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*Who are the Luo? Oral tradition and disciplinary practices in anthropology and history**

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ABSTRACT *What is oral tradition, and how can it help elucidate the past and understand the relationship between culture, social organization and identity today? It turns out that this question is complicated by the influence of early European narratives that described and defined African society and which have also indelibly marked the methods, assumptions and forms of narrative writing used by contemporary social science. This paper addresses this vexing issue with respect to research on the Luo-speaking peoples of Eastern Africa by examining how anthropologists and historians have approached 'oral tradition' and how their approach has influenced the way they write about Luo culture, society and identity.*

This paper examines the production of 'academic' knowledge, in the form of anthropological and historical texts, about the Luo-speaking peoples of Eastern Africa. Research on the 'Luo' began with early explorers, missionaries, administrators many of whom were influenced by the 'Hamitic hypothesis', a 'historians' myth' that significantly influenced academic and lay understandings about 'Luo' identity and their place in the Horn of Africa. Section 1 therefore examines the contribution of this early scholarship – in particular the significance of oral tradition – and its influence on subsequent anthropological and historical writing and on generations of Africans in the region.

In Section 2 I look at the significance of oral tradition for anthropological knowledge and writing, and in Section 3 I examine historical knowledge and writing on the Luo. I conclude by noting that despite the apparent convergence in the narrative accounts produced by anthropologists and historians, fundamental differences between the disciplines remain based upon methodological grounds and the approach taken to 'oral tradition/history'. Secondly, while we cannot control the manner in which our writings are used or understood by others, we should never

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the less alert our readers that our 'claim to authority' in representing ethnographic and historic 'others' is tenuous at best.

Two points need to be made at this point. First, within the scope of a paper there is a limit to the amount of material that can be discussed. This paper does not attempt to provide a definitive assessment of material about the Luo, nor have I thought it necessary to refer to the entire written corpus of the individuals who are cited. By and large my criticism is based on a critical reading of key monographs and I consider that within that format the author(s) had ample space to address the issues I discuss in this paper. Secondly, this paper should not be read as being in any way hostile to inter-disciplinary research; on the contrary, clarity about disciplinary methods and approaches is the foundation for inter-disciplinary work.

1. Early European scholarship, the 'Hamitic hypothesis' and 'race'

An understanding of the development of modern social science, and especially of its methodological and 'narrative traditions', requires if not an examination then an awareness of the influence of earlier narrative forms. For example Thornton's examination of nineteenth century travel writing and missionary scholarship on emerging forms of narrative in anthropology reveals the significance of work by missionaries (via their linguistic studies and translations of African languages) for nineteenth century 'arm chair' theoreticians (1983).

In particular, and quite apart from Malinowski's influence, early philological research bequeathed a tradition of fieldwork (including an insistence on collecting vernacular texts) to modern anthropology. A further, and often overlooked contribution of philological research came in the form of a professional, 'scientific' vocabulary that included new 'sociological' terms and an analytical focus on 'native terms' (i.e. *mana, totem, taboo*, etc.). Philological research sought to classify African languages and peoples and, in the process, coined new terms for large groups of 'related' peoples. Thus, in addition to terms like the 'Baganda' and 'Zulu'

> ... the term Bantu had even further reaching historical effects, both intellectual and political, since it came to designate, ambiguously, an imagined 'race', a conjectured common history, a family of languages, a *zeitgeist* or worldview, a 'stage of civilization', or a culture (Thornton 1983: 512).

While the term 'Bantu' emerged out of the study of African languages, other terms also entered into the scholarly discourse on Africa which derived entirely from European speculation. Thus the nineteenth century explorer Richard Speke (1863) is said to have originated the idea that the bearers of culture and civilization to Africa were light skinned Hamitic invaders from Christian Ethiopia. The 'Hamitic hypothesis', which was linked to the Biblical myth of the dispersal of the 12 tribes out from Israel, provided Europeans with a set of ideas that enabled them to 'explain' the physical and cultural differences they encountered in the region (Wolf 1994).

A close examination of the writing of Europeans and the newly educated African elite demonstrates the power of the Hamitic hypothesis – which functioned in social science as a modern myth and sociological model devoid of a factual basis – to explain European dominance and the savagery and barbarism of black Africa (MacGaffey 1978).

For example, historical research on the 'Abacwezi' – initially understood as a mythic account of an ancient dynasty of light-skinned kings in Central Africa – underwent a 'complete inversion' that turned mythic figures into 'living persons' 'who occupied the whole country then, suddenly, disappeared underground' (Berger 1980: 67). A key contributor to this theory was the British administrator Sir Harry Johnston who, in 1902, argued that the light-skinned Abacwezi were in fact 'alien rulers from the north' who were the source of African knowledge of plant domestication, iron smelting, music, etc. (ibid: 68). In the 1950s, the Hamitic hypothesis was also used by the African elite of Uganda to present themselves as the heirs of the 'exalted semi-European outsiders' for the purpose of enhancing their own claims and the status of their 'tribe'/kingdom (ibid: 70).

