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# LETTER TO V. G. FANSO

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*In November 1992, Sally Chilver wrote to the historian Dr (now Professor) V. G. Fanso of Yaoundé University discussing Eldridge Mohammadou's theories. In this letter, she drew his attention to the series of publications of the Institute for the Study of Languages and Cultures of Asia and Africa, Tokyo, which is available free to serious researchers.*[1] *The following pages reproduce the main body of that letter since it not only includes a serious critique of Mohammadou's position, but also well conveys the importance of Sally's continuing correspondence with scholars from all round the world, many of whom find her letters the best (and sometimes only) way of keeping up with developments in Cameroon studies.*

I thought I ought to draw your attention to the Tikar part of [Mohammadou 1990] in case you have not got it, and attach an epitome of his argument.

Well, I don't go along with it, but Eldridge Mohammadou's work is never to be sneezed at. He puts all his cards on the table, his sources are always clear, he never neglects archival or early ethnographic sources (in four languages), and he provides plenty of texts. When he advances a hypothesis, he makes a clear distinction between types of evidence. His book

1. Available from The Publication Service, African Languages and Ethnography, Institute for the Study of Languages and Cultures of Asia and Africa, Tokyo University of Foreign Studies, Nishigara 4, Kita-ku, Tokyo, Japan.

on Garoua, published by Bordeaux University/CNRS, is an important contribution and deserved the prize it received. He always makes one rethink one's own inferences, both on larger issues and on significant detail.

Now both Ian Fowler and I, from different standpoints, have been looking around for evidence that a raid or series of raids preceded, with quite a long time-lapse, the invasions of the Ba'ni associated with Gawolbe. There is some apparent convergence of traditions:

(a) the flight of an early Bamum ruler to Nkogham and his burial there (recorded by Tardits);

(b) the flight, on similar lines, from Kovvifëm to Taavisa' of Fon Nso';

(c) Babungo traditions of the invasion of the 'Montë', associated with the arrival of the refugee Ntuur master-smelter who helps start the improved furnace type (recorded by Fowler 1990);

(d) the insistence of the smaller Ndop chiefdoms that the first raiding long preceded the arrival of Bali-Kumbad;

(e) Western accounts of the passage past Bafut of raiders towards the Katsina Ala valley, sometimes called Muti or Mudi (Hawkesworth and others; Garbosa; Geary); and

(f) Tiv accounts of early disturbances in this area caused by mounted raiders (the Bohannans and R. G. Armstrong).

The past evidence has been examined by Richard Fardon (1988), from whose work we can tentatively pick up a possible set of precursors, the Péré. These are identifiable with Garbosa's Pyere; the Peli of the Bali chiefdoms (who still keep their own flags, e.g. Ga Konntan of Bali-Gham, Ga Sabum/Ga Muti of Bali-Kumbad), also known as Konntan; and the Potopo or Kotofo, the ruling stratum of the Kutin in the southern part of the Koncha lamidoship.

It is a pity that more work has not been done in the Furu–Mashi–Nser area, though. Having said that, we must also be aware, in comparing later and earlier accounts of apparently the same events, that oral tradition rearranges the past in terms of the present, that event time may be either telescoped or extended, that common story-telling conventions influence oral history, that names and ethnonyms suffer many kinds of corruption and displacement. Moreover, we now have to consider what Jack Goody calls the interface between literacy and orality and be on the look-out for the feedback from early European conjectures and Islamic historiography re-emerging at a later date with an aura of authenticity. Material now collected (say post-1970) will now also be influenced by unquoted pamphlet literature which has passed into gossip, e.g. Rabiato Njoya's pamphlet. These feedbacks can often be spotted and call for the same arts from the ethnohistorian as from, say, the student of Shakespeare's history plays; they are surface problems. There are the deeper ones which affect both informant and recorder, the 'cultural concepts' which affect the production of history anywhere and the polemical or didactic uses it can be put to, such as the definition of particular social identities. Seldom mentioned is the

common abhorrence for a vacuum in the record, oral or written. This tends to get filled up in various ways, for example, by plausible hypotheses (the evolutionist paradigm, the Hamitic hypothesis) or by plausible fictions which may become politically or artistically important, e.g. the fake Ossianic corpus for Europe's Celts. I am probably as guilty as others.

Eldridge Mohammadou advances his hypotheses clearly and modestly. They are directed to the solution of the 'Tikar problem', which Kaberry and I (1971) tried to convert into a non-problem for the benefit of the Grassfields Bantu linguists by suggesting that the dissemination of institutions from point to point, rather than the migration of peoples, could account for a lot. Mohammadou explicitly rejects our hypothesis. If we accept his, we have to fit in the processes of state formation in Bamum, Nso' and Bafut, to take the three biggest, into some 60–70 years; well, not impossible, but a tight fit. The chronology, upon which much hangs, is based on the assumption that the reported septennial king killings of Mbum were regular and real and adopted by the Tikar, or some.

The present Kimi rulers' claim to be Mbum is, he agrees (with Hagège and others), to be mistaken, and thus if Kimi had a Mbum dynasty it must have been prior to 1750. Nevertheless he retains a septennial chronology for the Kimi king-list after the presumed arrival of the new Tikar dynasty, which, he explains, may come from the general direction of Mbum but is 'really' Bare-Chamba, and neither Tumu (the autochthones) nor Mbum. Since much of the argument hangs on this chronology, one is bound to say it is a slender thread.

