TOWARD A RECONSTRUCTION OF PRE-COLONIAL CENTRAL AFRICAN HISTORY

by

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Introduction

In an earlier paper on "Class Formation, Class Struggles, Class Consciousness in Zaire"[1], I had attempted to give an overview of Zairian history from pre-colonial times to today. The present paper attempts to go beyond by focusing the discussion on the problems that historians are faced with when they try to reconstruct pre-colonial history. The object of this paper, therefore, is not so much to provide an interpretation of pre-colonial Central African history as to identify some of the major difficulties. This procedure was necessitated by a variety of reasons.

In spite of the number of monographs produced, there is still as yet no satisfactory text on pre-colonial Central African history.[2] Furthermore, the available material suffers from serious deficiencies. Most of the studies that exist have been produced from an implicit and/or explicit bourgeois problematic. Because these are the only available texts to the students, it has been necessary to constantly criticize the conceptual framework adopted in the above works. Given the conditions under which "field" research has to be carried out, it is going to be some time before satisfactory texts are produced from data collected on the basis of a dialectical materialist problematic. Thus, while criticizing the existing texts, the Marxist historian is still forced to rely on data contained in those very texts that he/she is criticizing, hence making the critical step even more crucial.

In many histories of African social formations there is a tendency to reduce the pre-colonial period to communalism, or an era during which antagonisms and contradictions were limited. The pre-colonial period tends to be seen or romanticized as a period during which various societies lived in almost total harmony.[3] Among Marxist writers this harmonization of pre-colonial relations of production has been conceptualized under the term 'natural economy'.[4] Presumably, the term is borrowed from Lenin's first chapter in the *Development of Capitalism in Russia*. Whatever the reasons for resorting to the concept of 'natural economy', it should be rejected on the grounds that there could never be such a thing as 'natural

economy'. The fact that Lenin used 'natural economy' is no licence to use it, nor is it a proof of its scientific validity, unless one chooses to use Lenin's writings as a catechism or a repository of the final truth. A rejection of the concept of 'natural economy' does not mean the rejection of Lenin's demonstration against the Narodniks of the existence of capitalism in Russia.

This brief introduction is meant to serve as a reminder that in reacting against bourgeois histories, Marxist historians may often find themselves reproducing the very problematic against which they are struggling. In a sense the earlier paper, "Class Formation, Class Struggles, Class Consciousness in Zaire", by its very title illustrates the failure to establish a Marxist problematic separate and independent from a non-Marxist one. Since one of the tenets of bourgeois pre-colonial histories was that there were no classes in pre-colonial history, it was thought that the task of a Marxist historian would be to prove that there were classes, or classes in formation. It would obviously be shortsighted to reduce the task of a materialist history of pre-colonial Africa to the objective of demonstrating the existence of class relations.

Non-Marxist Approaches

As productions of pre-colonial histories began to develop in the late fifties and especially early sixties, the most often discussed problems were those of collection of sources.[5] Various ways had to be found in order to overcome the paucity of written material. Thus, for a long time, and to a certain extent still today, J. Vansina's *Oral Tradition* was the standard text for young historians ready to begin their work on new areas.

While Vansina continues to be the object of dithyrambic reviews (especially, but not exclusively) from his students, his work has also come under very sharp and well deserved criticism. For example, any reader going through J.C. Miller's and Claudine Vidal's reviews of Vansina's *The Tio Kingdom* is bound to wonder whether the two reviewers read the same book. Their assessments are so widely divergent.

Claudine Vidal's critique revolves around one major point, namely that the practice of marrying history and anthropology produces unsatisfactory ethnography as well as unsatisfying history. Could it be that this is the reason for creating the new field of ethno-history? In fact, it may not be entirely correct to speak of 'marrying anthropology and history' for the basis from which many anthropologists operated during colonial rule required the recognition as well as non-recogni-

tion of history. This paradox was concretized through the notion of 'ethnographic present'. Through this device, it was possible for anthropologists to carry out research and write accounts which completely ignored the historical conditions under which those investigated populations were living. Anthropologists claimed that it was possible to describe village life, social customs, religious rituals as if they had been undisturbed by the arrivals of the colonizers.[6]

Aside from the notion of ethnographic present, there is a more serious problem which Claudine Vidal may not have sufficiently stressed: the constant intrusion of anthropological problematics in the production of most pre-colonial histories can be seen by the way in which it has even stamped Marxist scholarship.

This is not to say that all the work of anthropologists must be rejected. Quite the contrary. It is being argued that when trying to use the results of anthropological research, historians must remember the conditions which surrounded and determined the collection of the data. Admittedly, this is not always possible to do. However, it should be possible, in most instances, to determine the purpose for which any particular material has been assembled.[7]

Anthropological scholarship has not developed in a vacuum; it developed according to a certain dynamic which was in part linked, indeed generated by the colonization of most of the continent by European formations. Anthropology also developed an internal logic which, on the surface, may appear divorced from the above mentioned process. It is in part this internal logic that may have accounted for the intensive concerns among anthropologists for the study of kinship structures. It will be shown later that this is not entirely correct for the appearance of kinship structures among the so-called 'primitive' societies as their dominant characteristics can historically be accounted for.

From the early days of anthropology, the mark of a good anthropologist was whether he was capable of producing a theoretical work. A great deal of this theoretical work had as its object the study of kinship systems. While a large body of literature has been devoted to a thorough denunciation of the colonial and neo-colonial functions of academic anthropology, very little effort has gone into a critical examination of the scientific foundations upon which anthropology has been built.[8] It is very rare to find a member of the profession questioning the very basis and object of the theoretical exercises of the discipline.

