

Letters to the Editor

PASTORALISM IN TROPICAL AFRICA

Dear Sir,

I FOUND myself substantially in agreement with Professor Gulliver's response to my review of *Pastoralism in Tropical Africa* edited by T. Monod (*Africa* 48 (2): 203-4). But I would like to take the opportunity to clear up one or two points of misunderstanding.

Firstly, I was not intending to advocate any opinion in the single sentence of the review that Gulliver cites. To the contrary, I was merely summarizing a position that is widely held among students of pastoralism, including some of the contributors to the volume under review. I, too, find it tiresome and repetitive; indeed its limitations are pointedly suggested in the subsequent sentences of my review, which Gulliver does *not* cite. Secondly, I have no particular wish to defend the concept of 'ecological balance'. It is, of course, ambiguous in meaning and heavily laden with ideological overtones. However, I do not think the 'idea that the complex variables interact in constant change, continual flux, not ever settling down nor ever tending to do so' is the least bit helpful. Far from being a difficult idea to grasp, it is a simple device for dodging the whole problem of analysis and explanation. As such, it is subject to much the same objections as the opposite assumption of balance.

Perhaps one way forward would be to clarify more precisely what we mean by 'unbalance', a concept which Gulliver appears to find perfectly acceptable. I submit that we should distinguish between systems whose components are subject to high-amplitude oscillations, but which otherwise persist more or less in a steady state, and systems that are undergoing an irreversible *change* in state. In the arctic and subarctic, for example, the numbers of certain fur-bearing species are known to fluctuate markedly, but in accordance with a demonstrably cyclic pattern whose causes are quite well understood. For the trappers who depend on these species, life is a matter of coping with extremities, of abundance one year and scarcity the next. Perhaps the Californian trapper about whom Gulliver writes has had the same experience. Yet the system constituted of relations between the trapper, his prey and its food supply may be highly durable over time. By contrast, tropical ecosystems whose components are less subject to violent oscillations may nevertheless be far more fragile, in that the slightest disturbance may set in train a process of degradation—as from forest to grassland, or grassland to desert. Turning to pastoralism, it seems to me of critical importance to discover to what extent fluctuations in livestock numbers are, or were in the past, part of a regularly recurring cycle; and what is the threshold of durability beyond which irreversible degradation will occur.

Finally, there is the problem of what weight we should attach to 'socio-cultural variables'. I would certainly take the social relations of pastoral production as the starting point in any explanation of herd management strategy; for it is the social system that determines pastoral objectives, whereas the environment only places limiting conditions on their realization. But however much the actions of the pastoralist are culturally mediated, he remains organically as much a part of the natural ecosystem as the animals he exploits, and is thus subject to the same kinds of biological constraint as his non-human competitors (Gulliver's 'wild animals'). On this level, I contend that models derived from animal ecology, if correctly applied, can be of great assistance in coming to grips with the dynamics of the relations between humans, herds and pastures.

TIM INGOLD

MARRIAGE IN THE COMORO ISLANDS

Gentlemen:

THE appearance of Gillian M. Shepherd's article, 'Two Marriage Forms in the Comoro Islands: An Investigation,' (*Africa*, 47(4), 1977) should have been a welcome addition to

our knowledge of these influential, yet little known, islands in the western Indian Ocean. Instead, it was a disappointment.

Most outstanding is the fact that the information which she presents in her article is essentially the same as that presented ten years ago for Grande Comore by Rouveyran and Djabiri (1968). Yet their work is absent from her bibliography.