Colonial administrators in Central Africa developed an especially pernicious, racialist version of the Hamitic hypothesis to differentiate between African 'races' and to justify colonial forms of governance. In Rwanda and Burundi

> ... European conquerors ... seized upon the occupational categorization, imbuing it with a hierarchical racial classification. The Tutsi minority were identified as a Hamitic aristocracy, who ruled a state with such sophistication that they could only have originated from a place geographically, culturally and above all racially nearer Europe (DeWaal 1994: 3).

As Eltringham shows, this mode of thought was systematically pursued to the extent that in the census of 1933–34 'every Rwandese was assigned an 'ethno-racial' identity (15 per cent Tutsi, 84 percent Hutu, 1 per cent Twa) and issued with an identity card' (2004, p. 15). This racial schema was subsequently internalized by Rwandese and Burundians and contributed directly to widespread ethnic violence and to the 1994 genocide. Taylor writes that

> Tutsi extremists make use of their version of the hypothesis to claim intellectual superiority; Hutu extremists employ theirs to insist upon the foreign origins of Tutsi, and the autochthony of Hutu. No matter which side uses the Hamitic hypothesis, however unwittingly, it reproduces a colonial pattern: one that essentializes ethnic difference, justifies political domination by a single group, and nurtures a profound thirst for redress and vengeance on the part of the disfavourized group (1999: 57).

In the early 1950s when the Hamitic hypothesis was being institutionalized in central Africa, historical research elsewhere in the region was arguing for a 'modern' conception of regional history and culture.

In *The Lwoo* (1954), Father H.P. Crazzolara, supported by the Istituto Missioni Africane, sought to trace the history and customs of the (Nilo-Hamitic) Lwoo/Luo people from AD 1000. This history of Lwoo migration from the Bahr el Ghazal area of modern Sudan to present day Uganda and Kenya is based upon evidence derived from oral ('native') tradition – on the assumption that 'such narratives as present an

outward appearance of history, are built upon historic foundations' (p. 11) – and the presumed linguistic 'identity' between the languages of the Lwo, Dinka and Nuer (adduced from comparative lists of names for different parts of the human body).

Crazzolara's account is anchored in a conception in which distinct 'races' can be identified as the bearers of a distinct culture and tradition. Thus,

> ... I do not agree with the school of ethnology which prefers to use artificial and, in my opinion, meaningless terms such as 'Nilotic', 'Nilo-Hamitic', 'Sudanic' etc. as labels for groups of tribes with linguistic and cultural affinities. Here ... we are dealing with clearly distinct racial political communities, and it is both more accurate and more convenient to use the group names by which they have been known to one another for centuries, that is to say 'Lwoo', 'Lango', and 'Madi' (Crazzolara 1961: 141).

The significance of Crazzolara's work for the emerging disciplines of Anthropology and History is two-fold. First he provided a conceptual focus on culture (the concept of race was to be replaced with tribe, and subsequently by ethnicity[1]) as something that was ostensibly produced and carried by discrete and clearly bounded social groups. Second, he suggested a methodology for comparative research based upon the analysis of oral tradition. While the Hamitic hypothesis was eventually discarded as conjecture, the idea that history could be discovered by collecting and analyzing oral tradition remained despite an awareness – by Crazzolara and his successors – that the 'memory' contained in oral testimony altered with the passage of time. Even so, the prospect that oral testimony might unlock 'history' proved too promising a tool for anthropologists and historians to ignore.

To summarize the argument thus far, the Hamitic hypothesis represents a form of historiographic myth created by Europeans. The methodological issues surrounding its creation and perpetuation arose from European misinterpretation and misconception of African statements about the past, and a situation in which writers drew freely and without acknowledgement on each others' accounts, making it impossible to independently verify individual accounts. At the same time, socio-political pressures for decolonization, growing levels of literacy, and nationalist political fervour gave greater salience to the production of elite 'tribal' histories that borrowed ideas and concepts from European academic writing and discourse.

2. *Anthropology of the Luo*

While a growing number of books have delved into the history of British social anthropology (Kuper 1993; Stocking 1983; Goody 1995; Parkin 1990), they focus primarily on specific individuals/institutions or on the 'discipline'. For this

[1] In anthropology, attempts to conceptually delimit clearly bounded social groups (i.e. races, tribes, ethnic groups, etc) has been heavily criticized (cf. Southall 1970, Lentz 1995).

reason they are of little help looking at interdisciplinary research or work that was influenced by linguistics and history.

