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Comparative perspectives on Malagasy and Indonesian ethnography


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It was my principal intention in reviewing Dr. Bloch’s book, *Placing the Dead*, to call attention to and, indeed, to recommend to Indonesianists his remarkable case study of Merina social organization and mortuary practice. Neither in my passing criticism nor in my even more brief comments on certain puzzling and unsatisfactorily explained aspects of Merina ritual, did I wish to imply any slight on the contribution of Dr. Bloch’s ethnography. Dr. Bloch stands prominently among present ethnographers who are now, by careful case study, advancing and enriching our understanding of the complexity of Malagasy social life. But from this standpoint, it is both useful and instructive that my review has prompted Dr. Bloch to clarify, on the basis of his subsequent field work and further reflection, a number of topics inadequately dealt with in his book.

In my review I expressed what I consider to be reasonable criticism, namely, 1) that the book lacks suitable comparative reference to the considerable researches that have been carried out in Indonesia and, in general, throughout the Austronesian-speaking world and 2) that Dr. Bloch would have been better served had he been acquainted with this research and not adopted, for comparative purposes, models drawn from a West African context. It is on this point — my comparative perspective — that Dr. Bloch appears to have taken objection and it is on this issue that I feel compelled to offer some further comment.

I would like to begin by dispelling one gratuitously misleading criticism of this comparative viewpoint. It seems that Dr. Bloch happens to have read an article of mine on the Rotinese (Fox 1971) and has apparently seen in it some conceivable similarity to the Merina case. He has, therefore, supposed that this represents probable grounds for my comparisons and now he would like to joust over this. I, myself, never mentioned Roti in my review nor imagined that the Rotinese might present the most useful case with which to examine the Merina. My own field work on various societies (cf. Fox 1972 a, b, c, d) in a province of Indonesia that has over thirty linguistically distinct populations and a social complexity that could possibly rival an island like Madagascar makes me particularly wary of using the Rotinese as some kind of model even within eastern Indonesia.

Moreover I am suspicious of the typification of societies that I asso-
ciate with the theoretical work of Radcliffe-Brown. Dr. Bloch, on the other hand, cites and seems to subscribe to these methodological stric-
tures. This would appear to be the real issue that divides us — not that I have ventured a few comparative comments on Merina ritual. It seems doubtful to me that a taxonomic approach to the study of society — the sort that derives from Radcliffe-Brown's ideas — has much to offer toward a genuine comparative framework. It lends itself too readily to an identification of a society with one model of it and often leads a contentious ethnographer to defend his constructs as unique or, alternatively, as applicable everywhere. As he chooses, he can permit or deny the possibility of comparison.

The comparative endeavour of certain Dutch scholars, who combined literary and anthropological research in the study of the ritual and social systems of the Indonesian peoples, had quite a different focus. Their work in many ways carried forward the collaborative efforts of the early writers of the *Année Sociologique*. Their particular theories — involving, as they did, an historical reconstruction of ancient Indonesian society, various notions of descent, exchange, and connubium, a wedding of analytically separate forms of marriage, and speculation on complex forms of symbolic classification — may now seem too cumbersome to be defensible. Yet this comparative venture was both a prelude and a stimulus to more recent structural studies. It can be argued that these theorists slighted comparative economy or subsumed it as a mere reflex of ritual, that they occasionally ignored contrary evidence that could have been profitably pursued, and most unfortunately that just when the impetus for detailed ethnographic research was greatest, these scholars were prevented from undertaking the field work that would have undoubtedly advanced their theoretical perspectives. Nonetheless, they bequeathed a suggestive program within which comparative Indonesian research has developed.

