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Robert Buijtenhuijs

**The Revolutionary Potential of African Peasantries:
Some Tentative Remarks**



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Introduction

Much has been written on African peasantries in general and on African "peasant wars" in particular, and yet there is a remarkable gap in the existing literature. On the one hand, we find inspiring works on peasant wars generally (Barrington Moore Jr 1966; Eric Wolf 1973; J.C. Scott 1976; S.L. Popkin 1976), but the authors of these theoretical and comparative studies ignore Africa south of the Sahara, with the exception of J.M. Paige (1975) who devotes a chapter to Angola and a few pages to Kenya (in which, as we will see, he completely misinterprets the 1952-56 Mau Mau rebellion). On the other hand, there are brilliant studies of specific African peasant revolts and even a few attempts at generalizing on the continental level (K.W. Grundy 1971; B. Davidson 1981), but none of these works have probed the material on African cases in the light of the more general debates. This anomalous situation, is what the present article seeks to remedy, at least in part. "In part", because this essay is the first result of a research project which is only in a half-way stage; the following remarks and suggestions should therefore be regarded as quite tentative. They will more particularly address the rather tricky question of the revolutionary potential of different categories of peasants, a question that has been of importance in the literature on peasant wars for quite some time. This will be done by, first, presenting two general theories on the revolutionary potential of different categories of peasants, and then discussing these theories in the light of some African examples. Our conclusions, unfortunately, will be rather negative: the general theories under discussion here are only of limited use for understanding African peasant revolt.¹

Two general theories

Basing themselves mainly on examples from European and Asian history, with some additional evidence from Latin America, H. Alavi (1965) and E.R. Wolf (1973) have elaborated a very interesting theory on "Peasant Wars of the Twentieth Century", and more particularly on the revolutionary potential of different sorts of peasants. Most important is their distinction between rich, poor and middle peasants, described by Alavi in the following terms:

"The division of the peasantry into *rich peasants*, *middle peasants* and *poor peasants* suggests an array of the peasantry with the different strata arranged, one over the other, in a single order. This is misleading; the *middle peasants*,

¹I would like to thank Basil Davidson, Piet Leegwater, René Lemarchand, Henk Meilink and Terence Ranger for their helpful comments on the first draft of this article.

for instance, do not stand *between* the *rich peasants* and the *poor peasants*; they belong to a different sector of the rural economy. In the transitional historical situations we shall deal with, a distinction may be made broadly between three sectors of the rural economy. Firstly, we have the sector of which the essential distinguishing feature is that the land is owned by *landlords* who do not themselves undertake its cultivation. Their land is cultivated by landless tenants, mostly *share-croppers*, who are classed as *poor peasants*. The second sector is that of independent *small-holders*, who own the land which they cultivate themselves. They do not exploit the labour of others. They are the *middle peasants* (...). The third sector is that of *capitalist farmers*, who are described as *rich peasants* who own substantial amounts of land. Their distinguishing characteristic is that their farming is based on the exploitation of wage labour (...). Unlike *landlords*, they undertake the business of farming on their own account and employ capital in it. The *farm labourers*, who are paid a contractual wage, are referred to as the *agricultural proletariat*" (Alavi 1965: 244).

Analyzing the revolutionary potential of these different sections of the peasantry, Alavi comes to the following general conclusions:

"(...) the poor peasants are, *initially*, the least militant class of the peasantry (...). There is a fundamental difference between the situation of the poor peasant and that of the industrial worker. The latter enjoys a relative anonymity in his employment and job mobility which gives him much strength in conducting the class struggle (...). In the case of the poor peasant the situation is much more difficult. He find himself and his family totally dependent upon his master for his livelihood. When the pressure of population is great (...) no great machinery of coercion is needed by the landlords to keep him down. Economic competition suffices" (Alavi 1965: 274).

Alavi makes it clear, however, that the "backwardness" of the poor peasant is a relative, not an absolute characteristic. When the poor peasant is shown in practice that the power of his master can be irrevocably broken and when the possibility of an alternative mode of existence becomes real to him, the poor peasant may finally take the road to revolution (Alavi 1965: 275). On the other hand, the middle peasants, although initially the most militant element of the peasantry, are limited in their social perspective by their class position, and when the movement in the countryside advances to a revolutionary stage they may move away from the revolutionary movement (Alavi 1965: 275). Of

course, rich peasants and landlords, given their class position, will not easily commit themselves to revolutionary adventures.

E.R. Wolf adopted Alavi's theses and added some further elements. He first of all introduced the category of the "poor but free peasant", i.e. a peasantry located in a peripheral area outside the domains of landlord control, and claimed that these peasants, like the middle peasants, do have some internal leverage (Wolf 1973: 291). He then went on to say that:

"If we now follow out the hypothesis that it is the middle peasants and poor but 'free' peasants, not constrained by any power domain, which constitute the pivotal groupings for peasant uprisings, then it follows that any factor which serves to increase the latitude granted by that tactical mobility reinforces their revolutionary potential. One of these factors is peripheral location with regard to the center of state control (...). The tactical effectiveness of such areas is strengthened still further if they contain defensible mountainous redoubts" (Wolf 1973: 292-93).

Wolf concludes that:

"(...) ultimately, the decisive factor in making a peasant rebellion possible lies in the relation of the peasantry to the field of power which surrounds it. A rebellion cannot start from a situation of complete impotence; the powerless are easy victims" (Wolf 1973: 290).

Before turning now to a second general theory on the revolutionary potential of different categories of peasants, a warning with regard to Wolf's ideas might be in order, a warning phrased by R. Aya in the following words:

"To be most readily mobilized is not the same as being the 'most revolutionary' (...). If 'middle peasants', once mobilized, be revolutionary, it is only because their parochial rebellions feed into a national cataclysm whose outcome is an institutionalized order the peasants neither intend nor control (...). But taken on their own terms, the aims of 'middle peasants' have been conservative, even reactionary, in the literal sense: to conserve the economic and political basis of their identity, pride and meaning as a community and a class within it - usually in deliberate action against ongoing currents of social change" (Aya 1975: 132).

Quite a different theory on the revolutionary potential of different categories of peasants has been proposed by J.M. Paige. His ideas are not easy to summarize, but he basically formulates a theory of rural class conflict that tries to define:

"(...) recurring patterns of conflict in terms of interaction between the economic and political behaviour of cultivators and that of noncultivators and predicts the circumstances under which these conflicts lead to cultivator social movements in general and agrarian revolution in particular. The fundamental causal variable in this theory is the relationship of both cultivators and noncultivators (i.e. lower and upper classes R.B.) to the factors of agrarian production as indicated by their principal source of income" (Paige 1975:10).