Broadly speaking, the discipline developed out of the speculation of Victorian 'armchair' ethnologists whose theories were heavily dependent on research conducted by missionaries and colonial administrators. The 'facts' generated by such 'men on the spot' (James Frazer's term for those who sent data to the metropolitan theoreticians) was constrained by current theorizing, the absence of substantive data, evolutionist thinking, and a disregard for the study of language and linguistics (which, in British anthropology, lasted until the early 1960s; Henson 1974). Gradually 'metropolitan theoreticians' turned their attention to the methodological problems involved in soliciting 'facts' from 'natives' and, between 1841–1912 they produced a sophisticated questionnaire to be used by their 'men on the spot' (i.e. 'Notes and Queries in Anthropology'; Urry 1972). Toward the end of this period, and following on from the fieldwork undertaken by the 1898 Torres Straits Expedition, and by W.H.R. Rivers and Malinowski, the emerging discipline of social anthropology embraced fieldwork as its *sine qua non*.

Alongside the institutionalization of fieldwork there emerged a 'new paradigm', functionalism (first as a method and subsequently as a philosophy; Kuper 1993: 85-ff). One of the first post-war statements reflecting this paradigm is found in *African Political Systems* (Evans-Pritchard & Fortes 1940) which set the agenda for the comparative study of Africa by focusing largely upon lineages as universal social institutions (i.e. that regulated marriage, religion, ritual, economic relations, etc).

As it happens, Evans-Pritchard was the first professionally trained anthropologist to study the Luo in the 1930s (in the area around the Kavirondo Gulf in western Kenya). Illness shortened his visit (to less than 6 weeks) and restricted him to interviewing – in English – Luo mission converts (Evans-Pritchard 1965a: 228). His principal purpose was to survey the political structure of the Luo whom he believed to be closely related to other Nilotic peoples such as the Nuer. His account takes the form of a realist description of the area, its people, culture, and social structure (1965a, b).

In 1949 Aidan Southall conducted extended fieldwork among the western Luo of Uganda and wrote the first monograph length account of a Luo-speaking people, *Alur Society* (1953). Southall argued that the Alur were intermediate between the acephalous Luo of Kenya – to whom they were related by history, migration, language and culture – and the centralized political states of Bunyoro and Buganda. His objective was to describe and analyze the process by which the Alur had come to dominate other peoples without the use of force. In effect, Southall sought to define a fifth form of political system to those identified in *African Political Systems*, namely the segmentary state.

Alur Society differs from other ethnographies written in this period. First and foremost it is concerned with 'political theory'.[2] The monograph focuses primarily

[2] Its concerns derive from the work of Max Weber whose work was just appearing in English.

on social structure at the expense of social relations and beliefs, and there is relatively little use of vernacular terms and categories. In other ways, however, *Alur Society* does conform to the genre of ethnographic writing (see Marcus & Cushman, 1982). Southall developed a specialist vocabulary (a jargon or meta-language) to tease out the significance of fieldwork findings and convey meaning to an audience of anthropologists. The text takes the form of a generalized, somewhat static account of the organization and function of Luo lineages; notably the anthropologist is invisible (in the sense of being absent from the text) though his perspective defines the manner in which the Luo are represented. Furthermore, the text's validity is largely dependent upon the anthropologist's claim to have lived among the Luo.[3]

Okot p'Bitek, a British-trained Ugandan anthropologist, undertook fieldwork in 1962. A poet, dramatist and writer of fiction, Okot wrote and published primarily for African audiences. His *Religion of the Central Luo* (1971) argued against nineteenth century European theories, and in particular the Hamitic theory which, he believed, devalued the contribution of Africans to the history and culture of the Great Lakes. Okot also sought to refute the work of Christian missionaries, who he argued had mistranslated Luo into English to evangelize Luo-speakers. They had, he argued, misinterpreted Luo religion by turning it into the 'High God' of Christianity.

Religion of the Central Luo differs significantly from other ethnographies. The introduction merely defines the book's objective as describing 'the religious ideas and practices of the Luo people' resident in east and central Uganda. The book – which looks at theories of jok or spirit, ancestral shrines, witchcraft and sorcery, cursing, etc. – disavows participant observation and draws insights instead from myth, ritual, song and poetry. In so far as he identifies his sources, information comes from family members.[4] *Religion* self-consciously stands outside the established genre of ethnographic writing in the form it takes (i.e. as a rebuttal of European speculation), in the data it utilizes, and its concern with the contribution of the eastern Luo to African culture. Significantly, Okot is acutely aware of the limitations of oral tradition and myth as a basis for generalization. Not only does he argue that memory (recollection of the past) is strongly affected by the passage of time and by religious conversion etc., he notes that all myths 'are about the foundation of existing institutions and political groups'. Indeed, Okot argued that

> ... it is not possible to reconstruct the histories of the Northern, Central, and Southern Luo groups separately, it is not possible to trace the history of all the+Luo peoples to the first man, Luo. This is because oral traditions are

[3] For example, there is no discussion of his fieldwork nor about his research methods and the data they generated.

[4] As the son of teacher, he attended prestigious local schools prior to attending Bristol University, obtaining a law degree at Aberystwyth, and undertaking a doctorate in anthropology at Oxford (entitled 'Oral literature and its social background among the Acoli and Lang'o'; Heron 1976: 3).

