X* as chief or issued a given judgment.

The ethnographic genre also dictated the contents. Conventions ruled what should be found in monographs and what was not to be included there. These were based on widely shared theories as to what was significant in traditional society and what was not. Ethnographic questionnaires existed by the middle of the nineteenth century and blossomed from the 1880s on. Museum directors, sociologists, geographers, and administrative bodies developed them, copying each other to a great extent, as the listing by Olbrechts of questionnaires commonly used *ca.* 1930 demonstrates.[13] Anthropologists and amateurs followed the plan and layout of such questionnaires in their work. Authors who did not have them copied others who had used them. Professional anthropologists broke away from this model only in the 1920s. The first different monograph for this area was Evans-Pritchard's *Witchcraft, Oracles and Magic Among the Azande.*[14] Although this book did not become a prototype for later works, other professional studies from the late 1930s and 1940s did become models. Young anthropologists followed the example of famous elders such as Audrey Richards, Meyer Fortes, Max Gluckman, or Georges Balandier.

If the use of questionnaires biased the very observations made and their record, they nevertheless facilitated comparative studies by producing a uniformity which allows us to find comparable data on aspects of political systems in the thousands of articles or books written about the area. But it must be remembered that the categories of analysis utilized by common convention are not necessarily reliable or valid—witness the definitions of clan as listed in various editions of *Notes and Queries of Anthropology* (London). Questionnaires did not merely impose topics but whole models of reality, which were then rediscovered in the field. Indeed, in the Belgian Congo well before 1920, the administrative mind imposed the Durkheimian model of segmentary lineage systems, along with a model of feudal states and administrators reorganized local societies to fit the model.[15] Later observers then recorded it as a reality.[16]

The ultimate in the direction of patterned information is the record given in the Human Relations Area Files [*Ethnology*]. I compared its data for the fifty-three cases from the rainforests of central Africa with all the known evidence. The units HRAF used are the conventional tribes, the categories being devices ultimately drawn from Murdock's model of societies. The quality of the coding (done by graduate students) is not uniform. We have for instance AE 4 (Mongo and Nkundo), two names for the same people and the same locales. Nor is coding identical. A.5 Mbuti has two codings based on different sources and we are never told what to believe when the coding diverges.

The HRAF columns referring to political organizaton (19, 30, 31, 32, 67, 69, 71) show that the coding is not uniform. Thus we find larger settlements being called "hamlets," then others dubbed "villages" (col. 31). There are arbitrary categories. In column 67 class stratification allows only for "no classes," "presence of an aristocracy," and "distinctions of wealth not accompanied by distinct hereditary classes." In the rainforests heredity is not all that important in this matter, but most societies exhibit six or more recognized social strata. Similarly column 71 deals with slavery ("hereditary," "not hereditary," "absent") and does not allow coding for pawnship, serf, clientship, and other deprived statuses.

With unreliable units (tribes), arbitrary and often unsuitable categories and variable coding, HRAF data are not to be trusted. In each case the sources used for the coding must be reread, other relevant sources consulted, conceptual and epistemological bugs ironed out. What then is the use of these files?

The rules of evidence must be applied to ethnographic monographs and to their use even for comparative study. First we must establish where the observations were made and reckon them as valid only for such locales and a vicinity determined, ideally by information about population density, population mobility, and the frequency of inter-relationships among neighbors. In the absence of such data, I estimated that observations probably held true for a 50 km radius around the point of observation. In a few cases available information limits this area more, while in others the data suggest that the observations hold true for a wider area. By applying this rule it soon became apparent that as far as data published since the 1840s are concerned, over one-third of the rainforest areas remains unreported. Where population densities were higher, ethnographic coverage was better, probably because access roads and administrative and missionary facilities were more developed. Where population densities were very low, coverage was minimal.

With regard to the ethnographic present, the position is that the year of residence is the date for direct observation. But many data stem from interviews, and indeed from oral tradition. They then constitute indirect evidence, referring to a period earlier than the date of record. We must therefore seriate the sources according to time elapsed since the colonial conquest. A few, such as Bastian or Pechuel Loesche, date from before the conquest.[17] Others stem from the generation of conquest itself. Later ones must be classified according to the number of generations elapsed since the conquest. In most cases the generation of witnesses just before the conquest lasted for some thirty years or so afterwards. The second generation included witnesses who relied on information from their parents or elders. For a conquest date in 1900 the first generation would last until 1930, the second to 1960 and we now are in the third. Reliability diminishes with the number of generations. The later the observations were made, the more crucial a dwindling number of witnesses contemporary to the event becomes. Thus, among the Tio the colonial process began in 1880. In 1963 (third generation after!) I still found three people who were alive in 1880 and old enough to remember something about life then. As conquest dates vary by place (excepting Libreville and Angola), from 1879 to after 1920, the various levels of reliability also vary commensurately.

With regard to content, a careful distinction should be made between concrete cases reported and statements of norms. The latter are always more doubtful. The influence of pre-existing sociological or cultural models can be evaluated, as can the concern with questions or problems that were considered significant at the time. This will give a rough index of originality, as well as of the particular mindset of the author.

The main stumbling block in a critical study of this kind is the absence of background data. They often are most lacking for professional anthropologists. Even the prefaces or introductions to their books do not always specify where they resided or exactly when, whether or not they had relied on some informants more than on others, how fluent they were in the local language, or how they came to know specific data. For this, among other reasons, it would be highly useful to have access to their raw fieldnotes. Other authors, such as missionaries, administrators, or travelers are easier to track. Often we know from other data when they were where. The archival records show when information was gathered, and by whom (copyright did not apply to administrative records!) To look up such collateral data can be important if only to make certain that the main source was genuine. Contrary to popular belief, almost total fakes have been published and partial fakes have been frequent, especially in literature destined for a wider European public.[18]

Can the way of life of a given society really be reconstructed from documents? Most anthropologists used to deny this. Participant observation and intensive fieldwork were held to be essential. When *The Tio Kingdom*, which gave a picture of Tio life between 1880 and 1892 drawn from written and oral data, was published the reviewers accepted the reconstruction.[19] Since then the argument that only participant observation can yield valid results has not been raised again in central African scholarship.[20] One can establish a valid ethnographic baseline, and indeed this has been done for the major states and some other societies in the area.[21]