Given the fundamental causal variables chosen by Paige, four different agrarian class systems can be observed in practice, for each of which the author tries to define its relationship to unrest and revolution on the basis of an essentially logical and abstract argument:

1. In the first case, both the upper and the lower agricultural classes draw their income exclusively from land, a situation characteristic of the *commercial hacienda*. On the basis of a logical argument, which the limits imposed on this article do not allow me to summarize, Paige concludes that in such situations "few rebellions of any kind should take place", and that, where revolts (not revolutions!) do occur, they are focused on "the control and distribution of property in land" (Paige 1975: 42).
2. In the second case, the lower classes remain dependent on land, while the upper class is dependent on commercial capital rather than land. This combination of income sources is characteristic of a variety of *smallholding systems* (including middle peasantries R.B.) and leads to weak and dependent lower-class social movements whose "target is likely to be the middlemen who constitute the effective agricultural upper class" (Paige 1975: 47). Conflict may take political forms, but is more likely to express itself in economic warfare over control of the commodity market. According to Paige (1975: 48), the typical movement produced by this combination of income sources for the upper and lower classes should be called a reformist commodity movement. Focused on control of the market in agricultural commodities, it does not involve radical demands for the redistribution of property or the overthrow of the state. It is moderate in its tactics and limited in its goals.
3. In the third case the upper class is dependent on capital, while the cultivators depend on wages as their main source of income, a situation characteristic of *plantation*

agriculture. Such a combination of income sources produces a form of political conflict focused on income from property rather than ownership of property, and strong working-class political organization with radical overtones. As the upper class, however, is economically powerful enough to be able to bargain and make concessions, the most likely outcome of conflict in such situations is a reformist social movement focused on limited economic questions (Paige 1975: 48-49).

4. In the fourth and last case, the upper class is dependent on land as its main source of income, while the cultivators are mainly paid in wages. According to Paige (1975: 58), the typical form of social movement in these systems dependent on landed property and wage labour is revolutionary, and long guerilla wars will likely result. None of the other combinations of income sources has this potential for revolutionary war. This category, however, comprises two distinct forms of agricultural organization, i.e. *sharecropping systems* and *landed estates dependent on migratory wage labour*:

"In sharecropping systems the basis of group solidarity is economic class status, and the corresponding revolutionary movements tend to be based on socialist or Communist ideologies. In landed estate systems dependent on migratory wage labor the work force is only partly dependent on wages for its support. Since it must return to subsistence agriculture for the off season, it remains dependent on the traditional peasant or tribal village. When revolutionary movements do form in such systems, they are therefore likely to combine both wage laborers and traditional communal organizations. The ideology uniting these disparate elements cannot be based on class but can be based on national or racial hatred of a settler class" (Paige 1975: 59).

So much for Paige's logical argument developed in the first chapter of his book. In the second chapter he turns to the facts and considers the empirical relationship between agricultural organization and rural social movements in a population of 135 export sectors of 70 developing nations over the period 1948-1970. The analysis correlates the dominant type of agricultural organization for each export sector with the number of acts of rural protest observed in that sector, and leads Paige to the conclusion that the overall pattern of results supports the general theory of rural social movements outlined in his first chapter (Paige 1975: 120).

As for this second chapter, a few remarks seem appropriate. First of all, Paige only considers agricultural *export sectors*, which means that he excludes not only subsistence agriculture, but also commercial agriculture producing for the internal

market. As we will see later on, this has important consequences in so far as he misses several of the main (and certainly many of the minor) cases of rural protest in developing countries during the 1948-1970 period. This flaw in his argument is compounded by the inclusion in his population of only those export sectors that had a certain importance within the overall economy of the country under study². In doing so, he again misses important cases. Looking at the list of export sectors in Sub-Saharan Africa used by Paige (1975: 378) one is struck, for example, by the absence of cotton in Chad as well as in Mozambique, of groundnuts in Guinea-Bissau, oranges in South Africa, and cloves in Zanzibar (and this list is certainly not exhaustive). In my opinion export sectors are a far too limited field for studying agrarian unrest, and even if this were not so, Paige's way of identifying and using them has several important shortcomings and lacunae.

Secondly, the events of rural protest during the period under consideration have been identified with the sole help of newspaper reports. This means, again, that some cases are missing and, even more important, that Paige's interpretation of rural protest events is based on sources that are not always the most reliable. To give just one telling example: an interpretation of the Mau Mau revolt based on contemporary newspaper reports (even papers so distinguished as *The Times* and *The Guardian*) would certainly not satisfy those African and European historians who are working on this subject today. More generally speaking, revolutions are not always recognized as such by the next day's newspapers and they are therefore insufficient for correlating agrarian class systems with different types of rural unrest.

In spite of these criticisms, Paige's theory is sufficiently substantial and "logical" to deserve serious consideration, and more particularly to be compared with the Alavi/Wolf hypotheses. It is quite evident, in fact, that the two theories contradict each other on some important points. While Alavi and Wolf consider the middle peasants to be the category that, initially, will be the most easily mobilized for revolutionary endeavours, Paige holds that smallholder systems (including middle peasantries) are characterized by reformist social actions, while he attributes much more revolutionary potential to sharecroppers who, in the terminology of Alavi and Wolf, are poor peasants.

At another point, however, the two theories partly overlap. This is more particularly the case with Paige's landed estates dependent on migratory wage labour, an agricultural system that is not included, as such, in Alavi's agrarian sectors. The migrant labourers,

² For details on the criteria used, see Paige 1975: 74.

in fact, belong to two different agrarian sectors and play two different economic roles. As migrant labourers they belong to what Alavi called the agricultural proletariat, but at home they are still peasants, and in most cases smallholders, i.e. middle peasants. The question then is: is Paige right in viewing their situation as a specific and particular case or can they be counted unreservedly as belonging to the middle peasantry (or possibly agricultural proletarians) as the Alavi/Wolf scheme would imply? To this and other questions we will now try to find answers by using African data which have been ignored by Alavi and Wolf and used (but sometimes misused) by Paige³.