> concerned not so much with the ultimate and historical origins, but with the foundation and maintenance of existing institutions. Crazzolara's attempt to trace the history of the Luo people as far back as A.D. 1000 is thus a futile exercise (1971: 3).

Religion of the Central Luo is an account of a cultural 'insider': Okot was an Acoli, quite possibly descended from a chiefly lineage, the son of a Church Missionary Society teacher, and among the first generation of university educated Ugandans. His elite background raises issues about his interpretation of Acoli/Luo culture. How might the ethnography have differed if it had been written by a commoner, a Ugandan from a different ethnic group, or by a woman? Indeed, Okot's outspoken attack on the influence of European culture on African society suggests that his perspective was quite distinctive.[5]

A related issue concerns the anthropologist's competence in the vernacular language of the society she/he researches (Owusu 1978). For example, while Evans-Pritchard interviewed his Luo informants through an interpreter, Southall gained basic competence in Dholuo midway through his fieldwork (i.e. after 9 months), and Okot's competence is partly demonstrated by his use of vernacular concepts though he says nothing about his linguistic abilities. There is an ongoing debate in anthropology about the role of language in fieldwork between those who argue for a basic grasp of a language (quite possibly based on a limited grasp of phrases/locutions) and those who argue, *pace* Malinowski, that the ethnographers task – 'to grasp the natives point of view' – implies the need for fluency in the vernacular (Owusu 1978: 313–4).

While Owusu berates anthropologists for their lack of facility in the vernacular – indeed he argues for a need to 'speak and understand *several* local relevant vernaculars' to minimize data/translation and informant error (his emphasis, p. 313) – his emphasis on language competence is at the expense of other skills that are also important (cf. Henson 1974). Thus to focus on an ethnographer's ability to 'speak and understand' one or more vernacular languages, valuable though this undoubtedly is in comprehending social life, is to undervalue the linguistic training required to comprehend language use (e.g. language shift, the ability to deal with differences of dialect/argot, etc.) and the ability to accurately collect and transcribe vernacular texts. Even so, the anthropologist's task is not limited to accurately recording and reporting what her informants say or do, we are also tasked with the problem of interpreting and writing-up our fieldwork in a manner which is transparent and which addresses issues other than language.[6]

To conclude this section, we have seen that the ethnographic accounts of the Luo vary considerably from realist accounts to a dialogue between the ethnographer and her/his sources. Just as important, there is a conspicuous absence of any discussion of fieldwork, of research methods, and of the linguistic competence of the ethnographer. Furthermore, the contribution of social theory in interpreting fieldwork

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[5] See his interview in Lindfors (1980).

[6] See Altheide and Johnson (1994) for a recent discussion about the validity of ethnographic accounts.

findings is either conspicuous by its absence, or it tends to exclude detailed accounts of culture, beliefs, etc.

3. The 'new' African historiography of the Luo

Historical research in the region took a different route from that followed by anthropology. Historians initially argued that in the absence of documentary records ('facts') historical analysis was not possible (cf. Evans 1997), which meant that in sub-Saharan Africa historiography was based on European sources, archives, the work of missionaries and administrators, or fieldwork. An example of the historiography of this period is Roland Oliver's 'Discernible developments in the Interior, c. 1500–1840' which deals with Uganda and Lwo history (1963). With one exception (Oliver 1959), his account is based entirely on secondary sources – the accounts of missionaries, historians, anthropologists, and 'literate Africans' – and colonial district books. Indeed, while critical of certain claims, e.g., the 'doubtful etymological evidence' provided by Crazzolara (fn. 1, p. 172), he nevertheless relies on 'genealogical evidence' found in clan/tribal chronologies and 'special' (Nilo-Hamitic) clan names to explain the survival of groups incorporated by the Nilotic Lwo.

At roughly the same time a 'new African historiography' was developing based on the work of Jan Vansina (*Oral Tradition*, 1965). As Cohen noted, Vansina offered a way to 'join science to the study of African voices' (2001: 50), and it was not long before historians took up the challenge. In East Africa the new historiography was initiated by Bethwell Ogot, a Kenyan and a Joluo, in his *History of the Southern Luo* (1967). Ogot was the first professionally trained historian to engage systematically with oral tradition in the region. He distanced his research from work that blurred the distinction between collecting and analyzing oral tradition (particularly Crazzolara). Such accounts were problematic because they did not leave behind

> ... the vernacular texts, the circumstances under which the material was collected, and the manner of transmitting oral evidence in the particular societies they studied. Consequently, they presented us with accounts which are difficult to interpret. *Oral traditions need to be transformed into written texts before the historian's art can be applied* (my emphasis, p. 19).

While anthropologists used oral tradition, Ogot argued that

> ... they too lack the necessary historical training; and on the whole, they tend to concentrate on those aspects of oral traditions which relate to their theoretical models, especially myths, legends and genealogies (p. 20).