It is true that the work of E.R. Leach, among others (and especially among the Anglo-Saxons), has greatly contributed to shaking the establishment's complacent prejudices and assumptions. Leach's initiative incited one of his fellow enthusiastic iconoclasts to argue that "there is no such thing as kinship, and it follows that there can be no such thing as kinship theory."[9]

But even this critical exercise is fraught with serious problems. Essentially, Leach and some of his colleagues were simply suggesting that the *terminology* might be outdated. In saying that anthropologists should go back to the beginning, Leach argued that "first and foremost we are discussing a list of words, so it might well be to consider what sort of words they are before we lose sight of the original evidence in a welter of algebra."[10] For him, the way in which kinship studies were being carried out could only bring "marginal rewards". To overcome these diminishing returns he suggested "to concentrate our attention on the way that the linguistic performance of the individual (with respect to kinship terminology) varies as he moves from one social situation to another. This, in turn, will require us to pay more attention to the psychological as distinct from the sociological significance of particular words."[11]

One would have thought that after having produced his *Political Systems of Highland Burma*, Leach would have been theoretically more sensitive to the historical determinations of kinship categories and systems. From the quotations above it does not seem to have occurred to Leach that one of the most fundamental departures would have been to question whether the anthropologist's attachment to the study of kinship was not an infatuation with the study of what one could metaphorically call an archaeological aspect or mode of expressing a particular form of social relation; archaeological, not in the sense that it is completely dead, but that it is no longer a moving force. It is obvious, nowadays that kinship relations are still determining certain aspects of life, but those relations while *individually determinant are no longer socially as determining as they were when they were crucial for the production and reproduction of the social and material conditions of existence.*

Marxist Anthropology and Pre-Colonial History

After what has been said about the relationship between history and anthropology it might seem surprising that the greatest stimulus for reconstructing a materialist pre-colonial history has actually come from Marxist anthropologists.[12] Non-Marxists will probably react by saying that the

distinction would not necessarily go to Marxist anthropologists, and they might advance Claude Levi-Strauss' work as the shining example of how best to combine historical materialism with anthropology. Although a discussion of Levi-Straussian Marxism might carry us too far away from our present concerns, it might be useful to discuss its most salient features especially in view of the influence that his work has had on a Marxist like (for example) M. Godelier. The clearest statement can be found in the heavily autobiographical *Tristes Tropiques*:

> *Reading Marx was for me all the more enthralling in that I was making my first contact, by way of that great thinker, with the philosophical current that runs from Kant to Hegel. A whole world was opened to me. My excitement has never cooled: and rarely do I tackle a problem in sociology or ethnology without having first set my mind in motion by reperusal of a page or two from the 18 Brumaire of Louis Bonaparte or the Critique of Political Economy. Whether Marx accurately foretold this or that historical development is not the point. Marx followed Rousseau in saying - and saying once and for all, as far as I can see - that social science is no more based upon events than physics is based upon sense-perceptions. Our object is to construct a model, examine its properties and the way in which it reacts to laboratory tests, and then apply our observations to the interpretation of empirical happenings: these may turn out very differently from what we had expected. (emphasis added)*
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> *At a different level of reality, Marxism seemed to me to proceed in the same way as geology and psycho-analysis (in the sense in which its founder understood it). All three showed that understanding consists in the reduction of one type of reality to another; that true reality is never the most obvious of realities, and that its nature is already apparent in the care which it takes to evade our detection. In all these cases the problem is the same: the relation, that is to say, between reason and sense-perception; and the goal we are looking for is also the same: a sort of super-rationalism (emphasis in the text) in which sense-perceptions will be integrated into reasoning and yet lose none of their properties.*[13]

There are two key words in this passage: model and super-rationalism, both of which were to lead later to the elabora-

tion of *Structural Anthropology*, via F. de Saussure's *Course in General Linguistics*. For Levi-Strauss, Marxism was an accessory tool which could be used to unveil the hidden structures which are characteristic of all mankind:

> *In suggesting Man as the object of my studies, anthropology dispelled all my doubts: for the differences and changes which we ethnographers deal in are those which matter to all mankind, as opposed to those which are exclusive to one particular civilisation and would not exist if we chose to live outside it. Anthropology set at rest, what is more, the anxious and destructive curiosity of which I have written above: I was guaranteed, that is to say, a more or less inexhaustible supply of matter for reflection, in the diversity of human manners, customs, and institutions. My life and my character were reconciled.*[14]

Presumably the super-rationality was Levi-Strauss's medium through which he aimed at producing an anthropological science which would be above the subjectivity of scholars, hence his suggestion that value judgments (e.g. racism) in social science could be avoided through the use of mathematical language.[15]

In a sense it could be said that the Marxist method is structuralist because it operates on the basis of analyzing structures. Levi-Strauss also has argued that all societies develop around structures which are not visible (e.g. marriage as exchange of women), but which must be investigated in order to understand the process of change in those societies. However, Levi-Strauss's structuralism offers no method of how to go about discovering the so-called structures. It is simply asserted that structures can be uncovered through investigative work. More and more research is thus seen as the key to success. It is assumed that the multiplication of data will finally produce a pattern. The structures themselves are not seen as the result of historical processes, but rather as discoverable through sheer accumulation of data. Every new researcher can provide his/her own structural explanation of observable and non-observable data. This fundamental subjectivism and the claims of objectivity (through mathematical or computer treatment of data) may account for the popularity of structuralism among Marxists and non-Marxists. For the former, the work of Althusser, especially *Reading Capital*, may have helped to give structuralism greater academic respectability than it deserves. As was pointed out by a constructive critic of Althusser: "While deepening the theory, one should not loose sight of the reasons for which it was established."[16]

The extreme formalization of Marxism by Althusser certainly revitalized it, but it has also opened the way to theoreticism, which in the end, may overwhelm the positive aspects of his intervention. Ironically, while Althusser himself has moved away from his earlier formalism,[17] his 'followers' (see many of the contributions in *Economy and Society*) have pushed formalism to higher levels.

The affinity between the theoretical rigour of formalist Marxism (e.g. Hindess and Hirst in their *Pre-Capitalist Modes of Production*) and Levi-Strauss's structuralism is much closer than the former would like to admit in spite of their attack against the latter. While Hindess and Hirst are correct in their denunciation of the anthropological problematic which has dominated kinship studies, they failed to identify what distinguishes an anthropological problematic from a dialectical materialist one. In fact, it could be argued that there is no fundamental distinction between them and Levi-Strauss. The latter sees kinship as the appropriate concept for dealing with primitive societies while the concept of mode of production would be more appropriate for class societies. On their part, Hindess and Hirst suggest that

> *Kinship social relations are ideological social relations which may or may not correspond to the structure of certain genealogical relations between individual human animals. Under conditions of the primitive communist mode of production a network of kinship relations is a condition of existence of the complex redistribution variant of the mechanism of extraction of surplus labour. Since this mechanism requires the dominance of the ideological level in the social formation we may speak loosely of the dominance of kinship relations in some, but not all, primitive communist social formations.*[18]

For these two authors, it seems to be the ideological dominanc that determines or not the dominance of kinship relations and this position seems to be very similar to that of M. Godelier.