Were Dr. Bloch better acquainted with this comparative literature or had he chanced to attend to my remarks in the article he read, he might have realized that the idea of male and female complementarity is not something that originated from research on Roti. It is a construct that has a long and useful history within Dutch anthropology. Professor P. E. de Josselin de Jong, for example, in his Preface of *Ngaju Religion* remarks:

"It was a working hypothesis of several Leiden-trained anthropologists of the 'thirties and 'forties that a dualistic opposition of male and female (and associated concepts), so often encountered in Indonesia, might, in the field of social structure, be related not only to genealogical and/or territorial moieties, but also to an awareness (even among peoples with unilineal or cognatic kinship systems) of the opposition between the patrilineal and the matrilineal principles of descent, succession, and inheritance, and the latent, if seldom manifest, combination of the two."

The gulf that seems to divide Dr. Bloch and myself lies in our differing comparative perspectives and in our theoretical backgrounds. It is difficult to judge when Dr. Bloch will permit the possibility of comparing
the Merina to other groups in Madagascar or elsewhere and when he
will decide to reject comparisons as inapplicable. What I would like to
do briefly is examine a few instances of this on-again off-again compara-
tive perspective, especially in regard to points on which Dr. Bloch argues
that my previous remarks have been particularly misdirected.

Whereas Dr. Bloch admits that such notions as male/female comple-
mentarity “might apply more to other people in Madagascar”, he would
deny that they are “strictly applicable to the Merina”. Unlike some
former French commentators who tended to view all the other peoples
of Madagascar through Merina eyes, Dr. Bloch would, on this point,
prefer to view the Merina as distinct and separate. This is particularly
regrettable since recent researches in other areas of Madagascar, utilizing
these and related notions and directed, in part, toward the investigation of
mortuary practices, have begun to yield superb analyses that lend them-
selves readily to comparison with similar practices among the Indonesian
peoples. W. R. Huntington’s examination (1973a, b) of metaphors of
gender among the Bara of Madagascar provides a deft and detailed
presentation of Bara phenomenology of person and of social life.
G. Althabe’s *Oppression et Libération dans l’Imaginaire* (1969), an
investigation of the multiple dualities of village life among the Betsimi-
saraka during a period of considerable upheaval, is another fine study
that points to the ritual importance of male/female complementarity. In
particular, Althabe’s analysis of the Betsimisaraka burial ritual during
which the corpse is carried to the tomb, is worth quoting in this context:

“... les participants, divisés en deux groupes, masculin et féminin, jouent le rôle
complémentaire de l’homme et de la femme, et on fait jouer au cadavre le rôle
de produit de cette dualité sexuelle. Nous avons une véritable mise en scène de
la procréation: ... et l’ensevelissement prend la forme d’un véritable accouchement.
On peut trouver une confirmation de cette analyse dans la conscience
verbale qu’en ont les intéressés; les images de l’enterrement comme un accouchement,
et celles du sort du mort, qui se joue dans cette cérémonie, comme une
deuxième naissance sont employées par la majorité de nos interlocuteurs”

The ritual conception of Bara and Betsimisaraka should not be confused
with those of the Merina; each must be considered initially on its own.
But it seems unfortunate that comparisons, on this point, can be made
between the Indonesian peoples and some — but not all — of the
peoples of Madagascar.

A further difficulty is that Dr. Bloch, particularly in his book, seems
to imply that rituals can do little but reflect the descent constructs he
has defined for the Merina — “the strengthening of kinship”. At times,
however, he recognizes that this need not be so as, for example, in his
note of clarification where he points out the ritual expression of status
differences between wife-givers and wife-takers in the marriage ceremony
is the “opposite of the actual situation”. Although this adherence to
Radcliffe-Brown’s principles is not always consistent, where it is applied
it certainly reduces the subtleties of ritual communication to a rather
pedantic restatement of already established findings.
Many peoples in Indonesia, like the Merina, recognize some form of bilateral descent but this has not prevented various people from giving a ritual or symbolic identification to the descent groups they may possess. Dr. Bloch describes the social groups of the Merina as “segmentary, corporate groups”. In his note of clarification, he has identified a “patrilineal tendency” due to residence. He further argues that “the woman relative link is anti-descent” as is especially “the mother-child link” and he would now stress that the famadihanas [the ceremony of exhumation and reburial] “contains an anti-household element and is anti-individual mother-child link and pro-descent, that is pro all ancestors-all descendants link.” He goes on to explain that in a forthcoming analysis of the Merina circumcision ceremony he will demonstrate that the “mother-child link is associated with sterility and its break is linked with fertility”.