In addressing 'historical questions' Ogot pursued a radically different approach to collecting and analyzing oral tradition. To begin with, considerable effort went into collecting and translating lay/popular histories written in Dholuo, and in convening meetings with clan elders (18 clans of Luo origin in Uganda, and 4 Luo clans in western Kenya) with the aim of producing official clan histories. Discussions with assembled clan elders took place over several days and culminated in 'one named expert' relating the official story to Ogot who tape-recorded it. Clan

elders were also asked to comment on the 'official' history of other clans as a means of cross-checking accounts. Overall, Ogot collected oral evidence in the form of 'genealogies, migration stories, clan songs, histories of clan and tribal cults, histories of social organization, i.e. changes in the social structure, and evidence on place names' (p. 23). All oral testimony was transformed into text for analysis and was examined against other 'evidence'.

History of the Southern Luo is an 'ethnic'[7] history recounted in the third person as a 'factual' academic history. The following excerpt provides an indication of Ogot's method[8] of interrogating clan histories:

> Up to this point, the traditions are unanimous, and indeed from Acholiland it was the most likely route a migrating group of Pastoralists would follow. But there are conflicting accounts as to where the Luo went when they abandoned their temporary settlements in North Ugenya. According to one account, they first went to Manga in Wanga, and thence to Boro in Alego; and according to another account, they went straight from North Ugenya to Alego. But these accounts, both of which are given by experts on Luo traditions, are contrary to the accounts of some of the Joka-Jok themselves ... (Ogot 1967: 149).

Interestingly, Ogot eschews a search for absolute dates in favour of a 'relative' chronology defined as the estimated length of a generation, i.e., the time that elapses between the birth of a man and his first surviving child. Based on interviews, Ogot calculated a 'generation' as 26.5 years, which enabled him to date Luo migration to western Kenya back 16 generations (to AD 1490–1517 plus or minus 52 years; p. 28). In this manner Ogot sought to put historical research on a scientific basis.

A closer examination reveals that Ogot, like Crazzolara and some anthropologists, assumed the existence of clearly bounded and socially discrete 'clans' that were believed to exist through time as culture-bearing units.[9] In this way, clans were linked with the concept of 'generation' enabling a chronology to be calculated for Luo migration. Just as significantly, Ogot uncritically accepted the claims of the elders as the custodians of historical knowledge, thereby lending academic support to elite male views which were subsequently recorded as undisputed 'facts'. It is worth noting that 'traditions' are deployed by contending parties to contest and influence the outcome of disputes/litigation; as such tradition is better understood

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[7] 'Ethnic' history is one of several historical genres of writing/representing the past, in this instance it is produced by a professional historian. See Atkinson's discussion of academic reliance on the 'stylistic conventions of realist fiction' and the way in which different disciplines invoke specific criteria to 'partition' the field in an effort to situate it from other texts and disciplines (1992: 30).

[8] In the words of his successors, 'Like the larger and less honed work of J. P. Crazzolara, Ogot's *History of the Southern Luo* is essentially nomenclatural. The central technique ... was genealogical and the central mode of reconstruction was name-linkage' (Cohen & Odhiambo 1989: 17).

[9] 'Clan' boundaries appear to correspond with contemporary speech communities, an assumption that privileges existing Luo dialects, traditions, etc. In perceiving ethnic boundaries as fixed and timeless, this assumption fails to deal with historical change i.e. in language/dialect, and the movement of people, ideas, etc.

as situated knowledge that reflects contemporary socio-political processes rather than as a historical truth.

In addition Ogot's primary data, the transcriptions of officially agreed traditions, is 'archived' and is not available to his readers; instead he references the transcripts via footnotes (e.g. as *Padhola Historical Texts* vols. I, II, etc). This practice sidesteps concerns about how testimony was transcribed (Emerson, Retz & Shaw 2001) and about how linguistic and other problems involved in analyzing such material were handled. Interpretative problems are further compounded because the transcripts are subjected to the same type of analysis as other types of texts, i.e., they are scoured for consistency and 'nuggets' of data rather than to understand them as statements reflecting the socio-political status of 'clans' or as culturally encoded statements indicative of Luo perceptions of themselves and their world.

The successor to *History of the Southern Luo* is *Siaya, The Historical Anthropology of an African Landscape* (1989) written by David Cohen (an American) and Atieno Odhiambo (a Kenyan and a Joluo). *Siaya* takes up the challenge of providing a voice for the Luo by contrasting the views of anthropologists (who are said to see the Luo as an exemplar of a segmentary patrilineal society) with those of historians and differently positioned Luo who are represented as active social agents involved in producing their own culture and history.

Though the contrast overly simplifies disciplinary difference, it provides an interesting entrée into differing discourses about what it means to be a Luo. For example, separate chapters address the issue of social and geographic boundaries; the meaning of 'Siaya' as a homeland (for run-away fathers, migrants, and the elite); the problem of 'land hunger' (for the urban rich vs. the rural poor); and the paradox faced by rural widows whose efforts to gain control over their lives are frustrated by men.