Some African cases

Before we can try and see whether African examples of agrarian unrest tend to confirm or to negate the hypotheses summarized above, an interesting preliminary question has to be answered. Should one only use examples of "major" peasant wars, as was done by Wolf⁴, or is it better to take into account a whole range of phenomena, from minor disturbances like one-day demonstrations or strikes to the major wars, as was done by Paige, and also by J.C. Jenkins (1982) when studying Russian peasant uprisings during the period 1905-07? In the present stage of my research, I will have to opt for the first solution which, at first sight, seems to be the most logical way of proceeding. How indeed can one compare the turmoil and upheavals of Chad's protracted civil war with the peaceful demonstration of a few disgruntled peasants in an obscure provincial market-town? Obviously, at least so it seems, these two "events" do not obey the same rules and should not be used for purposes of comparison. And yet, on second thoughts, doubts creep in. Major civil wars, too, usually start as minor disturbances of the public order, and it can be argued that their metamorphosis from one stage to another does not depend only on the revolutionary potential of the peasants involved but also on other factors, amongst which, as is argued by C. Tilly, the reactions of the incumbents take pride of place: "(...) collective violence is a contingent outcome of interaction among contenders and governments, in which the agents of government commonly have the greater discretion and do most of the injury and damage" (Tilly, quoted in Berman 1976: 146)⁵. Wolf's option of studying only major peasant wars is

³ A last *caveat*, before turning to the African examples, has to be introduced: in real life different categories of peasants cannot be so neatly distinguished as sociological theory tends to suggest. Often there is some overlapping, as for example when independent middle peasants occasionally work as wage labourers for other agriculturalists. This makes the interpretation of events of rural unrest all the more difficult.

⁴ Wolf, in his major work (1973), uses the cases of Mexico, Russia, China, Cuba, Vietnam and Algeria.

⁵ This factor, although it certainly does not altogether invalidate all theories on the revolutionary potential of different categories of peasants, does at least indicate one of their limits: a tough government (or landlord) may provoke even the least militant peasants into action, while a more liberal and tolerant system might be able to live in peace with a potentially very "revolutionary" peasantry; or,

therefore not the only possible approach, and I am aware that, by following his example, i.e. by using the six cases of recent African peasant wars I am more or less familiar with, my argument is not entirely watertight.

Coming now to my examples, we will first pay attention to Guinea-Bissau where, from 1963 till 1974, Amilcar Cabral's PAIGC fought an all-out liberation war against Portuguese domination. This case is definitely in favour of Alavi and Wolf and runs counter to Paige's argument. All sources agree that it was in the Balante areas that the PAIGC won the most rapid and masive support during the war and, although in-depth studies on Balante agriculture are lacking, there is no doubt that the Balante are independent smallholders, in Paige's terms, or middle peasants in Wolf's terms. Moreover, and here Paige gets even more entangled in his inconsistent way of collecting data, the Balante rice growers in the areas that were the first to respond to the call of the PAIGC were commercialising part of their crops on the internal market, but not for export, so that Paige omits this case in his general inventory of events of agrarian unrest. This omission is all the more serious as commercial agriculture seems to have played an important role in the recruitment patterns of the PAIGC. Using unpublished research material collected by J. Cunningham, P. Chabal concludes that, unlike the majority of cultivators in Guinea, the Balante rice growers in the regions that were quickest to follow the call of Cabral and his friends, "were forced to trade through 'concessionaires' and not through the official commercial centres. The concessionary *pontas* (...) amounted to a monopoly control of trade in the area which was far more unfavourable to the local rice growers than trading through official channels would have been" (Chabal 1983: 69). Instead of a reformist commodity movement, as predicted by Paige's theory, the Portuguese government, however, found itself face to face with a full-scale nationalist revolution.

No need, therefore, to dwell much longer on this case, except to underline one important point, i.e. that even middle peasants are by no means always spontaneously revolutionary agents, as Amilcar Cabral himself admitted: "(...) nous savons (...) d'expérience, combien il nous a coûté de l'inciter à la lutte (...). En Guinée, à part certaines zones et certains groupes qui nous ont fait, dès le début, un accueil favorable, nous avons dû (...) conquérir leur appui à la suite d'efforts tenaces" (Cabral 1975: 143). This opinion was shared by Antonio Bana, one of the early PAIGC propagandists, who claimed that the mobilization of the peasants, even the Balante, was "far more difficult than the war itself" (Quoted in Davidson 1969: 55). Cabral and his

according to another possible argument, a tough government might scare into inaction and submission the most militant peasants, while a liberal and, particularly, more uncertain set of incumbents might give ideas to any "underdog", in the absence of any immediate threat of retaliation.

associates may have exaggerated their initial difficulties a little (which all the more lend credit to their final success), but there is no reason to doubt the essence of their testimony: peasants are basically suspicious people, even middle peasants.

As for the case of Mozambique, where Frelimo's struggle for independence lasted from September 1964 till April 1974, much the same picture obtains as the one we sketched for Guinea-Bissau. Here too, one ethnic group, the Makonde of the northern Cabo Delgado District, took the lead in the war and remained Frelimo's main recruitment reservoir throughout the struggle. In his case, too, we are dealing with middle peasants: 80% of Cabo Delgado's active population were non-salaried subsistence cultivators (Munslow 1983: 95), and their activities in wartime and revolution are a genuine argument in favour of the Alavi-Wolf hypotheses. Moreover, the data suggest that the Cabo Delgado peasants, like their colleagues in neighbouring Niassa District, had more "tactical power" than the peasants elsewhere in Mozambique:

"Free social space' within which peasants and rural workers could plan collective action undetected, existed or could be created more easily in both districts. Because they were considered marginal backwater areas, the state apparatus was appreciably weaker than in the more effectively policed southern districts" (Isaacman and Isaacman 1983: 67).

An interesting point to note here is that the leaders of the *Mozambique African Voluntary Cotton Society*, who were among the first Makonde cultivators to join Frelimo, although still basically middle peasants, were themselves relatively well-to-do people, according to the evidence given by one of them (Cf. Mondlane 1969: 134-135), and may have been on their way to becoming real commercial farmers ("rich peasants") when the war caught up with them. But this certainly does not contradict Wolf's thesis. The same holds for the fact that, during the 1950s and the early 1960s, many Makonde worked as migrant labourers in Tanzania and as far north as Mombasa in Kenya, forming an important basis for support to Frelimo during the struggle (Egerö 1987: 146). The special relations between middle peasants staying on the land and their sons and daughters they send to work in town are an integral element in Wolf's theoretical framework (1973: 292). The same fact, however, cannot be used as an argument in favour of Paige, mainly because Frelimo's war broke out in the wrong place. According to Paige's theoretical framework, such a nationalist revolution, had it occurred, would have had as its theatre the Tanzanian sisal plantations, because these, as an export sector, would form the relevant agrarian system, not the Makonde countryside.