What I find curious is that this groping, awkward, jargoned formulation of Merina conceptions is, if one dares to interpret it, not far removed from my passing suggestion that there might be some connection between the rituals performed on the bones of the dead and “the idea of a symbolically male descent group that continues through time by the fertility these bones provide” (Fox 1972e: 507). Dr. Bloch indeed tells us, on the very last page of his book, that “occasionally Merina leave bottles of water to stand on their tombs to capture the power of the ancestral group and so that the water can be sprinkled around the house to transmit good luck and fertility to the inhabitants” (1971a: 222). It is difficult to see what Dr. Bloch wishes to deny on this particular point.

A more fundamental difficulty arises when one attempts to interpret Dr. Bloch’s statements on Malagasy kinship. In an article which he intended as “a methodological preliminary to an ethnography of the various peoples of Madagascar considered within a single framework” he noted that one feature of the ethnographic situation in Madagascar was that “the kinship terminology is, with minor variations, uniform throughout the island” (1971b: 79). This is a remarkable statement from someone versed in the literature on Madagascar especially since he utilizes this claim to bolster a theoretical argument that the variations in “kinship organization” found on the island apparently deny “all connection between terminology and social organization”. All evidence to date, however, would indicate the contrary. The terminologies provided by Wilson (1967) for the Tsimihety, Lavondes (1967) on the Masikoro, Huntington (1973b) on the Bara and even the far less reliable bits of information from Linton on the Tanala (1933) demonstrate more than “minor variations”. There are on Madagascar a number of related but certainly distinctive terminological systems. Yet what is, to one investigator, minor variation can represent, to another, considerable difference. The real question for investigation is the significance of terminological variation in relation to the varieties of social organization that occur on the island. This cannot be done by imputing a uniformity that does not exist nor can it be achieved by imposing a single framework on all the peoples of Madagascar.

In his note of clarification, Dr. Bloch asserts that the “absence of the
notion of opposition between affines and kinsmen for the overall kinship system... differentiates the Merina system sharply from most Indonesian systems”. Again this striking assertion would seem to be a comparative overstatement. If one takes merely the Merina terminology published by Raharijaona and Verin (1964) there appears to be no sharp divergence between this system’s categorization of kin and affine and that of a large number of Indonesian systems. What is more, the Merina, like many Indonesian peoples, override, in address, affinal distinctions that may be used in reference. But this would not, one suspects, satisfy Dr. Bloch’s understanding of kinship. In discussing this, he has argued against an exclusively referential definition of terms in favour of more polysemic conception of kin terms, — what he calls their “moral meaning” and “tactical use”. In this regard, we would seem to be in some agreement. I have argued for a similar position on somewhat different grounds (1971). But if Dr. Bloch wishes to use his distrust of kinship terminologies either to deny or rather to disregard their comparative value, then he ought also to avoid grand assertions about the Merina kinship systems versus other Indonesian systems until he can make clear precisely what constitutes the comparative basis for his distinction between kin and affine.

Excellent ethnographic research is now being carried out in Madagascar and in the Indonesian world. Hopefully, this research will be mutually instructive in developing a comparative perspective. I have no wish to deny the ethnographer his specific insight nor to engage in petty polemic or chauvinistic quarreling. But on his own evidence, I feel that Dr. Bloch has failed to convince me that I was “inevitably wrong” in all my remarks in my review. Moreover I object to Dr. Bloch’s wholesale assertion that such an attempt at comparison between Indonesia and Madagascar is “ludicrous”. This position would also prohibit comparisons within Indonesia since the linguistic separation among the languages within eastern and western Indonesia is probably as great, if not greater, than the separation of some western Indonesian languages from those of Madagascar. Furthermore, there have been many evident cultural influences in the different parts of Indonesia. I hope, therefore, that other researchers are not tempted to adopt Dr. Bloch’s standpoint and that discussions between specialists on Indonesia and Madagascar may continue.