III

The Use of Oral Tradition and Linguistic Data

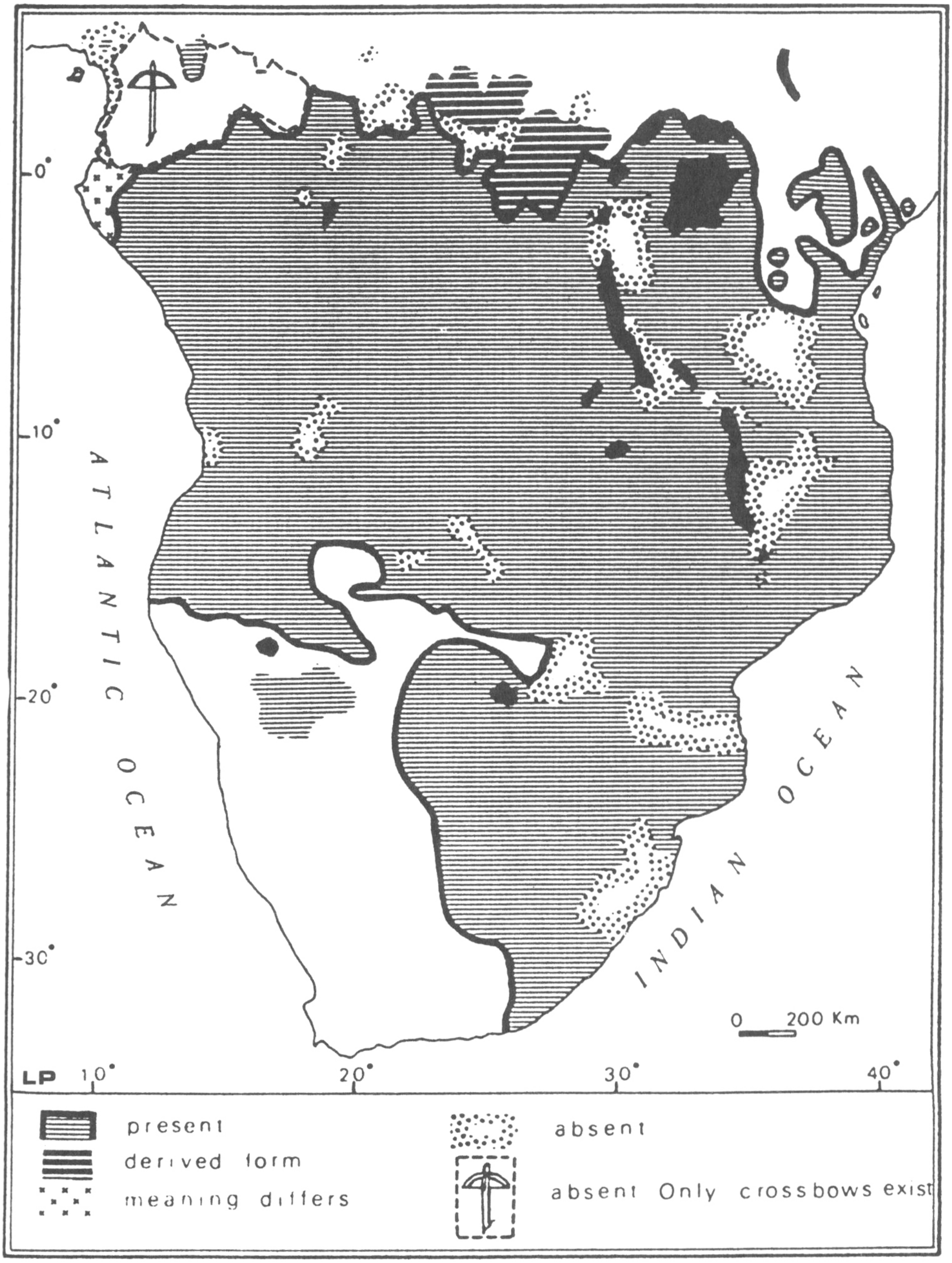
Among the indirect sources that can be applied to the study of past political systems in central Africa, oral traditions are useful for recent time periods.[22] The rules of evidence that apply to them are now fairly well known. For the last three or four generations before recording, traditions are reasonably full and often reliable. But the corpus is constantly altered by an active collective memory of which telltale signs can be detected. The precise depth of this body of tradition varies from society to society. The social organization of every society and the accompanying world view imply and include specific notions of relevant time depths. Relevant time depths for development agencies, such as AID, for example, often do not go beyond four or five years in the past. Just so, societies have rolling horizons concerning the details of their genealogies, kinglists, succession of age grades, and other frameworks basic to the operation of their societies. Beyond this limit concepts of time are linked to different considerations relating to the definition of communal selves and to origins or the meaning of the world.

Beyond this level exist traditions of origin. For the most part these represent cosmological speculation. Even if they do contain bits of information referring to events that occurred long ago, it is very difficult to spot these and even harder to interpret them. All information older than a few generations is couched in cliché form and robbed of precious detail. In between recent traditions and traditions of origin yawns a void. No information, or at times just a list of names, links the epoch of creation to the recent past.

In practice we know something about the political systems in the area going back to the end of the eighteenth century. In many places of the savanna a combination of writing and traditions allows us to go further back. There is information for the major states on the coasts south of the Zaire River reaching back to the fifteenth and sixteenth century, and to the seventeenth century elsewhere. Nevertheless, this still remains a shallow time-depth compared to the duration over which such political traditions have taken shape. Moreover Irwin rightly reminds us that oral traditions are not as detailed, circumstantial, or precise as written sources are.[23] Because tradition also acts as the unwritten legitimization for today's political tradition it tells us little about changing institutions or about the gaps between norms and reality. So even where traditions exist, they are helpful only to a limited degree.