In addition to this lack of familiarity with published materials on the Comoros, Shepherd demonstrates, through a number of mistakes, that she has only marginal familiarity with the Comoro Islands themselves. Relying on a group of Grande Comorians (Wangazija) in Kenya she describes the Grande Comorian system of big and little marriages stating that 'much of it holds good for Anzwani (sic) too' (1977: 348) and cites Robineau (1966: 65–70) for support. Such a statement is simply incorrect. Although both islands, Ngazija and Nzwani, have 'big marriages' the two systems are markedly different in conception, in detail and in their place within the overall structure of society in the two islands (Ottenheimer 1971). There is no term on Nzwani for the 'little marriage' (*mna daho* on Ngazija); the 'big marriage' is called *grande mariage*, not *ada*; there are only informal restrictions on Nzwani on the order in which siblings may marry, not rigid ones as on Ngazija; the 'grande mariage' is most often the first one for those men who can afford to have it at all, not the last as on Ngazija; the dances are different; the order in which they occur is different . . . One could go on at length but two quotes will suffice to demonstrate the order of difference that exists. The reader need only compare Shepherd with Robineau, whom she cites, on the necessity of making a 'big marriage'.

Shepherd says:

The institution is too coercive to be easily ignored. A man who falls behind his *hirimu* mates as they begin to make their *ada* marriages soon finds himself forbidden their company at public events and shamed by being forced to sit with much younger men. His delay may hold up sons or sisters' sons, ready but unable to make *ada* marriage because of him. If he attempts to speak publicly at a village meeting he will be told, 'Who are you? We have eaten nothing from you,' by the *wafaume wa mji* even if he is their senior in chronological age. (1977: 353)

Robineau, on the other hand points out that:

Theoretically, for a man to have a voice in village or mosque meetings, he must have been married according to the custom. In fact, when he reaches 25 or 30 even if he has not yet celebrated his 'grand-marriage', the collectivity of the town, quarter or mosque, gives him credit for the marriage and admits him into their assemblies. (1966: 65; our translation)

Shepherd's reliance on her Kenya Grande Comorians for data on the rest of the Comoros is clearly too heavy and the amount of misinformation which results is disappointing to see in published form. Another illustration of this problem can be seen in the widely held belief that Comorian languages are dialects of Swahili. While some *wangazija* who have learned Swahili believe that their language may be a dialect of KiSwahili and thus call it KiNgazija, it is, in fact, a separate language, pronounced by non-Swahili speakers in Grande Comore as HiNgazija. ShiNzwani, the language of Nzwani island is distinct both from HiNgazija and KiSwahili (Ottenheimer and Ottenheimer 1976). Most of the difficulty with the classification of these languages has resulted from exactly the kind of paper that Shepherd has attempted to write. The Comoro Islands are the scene of a complex group of cultures whose relations with the rest of the Indian Ocean are ancient and intriguing. To generalize about even two of the islands, as Shepherd has done, is to oversimplify and to misinform.

One cannot expect to be able to arrive at a reliable anthropological account of the customs of any group of people by collecting information from their neighbors. Shepherd should have limited herself to an account of the customs of Grande Comore as related by the Grande Comorians in Kenya. Let us hope that any confusion that has been generated by the printing of this article can be cleared away as soon as possible.

MARTIN AND HARRIET OTTENHEIMER

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- Shepherd, Gillian M. 1977 'Two marriage forms in the Comoro Islands: an investigation,' *Africa* 47(4): 344-59.

Dear Sir,

I was somewhat surprised by the Ottenheimers' letter. The article they accuse me of ignoring is not only in my bibliography, but is also referred to three times in the text of my article. It turns out, however, that the volume of *Tiers Monde* in which it originally appeared was incorrectly cited when the article was reprinted in the now defunct Comorian journal *Promo al Kamar*, where I first came across it. Perhaps my reproduction of this error is what misled them.

The information about what takes place in Ngazija marriages collected by Rouveyran in 1967 (apparently from a single informant) and by myself in 1973-74 is similar, but our overall aims are rather different. Over half his article is devoted to a descriptive account of *ada* marriage and its place in the life of the individual, while my account is as brief as I could make it and my main focus is anthropological interpretation of the broader meaning of *ada* marriage.

This is not to say that Rouveyran attempts no sociological explanation. He does, but in a very general way, suggesting that *ada* marriage 'favours social cohesion' (1968: 119) and that, 'its primary benefit . . . is, in our opinion, psychosociological' (*ibid*: 120). He is clearly of the opinion that it provokes irrational expenditure, indicating, 'the primacy . . . of psychosociological security over economic calculation' and 'the still archaic nature of Comorian society' (*ibid*: 122).