Basically the question of the appropriateness of kinship relations to pre-capitalist relations of production is posed in the following terms: do kinship relations describe the actual concrete material relations of production or do these kinship relations mystify and misrepresent the concrete reality? M. Godelier answered this question by asserting that kinship relations actually function as if they were relations of production, political relations and ideological structure. In other words, kinship relations represent at the same time

the infrastructure and superstructure.[19] Such a position, as pointed out by E. Terray, reproduces the classical anthropological interpretation of kinship systems. Kinship systems are plurifunctional.[20] They are plurifunctional because they are dominant, and they are dominant because they are plurifunctional. Such tautological circling is made possible because of the empiricist practice of classical anthropology.

In an excellent critique of this position Alain Marie has pointed out that it is not enough to say that kinship relations function as relations of production, one still has to explain why. Does 'function as' mean 'equivalent to'? If the problem is to prove that kinship relations function as production relations then one should explain why these economic relations take the form (*se realise*) of kinship relations.[21] Admittedly, Godelier was trying to avoid vulgar economism, but in doing so he moved completely out of a Marxist problematic which stipulates that it is the material conditions and relations that determine all social relations. Finally, to say that kinship is dominant is not the same as saying that is determining.

By posing the question in the above terms, it becomes immediately apparent that the question of kinship relations (their dominance as distinguished from their determinacy) should be examined from a historical perspective. The historical conditions under which kinship relations emerged as specific forms of organizing social formations ought to be specified and not universalized. Kinship relations ought to be treated as the surface appearance of specific social relations. From this perspective Hindess and Hirst would be correct in saying that now and then kinship relations could be actual relations of production, but they failed to analyse the historical conditions of this determination.

Anthropologists, however, through their practice have elevated kinship relations to a permanently determining feature of all pre-capitalist social formations. And it is in part through this practice that there emerged a picture of pre-colonial Africa as an amorphous mass of unchanging reality. Not only was pre-colonial Africa described in that way, but it was even assumed that this pre-colonial world remained unchanged during colonial rule. When going in the field, anthropologists were encouraged to select groups of people living as far away as possible from 'modern' civilization. If this were not possible, the notion of 'ethnographic present' could always be used for pretending that the traditional world was still untouched by modernity.[22]

Whereas the ahistoric assumptions and presumptions of the so-called ethnographic present have been easily identi-

fied with regard to what was happening during colonial rule, the same cannot be said with regard to the pre-colonial period. There are very few histories of pre-colonial rule which integrate the onslaught of Atlantic slavery on the various social formations which suffered from it.[23] Atlantic slavery is most often treated as a separate process, but very little is said about how it intensified the commoditization process, how it undermined the internal dynamics of production and reproduction processes of those social formations which were and were not directly confronted with Atlantic Slavery.[24] Atlantic Slavery resulted in transformations of social and economic relations, the extension and intensity of which has not been fully assessed. One may very well ask whether it was not this destructive phase which may have accounted for the anthropologists' focus on kinship relations.

To say that anthropologists have erroneously singled out kinship as the focus of their studies is not sufficient. One still has to explain why kinship was singled out. Could it be that material conditions were such that in certain cases kinship relations had again become determining? Kinship relations may have acted as the last refuge, the last kind of soci relations into which members of the social formations could retreat to protect themselves and their closest relatives.

Slavery and later colonialism had the effect of completely destroying the political and economic bases of many African social formations to such an extent that the surviving social institutions (kinship systems) were those which has lost their determinacy after they had been superseded by slavery and, in some areas, by feudalism. This historical and materialist analysis of why kinship may have appeared as the dominant feature of African social formations also casts the so-called errors of H. Morgan's *Ancient Society* in a different light. We shall turn to him in a moment, for the manner in which he has been criticized (as an apology for Engels' subsequent errors) reveals the cornerstone of classical anthropology, which is empiricism.

This has already been mentioned earlier with regard to plurifunctionality. Because anthropological data are often highly descriptive, it is assumed that all materialist historians need is to simply interpret the data in their own framework. Such empiricism may have counterproductive effects on a materialist analysis if it is not realized that the descriptions offer a view of society which may already have been fetishized by the informants themselves. Thus, when rights a described, say between people and nature or between people and people, they do not describe real relations. Most of the property relations described in anthropological monographs often refer to juridical or legal relations and not to real relatio

This lack of discernment between legal and real relations has been one of the greatest obstacles in the way toward a materialist understanding of pre-capitalist property relations.

Suspecting this difficulty, P. Ph. Rey has wondered whether "the particular importance given to real kin relations in the 'primitive' societies by anthropologists is not a mystification."[25] Unfortunately, Rey did not pursue this line of inquiry, possibly because it might have led to a rejection of the concept of lineage mode of production. The mystification could be further increased if one relies on specific forms of kinship organization to construct modes of production and reproduction. This was one of the errors attributed to Morgan when he attempted to offer an evolutionary view of society which placed matrilineal and patrilineal societies in a chronological sequence. Morgan's error was attributed to a lack of data, which is of course the most convincing way a science based on empiricism can explain its errors. It would be wrong to see in the various forms of descent the representation of various stages of history. One of the reasons why such interpretations die hard may have to do with the apparent coincidence that exists between certain forms of descent and modes of paying bridewealth.[26] There are obviously historical as well as conjunctural causes for the variations in systems of descent, but they should not *necessarily* constitute the basis upon which one reconstructs pre-capitalist modes of production. For example, the Luba and the Lunda of central Zaire are respectively patrilineal and matrilineal, and yet it will be argued that the expansion of these two social formations was carried out through extraction of surplus labour and surplus product from slaves and/or infeodated social formations.