The study of the political repertory in central Africa must rely heavily on linguistic and ethnographic traces from the past. Of the two, ethnography is the less useful. At the time of our baseline it yields a number of descriptions from which distributions of particular items (tangible and intangible) can be plotted. Where independent invention can be excluded, these are the residue of processes of diffusion over time. They document traces of the past. It has proven impossible to use *a priori* models of change to interpret such distributions. The size and the shape of distributions do not allow for conclusions, however much they may prove if applied to biological or linguistic data. This is true even for the application of the age-area hypothesis.[24] Other ways to use comparative data for historical purposes have had only limited success.

At first glance linguistic data seem similar to ethnographic data. Yet they can be used because language is arbitrary and every language forms a system. Social behavior and cultural conceptions or expressions, however, are the very stuff of life and hence could not be arbitrary, nor do they form a system in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present
derived form
meaning differs
absent
absent Only crossbows exist

Map 1
C.S. 1631: Reflexes of **-tá* "bow"

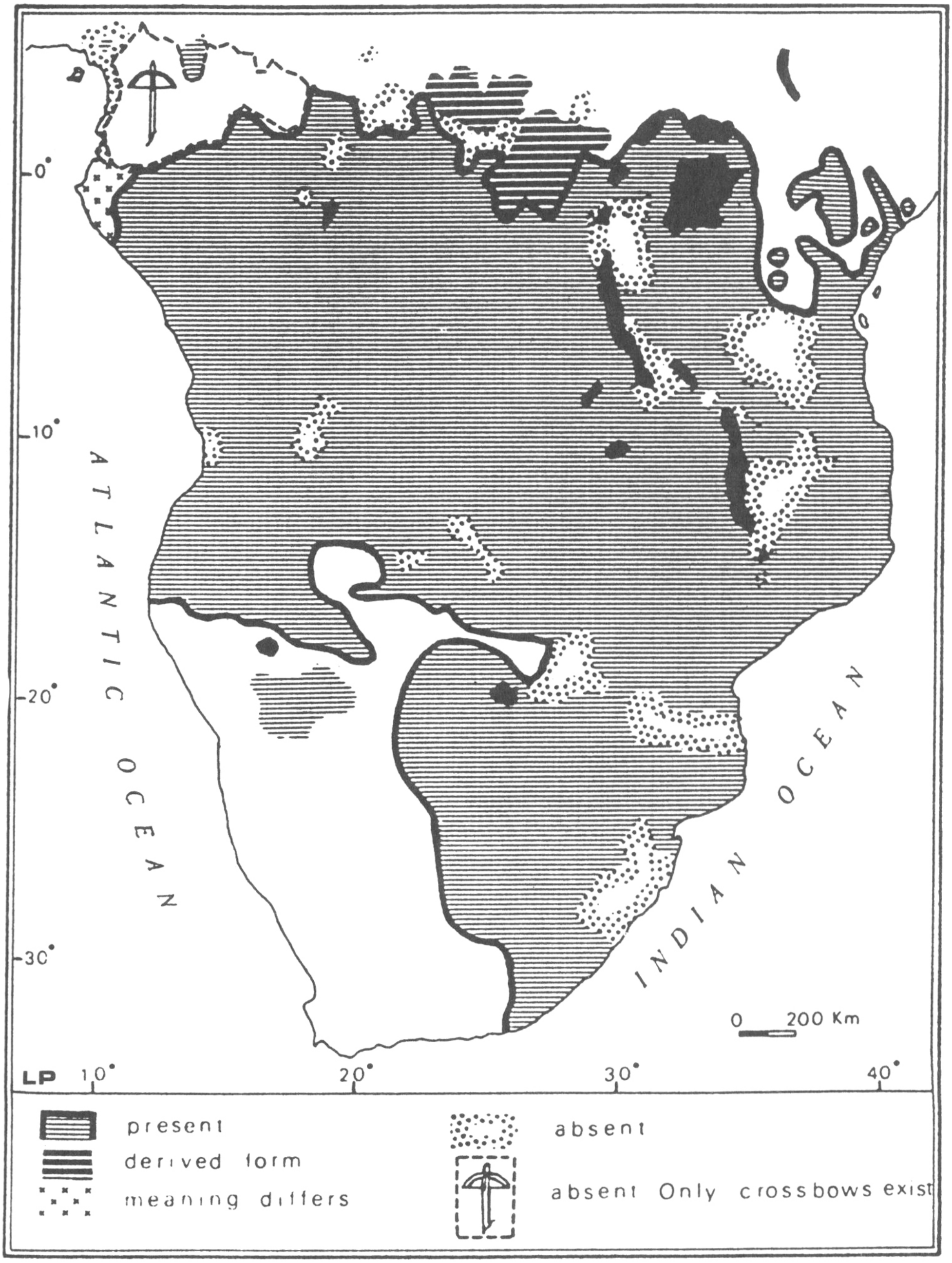
the sense in which language forms a system.

The comparative method in linguistics has developed detailed canons of validity.[25] Such data yield reliable historical information in several ways. Best known is the reconstructions of language families, with associated rules about proto-vocabulary and rules to surmise points of origin for ancestral languages. For the study of political tradition this technique is of real interest, because it attests to a common political tradition as a point of departure. But in general its value remains limited, because time depths involved in reconstruction of language families usually exceed the periods most historians are interested in.

The approach most useful to historians is known as "words and things." It compares words across languages and exploits the arbitrary relationship between a term and its meaning, cases of onomotopoeia and babytalk excepted. Because the relationship is arbitrary, it is highly unlikely that two peoples will invent the same term independently for the same item. Comparative studies of unrelated languages show that there is at most a five percent chance that this will happen by chance. Conversely it is ninety-five percent likely that when two peoples use the same term for the same item, they do so because either one language borrowed from the other, or because the languages are related and both derive their term from a common ancestral term. In mass comparisons chance can be ruled out, while babytalk or onomatopeoia are easily detected. For example, an emblem of authority in central Africa is the clapperless bell, often called *ngonge, ngunga, longa*—a term that, like our *gong*, is onomatopoeic. It cannot be safely used to prove anything without a great deal of detailed study.