In the text Siaya is simultaneously:

- a historical locale in western Kenya where Dholuo-speaking peoples have settled and from which many have emigrated;
- an administrative district;
- an (ethnic) 'homeland'; and
- a socio-cultural landscape central to the reproduction of contemporary Luo culture in which individuals confirm their ethnicity (e.g. through building a house, burial, performing ritual, etc.).

The Luo are said to constantly redefine their history through a process of 'rehearsal' in which current themes and concerns are reworked by the elders who, in effect, rewrite 'custom' (the past) and in the process validate their authority as 'custodians' of clan history, land, etc.

Cohen and Odhiambo take their argument a step further in *Burying SM: The Politics of Knowledge and the Sociology of Power in Africa* (1992) which chronicles the use of 'custom' in litigation and in particular the role that academic and other texts play in disputes as a 'factual' record of the past. While the case of SM is unique, the political use made of 'custom' in resolving disputes should

remind academics that disciplinary accounts are also read by laymen who apply their own criteria of 'validity' when they read our work.

While the narrative device of contrasting disciplinary and Luo perspectives makes for interesting reading, it obscures important issues. Perhaps the most obvious concerns the basis on which specific ethnographic examples are selected by the authors and inserted in the text to validate their argument. For example, in chapter five there is an extended account of a visit by a jogam (the intermediary sent to negotiate for the hand of a man's intended wife; pp. 103–9). Not only is it the only account of jogam provided, very little information is given about the context, the participants, etc. (i.e. information that would allow readers to assess the extent to which the event is 'typical'). Furthermore, the account is a partial and partisan recollection of one man.

The issue of perspective is complex. Whereas Ogot's history is written from a perspective that purports to be above and outside individual/clan concerns, this is not the case inasmuch as both books incorporate transcribed field data in the form of 'stories' and first person narratives from informants. As Atkinson has remarked of such practices, not only has 'a great deal of tacit and invisible authorial work in collating, editing and rewriting the personal narratives' taken place, the net effect is to produce a 'highly conventionalized' representation that supports the authors' perspective as against demonstrating the views of other social actors (1992: 25).[10]

Finally, Cohen and Odhiambo do not discuss their fieldwork, the methods they employed to gather data, nor their competence in Dholuo (though poems, proverbs and popular songs are cited in the vernacular and in English). Interestingly, such material does not seem to be drawn from extended observation nor is it discussed in relation to specific settings or events; instead it is cited to support the authors' arguments regarding the significance of particular kinds of discourse. Finally, the narrative format of *Siaya* and of *Burying SM* shows greater similarity to ethnographic accounts – in terms of the invisibility of the author in the text, the realist writing conventions adopted,[11] and the implicit assumptions about ethnicity, social boundaries, etc – than with *History of the Southern Luo*.

4. Conclusion: disciplinary methods, oral tradition and explanation

Over the past 40 years there has occurred a remarkable convergence between anthropology and history: both disciplines undertake fieldwork, both rely to varying degrees on working in local vernaculars, and both make use of oral

[10] Stamp, for example, looks at the burial of SM in terms of its implications for women (1991). The issue of perspective is complicated when an author is writing about his/her own ethnic group.

[11] A further issue concerns the intended audience for a publication. David Anderson has suggested that both books by Cohen and Odhiambo were primarily intended for a literate Luo audience, and specifically for a group of Luo 'professors' (comments made during the discussion of the paper).

testimony in writing their narratives (Faubion 1993). Indeed, Krech has argued that the convergence is driven by shared theoretical concerns and that disciplinary boundaries and genres have become 'blurred' (1991: 350).

However, while it is clear that the narrative/textual forms of writing employed by the two disciplines converge in their reliance on realist conventions of representation, considerable differences remain. A notable difference is found in the manner in which each discipline 'partitions' the field: historians adopt certain criteria to establish or define a specific genre of writing such as 'ethno-'/'ethnic' history (Krech 1991); in anthropology one finds 'regional' ethnographic traditions (Fardon 1990). Just as significant has been the manner in which each discipline addresses, or fails to address, key methodological issues including: linguistic competence in fieldwork; problems of translation and transcription of texts; the link between theory, method and data in the production of disciplinary knowledge; and the vexed question of textual writing strategies in representing ethnographic/historic 'others'.

I suspect that the very different way which the two disciplines have made use of oral tradition is a reflection of the recent turn to fieldwork by historians *and* of deep seated differences regarding how to find and interpret 'facts'.[12] In this regard Evans' claim (1997: 77) that in history 'facts thus precede interpretation conceptually, while interpretation precedes evidence' obscures a deep division between historians regarding the reliability of certain types of data and a stubborn reliance by those who prize 'oral tradition' over other sources.[13]

Significantly, nearly all the writers discussed in detail look principally at the verbal element/aspect of oral testimony and in doing so tend to adopt a literalist approach – reflected partly in an unproblematic transcription of an account onto paper – and gave little attention to allegory, metaphor or the social context in which 'tradition' was/is orally performed/ transmitted. Nor do they appear to be aware of the significant differences between different 'genres' of oral performance – i.e. between the verbal 'arts' (poetry, song, narrative recitations) and dance and 'theatrical' performances, much less a concern with metre and different types of audience participation.