One of the most lucid attempts made to unravel the relationship between kinship relations and relations of production is M. Bloch's study of two social formations in Madagascar.[27] The following can only be an abbreviated account of the most salient features of that study. M. Bloch, somewhat like Godelier, wanted to establish whether or not kinship relations could be treated as relations of production. In more general terms he wanted to establish whether or not kinship relations were simply an ideological representation (superstructure) of relations or production (infrastructure).

He proceded by examining the concrete material conditions of existence of each group, and what kind of social, political and economic relations emerged as a result of those conditions. From the property relations that existed, Bloch was able to establish whether these reflected actual relations of production. For example, in the case of the Merina, he showed that the relations that seemed to dominate were described as relations between land and people. This, he argued, could not

be the case,for land (rice fields in terraces) itself was not just any kind of land, but was already the result of definite transformations through labour. In other words,the relationship between land and people was in fact a relationship betwee people: those who had transformed the land through their labou and the real owners of land.

This point cannot be overstressed in view of the way in which land has been treated in anthropological literature. Claude Meillassoux has already drawn attention to the importance of distinguishing between land as object of labour and land as means of production (subject of labour). As an object of labour, land is directly used without any transformation, a situation which prevails under primitive communism. As an object of labour, land is not transformed, no labour is invested in it in order to transform it. When land has become a means of production (or subject of labour), it means that it has been transformed, and is in fact useless unless it is actually continuously transformed. Therefore, to simply talk about availability of land without specifiying the conditions under which it exists (i.e. whether or not it has been transformed through labour and to what extent) is to fetishize land and the relations that are structured around it.

Thus in all the African pre-colonial formations where it is argued that land ownership is vested in the ancestors or the lineage heads or clan elders, one is confronted with actual or potential distortion of real relations of production. With the risk of overstating the case, it could be said that such distortions occur in almost all if not all pre-colonial centralized formations. In these formations kinship relations are an ideological misrepresentation of the social reality.

Bloch's contrasting case in Madagascar was that of th Zafimaniry who describe their relations of production through their kinship relations. The swidden cultivation which they practice has meant that the only way more land could come under cultivation will be through recruitment of more labour. As a result of this necessity, the Zafimaniry have maintained an open kinship system which facilitates recruitment of more members into the lineage, clan, village. The Merina, on the contrary are very well known for their highly stratified society which maintains a rigid distinction between 'insiders' (owners of the land) and 'outsiders' (often referred to as slaves).

By emphasizing labour as the crucial element in production relations, and reproducing this emphasis in the organization of their kinship system, the Zafimaniry did not misrepresent the concrete reality. By operating around the ques-

tion of labour, Bloch offers a better understanding of the relations of exploitation and domination in pre-colonial social formations. Specifically it also opens the way for a better understanding of the exploitation and subjugation of women.

Pre-Capitalist Social Formations and the Woman Question

The purpose of this section is very limited: to establish what may have been the historical conditions which accounted for the subjugation of women.

F. Engels' *The Origin of the Family, Private Property, and the State* is often used as a starting point for analysing the above question. Insofar as Engels' analysis is rooted in an evolutionary anthropological problematic, many of Engels' critics who did not recognize this were bound to reproduce this problematic or a derivation of it. More seriously: even with such a recognition, it has not always been possible for critics of Engels to rid themselves of evolutionary schemas. How to overcome this anthropological problematic has been discussed above, but may be worth repeating again, for the discussions of Morgan's *Ancient Society* on which Engels' *The Origin of the Family, Private Property and the State* is based, have shown, there cannot be a blanket dismissal of Morgan's work.[28]

The often repeated assertions that the weakness of Engels comes from his having had 'wrong' or insufficient anthropological data completely miss the most important point namely why did kinship relations appear to many of the nineteenth century observers as the most characteristic features of the societies under observation? Having failed to pose the question of the historical conditions under which their observations were being conducted, anthropologists then universalized their findings. Although Morgan did establish the existence of different stages, the subsequent practice of anthropology has been to ignore those different historical epochs, and instead to challenge Morgan's findings by adding new ethnographic data which were gathered in a similar fashion. Despite Godelier's critique of Morgan, there is no reason why it cannot be argued that if women are oppressed today, it could be because this oppression coincided once in their history with exploitative relations.[29] This may be a very simplistic way of posing the problem, but it comes to mind through looking at the position of blacks in European and American social formations. The oppression of blacks in these formations has been determined by their history, a history which is closely linked with Atlantic slavery. All blacks who were shipped from the continent were shipped as slaves, that is to say as a class. In the history of blacks in the United States, there was at one point

congruence between the colour of their skin and their belonging to a specific class. This is not to say that they became slaves because they were blacks: the continent became a target for slave traders because of the particular level of development reached in relation to the European formations.

Taking the history of blacks in the United States as an analogy, could it be argued then that women may have been historically in a similar position: they were *socially* the target of entire social formations, and in the process, because of the specific needs they were fulfilling as additional labour, they became an oppressed as well as an exploited class? This exploitation of women as the sources of additional labour was crucial for the transition from primitive communism to slavery. If there ever was a lineage mode of production, the only period during which it could have been determinant was during that transitory stage from primitive communalism to slavery. Once the subjugation of women had been accomplished, it could be reproduced *without necessarily* resorting to exploitative relations. Parts of this process will be examined when looking at the relationships between pygmies and villagers, or in technical terms, between hunters and gatherers and sedentary agriculturalists. With the development of more efficient means of acquiring extra labour (various forms of slavery and feudalism), the position of women will no longer be the economically determining relations for the reproduction of social and material conditions of existence. This statement should be qualified by pointing out that the relationship between various pre-capitalist modes of production is of a different nature to that which exists between a pre-capitalist mode of production and a capitalist mode of production. In the former case the subjugation of women, once instituted was more crucial to the reproduction of subsequent pre-capitalist modes of production than in the latter case.

Pre-Colonial History and State Formation

As in the previous sections, it has proved difficult to deal with the process of pre-colonial state formation without first identifying the most common errors that are found in the literature on the subject. Along with the anthropological problematic already mentioned in the general introduction, there are a number of theoretical and ideological presuppositions which characterize the manner in which historians have written on state formation.[30]

It is unfair to single out one author or a single work of one author to illustrate a case, but when the work itself is based on a review of previous work with the explicit aim of contributing novel interpretations, then such a work

should be the focus for serious critical assessment. J.C. Miller's ambitious and extremely well researched *Kings and Kinsmen*[31] falls in this category. It will be the point of departure for our analysis of the process of state formation in Central Africa.