Given the relation between term and meaning, comparisons will yield situations where, in different languages both term and meaning are identical, situations where the terms are the same but the meaning differ as in English *sea*, German *See*, meaning "sea" and "lake," and situations where the meanings are the same but terms differ, as in English *dog* and German *Hund*. Often the distribution of a single term shows partial identity of meaning in some places, full identity elsewhere. Moreover, the specific form of a term found in related languages shows whether it is regular or skewed; that is, whether or not it confroms to the general soundshifts that characterize each language. If the form is regular, ancestral origin must be accepted. If it is skewed, loan is most likely. The major work of Malcolm Guthrie, *Comparative Bantu*, lists the known soundshifts for Bantu languages and all terms that seem to have a common grounding in the ancestral languages of western Bantu, eastern Bantu, and even further back in time. Each meaning is listed as a comparative series (C.S.) followed by a number. For example, the term meaning "bow" is C.S. 1631, and when discussing Bantu below these numbers will be listed.

The maps illustrating the comparative series for "bow" and "chief" show the distribution of common ancestral terms for the whole of Bantu-speaking Africa. Not all languages have retained the ancestral forms, which is normal since innovations have occurred. In the case of "bow" the term for a regular bow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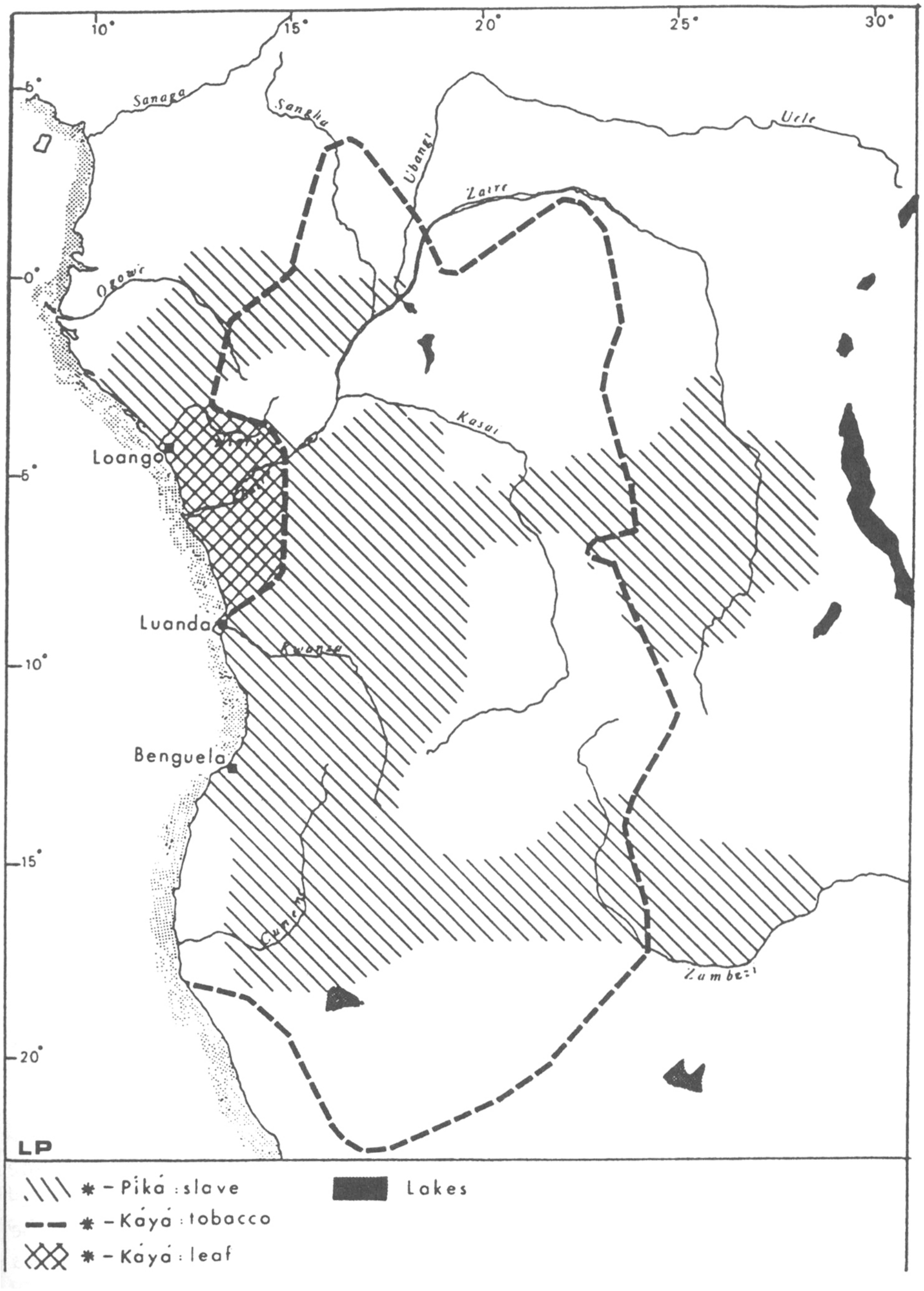
present
derived form
meaning differs
absent
absent Only crossbows exist

Map 2
C.S. 1265. Reflexes of **-kúmù* "leader"

has disappeared in the area where crossbows were adopted. In some cases meaning has shifted, such as where the term "bow" came to refer to "musical bow" only. The reflexes, or the real form in every language, documented in the comparative series for "bow" and "chief," conform in both cases to known sound-shifts and hence reflect common ancestry. A comparison of the situations for "bow" and "chief" shows that the latter is much less widespread than the former. Chief has two forms "*kúmú*" and "**fúmú,*" the first being western Bantu, the second eastern Bantu. Both derive from ancestral forms in the Nigeria/Cameroun area, and in a portion of east Africa its meaning shifts from "chief" to "medicine man." Innovation naturally has affected the core vocabulary of political tradition much more than the vocabulary for "bow." Shifts in meaning have occurred more often, in part simply because a bow is a tangible object and chief is an intangible concept. Even so, such shifts reveal conceptual shifts, traces of real changes in the representation of chiefs and hence authority, for everywhere when **-kúmú* or **-fúmú* occurs as "chief" it also occurs with the meaning "authority."