A last point to be mentioned is that, like Guinea's Balante, the Makonde did not spontaneously take up arms. Frelimo, first of all, sent some of its militants to Algeria for military training, and it were these soldiers, and other propagandists, who mobilized the local population and who initiated the war, at a date fixed by Frelimo (Mondlane 1969: 128-139). As in Guinea, these early propagandists experienced "peasant scepticism", and the mobilization phase took nearly two years (Isaacman and Isaacman 1984: 141).

Another case that seems perfectly in accordance with Wolf's ideas takes us to independent Chad which, from the end of 1965 till the beginning of the 1980s, has been beset by a series of loosely connected peasant revolts that gradually evolved into a political-military organization (Frolinat) which, in its turn, tore apart the whole country, brought down the incumbent (military!) government and finally took power in N'Djamena⁶. Again, the Chadian peasants who, in this case spontaneously, rose in rebellion before the establishment of an outside based political organization, are definitely middle peasants. Although many of them live in areas formerly, and still formally, controlled by Moslim Sultans and Alifas, there is no question in North and Central Chad of landlords, even less of "feudalism", as was the case in Ethiopia or in some parts of Northern Nigeria.

Although not calling for any special comments as far as *peasants* are concerned, the Chadian example is interesting in that it introduces a category neglected in most comparative studies of peasant wars, i.e. the *nomadic pastoralists*. What is their position with regard to rebellion and revolt? One can first of all note that, providing one reads cattle for land, most pastoralists do answer Alavi's definition of middle peasants, in that they own their means of production and exploit them themselves, with the help of members of their family but without using paid labour (Cf. Saul and Woods 1979: 105). At least this is the case of the Chadian Toubou who, although rather slow starters in the Frolinat rebellion (their first actions only date from the beginning of 1968), have been enthusiastic guerrilla fighters ever since and are largely responsible for the rebellion's final victory. This is certainly not by accident. As I have demonstrated elsewhere (Buijtenhuijs 1987: 87-90), the Toubou, by virtue of their traditional way of life, are born guerrilleros or, as Wolf would say, they possess an enormous amount of tactical power and freedom. First of all, the vast desertic reaches of Chad's northern BET *préfecture* are particularly well adapted to guerrilla warfare and, as was already

⁶ Hissein Habré, Chad's current President, started his career as the leader of one of the main branches of Frolinat. He was the first guerrilla commander to overthrow an independent African government, to be followed a few years later by Uganda's Museveni.

noted by J. Chapelle, when analyzing the precolonial wars of the Toubou, they themselves, in turn, are particularly well adapted to their natural environment:

"Leur résistance à la fatigue et à la soif est, en effet, extraordinaire et supérieure à celle de tous les autres nomades (...). Les raids qu'ils accomplissent, avec des provisions insignifiantes d'eau et de dattes, dépassent certainement les exploits analogues des autres Sahariens, et ne sont limités que par la résistance de leurs montures. A pied, ils sont imbattables" (Chapelle 1957: 16-17).

This is not only a question of physical qualities and endurance. Their warlike spirit and their traditional way of life, too, mark the Toubou as born guerrilleros. "La société précoloniale toubou", writes C. Baroin, "(...) était une société guerrière (...). L'état de *feud* y était quasi permanent" (Baroin 1985: 74). The same author also notes that in traditional Toubou culture "l'agression d'autrui est normale (car ni le vol de bétail, ni le meurtre ne sont pour les Toubou des actes en eux-mêmes répréhensibles)" (Baroin 1985: 91). It is therefore not surprising that, in Toubou society, "à ses yeux, aux yeux de sa femme, de ses enfants et des gens qui l'entourent, l'homme est 'homme' par le port des armes et par son adresse à les manier" (Chapelle 1957: 329). Chapelle, who emphasizes the bonds of affection that unite a Toubou man with his wife and children, also describes how a married man often leaves his "tent", in order to take care of his herds or to engage in trade or warfare, while his wife, in his absence, quite naturally takes charge of the running of their camp. Thus:

"Malgré les obligations qu'il a envers sa famille et le sentiment de ses responsabilités, l'homme se trouve entièrement libre. S'il quitte sa tente, c'est pour faire son métier d'homme, et une fois en route il suivra son inspiration ou sa fantaisie. Ses voyages se prolongeront pendant des mois, sans que sa femme s'inquiète de savoir où il est ni ce qu'il fait" (Chapelle 1957: 290-291).

We can therefore conclude that the traditional way of life of a Toubou man is, in important respects, quite compatible with that of a guerrillero, much more so than the traditional way of life of a sedentary peasant (rich, middle or poor). A Toubou man enjoys an uncommon amount of "tactical power", and the question that arises is whether this situation is particular to the Toubou or whether other nomadic pastoralists find themselves basically in the same situation? The political upheavals that currently plague the countries of the Horn of Africa, where pastoralists make up an important part of the population, seem to indicate that we have here a very interesting field for further

study, although one has to be very careful in generalizing: the Bororo nomads of Niger, but who are also represented in Chad, never joined any guerrilla band.

Another interesting case with which I am somewhat familiar is the Malagasy insurrection of 1947. Of all the examples analyzed here this case is the least well documented, in spite of the excellent work done by J. Tronchon and R.B. Ramanantsoa, and it is therefore not easy to come to any definite conclusions with regard to the recruitment patterns of this rebellion. A few things, however, seem well established. First of all, although the insurrection was planned, insofar as there was any planning at all, to involve the whole of Madagascar, not more than one sixth of the island's total surface (i.e., the central areas of the East coast) was finally affected by the revolt. As far as the existing literature permits any definite conclusions, these areas were inhabited by middle peasants, which would seem to be an argument in favour of the Alavi-Wolf hypotheses.