By choosing the above title, Miller seems to suggest that the history of the Mbundu pre-colonial states constantly vacillated between kingship and kinship. The two determining forces of the pre-colonial Mbundu state are identified as kingship and kinship. I have already explained in the general introduction why kinship cannot be seen as permanently determining social and production relations and not much more can be added here.

Miller's theoretical and methodological point of departure is M. Fortes' and E.E. Evans-Pritchard's introduction to *African Political Systems*. Although he disagrees with their simplistic distinction between stateless and state societies, his own framework is very much derived from that simplistic dichotomy:

> *As a historical study of non-literate Africans, however, it (Kings and Kinsmen) necessarily borrows eclectically, and hopefully wisely as well, from the ethnographer's conceptual tool-kit in order to explain the thought and behaviour of Mbundu state-builders.*[32]

The admission shows not only the way in which anthropological conceptions have determined how historical questions are posed, but also how concepts like "state-building" are introduced without establishing their theoretical status and/or their historical applicability. The situation is even worse, for the process of state formation is seen as the result of enterprising subjects called "state-builders".[33]

Miller does admit there is a distinction between the concerns of anthropologists (and political scientists presumably) and historians! While the former tend to analyze structures and systems in a static framework, the latter point out that structures and systems change; it would therefore be historically wrong to divide pre-colonial social formations between state and stateless societies as if there were no relationship between the two, as if one social formation could not historically change from one to the other and vice-versa. But why should the history of pre-colonial formations be perceived or apprehended through relationships determined by kinship loyalties and those determined by kingship demands? Should one assume that this is the only possible way in which the history

of pre-colonial Mbundu social formations can be perceived? One suspects that Miller would argue that it was the evidence given and not Fortes and Evans-Pritchard who guided his analysis. But, in part the problem with Fortes and Evans-Pritchard is that they also derived their analysis from the empirical evidence. This then raises the question of the kind of empirical evidence used to reconstruct pre-colonial history. To what extent is the empiricial evidence given not itself already the result of ideal and/or ideological transformations? By itself the empirical evidence cannot provide the answer. For example, if a worker in a capitalist firm says that he/she is not exploited, that does not mean the particular firm is not capitalist and does not extract surplus-value from its workers. Exploitation, i.e. extraction of surplus-value, is a concrete and objective reality of the capitalist mode of production whether or not the subjects living this reality acknowledge it or not.

Likewise with pre-capitalist relations of production, and how to proceed with the analysis of the data. Social and political institutions are not *sui generis*. They are the product of social relations, and they cannot be given the status of a material reality without pointing out that they are the product of social and historical transformations. Herein lies the problem. Miller did not seem to know how to relate empirical evidence to a theoretical framework. This uncertainty comes out very clearly in the following passage:

> *The basic question, for comparative purposes, might be phrased: how have the institutions resembling the conventional notion of a 'state' been formed in the context of strongly autonomous descent groups in the case of the Mbundu of Angola? This formulation is intended to postpone the need for a precise a priori definition of 'state' since the entire study represents in one sense a search for an empirical identification of Mbundu 'political structures' based on their historical experience.*[34]

The relationship between empirical evidence is not clarified in part because Miller himself suggests that the em ical evidence does not always support theoretical definitions But it is obviously clear that Miller implicitly accepts the notion that state-like institutions or structures are distinc ly and pre-eminently the domain of political relations. Thus it is more or less assumed that the greater the number of peo living together the more likely they are to develop social relations which do not derive from relations between kinsmen. On other occasions it will be a question of unrelated people coming together which produces the necessity to develop new

systems of relationships:

> *The assumption which lies beneath all the explanations I have offered is that people tended to create institutions in response to felt needs, specifically that the desirability of contact between unrelated members of different lineages forced the Mbundu to find ways of structuring relationships between non-kinsmen. These relations, by definition (emphasis added), were political, and the variety of state institutions already noted emerged in response to this need.*[35]

Why these new relations would be political by definition is not really explained. Presumably, their political nature derives from their organizing relations which could not be taken care of through kinship relations. Should one then assume that any social relations which are not explainable through the kinship system belongs to the realm of politics? This can be rejected on empirical grounds alone: cases of clan fusion have been documented for many areas in Central Africa. Unrelated people can easily relate to each other on the basis of existing kinship systems. Of course, if one attributes a determining role to kinship relations (or to individuals), then it becomes understandable why the fact of relatedness or unrelatedness could be perceived as a necessary source of antagonism:

> *As these unrelated individuals congregated about the salt pans or iron diggings, the etiquette provided by purely kinship links would not have sufficed to regulate their contacts with one another which, we may assume, would not always have been friendly.*[36]

In his concluding chapter, the author eventually opts for empiricism:

> *Mbundu history shows that, whatever the structural tendencies prompting people to create political structures, non-structural (i.e. non political?- J.D.) historical circumstances determined which of the myriad cross-cutting institutions grew to sufficient size and lasted long enough to be termed 'states' (emphasis added).*[37]

The author then classified the 'sorts of historical circumstances' which accounted for the rise of the various Mbundu states. Ironically, in listing the nine various ways which could lead to the rise of states, Miller falls back into

an ahistorical treatment of historical processes. This can be seen by the way in which he relates agricultural production to state formation:

> *no sedentary state could appear without agricultural techniques (emphasis added) capable of producing a surplus to support the agriculturally non-productive specialists in magic, war, and arbitration who ran the state machinery.*[38]

But then he goes on to say that this is not always the case:

> *The history of the Imbangala showed only that states may exist without agricultural base whatsoever if they move continually, seeking new areas to plunder as they devastate the regions where they have passed.*[39]

This reasoning does not disprove the agricultural surplus agrument. As Marx once pointed out, even in cases of plunder, there has to be something from which to plunder. What the above argument says is that the surplus did not originate from the Imbangala themselves. A similar process occurred among the Lozi, where the kingdom was reproduced on plundering outlying areas.[40]

The same can be said for slavery which is another factor advanced by Miller to explain the rise of states. Slaves in pre-colonial Angola tended to come from two sources: throug settling debts between clans or lineages and through raiding to overcome shortages of labour. In the latter case, the practice developed of raiding neighboring and weaker social groups. This system of acquiring slaves was further intensified with the onset of Atlantic slavery, so much so that it could be argued that since slaves did not come from within the social formation, slavery did not constitute one of the bases upon which states were built.