Shifts in meaning allow for the scientific reconstruction of etymology and on occasion quite significant evidence is thereby uncovered. For example, a western Bantu form **-bánja* (C.S. 55) has the general meaning of "place of authority." In the northwest it is applied to the men's clubhouse, of which there was one per basic political unit and several per village. In Gabon, south of the Ogowe, the term was applied to the village assembly house and temple. Further to the south and southeast it meant "capital" as in *Mbanza* Kongo, the capital of the former kingdom of Kongo. Yet other meanings, such as "meeting place for a council," or "quarter of a village and its central courtyard," and closer to the general meaning of "courtyard" in eastern Bantu. Thus the concept of capital in Kongo developed out of the word applied to the political center in a village, which itself developed out of the meaning "political center of the smallest political unit" (whether a House or a courtyard). The geographical distribution suggests a development in complexity probably from north to south--provided we assume that the development went from less complex to more complex.

The second set of terms (map 3) shows the diffusion of loanwords from the coastal area to the interior. **-káyá* "tobacco," a plant imported *ca.* 1600 just north and south of the mouth of the river Zaire, is derived from the same term, meaning "leaf" in the coastal languages. **-pika* "slave" (CS 1517) spread as the result of the slave trade that began on the coast soon after 1500. This is certainly not the only term for slave or slavelike status in the area, but it came to refer to the *traded* slave.[26] The map shows the immediate hinterland of the harbors of Luanda, Benguela, and Loango plus smaller harbors between them. The absence of the term **-pika* elsewhere along the coast indicates that slaves moved through other networks delineated by other terms for slaves. Its absence along the major slave trading route on the Zaire river upstream from Kinshasa, where another term covers a large trading area, indicates another network, that of the Malebo Pool, which was a major bulking center with its own hinterland. A similar situation occur in much of the Lunda empire.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Luanda
Kwanza
10°
Benguela
15°
Cunene
Zambezi
20°
LP
✱ – Piká : slave
✱ – Káyá : tobacco
✱ – Káyá : leaf
Lakes

Map 3
Some Traces of the Slave Trade, *ca.* 1600-1900

The distribution for **-káyá* is included for comparison. The plant was new, and the term spread much further than the area directly in the orbit of the coastal harbors. Still the map is only a partial representaton. It can be complemented by the full distribution of the tobacco plant and all the terms everywhere used to designate it, and by maps considering terms for slaves used in other harbors along the coast (Gabon, Cameroun) and along other major networks such as the Zaire river above Kinshasa and the Lunda empire network, as well as terms for slave status all over central Africa. The map shows then the usefulness of loanwords for historical inquiry, even for recent centuries. It also shows that the area of diffusion of a loanword can be quite large, so scale alone cannot be used to differentiate loanwords from common origin.

Ethnographic distributions become useful when they are linked to the distribution of words. Since traces of the past, preserved in distributions of words and things, have been retained over the whole two or more millennia affecting the political tradition of central Africa, a systematic examination of the political vocabularies, traditions, and ideology will eventually reveal how the common political tradition developed. Yet there remains a serious flaw in the comparative method. It lacks chronology. Ancestral origin can be separated from loanwords in most cases, but loanwords cannot usually be arrayed in temporal sequence by an examination of their formal features. Outside data are required, as in the case of **-káyá* "tobacco," or as in the Kuba case, where oral tradition tells us at least about the chronology of contacts between the Kuba and speakers of other language groups.[27] The shape and size of distributions, evidence from word derivation, the number of words for a given meaning, can all yield suggestive clues, but nothing more. For the moment we can seriate between ancestral western Bantu terms (including some relating to the political repertory), loanwords before *ca.* 500 AD (thus far biological items only), loanwords and innovations between *ca.* 500 and 1500 AD (including items of the political tradition), and distributions from the sixteenth century to the colonial conquest and after. Almost certainly we will one day be able to establish a relative chronology for the history of political institutions and ideology, especially in recent centuries. However, progress will be quite slow, not just because of the immense task of comparison involved, but because chronological pointers depend on the accumulation of masses of information from different disciplines where dating is possible.

Despite its promise, the "words and things" approach is rarely used systematically mainly because it is time-consuming and requires so much effort to establish a single distribution. Often the results for one meaning or one term are not conclusive and require further research. Researchers are not used to a research design that requires a calculation of the effort to be invested versus the relative values of the expected returns. This sort of cost accounting requires a lucid appraisal at each stage of the research. The question to be resolved and its relationship to the most economical means of answering it must be constantly kept in mind. A flexible and yet precise research design is essential. Most historians or cultural anthropologists are not used to this type of situation, and that is a bigger stumbling block than the fact that new methodologies must be learned; hence the next section deals with concrete illustrations of research designs.

IV

Research and Design and Comparison

The use of comparative linguistic and ethnographic data to establish processes of change allows for great flexibility. The level of comparison can be exactly tailored to the goal pursued. The range of possibilities ranges from a focus on the past of a single society to a focus on all societies that share the phenomenon to be studied. To illustrate the range and the practical results, this section considers almost opposite extremes: a limited comparison focusing on a very few societies belonging to a small set of languages deriving from a recent ancestral language, and a large-scale comparison involving many societies sharing a common ancestral language at the remote level of western common Bantu. The focus of the comparison deals with the political repertory. The first research design will be limited to a discussion of terms of leadership only. The second will encompass the whole political system of all societies compared in order to illustrate the questions involved in a very complex research design.