However, most other Malagasy cultivators seem to be middle peasants too, and it is therefore of some interest to discover why only the East Coasters engaged in sustained rebellion? A first point to emphasize is that the dense mountainous forests of the East are particularly favourable to guerrilla warfare, and that the secret societies that initiated the armed revolt were well aware of this and may even have taken the conscious decision to limit the insurrection to these areas. At least this is suggested by Tronchon (1974: 108) who seems to forget, however, that, according to most of the sources, the insurrection was definitely meant to erupt elsewhere, and more particularly in Tananarive, the capital town, where it was only cancelled at the last minute. Tronchon may therefore be mistaken on this point, but the restriction of the revolt to the East coast, anyway, strengthens Wolf's remarks on the importance of defensible mountainous redoubts (Wolf 1973: 293).

Another feature is mentioned, however, by several authors. The East coast is, in fact, also the area "where almost all the export crops were produced and where the Malagasys had suffered most from spoliation of land and requisitioning of labour" (Thompson and Adloff 1965: 55). Intensive colonisation by people originating mainly from the neighbouring island of Réunion has indeed characterized the economy of the areas involved in the rebellion, and these "petty colonists" have often benefitted from their privileged political status in order to obtain more or less forced and certainly cheap labour with the help of the colonial administration (Althabe 1969: 57-60). This point is significant, because at least one of the secret societies that was at the basis of the revolt had, according to the evidence given by one of its main leaders, as its goal: "de former

idéologiquement les jeunes, surtout les jeunes paysans des *concessions coloniales*, les organiser et les entraîner à la lutte politique" (Quoted in Ramanantsoa 1986: 61; my italics), while Tronchon, in his chapter "The price of war" claims that:

"Le secteur le plus éprouvé est celui des exploitations agricoles, ce qui se comprend quand on sait que la plupart des plantations européennes de la plaine orientale (...) ont été systématiquement soumises au pillage par les forces malagasy. Rares sont les colons qui ont pu garder presque intacte leur propriété" (Tronchon 1974: 69).

Although the data on the East coast estates are too scarce to allow any definite conclusions, we have here very probably a case of a landed estates system dependent on migrant labour that has produced exactly the kind of nationalist "revolution" predicted by the theory of Paige, although, ironically, Paige's crude ways of measuring and of defining his initial population of export sectors made him miss this very interesting case.

However, Paige did not miss the case of Angola which he uses as one of the main pieces of evidence in favour of his theory, and more particularly in order to demonstrate that a landed estate system dependent on migratory labour is likely to lead to nationalist revolution. Undoubtedly, he has a strong argument here. The March 1961 insurrection in northern Angola involved an area where Portuguese coffee planters dominated the local economy, where they were in conflict with a substantial class of African smallholders (of Bakongo and Mbundu origin), and where the substantial profits made by them during the boom years from 1950 to 1960 "were based on two aspects of agricultural organization which were inextricably linked to colonial rule - the forced expropriation of native lands at little or no cost and the forced recruitment of African labour" (Paige 1975: 237). With regard to the last point, Paige specifies that in the northern coffee regions virtually the entire population was affected by the demand for estate labour (Paige 1975: 247), while contract labourers were also recruited from other parts of Angola; in both cases, direct or indirect compulsion had to be used because the prevailing wage rates were too low to attract sufficient numbers of workers (Paige 1975: 250). Paige, moreover, does not limit his analysis to this general picture of the socio-economic conditions of northern Angola, but goes one step further by trying to test his hypotheses empirically by computing ecological correlations between measures of the coffee export economy and measures of revolutionary nationalist events for

political sub-units within the area⁷. This exercise leads him to the conclusion that only in areas where settler estates were in direct competition with the local population for land and labour were there any substantial numbers of nationalist events in the first months of the uprising (Paige 1975: 269).

There is no reason to doubt Paige's main argument on the Angolan insurrection, the more so since it has been confirmed by several specialists on ex-Portuguese Africa (Chabal 1983: 196; Pélissier 1978: 148).

This is not to say, however, that his treatment of the Angolan material is without fault. The most important error is that Paige completely misses another "revolutionary" event that occurred in Angola, in January 1961, i.e. a "very mysterious messianistic jaquerie" (Pélissier 1978: 394) known as "Maria's war". J. Marcum supplies the following details on this event:

"Trouble (...) broke out in the cotton-growing country of Kasanje (...). A fall in cotton prices was followed by failure to pay African growers, then strikes, retaliatory beatings and arrests, and finally, by mid-February, mayhem and destruction throughout the countryside" (Marcum 1969: 124).

Marcum qualifies this movement as a "religious crusade for 'independence'" and reveals that the demonstrators sang militant hymns to Lumumba and to northern Angolan political leaders, but he adds that later African nationalists described the movement as "peaceful protest", because the arms of the rebels "were not used to attack persons but only to level property and kill cattle" (Marcum 1969: 124-125). This spontaneous and localized uprising, for which no Angolan nationalist movement claimed credit and which was not publicized at the time, was harshly and rapidly crushed by the Portuguese, so that there was no "follow up", contrary to the coffee areas, where low-keyed guerrilla warfare led by a Zaire-based nationalist movement remained a reality until 1974.

The fact that Paige never mentions the "Maria war" demonstrates that his argument is not watertight. First of all, why did he miss this case? As it was not mentioned at all at the time in the press, one might suppose that Paige's exclusive use of newspapers as his source for identifying revolutionary events must have played him tricks here. It certainly indicates the limitations of this method. Maria's war, however, as we have

⁷ The data concerning revolutionary events are based on newspaper sources, supplemented by the chronology of the 1961 uprising contained in *Guerra em Angola* by Helio Fergas, the former governor of the northern province of the Congo.

seen, is mentioned by Marcum, who is quoted at least once by Paige, so that the latter cannot pretend to ignore what happened. This, then, leads us to another flaw in his theoretical framework: for reasons that I do not understand, the Angolan cotton sector is not included in Paige's list of African export sectors (Cf. Paige 1975: 378), and this has probably induced him to disregard Maria's war; a rather unsatisfactory solution that casts doubts not only on his treatment of the specific case of Angola, but, again, also on his general treatment of "world patterns".