Miller's fundamental error in listing all the factors lies in this ahistorical treatment of the evidence in the sense that the factors themselves are not seen as the results of previous historical processes. It was precisely because of this particular treatment of empirical data that many historians of the trade and politics school were misled into treating trade as the basis of centralized kingdoms. Indeed, Miller does identify such a things as a 'trading state'.[41]

In a way, this error is not surprising for it is one which runs throughout Miller's book. It is related to the one mentioned earlier regarding the strict dichotomy between the

political and non-political spheres. From a historical point of view it ought to be clear that such a sharp distinction cannot exist. While it is possible to identify such distinct spheres, historically one cannot exist without the other. Even when so-called political relations are dominant, they are dominant on the basis of non-political relations. Likewise with the various ways in which states *appear* to have come about. A 'trading state' - assuming there ever was such a thing - could not have come about as a result of trade alone.

History cannot be reduced to processes which take place when certain characteristics are satisfied, and yet this is how Miller tries to explain the process of state formation. Take for example his first illustrative example which is control over a scarce but valuable resource. No doubt one could document a number of states and a number of kings who controlled access to valuable resources. This is not contended. What is contended is the direct relationship between 'scarce and valuable resource and state formation'. After all, what a scarce and valuable resource is must be historically, that is to say socially, determined. Miller gave the example of salt pans. From our distance it may seem obvious why salt was valuable, but this may not have been necessarily so at all times.

The value of commodities is determined by social relations which are themselves the result of historical developments. The value (exchange value) of slaves in pre-colonial formations was different in those times preceding the onslaught of Atlantic slavery and in the years when it reigned supreme. Likewise with other commodities whose value changed from use-values to exchange-values: e.g. rubber and ivory. Therefore control of access to these commodities cannot alone account for the rise of states. At best they may help explain processes of reproduction or consolidation of states, but they cannot be treated as discrete causes in the way in which Miller perceives them:

> *The economic value of these natural resources stimulated just the sort of social circumstances in which political institutions might appear and led to the eventual emergence of states. A rare but necessary source such as iron or salt presumably attracted unrelated persons from a large area in search of the desired commodity. As these unrelated individuals congregated about the salt pans or iron diggings, the etiquette provided by purely kinship links would not have sufficed to regulate their contacts with one another which, we may assume, would not always have been friendly.*[42]

Salt pans and iron diggings, and not the relations developed around their production, are treated as universally valuable and hence capable, in and by themselves, of 'attracting unrelated persons from a large area in search of the desired commodity'.

Anti-hamitic

In his introduction, Miller also sought to bury once and for all the hamitic theory. Quite correctly, he links the hamitic theories of explanation of state formations with the diffusionist school which, in its racist overtones, could not conceive of barbaric or non-civilized people being able to produce sophisticated artistic, cultural or social and political achievements. Correctly too, Miller points out that while the most objectionable aspects of the hamitic theory have been discarded, the central premise - that outsiders brought "statecraft" to Sub-Saharan Africa - was retained.[43] He goes on to document how historians in the early 1970s still continue to explain the process of state formation by attributing this performance to 'invading forces'.[44]

However, Miller himself does not seem to be able to shake off completely from a diffusionist problematic. In his concluding chapter he seems to hesitate between complete rejection of the theory and partial acceptance. Thus on the one hand one reads:

> *Neither simple migration nor diffusion hypotheses made much sense of state-formation in the case of the Mbundu.*[45]

and on the other hand:

> *Diffusion hypotheses, while closer to historical fact in some ways, must be applied very carefully, since the experience of the Mbundu shows that the simple availability of an idea diffused from the outside did not guarantee its implementation or long term success.*[46]

Thus as was the case with his attempt to reject the anthropological problematic and eventually operating within it, Miller's rejection of the diffusionist school is not a rejection, but a refinement:

> *In actual fact, the appropriation by local ambitious and clever men of someone else's good idea seems a far more likely explanation of most early Mbundu states. It was thus the*

> *idea or the institution which travelled in most cases, while the basic population of the Mbundu region has remained relatively stable for a long time. (emphasis added)*[47]

The idealism of the diffusionist theorists is reproduced, but this time the idealist nature of the theory is compounded by attributing idealist motives to 'local ambitious clever men'. Thus before the state ever existed such 'local ambitious and clever men' fabricated state institutions in their heads:

> *Thus diffusion did not explain state-formation but merely provided the opportunity for local innovators to change an outside idea into a form which they could use to create new states.*[48]

The notion that state formation could be brought about by a single individual is not only idealist, but is also derived from the arsenal of ideological conceptions of the capitalist state, an issue which will be discussed in a moment. More seriously, it violates and contradicts Miller's position that the existence of individuals in lineage societies is completely dominated and determined by allegiance to his/her kin.

Miller's adherence to diffusionist explanations is somewhat puzzling in view of his awareness of how diffusionist interpretations intrude into oral data. It is rare to find social formations which do not have some sort of standard 'genesis' type of myth which traces their arrival from somewhere outside the place they are currently living. Unfortunately, Miller's analysis of this kind of data reinforces the conception of the state as an institution which is totally distinct from all others. Indeed the supreme authority who embodies this institution - the king - is seen as an outsider. While avoiding one ideological trap of the social formation under examination, Miller fell into another one, but this time resulting from his own ideological understanding of the capitalist state.

Ideology

More often than not historians realize that when they are dealing with history and historical sources they are dealing in part with ideological discourses. In his book on *Oral Tradition*, J. Vansina explains in detail how ideologies of ruling clans, ruling lineages will shape the transmissions of oral testimonies. Amazingly, Vansina has very little time for analyzing the ways in which the historian's ideology will shape how he chooses to interpret the data he has collected. Like

many other historians he points out the technical, cultural and psychological difficulties to be overcome by Western historians, but beyond that there is no analysis or even a warning of how the ideology of the researcher will transform the data he/she has collected.