I would now like to discuss what Dr. Bloch has labeled as “specific errors” in my review:

a) I never wrote that Dr. Bloch’s initial training in France was in anthropology. Many anthropologists have had earlier training in other fields before they turn to anthropology. I see, however, that my statement could be interpreted in this way. This was not my intention since French anthropological training was what I found lacking in his book.

b) My reference to Sibree was in connection with Merina funeral ceremonies and I chose this early missionary description as a vivid example of a common European reaction to such conspicuous extravagance. The quote is taken from a section of Sibree’s book dealing with
the “Tombs and Burial Customs of the Hova...”. The reference is to the depositions of wealth that were provided Radama I and Queen Rasohetina at their burial: “... an immense quantity of costly articles of dress and furniture and other valuables were placed in their tombs, as well as money. Their coffins were made of plates of silver formed from dollars hammered out, while many thousands of dollars were also put into the coffin as a bed for the deceased sovereign. There is, to European ideas, a prodigal and painful waste of money” (Sibree 1880: 231). If Dr. Bloch feels this too restricted and therefore an unrepresentative reference to Merina funerals, I could quote him Dr. Huntington’s recent description: “Best known are the exhumation rites (famadihanas) of the dominant Merina people at which hundreds of kinsmen gather and celebrate for several days with drinking, feasting and dancing to the music of professional performers. The expenditures of time and resources for the maintenance of tombs and the production of these ceremonies are considerable, especially in light of the meagre economic base” (1973a: 65). (See also Bloch: 1971a: 148).

c) Dr. Bloch’s lack of comparative reference to research on the descent systems of Indonesia, Melanesia, and Polynesia has already been discussed. I note with interest that Dr. Bloch considers his article (1971c) with its single reference each to the work of Eggan, Hudson, and Goodenough makes up for this lack.

d) An interpretation of Professor Fortes’ monumental works is quite beside the point. My remark was merely that Dr. Bloch’s “vocabulary” reflects the influence of Professor Fortes’ Tallensi ethnographies, one of which is entitled The Web of Kinship Among the Tallensi. My review suggested that Dr. Bloch’s interpretation and application of this West African model to the Merina situation was somewhat inappropriate.

e) I certainly agree that Merina marriage can be described without resort to paradox. I was not accusing Dr. Bloch but merely quoting his statement about “the paradox that is marriage in Imerina” (Bloch 1971a: 53).

f) Whatever explanation Dr. Bloch imagines I was “driving at”, I made it clear in my review that I felt that rules regarding Merina preferences were insufficiently described to permit any adequate explanation. Again I can only quote Dr. Bloch’s own crucial statements which lead to his remarks on paradox and the importance of incest: “It would be quite pointless for me here to attempt to list the rules for the various demes because I could get no agreement. Many people were willing to discuss this matter and attribute certain rules to certain demes, but evidence from other people was almost always contradictory. Nevertheless, the differences which informants attributed between demes and the difference of opinion amongst them always concerned the kinship relations already discussed. The explanation of this apparently baffling diversity became quite clear from the statements of informants. It is the result of the paradox that is marriage in Imerina. All these marriages, as indeed all marriages between cognates, are in various degrees incestuous” (Bloch 1971a: 53). I am of the opinion that, in social anthro-
pology, it is still useful to have a description of supposed rules, however baffling they may appear, before they are explained away.

g) This point, too, has already been discussed. I am willing to accept Dr. Bloch’s statement as an ethnographer among the Merina. But I am still critically suspicious of it, especially in light of the other evidence Dr. Bloch has himself presented on the Merina. I suspect that “prodding” people to state, formally, beliefs that they express primarily in ritual is not a conclusive means of obtaining confirmation of certain ideas.

