LETTER TO V. G. FANSO

SALLY CHILVER

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. 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 con-
tribution 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 Kovvifém to Taavisa’ of Fon
Nso’;

(c) Babungo traditions of the invasion of the ‘Monté’, associated with
the arrival of the refugee Nturu master-smelter who helps start the im-
proved 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 oth-
ers; 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 Péli of the Bali chie-
fdoms (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 Kon-
cha 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 rear-
ranges the past in terms of the present, that event time may be either tele-
scoped 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 col-
lected (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 Os-
sianic 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 Hagege
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 Mo-
hammadou 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 ear-
lier ‘segmentary’ or acephalous’ polities of the Grassfields. All this hap-
pens without leaving any linguistic traces (except in the Bali chiefdoms, of
course, where, with the help of the Fon’s secretary, A. W. Daiga, vocabu-
laries of Wute, Mbum (Nyamllyam), 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 light-
n ing linguistic conversion of the conquerors by the conquered?

So there are, to my mind, too many interlinked hypotheses for com-
fort.

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 Lamide that the raids
he ascribed to ‘Dingdings’ (Dingyi? i.e. Chamba) softened up the opposi­
tion to Mwɔmbwɔ 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 one­
self with onomastic questions viewed historically and in the light of the his­
tory 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 evi­
dence as we have from archaeology, palaeobotany and biogeography, cli­
matic 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, Pére 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 Mehmmadou’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 Kuparé, though one can't be dead certain that it was the Kuti or Tsên 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).

REFERENCES