It then follows that if the Bamum and Nso' dynasties (and those of Bafut, Baleng, etc.) are 'really' Bare-Chamba ('Tikar'), they are of very recent origin—elder cousins of Bali-Nyonga and Bali-Kumbad, as it were—and that we can dismiss antecedent sites and legends: the 'miraculous princes' established themselves by force of arms. It may well be that Mohammadou has dealt with some of the cracks in the argument in earlier publications (e.g. 1986) which I don't have.

But I find it more disturbing that we are told, in the style of Champaud (1965), of Tikar-Ndobo 'waves of migration' that, after the settlement of Bankim/Kimi, roll over the land, first after 1760 or so and then again after a second Bare-Chamba arrival, and rapidly form chiefdoms out of the earlier 'segmentary and acephalous' polities of the Grassfields. All this happens without leaving any linguistic traces (except in the Bali chiefdoms, of course, where, with the help of the Fon's secretary, A. W. Daiga, vocabularies of Wute, Mbum (Nyamnyam), Tikali (Tumu), and distinct 'Mbam-Nkam' languages close to Munggaka could be collected as late as 1960). And would one not expect some more loan-words, supposing such a lightning linguistic conversion of the conquerors by the conquered?

So there are, to my mind, too many interlinked hypotheses for comfort.

Now for plausibilities: that the pre-Fulani raids galvanized a process of compaction, resisting and greater centralization (as Warnier in Nkwi and

Warnier 1982 had already suggested) in the Grassfields is entirely plausible. So is the proposition that the Bare-Chamba arrived in two main groups and that the, or a, or some of the earlier lot encamped, or settled among or near the Tumu-speaking 'Tikar' groups. The rough dates he suggests for the arrival of the earlier contingents is not implausible per se; though one would like more cogent reasons than either his (or mine). Nor can one quarrel with the proposition that the insecurity and fear inspired by raids occasioned movements of groups to safer areas. One recalls, for example, the story collected by P. F. Lacroix from the Banyo Lamido that the raids he ascribed to 'Dingdings' (Dingyi? i.e. Chamba) softened up the opposition to Mwömbwö of Bamum, enabling him to incorporate and enserf many of the 'Pa Ghet' chiefdoms or send them fleeing across the Nun.

It is easy to pick holes in other people's work and cavalier to do so when the writer has covered so much ground one has not tackled oneself and has made a prodigious study of the literature and archives. One should try to offer a better alternative. One would certainly have to concern oneself with onomastic questions viewed historically and in the light of the history of 'Tikar' ethnogenesis. But if one is concerned with 'real history' and not 'mentalities' one would also have to concern oneself with questions he barely touches: the linguistic evidence and its interpretation, such evidence as we have from archaeology, palaeobotany and biogeography, climatic records, even equine veterinary science. A life work, too late for me to start!

In so far as the Chamba themselves are concerned, it would be hard to go beyond what Fardon (1988, 1991) has written, except on the Donga and Nigerian border side, but perhaps too late for that. Much of the 'Tikali' evidence will have vanished under the barrage lake for good, apart from the snippets rescued by Hurault. But to return to a point Fardon makes about the transformation of the Batta, Chamba, Wute, Péré and some Mbum and Tumu-Tikar into predators, a south-north look at the Arabic literature might reveal some clues. Can we explain the trajectories solely in terms of an expanding slaving frontier, leaving burnt-out areas behind, or, in the second push, solely in terms of Fulani pressure? Should we neglect Frobenius' reports, and those of the Chronicles of Bornu recording a series of eighteenth-century droughts and famines—there are some Mandara ones too. Can the palaeoclimatologists help?

Finally, one wonders why it is assumed as axiomatic that the peoples of the Grassfields were incapable of inventing and developing chiefdoms for themselves and were relatively primitive? Two decades after the last Bare-Chamba raids, Barth's trader informants (appendix to Vol. 2, 1857) give a different picture of 'Mbafu'.

I've said nothing in detail about Eldridge Mohammadou's handling of the Ti which he equates with the Bantu speakers of the Mbam and Sanaga areas. If he is right, they must be distinct from Koelle's Pati, the Kpati who turn up in Takum, the Bati of Bali-Kumbad, the Ti-Gawolbe of Bali-Nyonga and their 'brothers' who wandered to Banssoa. Given that Nyong-

pasi was supposedly a Chamba on his mother's side, his father Pati (the Chamba proper have a complicated double unilineal descent system), the plot thickens and one might say that there is a 'Pati problem'. So my priority for archaeological research might now be that part of south-west Bamum called Kuparë, though one can't be dead certain that it was the Kuti or Tsën of Bali tradition, said to be the base to which elements of Gawolbe's army returned. There is more than one Kuti.

There are two odd omissions from Eldridge Mohammadou's bibliography. One is Thorbecke's four-volume geographical survey of the trans-Mbam Tikar–Wute area (1914–24), and the other is the original, fuller, edition of Barth's travels (five vols.). He only quotes M. P. (Frau) Thorbecke's book, which is odd, given the other early material he has dug out. Also missing is Hurault's brief report in Africa (1988).

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