Our limited comparison deals with the term *kolomo* as it is found in a limited range in north Kasai. The term is found in the Kuba, Lele, Ndengese, Tetela, Yela, Hindu, and Luba (Kasai) languages and the formal peculiarities allow us to trace the direction of its diffusion.[28] The meaning varies as follows: it means "elder, notable" except among the Kuba where it means "titleholder," among the Lele where it means "person pawned," and among the Luba where it means "old, used man." How is it possible for one term to have such a range of meanings for *kolomo*, going from "pawned person" to "titleholder"? The solution is to realize that it forms part of a pair, the other part being **-kúmú* "lord, chief, master" (map 1). The Kuba pair, "chief" (*kúm*) --"titled official"(*kolm*), is analogous to the Lele pair, "master/person pawned to the master." Unlike the Kuba, the Lele had no chiefs but lived in autonomous villages. Among the Ndengese, an area from where the Kuba people stem, *kolomo* refers to rich elders whose association governed sets of villages in the nineteenth century. Collectively such men were *kolomo*, individually they were *kumu*. The Luba usage stresses age. They focused on the decrepitude of age (*kolomo*= "old, used man") and left aside all political connotations, which made no sense in their political structure.

Oral traditions and distributional data tell us the **-kúmú* as leaders sporting titles are old (1400 AD or earlier) in this area. *Kolomo* is an innovation is Bantu, being a derivation from a verb "to become strong" plus a stative. Hence "to be strong" and the noun form applies this to a person. Hence *kolomo* means "a strong person," "a big man." This innovation developed as two different types of "big men" developed and **-kúmú* became confusing to designate both. In Ndengese and proto-Kuba the pair probably meant "leader" and "big men following the leader." The Kuba developed into a kingdom, hence the present meaning of "titleholder." The Lele abandoned chieftainship for village autonomy with collective leadership and the pair was reduced to "master and pawned person owned." Meanwhile the Ndengese developed an association of "big men," so that *kolomo* acquired a collective connotation and **-kúmú* retained the individual connotation.

The terms give us an insight into a history of political institutions that ranges perhaps over five centuries (an estimate based on the chronology of the Kuba kingdom). The comparison does not assume that people *A* remained unchanged over this long period of time while people *B* did change. There are no social fossils hidden in the account. This is worth underlining because the most common error in the use of ethnographic comparisons is to assume that one of the societies compared represents a "pristine" situation, which usually is not defensible.

This type of research design can be used very widely. As an example, consider this design relating to the problem of how the political system of the core of the later Lunda empire, the Rund, developed. In linguisitc and geographic terms the societies closest to the Rund are the Sala Mpasu (Akawand) and the Kongo-Dinga.[29] In the nineteenth century the Sala Mpasu, just north of the Rund, lived in sizeable villages governed by "warrior leaders" or *tulambá tumbánji*. Associations of "young male warriors" or *migongo* were crucial. Big men competed with each other and collaborated with each otherto repel Rund slaving raids. The Kongo-Dinga had "leaders" or *tulambá* ruling over smaller villages, the inhabitants of which were perceived as kin groups.Our sources say that the early Rund were led by "wealthy persons" or *tubung*, each one ruling one or several villages strung out along the banks of the Nkalany river. When we compare the Rund, Sala Mpasu, and Kongo-Dinga societies of the late nineteenth century, the similarities in both meaning and ethnographic contexts become obvious. War, wealth, and claims of legitimacy were all involved in the concepts of leadership. Suggestively, the term *tulambá*, derives from **-dambá* (CS 487) "cloth," which also produced *mulambó*, "tribute," in many languages of the area. The next step will be to trace the distributions of this terminology for each word, and account for them in linguistic terms. In the end the data will allow us to propose a valid reconstruction, not only for the development of the early Rund system, perhaps as late as 1600 AD, but also for other systems drawn into the comparison and for the system that must have been common to all of them when they still spoke the same language, perhaps only a century or so before 1600. Implications relating to size, location of village, relative differences in housing, wealth, and emblems of leadership could then be exploited to allow archeology to test the hypothesis derived from the research design.

Limited comparison of the kind illustrated above is very fruitful as a research design and covers the timespans historians are most interested in. They should allow the history of political repertories to be traced for the last half a millennium and longer, perhaps to the turn of the millennium. But these comparisons cannot be applied to cases where the societies and languages compared are nearly identical. The remaining differences then become so small that no evidence of past processes remains. This unfortunately occurs quite often in portions of central Africa. Limited comparison also does not cover the whole time depth we are interested in--the change in political systems and repertories since the arrival of farmers. In such cases one must turn to larger-scale comparisons.

For these reasons I have developed a large-scale comparison for all the people living in the rainforests. Hitherto this huge area with its hundreds of societies has been perceived as having extremely similar sociocultural ways of life.[30] Ethnographic comparison in the late nineteenth century shows a range of political systems from big men with their kin and dependents living in independent villages all the way to kingdoms (three levels of territorial organization, single leadership).

One can go from one end of the scale of complexity to the other by setting up a model of transformations that takes all the cases into account. When this is done it becomes clear that kingdoms can grow out of chiefdoms, or out of a single big man's house, or out of government by an association, encompassing many settlements, where leaders move from rank to rank. As soon as the highest rank is limited to a single incumbent, a kingdom emerges. Kingdoms can result from the transformation of the institution whereby wealthy men take titles, publicly acknowledged by their communities during sumptuous festivities and accompanied by emblems and subsidiary titles given by the big man to his spouses and followers. There is even a case where a set of stable alliances between equal houses developed into a chiefdom. The So did this in their bilateral or patrilineal society by using a matrilineal succession for the leader, so that succeeding chiefs would always govern from different villages, residence being virilocal.[31] Thus, multiple tracks must be considered in the model.

All the political systems of the peoples of the forests can be fitted into a single diagram showing multiple pathways going from simple to complex systems and vice versa. This yields a logical explanation for all the cases. With what we know about the original political tradition of the western Bantu speakers, we can establish the direction of development as well.