My explanations try to investigate culture in a less timeless way and are concerned to stress the *rationality* of *ada* marriage in terms of the political structure of the colonial period, the current economic situation and the religious and racial hierarchy within which the Ngazija live. During my two visits to Ngazija in 1977, everyone I asked expressed relief that *ada* marriage had been banned the previous August by the then socialist government, even while they were highly critical of other changes. It was plain that *ada* marriage had been a burden, not a psychological satisfaction, yet one that few individuals had dared unilaterally to lay aside.

The Ottenheimers are annoyed that I regard Ngazija and Nzwani marriages as comparable. My evidence for this is from Robineau (1966) not from 'Kenya Grande Comorians'. I made it quite clear that my material drawn from Ngazija informants (in both Kenya and Ngazija) is about Ngazija only, 'where the (marriage) system is more marked' (1977: 344, 348, 356). The objections the Ottenheimers raise to my assertion all seem trifling except for the contrast, illustrated by two quotations, between the compulsory nature of customary marriage in Ngazija and its apparently optional nature in Nzwani. Yet Robineau's very next sentence after the section they quote goes on to make it clear that well-born young men, at least, 'are virtually compelled . . . to make a customary marriage' (Robineau 1966: 65). There may be a bending ('*assouplissement*') of the rules, which allows such young men to receive some of the benefits of a customary marriage in advance, *but the marriage itself must be in the making*. It would be fascinating to know why poorer Nzwani do not aim for big marriages as their counterparts might in Ngazija. Is land perhaps the crucial factor? A far higher proportion of usable land has been lost to plantation companies in Nzwani, and a larger percentage of the population are landless, than in Ngazija.

'Nzwani' is indeed better usage than 'Anzwani', and it would have been more logical for me to have followed local pronunciation and called Ngazija's language *ShiNgazija* rather than *KiNgazija*.¹ *HiNgazija* appears to be a rare usage, for when I checked written sources I found that Steere (1869: 18) called the language *ShiAngazidja*, Sacleux (1909: 17) wrote *çingazidya* (= *Shingazidya*), the texts collected by Heepe (1920: 118, 120) record native pronunciation as *šingazidja* (= *Shingazidja*) and Tucker and Bryan (1970: 376) have *Shingazidja*.

The Ottenheimers' statement about the relationship between *Swahili* and *Ngazija* also calls for comment. It is well established that the Coastal Bantu language group is divided into two even more closely related sub-groups:

- a) The *Swahili* dialects and the *Komoro* (Comorian) dialects;
- b) *Mijikenda*, *Pokomo*.

Though *Swahili* dialects and *Komoro* dialects are no longer mutually intelligible, we know they all derive from a single common ancestor (Derek Nurse, personal communication). *Ngazija*, for instance, shares certain features with both Northern and Southern *Swahili* and some of its more striking phonological and grammatical idiosyncracies reappear in the *Swahili* dialects *Vumba*, *Mtang'ata*, *Pemba*, *Hadimu* and *Tumbatu* (Bryan 1959: 126–8). The distinction between 'dialect' and 'language' is in any case one of degree, not of kind (Lyons 1971: 34ff) and in this case the words do no more than indicate different stages in a process. The differences between *Ngazija* and *Nzwani* are no more striking than the plethora of variants encountered in the *Swahili* dialects, and though precise understanding of the chronology of the whole group must still await the expert, that all are intimately related is not in doubt.

It is always a temptation for those who know an area well to take up an extreme cultural relativist position, but by doing so the Ottenheimers are in danger of deluding themselves about the remoteness and uniqueness of Nzwani, the island where they worked.

G. M. SHEPHERD

NOTE

- ¹ Italics are used to distinguish languages from the geographical locations.

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- For Robineau 1966, Rouveyran 1968 and Shepherd 1977 see *References section of the previous letter*.