It is true, however, that the northern Angolan case, as far as the UPA insurrection in the coffee areas is concerned, does confirm Paige's theory, although, ironically, it cannot be used against the Alavi-Wolf hypotheses, as the local African cultivators who participated in the revolt were middle peasants. Maria's war, however, is not consistent with what Paige would have us believe; technically, the Kasanje cotton growers were middle peasants in that they still owned the land they were cultivating, and there is no question here of landed estates using migratory labour. However, when one reads Pélissier's description of conditions in Kasanje, one wonders whether the term "middle peasant" has any sense in this case:

"Un point ne prête plus à discussion: la culture obligatoire du coton faisait de l'Africain de la Baixa de Casange, qui y était soumis, un homme dont la vie dépendait de puissances économiques et administratives sur lesquelles il n'avait certes pas la moindre influence, mais qui, en plus, par la répétition des contrôles et des contraintes, le réduisait à un état proche de la servitude" (Pélissier 1978: 397-98).

This case seems to enter neither into the theoretical framework of Alavi and Wolf, nor into Paige's theory.

Turning now to my last case, the Mau Mau revolt in Kenya (1952-1956), I would like first to make a preliminary remark with regard to Alavi's statement that the middle peasants are *initially* the most militant elements of the peasantry. What is exactly meant by this? Does Alavi refer here to the first people to start organizing a protest movement, *even if they do not take up arms themselves*? Or should we only take into account people who actually initiate *and participate* in armed rebellion, when assessing the revolutionary potential of different categories of peasants? As far as I have been able to discover, neither Alavi nor Wolf answer this question and yet, as the case of the Mau Mau revolt will show us, the issue is not irrelevant.

The social composition of Mau Mau is an intricate question and some points are still the object of academic controversy (Cf. Buijtenhuijs 1982: 48 ff), but it can be said that three groups contributed heavily to the Mau Mau forest armies:

1. The Kikuyu squatters working on the European farms on the White Highlands. Although the extent of their contribution to the forest fighting may have been exaggerated, they were certainly the initiators of the oathing movement that was later to become Mau Mau (Cf. Furedi 1974).
2. Nairobi urbanites, mainly Kikuyu, who were responsible for the radicalisation of the oathing campaigns in the early 1950s. Most of them had still one foot, if not two feet, in the countryside, and although they were proletarians by class position, they were peasants at heart, i.e. as far as their ideology and outlook was concerned.
3. The *Kikuyu Land Unit* dwellers. Although this group did not initiate the oathing campaigns, it undoubtedly supplied the bulk of the forest fighters, once the war broke out.

Let us first consider the squatters. There is no doubt that this category took the lead at least twice in the political struggle of the Kikuyu people against colonial domination. First of all in 1946-47, when they transformed an existing "non-violent" political oath that was only administered to trusted (male) leaders by the then dominant *Kikuyu Central Association* into a much more militant oath administered to whole communities, including women and children, in order to unite them irrevocably in the political struggle. A second time, by the middle of 1951, when youth-wingers of the KCA in the settled areas initiated the *batuni* or fighting oath, to be administered only to a small group of selected militants who, by taking the oath, committed themselves to violent political action which, in the strict sense of the word, means that they were the real founders of Mau Mau as an armed revolt (Cf. Tamarkin 1976).

If we consider these people as the initiators of the Mau Mau revolt, then this example goes against the Alavi-Wolf hypotheses. The squatters, indeed, held no rights in land and worked as more or less permanent labourers on the European farms where they were paid partly in wages and partly by being permitted to cultivate a few acres of their employer's land for their own use. As, according to an official report (*East Africa Royal Commission...: 167*), only 25 per cent of their income was provided by money wages and 75 per cent by the produce of their independent agricultural and pastoral activities, they may best be described as *poor peasants* rather than as agricultural proletarians.

Their protest, in fact, was mainly sparked off by the attempts of their European employers, after World War II, to reduce them from would-be "independent producers" to outright rural proletarians by severely limiting their access to agricultural and grazing land (Cf. Throup 1987: Chapter V). Their early commitment to Mau Mau, then, cannot be explained by the views elaborated by Alavi and Wolf. In spite of their limited tactical powers, they were amongst the first to conspire against European rule, and they did participate in considerable numbers in the forest fighting although, admittedly, such participation gained momentum only after they were more or less forcibly driven off the White Highlands during the first months of the Emergency. From that time on they had plenty of tactical freedom and mobility, but in a rather unexpected way, i.e. in the sense that they had nothing to lose anymore, a point to which I will come back later.

The squatters, however, cannot serve as an argument in favour of Paige's theory either. The White Highlands, in fact, have all the characteristics of the *hacienda* system, and Paige is completely wrong when he tries to use the Kenyan case as evidence for his own views. In his book he gives a description of the Kenya coffee estates, an important export sector, which indeed corresponds to his criteria of landed estates dependent on migratory labour, and then jumps to the conclusion that this agricultural system must have been at the root of the Mau Mau revolt (Paige 1975: 68-69), which is entirely incorrect. The Rift Valley squatters, not the migrant labourers on the coffee estates, made Mau Mau, and Paige is here, again, a victim of his fixation on export agriculture. If, as I believe, the White Highlands correspond to the *hacienda* type of agricultural systems, then the Mau Mau revolt, according to Paige's theory should not have occurred at all or it should have taken the form of agrarian revolt, not of a nationalist revolution⁸.

What about the *Kikuyu Land Unit* dwellers who, although late starters in the oathing campaigns, contributed an important number of fighters to the Mau Mau forest armies? Many of them were middle peasants, but a more refined analysis of the data demonstrates that Mau Mau recruited more particularly amongst the "poor peasants, tenants and members of the junior lineages of *mbari* (sub-clans) in the Kikuyu reserves who (...) were being transformed into a landless rural proletariat as the senior lineages attempted to establish their exclusive access to land" (Throup 1987: 11). This, too, is

⁸ Those familiar with the literature on Mau Mau might object here that Mau Mau had, in fact, as much to do with agrarian revolt as with nationalist revolution. Referring to *hacienda* systems, Paige (1975: 42) claims that: "When (...) revolts do occur, they are focused on just those issues specified by the theory - the control and distribution of property in land". Land played an important part in Mau Mau ideology; therefore, Paige, although wrong on all the details, may still be partly right as far as one of his main arguments is concerned.

certainly not in favour of Paige's theory and only of limited value for supporters of the Alavi-Wolf hypotheses.