But such a diagram does not show genuine *historical* development. In order to come to this point we have taken into account all known actual transformations—for example, the So case or the case of the Mangbetu kingdom--which developed out of a single big-man-with followers system into a large kingdom within two generations. We incorporate this into our diagram. This diagram then tells us what the parameters of the political systems in this large universe are. We know what to expect and what not to expect. We learn that what looks like a process over time may in fact be two expressions of the same system according to circumstances. For example, one source tells us of a series of small villages equally distributed over the land in a given district. A few years later another source notes the presence of a few very large settlements surrounded by hamlets in the same district. One is tempted to see the second situation as a transformation of the first one. But a few years later a third observer in the district notes a return to small villages equally distributed over the land. Did the second situation transform into the first one again? Further examination shows that the second situation occurred in time of war or when a trading route momentarily was diverted through the district. The answer to our questions is, in short, *both*: villages of the same size, equally distributed, and villages of unequal size in clusters are but two facets of a *single organization*. Within a few years and according to need, one aspect or the other can come to the fore. The organization is not rooted at the level of the village, but at the level of the district. This example was very common in the forest areas of central Africa just before 1900. We can thus learn what the dynamic possibilities as documented from available history are. In this way, we know what kinds of hypotheses can account for the whole time span and all the systems.

Once this is done one can closely examine the vocabulary relating to the political repertory. Part of it is common to western Bantu. For every term such as **-kúmú* we must record the variant meanings found and propose a history of meaning. The **-kúmú* means "chief" but also in many cases "wealthy person." One meaning may have been derived from the other, but "chief" is a proto-western Bantu meaning because it is the only form which has this meaning. Probably the relative shift of meaning from "chief" to "wealthy person" and vice versa occurred many times during the long political history of the area. We can also take note of items that developed or were introduced later as shown by word distributions, linguistic innovation, or in some cases the distribution of objects such as emblems.

Merely listing this agenda shows why it is reasonable not to be too dogmatic about the political system of the earliest western-Bantu-speaking farmers that entered the area. Still, we can say that they lived in villages and recognized political leaders as distinct from religious or technical specialists. Wealth and authority were probably closely related and we can imagine the leader as a big man whose wealth was used to keep his followers. More tentative data suggest the existence of a chief's council and/or court of law. We can think of some emblems of chieftaincy such as drums. Succession was ill-defined. So far, and rather surprisingly, no linguistic link between chief and sacrality of any kind has been forthcoming in western Bantu, unlike the situation in eastern Bantu. Ethnographic comparison suggests, however, that chiefs were thought to have preternatural powers of some kind, since every single political system in the area showed this feature in the nineteenth century. Terms for "charm," "witch" (probably two), "medicine man," "ritual avoidance," and others are well attested in western Bantu. As to the size of the political arena, the evidence indicates that several villages or a single village constituted the largest recognized political units.

What matters most, however, is that this constitutes a genuine point of departure for the political heritage of these peoples in the rainforests. Both our point of departure and the points of arrival represent historical situations. In going from the baseline backwards we can take genuine historical change into account, such as the impact of the slave trade from *ca.* 1530 to 1880. This involves studies of other sets of linguistic distributions such as the one for **-pika,* which can secure a greater time depth for known political developments in the last centuries. The range of possible parameters for political systems in our pathway diagram is narrowed down as a result. While it is too early now to present conclusions, the outlines of a genuine political history are beginning to appear in the area as a whole.

This shows that it is possible to elaborate a general hypothesis about political developments in the area and to establish its broad correspondence to known elements of historical change in the past. Such a hypothesis remains rudimentary, but it has three qualities. Parts can be tested by archeology, it suggests directions for further research to confirm or reject portions of the whole hypothesis, and it is complex.

The issue of complexity needs some amplification. The plausibility of any given hypothesis increases with the complexity of the data base it integrates. A hypothesis involving a limited number of cases and variables is less solid than a grand design accounting for a large number of cases and variables. In the first instance more alternative hypotheses are possible, thus lowering the probability that any single one of them is correct, even if it looks the most plausible among those considered. The probability that some alternatives are not even considered is the highest for middle range hypotheses, those that involve more than two or three cases or variables. However, the likelihood that an alternative hypothesis can account for a truly large number of cases and variables becomes quite small. Add to this the fact that the more economical the hypothesis, the more elegant. It follows, paradoxically perhaps, that in principle the grand design should be better than the limited hypothesis! For it accounts for a larger universe and is more economical than a set of hypotheses concerned with small universes. The danger of the grand design remains, of course, that no hypothesis will be found to account for all the features that must be explained.

Most of us shrink from the elaboration of truly complex hypotheses. We rarely feel the need to do so because we usually accept without question the existing general historical frameworks for the area and periods we study. In such cases we often do not even realize that historical frameworks are themselves complex hypotheses! Where such a framework does not exist, research simply cannot some to grips with the problems to be elucidated. The past history of the peoples of the rainforest is a striking example of such a situation. Instinctively researchers shy away from cases like this. They feel that it is presumptuous to create a hypothesis on a grand scale because, in its first formulation, its chances to be entirely correct are dismal. So, to set up such hypotheses is felt to be a waste of time that will amost certainly result in a loss of face. Moreover, the amount of data needed to test such a hypothesis thoroughly is so vast that a lifetime may pass before all the results are in.

But with the guidance of such a hypothesis, however imperfect, research can be brought into focus. Opportunities for testing precise points are now given, whether by a search for direct evidence, as in archeology, or indirect evidence, by the use of research designs using limited comparison but adducing much richer data. New knowledge is now generated and it is cumulative. The field progresses as the general hypothesis is amended and generates further questions for testing, until sufficient knowledge provides full answers to what can be known.

In the example of the hypothesis about political developments among the inventories or rich ethnographic and linguistic data must be set up. This will require much fieldwork. Archeological research, just now beginning, needs to be immeasurably intensified. Very limited comparison research designs will have to be designed to test every single part of the grand design. It may take a generation of research, but in the long run a rich and valid reconstruction for the past two millennia will emerge, and that is no small achievement.

NOTES

1. This paper has grown out of research in progress to trace the past of the peoples living in the rainforest of equatorial Africa. Hence I concentrate more on this portion of central Africa than on the southern savanna. I am grateful to Roderick J. and Susan Keech McIntosh for their comments and editorial work on this paper.

2. W. De Craemer, Renée Fox, and Jan Vansina, "Religious Movements in Central Africa: A Theoretical Study," *Comparative Studies in Society and History,* 18 (1976): 458-75.