The way in which Miller conceptualizes the state or one of its manifestations in the form of kingship is clearly tainted with the way in which ideologues of the capitalist social formations describe the state. For these, the state is seen as an institution above society as an institution which treats equally all members of the society. Kings are outsiders. They are said to be outsiders because of the process through which they have to go prior to assuming their position. The rituals in question stressed the fact that the king was no longer part of the lineages:

> *Mbundu kings were themselves outsiders, removed from their descent groups and kinsmen through initiation ceremonies which placed them in a non-lineage limbo where they acted as theoretically neutral arbiters in disputes which divided competing groups of kinsmen.*[49]

And further down:

> *Political history among the Mbundu was to a large extent the history of 'outsiders', the kings, in their attempts to extend their authority over the relations between strangers.*[50]

And then the evidence on which this conception of the state is based:

> *And the Mbundu traditions' unanimous attribution of state-founding to such 'outsider' hunters and conquerors as Ngola Inene and Cibinda Ilunga provides metaphorical confirmation that the Mbundu themselves saw things in this way.*[51]

And yet, in another essay, Miller has shown very clearly that the apparent distinction between the world of lineages and that of outsiders was not as sharp as he was attempting to make it here.[52] In that essay, Miller showed how the institution of *Kinguri* (king among the Imbangala) came about, how the *Kinguri* was the creation of the most important clans. The ritual was an important step toward establishing the legitimacy of the king over 'outsider' clans. With the increased destruction brought about by Atlantic slavery, kinship relations

social relations and relations of production were increasingly commoditized. Whereas originally the strength of the *Kinguri* depended on the strength of the most important clans that had elected him, he began to develop his own independent social and economic base by building a strong army which could then go and raid for more and more slaves to strengthen his army as well as swell the ranks of those who were responsible for feeding the court. The Kinguri saw his power increase even more with the Portuguese introduction of monopolistic practices.

As long as this system was maintained, as long as monopolistic tendencies existed on either side, it was difficult for lineage or clan heads to restrain the power of the *Kinguri*. They had created an institution over which they thought they were going to maintain control, but the increased commoditization of relations of production led to a more and more independent *Kinguri*. Soon, however, the lineages realized that they did not have to comply with the monopoly imposed on them by the Portuguese (with the help of the *Kinguri*). Adventurers on the one hand and lineage and clan heads on the other realized that they could acquire some of the commodities that the *Kinguri* was able to amass if they simply refused to send the slaves they were supposed to send annually to the *Kinguri's* court.

Conclusion

There is one problem which seems to resurface constantly, namely how to account for the transitory stages in pre-colonial African formations. For example, in order to understand the transition from hunting and gathering social formations to slave formations, one must break away from the ethnographic practices of focusing on the histories of tribes. The various slave and feudal formations which emerged during the pre-colonial period cannot be understood unless they are studied in conjunction with social formations which reproduced themselves on different economic bases.

It will also be necessary to move away from the study of so-called important tribes. In order to understand the expansion of the Luba and Lunda empire for example, it will be necessary to understand more precisely how the smaller groups reproduced themselves. From a technical point of view, the understanding of sedentary agriculturalists will not be possible unless one understands them as dialectically and historically springing from hunting and gathering social formations. Fascination with the victorious empires of the pre-colonial period has actually prevented historians from understanding the role played by less prominent, less visible, but nevertheless

crucial social forces, groups or classes in the emergence of those empires.

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Author's postscript: *This paper was presented to the History Department Seminar at the University of Dar es Salaam and I must thank the participants for their comments and criticisms. I would concur with them that the criticism against the use of natural economy may have been overstated. As long as it is used as a residual category it should be acceptable. On the woman question I would agree, partially, that it is incorrect to draw a parallel between the position of women and that of blacks.*

Footnotes:

1. Department of History Seminar Paper, January 22, 1976.

2. See the various attempts by T.O. Ranger, ed., *Aspects of Central African History* (London: Heinemann, 1968); H.W. Langworthy, *Zambia Before 1890* (London: Longman, 1972); B. Pachai, ed., *The Early History of Malawi* (London: Longman, 1972); J. Vansina, *Kingdoms of the Savanna* (Madison: University of Wisconsin Press, 1968); A.D. Roberts, *A History of the Bemba* (London: Longman, 1973); M. Mainga, *Bulezi Under the Luyana Kings* (London: Longman, 1973); W.G.L. Randles, *L'Ancien royaume du Congo* (Paris: Mouton, 1968); G. Balandier, *Daily Life in the Kingdom of the Kongo* (New York: Pantheon Books, 1968); L.H. Gann, *A History of Northern Rhodesia: Early Days to 1953* (London: Chatto and Windus, 1964); L.H. Gann, *A History of Southern Rhodesia: Early Days to 1934* (London: Chatto and Windus, 1965); B. Davidson, *A History of East and Central Africa* (Garden City: Anchor Books, 1969).

3. See for example the works of B. Davidson, and to a certain extent Walter Rodney's *How Europe Underdeveloped Africa* (London, Dar es Salaam: Bogle-L'Ouverture and Tanzania Publishing House, 1972).

4. Bradby, B. "The Destruction of Natural Economy", *Economy and Society* 4, 2, pp. 127-161.

5. Note the discussions on sources and methodology in H.W. Langworthy, *Zambia Before 1890* and in A.D. Roberts, *A History of the Bemba*.

6. For a critique of this procedure see J. Depelchin's review

of A. Merriam's *An African World* in *International Journal of African Historical Studies* IX, 4 (1976).

7. M. Legassick has drawn attention to and criticized the uncritical use of material collected by early missionaries. My thanks to D. O'Meara for drawing my attention to his "The Sotho-Tswana Peoples Before 1800", in Leonard Thompson, ed., *African Societies in Southern Africa* (London: Heinemann, 1969).

8. The literature on this has grown to unmanageable proportions. For references see Merriam's review cited in footnote 6. Note also the recent excellent essay by B. Magubane, "The Poverty of Liberal Analysis: A Polemic on Southern Africa", *Review* I, 2 (1977), pp. 147-166.