A final point has to be made with regard to Mau Mau, and more particularly with regard to the initiators of the armed uprising. As I have argued elsewhere (Cf. Buijtenhuijs 1971), when Governor Baring declared a State of Emergency, in October 1952, there did not exist, neither in Kikuyuland nor on the White Highlands, a well-structured revolutionary organization ready and capable to initiate a general insurrection and, apart from a small minority of young, uneducated semi-urbanites, nobody contemplated the use of violence in the short run. There simply did not exist a Mau Mau army and the army that gradually emerged in the Nyandarua and Mount Kenya forests owed its existence more to the inconsiderate actions of the colonial government than anything else:

" (...) as Government pressure mounted during the first few months of the Emergency a growing stream of Kikuyu, Embu and Meru peasants began drifting into the bush or forested areas bordering their homes. This movement was slow, sporadic and, at least in the early stages, unorganized. It was by and large a reaction to external stimuli rather than the unfolding of a well-laid plan for revolutionary action or guerilla warfare. In general terms, this movement to the forests might be described as a 'withdrawal', stimulated in the main by fear of Government repressive measures and reprisals" (Barnett and Njama 1966: 149).

Barnett's opinion is shared by Dedan Kimathi, the overall military leader of the Mau Mau Nyandarua Army, who, in August 1953, wrote an Open Letter to the British Authorities:

"Because of the Government's policy of moving people without any consideration, and of harassing them in the Reserves many people have come to the forest for fear of being killed or badly beaten. As a result, Mau Mau has increased a thousand times" (Quoted in Maina wa Kiniyatta 1987: 57).

This brings us back to Tilly's remarks on violence often being initiated by the incumbents which, as I said before, casts some doubt on Wolf's speculations about the "tactical powers" of middle peasants and poor but free peasants. Indeed several books by ex-Mau Mau fighters suggest, in accordance with the views expressed by Barnett

and by Dedan Kimathi, that many ordinary militants joined the armed struggle simply because they had no other alternative. N. Kabiro is quite representative for these people:

"No one knew what the next day might bring or if he would be alive to see it. For my part, I decided that it was time I joined the Mau Mau fighting forces; life outside was becoming very hard to bear" (Kabiro 1973: 61).

Kabiro's "tactical powers", then, seemed rather limited, although it is true that he could also have joined the Loyalist Home Guard at the time. I suspect that situations such as described here are relatively frequent and will occur on many occasions when the incumbent government is the agent who first opts for violent action. Under such circumstances neither poor nor middle peasants have much tactical power left, and their joining a revolutionary movement does not necessarily demonstrate the intrinsic revolutionary potential of the category to which they belong.

A general argument: the exit option

What - admittedly very tentative - conclusions can be drawn from the foregoing analysis of six major African peasant wars? With regard to the theory of Paige, the results are rather negative. At least three cases defy his predictions (Guinea-Bissau, Mozambique, and Chad), while the case of Mau Mau, as interpreted by him, does not fit either, although a more correct reading of the facts, as I have suggested, might eventually be more in his favour. Only one case (Angola) does fit into his model, while the example of Madagascar probably does too. Two cases out of six seems a rather poor performance for a theory that aims at, and pretends to have, predictive value. Yet, this verdict might be a little bit too severe. The agrarian systems that, according to Paige, are likely to give birth respectively to agrarian revolt and nationalist or communist revolution are rather exceptional cases in Africa south of the Sahara, and the fact that several of them have in fact been beset by internal troubles should lead us not to disclaim Paige's theory altogether, but rather to try to amend it by identifying where it goes wrong and on which points it may be useful.

As for the Alavi-Wolf hypotheses, our African material seems, at first sight, to be much more in favour of them. In all the six cases examined, middle peasants played an important role, although, as we have seen, this statement has to be qualified for the case of Kenya (where poor peasants were conspicuously present on the rebellious scene), as well as for the cases of Madagascar and Angola, where the rebellious middle peasants were at the same time migrant labourers, which, following Paige, leads us to the

question: did they rebel as middle peasants or as migrant labourers? Probably, the two roles are inseparable, which means that Alavi and Wolf's hypotheses stand in the need of improvement or refinement.

Another argument, moreover, can be used to cast some doubt on their line of reasoning. Contrary to the agrarian systems that, according to Paige, give rise to agrarian revolt and nationalist or communist revolution, middle peasantries form the overwhelming majority of the agrarian systems in Africa south of the Sahara⁹; instead of counting the number of cases favourable to the Alavi-Wolf hypotheses on the relatively low total of peasant wars that have erupted in Africa south of the Sahara over the last decades, one might rather quote as evidence against them the fact that there have been, on the whole, so few eruptions of agrarian unrest. Concerning the period up till 1959, for example, R.A. Levine notes that: "One of the outstanding facts about the past fifteen years of nationalist turmoil in subsaharan Africa is the infrequency with which Africans have resorted to violence against their European rulers" (Levine 1959: 420). Although Levine's counts are incomplete (he misses the cases of Madagascar and Cameroun) his conclusions are basically correct. It is true that large-scale violence did break out, after 1960, in what remained of colonial Africa (Guinea-Bissau, Angola, Mozambique, Zimbabwe and Namibia) but, for reasons which space does not allow me to develop, this period is rather atypical. As for independent Africa, again, violence has been the exception: only two cases of major peasant wars (the Zaïre rebellions of 1964-65 and Frolinat's armed insurrection in Chad) have occurred since independence, which is a surprisingly low total. If the middle peasant hypothesis of Alavi and Wolf were true, would one not have expected more cases of agrarian unrest?

Admittedly, an objection might be made here. Over the last thirty years waves of violence have engulfed important parts of Africa south of the Sahara, as is shown, for example, by the fact that Africa has by far the largest number of refugees of all the continents. Much of this violence, however, has had to do with ethnic wars, attempts at separation, etc., and one can doubt whether they should be counted as "peasant wars" at all. Paige, for example, made a clear decision when listing the acts of rural protest used in his chapter on "World Patterns":

"Movements of regional secession have (...) been excluded from the analysis if there is clear evidence that the movement is based on urban commercial or industrial interest groups or if it involves a coalition between urban groups and

⁹ I have no reliable statistical data at my disposal, but the above statement is certainly correct generally speaking.

the rural upper classes. The American Civil War would not be considered a rural social movement by this criteria, nor would the Nigerian civil war or the Katanga secession" (Paige 1975: 91).