Interestingly, the key issue here is not, strictly speaking, disciplinary. As Vail and White (1991) have argued, research on oral tradition has with few exceptions adopted the same approach. By affirming a problematic distinction between written/printed literature and oral performance, academic research has tended to focus on the narrative element of oral performances without examining the context in which they were performed which embues a narrative with meaning

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[12] The extent to which this reflects earlier disciplinary orientations – that anthropologists study people, while historians study documents – remains to be seen.

[13] Miller's *The African Past Speaks* (1980) argued for the unequivocal value of oral tradition above all other sources, while Spear (1981), Waller (1999) and others have argued that oral evidence should be integrated with data from comparative linguistics, archaeology, ethnography etc. and not treated as something unique.

(e.g. as satire, elegy, social commentary or praise). Equally problematic is the lack of attention to issues of power involved or reflected in the oral performance, which results in a reductive approach to the words/narrative as reflecting some historical 'truth' rather than as an evolving aesthetic or poetic tradition (p.71). Their ability to invoke 'poetic license', however, depends critically not merely on what is said but also on how it is said and performed to his/her patron, ruler and audience (Furniss & Gunner 1995).

It should be clear, therefore, that transcribing and transforming oral testimony into a written text is hugely problematic. First, the task is methodologically fraught and may divorce verbal elements from their cultural context. By becoming fixed in print, a commentary/tradition may loose its flexibility and its social value as an independent commentary on life. Just as important, some forms of oral tradition may be secret and access to it may be controlled because the knowledge encoded in it is power.

At the same time, academic texts circulate alongside other written (written and oral) accounts; not surprisingly academic writing may inform lay understandings and behaviour in unpredictable ways. The longevity and salience of the 'Hamitic hypothesis' was noted, as was the use made of published ethnographies in Kenyan courts. Similarly, Shetler (2002) draws our attention to the recent spate of written 'tribal' histories in south central Tanzania that purport to tell the history of an 'ethnic' group. Shetler's account is a useful reminder of the pitfalls that confront academics, and his proposed solution is worth serious consideration. He reminds us about the importance of the wider political context in which laymen – it is almost always literate men, not women – use writing to 'translate the past into a useable idiom for the present', one which often promotes particular political ends (ibid: 424). Secondly, he reminds us that we cannot control the way our writings are used or understood by others. Even so, Shetler suggests that the problems posed by laymen who use academic texts can be met in part by writing and publishing accounts in a format (e.g. an edited volume) that forces readers to compare preferred accounts (i.e. an 'ethnic' history) to other accounts in an attempt to 'reveal the constructed nature of tribal histories and to bring to light their common cultural roots' (ibid).

I would only add, that in view of the many limitations of academic research and of the myths that it has helped promulgate, we need to recognize that our claims to authority – in terms of our ability to represent other cultures and/or to capture and define an authentic African 'voice' – are limited. These limitations should be explicitly acknowledged in our accounts by discussing problems of research method, language competence, etc. and by stressing how comparative study is important (e.g. to understand shared culture, and that 'historical' accounts are constructed) so that lay readers are not left with the impression that research is only or even primarily about discovering cultural difference.

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REFERENCES

Altheide, D. & J. Johnson. 1994. Criteria for assessing interpretative validity in qualitative research. In *Handbook of Qualitative Research*, ed. by N. Denzin & Y. Guba, pp. 485–99. London: Sage.

Atkinson, P. 1992. *Understanding Ethnographic Texts*. (Qualitative Research Methods 25). London: Sage.

Berger, I. 1980. Deities, dynasties and oral tradition: the history and legend of the Abacwezi. In Miller (1980), pp. 61–81.

Cohen, D.W. 2001. African historians and African voices: Bethwell Alan Ogot and the changing authority of the 'African Voice.' In *African Historians and African Voices*, ed. by E.S. Atieno-Odhiambo, pp. 47–55. Basel: P. Schettwein.

Cohen, D.W. & E.S. Atieno Odhiambo. 1989. *Siaya. The Historical Anthropology of an African Landscape*. Oxford: J. Currey.

——. 1992. *Burying SM: the Politics of Knowledge and the Sociology of Power in Africa.* Oxford: J. Currey.

Crazzolara, J.P. 1954. *The Lwoo*. Verona: Editrice Nigrizia.

——. 1961. Lwoo migrations. *Uganda Journal* 25 (2): 136–48.

DeWaal, A. 1994. The genocidal state, Hutu extremism and the origins of the 'final solution' in Rwanda. *Times Literary Supplement* (July 1).