9. Needham, R. "Remarks on the Analysis of Kinship and Marriage", in R. Needham, ed., *Rethinking Kinship and Marriage* (London, New York: Tavistock Publications, 1971), p. 5.

10. Leach, E.R. "More About 'Mama' and 'Papa'", in R. Needham, ed., *Rethinking Kinship and Marriage*, pp. 75-76.

11. *Ibid.*, p. 77.

12. A good number of works by French anthropologists has been translated. See the recent *Relations of Production* edited by Seddon. See also F. Pouillon, ed., *L'Anthropologie Economique* (Paris: Maspero, 1976).

13. Levi-Strauss, Claude. *Tristes Tropiques*. New York: Atheneum, 1970, p. 61.

14. *Ibid.*, p. 62.

15. On mathematics and anthropology, see Ph. Richard and R. Jaulin, eds., *Anthropologie et Calcul* (Paris: Union Generale d'Editions, 1971). On Marxism and structuralism, there is a very large body of literature, but most of the critical literature in French has not been translated. See V. Leduc, ed., *Structuralisme et Marxisme* (Paris: Union Generale d'Editions, 1970). Levi-Strauss himself later made heavy use of mathematical and computer formulas in the series on myths: *Mythologiques: Le Cru et le Cuit* (Paris: Plon, 1964); *Mythologiques: Du Miel aux Cendres* (Paris: Plon, 1966); *Mythologiques: L'origine des manières de table* (Paris: Plon, 1968).

16. The critical literature on Althusser has become voluminous. Aside from the recent work of J. Ranciere (an early

collaborator) one should note the Trotskyist collective *Contre Althusser* (Paris: Union Generale d'Editions, 1974) and also H. Lefebvre's cutting "Les Paradoxes d'Althusser ou le Nouvel Éléatisme", *L'Homme et la Societe*, 1969, 13 which focuses specifically on Althusser's structuralism.

17. See his recent *Ce Qui ne Peut plus Durer dans le Parti Communiste* (Paris: Maspero, 1978).

18. Hindess, B. and P.Q. Hirst. *Pre-Capitalist Modes of Production* (London, Boston: Routledge & Kegan Paul, 1975), pp. 73-74.

19. Godelier, M., ed. *Sur les Societies Pre-Capitalistes*. Preface, p. 139.

20. Terray, E. *Le Marxisme Devant les Societes 'Primitives'* (Paris: Maspero, 1969),p. 139.

21. This section relies heavily on Alain Marie's "Rapports de Parente et Rapports de Production dans les Sociétés Lignagères", in F. Pouillon, ed., *L'Anthropologie Economique*, especially pp. 86-101.

22. *Notes and Queries on Anthropology*, Sixth Edition revised and rewritten by a committee of the Royal Anthropological Institution of Great Britain and Ireland (London: Routledge & Kegan Paul, 1967), particularly Chapter I.

23. One of the few exceptions being Claude Meillassoux, ed., *L'Esclavage en Afrique Pre-coloniale* (Paris: Maspero, 1975) and the non-Marxist but excellent essay by J.C. Miller, "Slaves, Slavers and Social Change in Nineteenth Century Kasanje", in F.W. Heimer, ed., *Social Change in Angola* (Munich: Weltforum Verlag, 1973), pp. 9-29.

24. Exemplified by Philip Curtin's *Economic Change in Pre-Colonial Africa* (Madison: University of Wisconsin Press, 1975) where slavery is reduced to a question of trading commodities. See review article by J. Depelchin, "Neology and Quantification in African History", forthcoming in *African Review*.

25. Rey, P. Ph. *Colonialisme, Neo-Colonialisme et Transition au Capitalisme* (Paris: Maspero, 1971), p. 207.

26. On these correlations see J. Vansina, *Introduction à l'Ethnographie du Congo* (Editions Universitaires du Congo, 1966).

27. Bloch, M. "Property and the End of Affinity", in M. Bloch,

ed., *Marxist Analyses in Social Anthropology* (Malaby Press, 1975).

28. Note the different positions on Morgan's evolutionary schemas by M. Godelier, *Horizon, Trajets Marxistes en Anthropologie* (Paris: Maspero, 1973), pp. 174-182; E. Terray, *Le Marxisme Devant les Societes 'Primitives'* and R. Makarius' introduction to the French translation of *Ancien Society: La Société Archaique* (Editions Anthropos, 1971).

29. This position is in opposition to Godelier's and closer to R. Makarius. See note above.

30. The works on which this is based are the same as those listed in footnote 2.

31. The criticism made here for Miller would apply *mutatis mutandis* to S. Feierman's *The Shambaa Kingdom* (Madison: University of Wisconsin Press, 1974). Both books are similar in that they aim at establishing new points of departure at the theoretical level.

32. Miller, J.C. *Kings and Kinsmen* (Cambridge: Oxford University Press, 1976), p. 3.

33. This is reminiscent of the state-building jargon developed by political scientists immediately following independence of many African countries.

34. Miller, *Kings*, p. 3.

35. *Ibid.*, p. 271.

36. *Ibid.*, p. 272.

37. *Ibid.*, p. 271.

38. *Ibid.*, p. 275.

39. *Ibid.*

40. Mainga, M. *Bulozi Under the Luyana Kings* (London: Longman, 1973), especially Chapters 2 and 3; and M. Gluckman, *Economy of the Central Barotse Plain*, Rhodes Livingstone Institute, Paper No. 7, 1941.

41. See, among others, S. Lemelle's review of A. Roberts' *A History of the Bemba* (unpublished) and Walter Rodney's review of R. Gray and D. Birmingham, eds., *Pre-colonial African Trade*, O.U.P., 1970 in *Transafrican Journal of*

History 2, 1.

42. Miller, *Kings*, pp. 271-2.

43. *Ibid.*, p. 7.

44. *Ibid.*, p. 9.

45. *Ibid.*, p. 280

46. *Ibid.*, p. 281.

47. *Ibid.*, pp. 280-1.

48. *Ibid.*, p. 281.

49. *Ibid.*, p. 277.

50. *Ibid.*

51. *Ibid.*

52. Miller, "Slaves, Slavers,...", *Social Change in Angola.*

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