I certainly do agree with Paige's general remarks and also with his more specific suggestion to deny to the cases of Biafra and Katanga the qualification of peasant wars. I am not sure, however, whether his remarks hold for all "movements of regional secession", at least as far as Africa south of the Sahara is concerned. At the grass-roots level, quite a few of these movements are made up of peasants, and the question is, therefore, in each specific case: Why do these peasants fight and in which capacity? As members of a specific ethnic group or religion, as it would seem at first sight, or maybe also, and even mainly, as peasants defending their rights to ancestral lands? In some cases, the last proposition would seem to contain at least part of the answer. Analyzing the Diola agitation in the southern Senegalese region of Casamance, in the beginning of the 1980s, P. Geschiere and J. van der Klei argue that the immediate cause of the unrest had to do with the question of tribal lands, and this more particularly because of the introduction, in 1964, of the *Loi sur le domaine national* :

"L'installation des communautés rurales qui devaient gérer la terre en remplaçant les aînés du village, et la rumeur selon laquelle le gouvernement accorderait des terres incultes à des étrangers, renforçait chez les Diola l'idée que les Nordistes étaient en train d'accaparer leurs terres" (Geschiere and van der Klei 1987: 321-322).

As these authors conclude, the revolt only became a reality "lorsque le gouvernement commençait en effet à s'occuper de la gestion de la terre" (Geschiere and van der Klei 1987: 334). I do not think that the Diola case is unique, and one should therefore take into account at least part of the examples of "ethnic" violence when testing the Alavi-Wolf hypotheses. I am thinking, more particularly, of the civil war in Southern Sudan and the different revolts in Ethiopia.

None the less, it still remains true that many parts of Africa where middle peasants form the majority of the rural dwellers have remained remarkably calm since independence, which seems to indicate that middle peasants are certainly not always and under all circumstances inclined to rebellion and revolt. How can one explain this? In a way by using the same concept of "tactical freedom" which Wolf uses to explain why middle peasants are, initially, more easily inclined to follow the path of revolt and revolution. Tactical freedom, or tactical power, in fact, is a two-edged device. It does allow the

middle peasant to opt more easily for disobedience and revolt than other groups of peasants, but it also leaves him the option *not the revolt*, to have recourse to other means of manifesting his discontent.

This becomes particularly clear when one introduces G. Hyden's well-known thesis on the "uncaptured" African peasantry into the discussion. Hyden, in fact, argues that Africa is the only continent where the peasants have not yet been captured by other social classes, i.e. made subordinate to the demands of such classes. By being the owners of their means of production, the smallholder peasants of Africa have enjoyed a degree of independence from other social classes large enough to make them influence the course of events on the continent:

"The peasants are the owners of the means of production (...) and thus they can always seek security in withdrawal (...). While it is true, as Francis Hill argues, that in the administrative regimes of contemporary Africa, peasants have few opportunities to use citizen rights to circumvent bureaucratic power, they do have the freedom to stay outside the state system. To use Hirschman's terminology, they have the option to 'exit' out of the system" (Hyden 1980: 25).

Although Hyden has been severely criticized (Cf. Geschiere 1984) it is nevertheless true that African peasants, although they do need the *market*, can afford, at least for some time, to do without the *state*. They can also "use the market against the state", by withdrawing from cultivating crops that have become economically unattractive, "thereby evading some of the adverse consequences of government policies" ((Bates 1981: 82). These "exit" options are often less costly for the peasant than open war, and these forms of "silent" guerrilla war (Hyden 1985: 199), moreover, can be decided and implemented individually without having recourse to collective action and organization, all things that are difficult to initiate for independent and scattered smallholders. In other words, Wolf and Alavi are right when they claim that middle peasants are free to revolt, but they tend to forget that they are also free not to revolt and to use instead Hyden's exit option. Quite a few African peasants seem to have made the latter choice, as is demonstrated by "le refus de l'arachide" in Senegal, cocoa smuggling in Ghana (Konings 1986: 122-123), the failure of the *ujamaa* movement in Tanzania and of similar attempts at collective farming in Mozambique, to quote only a few examples. Moreover, exit options do not only exist in the field of economics and politics, but also in the field of religion. As I have demonstrated elsewhere (Buijtenhuijs 1976), independent churches and messianic movements are often used as ways of "libération

dans l'imaginaire" (Althabe 1969), as an alternative to open revolt. Of this exit option, too, African peasants have made extensive use.

Conclusion

The two theories on the revolutionary potential of different categories of peasants examined in this article seem logically coherent and intellectually satisfying. However, when put to the test of materials on African peasant wars they are unable to account for all the cases under study. Both theories may, up to a certain point, be correct on a very general, abstract level, i.e. a revolutionary potential exists probably for *several* categories of peasantries (middle peasants, migratory labour estates, share-cropping systems, and even *hacienda* estates), but this is tantamount to saying that the question of revolutionary potential is not the question that really matters, i.e. that there does not exist a revolutionary or militant class *as such*. I absolutely agree here with P. Worsley's statement: "In sum, there is no single absolute general proposition that one can make about any particular *type* of class, universally, as being *the* or even *a* revolutionary force" (Worsley 1972: 227). More particularly, Worsley concluded that "no social class is 'inherently' anything (...). Where they go depends on who approaches them and how" (Worsley 1972: 223).

Different categories of peasants may initiate a revolt, under certain circumstances, or may be mobilized by outsiders if the right arguments are used. A shrewd and informed observer might, beforehand, have tipped the Kikuyu, or the Bamileke, as the possible initiators of revolt in Kenya, respectively Cameroon, but who would have tipped the Balante or the Makonde (in spite of their quality of middle peasants)? The revolutionary potential is only one factor that plays a role in the making of "revolutions" and it will only work when combined with a multitude of other factors, the identification of which I will hopefully pursue in later publications.

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The Institute goes out from the premise that the research which is carried out in Africa must be of such a nature that the results are directly or indirectly relevant to the population in the country concerned. Other objectives include the systematic promotion of research and education covering the afore-mentioned areas, and also the spread of the knowledge of African societies and cultures.

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The main emphasis of the department of Social and Economic Studies is on rural development, food and nutrition and trade in agricultural products. The research is policy-oriented; the most important programme for the period 1989-1993 is the Food and Nutrition Studies Programme which has as its main objective to analyse contemporary trends and future needs concerning Food and Nutrition in Kenya.

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In addition to the research departments, the Institute has a library and a documentation section.

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The African Studies Association cooperates closely with the African Studies Centre in promoting research and education relevant to African studies in the Netherlands. The Association advises the Netherlands Foundation for the Advancement of Tropical Research (WOTRO) on applications for research funding in the social sciences and the humanities concerning Africa. The secretariat is based in the Institute's offices.

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