Eltringham, N. 2004. *Accounting for Horror: Post-Genocide Debates in Rwanda*. London: Pluto.

Emerson, R., R. Retz & L. Shaw. 2001. Participant observation and fieldnotes. In *Handbook of Ethnography*, ed by R. Atkinson, A. Coffey, S. Delamont & L. Lofland, pp. 352–368. New York: Sage.

Evans, R.J. 1997. *In Defence of History*. London: Granta.

Evans-Pritchard, E.E. 1965a. Marriage customs of the Luo of Kenya. In *The Position of Women and other Essays*, pp. 228–244. London: Faber & Faber.

——. 1965b. Luo tribes and clans. In *The Position of Women and other Essays*, pp. 205–227. London: Faber & Faber.

Evans-Pritchard, E.E. & M. Fortes. 1940. *African Political Systems*. London: International African Institute & Oxford University Press.

Fardon, R. (ed.). 1990. *Localizing Strategies. Regional Traditions of Ethnographic Writing*. Edinburgh: Scottish Academic Press; Washington D.C: The Smithsonian Institute.

Faubion, J. 1993. History in Anthropology. *Annual Review of Anthropology* 22: 35–54.

Furniss, G. & L. Gunner (eds.). 1995. *Power, marginality and African oral literature*. Cambridge: University Press.

Goody, J. 1995. *The Expansive Moment. Anthropology in Britain and Africa, 1918–1970*. Cambridge: Cambridge University Press.

Henson, H. 1974. *British Social Anthropologists and Language*. Oxford: Clarendon Press.

Heron, G.A. 1976. *The Poetry of Okot p'Bitek*. London: Heinemann.

Kuper, A. 1993. *Anthropology and Anthropologists. The Modern British School.* London: Routledge.

Lentz, C. 1995. 'Tribalism' and ethnicity in Africa. *Cahiers Science Humaine* 31 (2): 303–28.

Lindfors, B. 1980. *Mazumgumzo*. Athens, Ohio: Ohio University Centre for International Studies, Africa Program.

MacGaffey, W. 1978. African history, anthropology, and the rationality of the natives. *History in Africa* 5: 101–21.

Marcus, G. & D. Cushman. 1982. Ethnographies as texts. *Annual Review of Anthropology* 11: 25–69.

Miller, J. 1980. *The African Past Speaks. Essays on Oral Tradition and History*. Folkestone: Wm. Dawson & Sons.

Ogot, B. 1967. *History of the Southern Luo*. Nairobi: East African Publishing House.

Okot p'Bitek, J. 1971 [1979]. *Religion of the Central Luo*. Nairobi: Kenya Literature Bureau.

Oliver, R. 1959. Ancient capital sites of Ankole. *Uganda Journal* 23: 51–63.

——. 1963. Discernible developments in the interior c. 1500-1840. In *History of East Africa*, ed. by R. Oliver & G. Matthew, vol. 1, pp. 169–210. Oxford: Clarendon Press.
Owusu, M. 1978. Ethnography of Africa: the usefulness of the useless. *American Anthropologist* 80: 310–34.
Parkin, D. 1990. Eastern Africa: the view from the office and the voice from the field. In Fardon (1990), pp. 182–203.
Shetler, J. 2002. The politics of publishing oral sources from the Mara region of Tanzania. *History in Africa* 29: 413–26.
Southall, A. 1954. *Alur Society*. Cambridge: Heffer & Sons Ltd.
——. 1970. The illusion of tribe. *Journal of African and Asian Studies* 5: 28–50.
Spear, T. 1981. *Kenya's Past. An Introduction to Historical Method*. London: Longmans.
Speke, J.H. 1863. *The Journal of the Discovery of the Source of the Nile*. London: Denton.
Stamp, P. 1991. Burying Otieno: the politics of gender and ethnicity in Kenya. *Journal of Women in Culture and Society* 16 (4): 808–45.
Taylor, C. 1999. *Sacrifice as Terror. The Rwandan Genocide of 1994*. Oxford: Berg.
Thornton, R. 1983. Narrative ethnography in Africa, 1850–1920: the creation and capture of an appropriate domain for anthropology. *Man* (N.S.) 18: 502–20.
Urry, J. 1972. 'Notes and queries in Anthropology' and the development of field methods in British anthropology, 1870–1920. *Proceedings of the Royal Anthropological Institute* (1972): 45–57.
Vail, L. & L. White. *1991. Power and the Praise Poem. Southern African Voices in History*. London: J. Currey.
Vansina, J. 1965 [1961]. *Oral Tradition: A Study in Historical Methodology*. London: Routledge, Kegan & Paul.
Waller, R. 1999. Pastoral poverty in historical perspective. In *The Poor are not us: Poverty and Pastoralism*, ed. by D. Anderson & V. Broche-Due, pp. 20–49. Oxford: J. Currey.
Wolf, E. 1994. Perilous Ideas. Race, Culture, People. *Current Anthropology* 35 (1): 1–12.