h) As I explained in my review, it is difficult to assess the differences between Dr. Bloch and Professor Condominas in their interpretation of the Merina. Professor Condominas does cite Radcliffe-Brown in a footnote, not to obtain a definition for a clan, but to distinguish a clan from a lineage. He does this to define the fokon’olona as a patrilineal and patrilocal group whose “tomb constitutes the mystic pole where the group comes to recover its cohesion” (Condominas 1960: 24). Dr. Bloch, in his book, admits a “certain ideological stress on patrilinearity”, but he argues that the fokon’olona is neither patrilineal nor a clan as is “usually understood by anthropologists” (1971a: 44). And he, too, cites the same article by Radcliffe-Brown. Clearly Dr. Bloch and Professor Condominas have differing views of the fokon’olona. Professor Condominas refers to it as a clan, Dr. Bloch rejects this. One can only surmise that, in practice, Professor Condominas does not share Dr. Bloch’s rigid commitment to Radcliffe-Brown’s strict definition. But it is also interesting that in his note of clarification, Dr. Bloch now recognizes a “patrilineal tendency” in Merina society due to residence.

i) In my review I remarked that Condominas’ term for clan, fokon’olona, did not seem to be used in the same sense in Dr. Bloch’s main area of research. I can only quote Dr. Bloch’s statement that led me to make my remarks: “Some writers on Madagascar who refer to these groupings use the word foko (e.g. Condominas) and translate it as ‘clan’, but my informants did not understand the word foko in this sense and only seemed to use the word in church hymns for nation or in the compound, fokon’olona, which roughly means ‘local community’ (see p. 19 and Bloch 1971). It was only when I began to use the word karazana that I was understood and got the information I wanted” (Bloch 1971a: 42).

j) The only area that Dr. Bloch discusses in some detail, chapter three of his book, is the Ambatomanoina district “beyond the northern frontier of traditional Imerina” (1971a: 74). In the context of Dr. Bloch’s disagreement with Professor Condominas about the nature of Merina ‘clans’, it seemed that this was the area he was discussing when he referred to his “informants”. The problem may lie in Dr. Bloch’s attempt to characterize all Merina and avoid the difficult problem of diversity within this large grouping.

k) Again Dr. Bloch seems to have misread what I wrote. I said nothing about water seeping into tombs. Dr. Bloch states 1) that Merina “leave bottles of water to stand on their tombs to capture the power of the ancestor group”, 2) that this water is sprinkled around the
house to bring “good luck and fertility” (1971a: 222) and 3) that the ‘blowing on water’ blessing (tsadrano) is requested of the dead on the occasion of a famadihana by its main participants and is actually, at times, mimed in dances in the presence of the exhumed bones (Bloch 1971a: 163). I interpreted these statements to imply some association between water, the bones of the dead, and ideas of fertility and prosperity. If it is not water itself that seeps from the tombs, it seems to be a ‘power’ of these bones transmitted via water to the living.

1) My comment had nothing to do with an individual or collective representation of death. I merely expressed surprise at Dr. Bloch’s brief three page discussion of Merina eschatology and his own comment that “the very lack of interest of Merina in the after-life may account for the many inconsistencies in their eschatological beliefs” (1971a: 124). It is possible, as Dr. Bloch claims, that the Merina have “undeveloped ideas about death” but I would prefer to be sceptical of such assertions as long as the symbolic aspects of the complex mortuary rituals in which these ideas seem to be developed remain to be carefully studied.

m) In conclusion, I can only note that Dr. Bloch lists my regret at his not including a list of Merina kin terms as another of my “specific errors”. One senses, in this, the reason why, in Dr. Bloch’s opinion, I was “inevitably wrong” in my review of his book. To comment on what is evident seems to provoke, in Dr. Bloch, the accusation of error.

Discussion closed - Editors

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