3. Leadership (*-*kúmú* in Common Bantu) was a role bolstered by both beneficial and malevolent forces, the 'power by day and the power by night.' The role was interconnected with those of witch, healer, magician, and prophet (or diviner) in a stable intellectual pattern. Hence power was at the same time legitimate and illegitimate. It was the power of a 'big man' even if the leadership passed to a relative. Moreover, this view was accompanied by a perception of the community as a bilateral family writ large established over a 'land' or district. From that perspective the leader was a 'father' who should protect his 'offspring' even while he governed them with a firm hand. This image also justified why considerable autonomy should be left to local units in the 'land.' Overlords should arbitrate relations only between such units, never intervene directly between local 'fathers' and their 'families.'

4. Vansina, "Western Bantu Expansion," *JAH,* 25 (1984): 129-45.

5. Vansina, "Esquisse historique de l'agriculture en milieu forestier," *Revue Muntu* 2 (1985)

6. George P. Murdock, *Africa. Its Peoples and Their Cultural History.* (New York, 1959).

7. Mumbanza mwa Bamwele, "Histoire des peuples riverains de l'entre Zaire-Ubangi: évolution sociale et économique," (PhD, UNAZA/Lubumbashi, 1980), 909-26.

8. P. van Leynseele, "Les Libinza de la Ngiri: l'anthropologie d'un peuple des marais du confluent Congo-Ubangi," (PhD, Leiden, 1979).

9. J. Maes and O. Boone, *Les peuplades du Congo belge: nom et situation géographique.* (Brussels, 1935); I. Dugast, *Inventaire ethnique Cameroun* (Dakar, 1949);

Boone, *Carte ethnique du Congo--quart sud-est* (Tervuren, 1961); J. Redinha, *Distribuçâo etnica da Provincia de Angola* (Luanda, 1969); Boone, *Carte ethnique de la République du Zaire--quart sud-ouest* (Tervuren, 1973); *Atlas du Cameroun.* ed. G. Laclavère (Paris, 1979); *Atlas du Congo.* ed. P. Vennetier (Paris, 1977); *Atlas du Gabon.* ed. L. Perrois(Nancy, n.d.).

10. M.C. Dupré, ""Naissances et renaissances du masque Kidumu: art, politique et histoire chez les Teke (tsaayi)" (Doctorat d'état, Université de Paris V., 1984): 43-70.

11. *Bulletin Officiel,* quoted in R. Buell, *The Native Problem in Africa* (2 vols.: New York, 1928), 2:568.

12. E. E. Evans-Pritchard, *The Nuer* (Oxford, 1940).

13. F. Olbrechts, *Ethnologie. Inleiding tot de studie der primitieve beschaving* (Antwerp, 1936), 276-79; Vansina, "Knowledge and Perceptions of the African Past" in *African Historiography,* ed. B. Jewsiewicki and D. S. Newbury (Beverly Hills, 1985),

14. Evans-Pritchard, *Witchcraft, Oracles, and Magic Among the Azande* (Oxford, 1937).

15. G. van der Kerken, *Les sociétés bantoues du Congo belge et les problèmes de la politique indigène* (Brussels, 1920).

16. A. Wolfe, *In the Ngombe Tradition. Continuity and Change in the Congo* (Evanston, 1961).

17. A. Bastian, *Ein Besuch in San Salvador* (Bremen, 1859); E. Pechuel Loesche, *Volkskunde von Loango* (Stuttgart, 1907).

18. K. Piskaty, "Ist das Pygmäenwerk von Henri Trilles eine zuverlassige Quelle?" *Anthropos,* 57 (1957): 33-48.

19. Vansina, *The Tio Kingdom of the Middle Congo, 1880-1892* (London, 1973).

20. In the ethnography of North America the critiques of "memory culture," former ways of life reconstructed from a single informant's recollections have not abated, however. The deficiencies in the quality of the sources more than the principle itself keep reservations about "historical ethnography" alive. Full-scale reconstructions of the ways of life of other peoples of central Africa for a given generation, rather than as general statements, have not yet been undertaken.

21. *History of Central Africa,* ed. David Birmingham and Phyllis Martin (2 vols.: London, 1983)

22. David Henige, *Oral Historiography* (London, 1982); *The African Past Speaks,* ed. Joseph C. Miller (Folkestone, 1980), passim; Vansina, *Oral Tradition as History* (Madison, 1985).

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24. Margaret T. Hodgen, *Change and History* (New York, 1952); idem., *Anthropology, History, and Cultural Change* (Tucson, 1974); Vansina, *Le royaume kuba* (Tervuren, 1964); idem., "The Use of Ethnographic Data as Sources for History" in *Emerging Themes in African Hisotry.* ed. T. O. Ranger (Dar es Salaam, 1968); *Historische Völkerkunde.* ed. C. A. Schmitz (Frankfurt, 1967); J. Zwernemann, *Culture History and African Anthropology* (Uppsala, 1983); Adam Kuper, "Regional Comparisons in African Anthropology," *African Affairs,* 78 (1979): 103-13.

25. R. Anttila, *An Introduction to Historical and Comparative Linguistics* (New York, 1972); T. Bynon, *Historical Linguistics* (Cambridge, 1977); H. H. Hock, *Principles of Historical Linguistics* (Berlin, 1986).

26.E. De Jonghe, *Les formes d'asservissement dans les sociétés indigènes du Congo belge* (Brussels, 1949).

27. Vansina, *The Children of Woot* (Madison, 1978).

28. Ibid., 304.

29. William Pruitt, "An Independent People: a History of the Sala Mpasu of Zaire and Their Neighbors" (PhD, Northwestern, 1973); J. H. C. Ceyssens, *Pouvoir et parenté chez les Kongo-Dinga du Zaire* (Meppel, 1984).

30. Murdock, *Africa;* Hermann Baumann, *Die Völker Afrikas und ihre traditionellen Kulturen* (2 vols.: Wiesbaden, 1975); Baumann and Dietrich Westermann, *Les peuples et les civilisations de l'Afrique* (Paris, 1948).

31. J. Fraessle, *Meiner Urwaldneger Denken und Handeln* (Freiburg, 1923).