Multilingualism at the Crossroads of Africa: A Response to Commentaries on "Remaking Late Holocene Environment of Western Uganda: Kansyore and Later Settlers in the Ndali Crater Lakes Region"

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We are grateful for the care and interest taken by the commentators on our study of human–environment interactions and interchanges of socio-linguistic groups in the Ndali Crater Lakes Region (NCLR) of western Uganda (Schmidt et al., 2024b). Our longitudinal research has involved significant local collaboration and has enhanced the growth of local archaeological capacity, nicely summed up by Reid (2024): "It is by some distance the most inclusive publication in the history of the archaeology of Uganda and suggests that a cadre of indigenous archaeologists may be beginning to emerge." Indeed, our interdisciplinary team included multiple generations of scholars and fundamental contributions by Ugandan scientists and community members.

As Schoenbrun (2024) observes, our research complements and adds to previous studies of linguistic groups linked to some phases of Kansyore pottery production: The Kansyore of western Uganda is related to Central Sudanic and Sog Eastern Sudanic speakers. Long overlooked is that the Kansyore or, more appropriately, the Oltome (Collett & Robertshaw, 1983; Robertshaw, 1982; Robertshaw & Collett, 1983; Robertshaw et al., 1983), of the last millennium BCE and early first millennium AD in western Kenya is also related to Sudanic speakers (the Rub/Kuliak) (Ehret, 1982, 1998; Schoenbrun, 2024). The unveiling of the different origins of these two facies of Kansyore is critical to the further development of archaeology in eastern Africa. We do not specifically address—and this is a substantial anomaly in eastern African archaeology—why the Kansyore (or any archaeological culture, for that matter) of western Kenya and northern Tanzania is represented as continuous when it has several chronological gaps of approximately 2000 years from its origins to the early first-millennium calAD (for examples of disparate dating see Dale & Ashley, 2010; Collett & Robertshaw, 1983; Jones & Tibesasa, 2022; Lane et al., 2006; Lane et al., 2007; Prendergast, 2010; Prendergast et al., 2014; Robertshaw et al. 1983). The issue of significant discontinuity hovers, like a pale, over any discussion of Kansyore.

Our study of the Kansyore of western Uganda as a facies dating to the early first millennium highlights the prescient research of Collett and Robertshaw, 1983, which recognized that the later phases in Kenya differed significantly from Kansyore Island, western Uganda. Moreover, as Reid observes, this research revises Kansyore studies in eastern Africa, with the Kansyore of western Uganda, "…associated with mixed farming communities as opposed to the opportunistic exploiters of natural resources near fast flowing rivers and lake shores [in western Kenya]."

The core contributions of this regional scale archaeological research include unique settlement patterns, artifact finds (particularly related to the first millennium AD), including discussion of 15 sites of the more than 60 archaeological sites documented, multiple newly engaged ceramic traditions, food remains, and unique burials alongside evidence for environmental change itself drawn from sites and many caldera lakes and marshes in the NCLR. From multiple field seasons of research overlain onto the backdrop of broader interdisciplinary scholarship, the project re-represents the archaeology of Uganda and the region immediately surrounding the NCLR. The often fragmented (in both space and time) people and pasts of central-eastern Africa are reinterpreted through empirical evidence from archaeology and shown to be entangled in multiple ways.

To reprise, the project findings remake extant understandings of Kansyore and Boudiné ceramic traditions, as well as add significant contextual nuances to Transitional Urewe as a ceramic practice engaged in fluid experimentation with other traditions, specifically, Kansyore. Reid acknowledges the consequences of the now firm dating of Boudiné ware, resulting in "a

radically improved understanding of the ceramic, possibly the most important cultural contribution of this NCLR work." We appreciate this recognition but hasten to add that it is only one of many other equally important contributions. What stands out in the history of Boudiné ware is that it arises from Urewe ceramic producers who lived in multilingual communities, sharing ideas with their neighbors and kin—Sudanic speakers who produced Kansyore ceramics.

In our conclusions, we state our interpretation that the Bantu speakers of the NCLR were multilingual, a point that Schoenbrun also appropriately makes in his comments and that we want to reinforce. Living in mixed, contiguous settlements, Bantu speakers would have found, by necessity, that speaking Sudanic languages proffered significant advantages, as they adopted foodways (seen in linguistic borrowings) and associated pottery technologies—both physical and decorative. The archaeological data speak strongly to the borrowing of Kansyore ceramic elements, particularly in funerary settings. Thus, similar elements in ceramic traditions, among other material elements such as subsistence practices, support a millennium of borrowing indicative of multilingualism in communities.

These robust interpretations of a social mosaic in the NLCR counter the framing of cultures as ethnic boxes that characterize earlier problematic scholarship. Culture change, innovations (of all sorts, material and immaterial), and collaboration characterized ancient African communities in the NLCR. However, Kessy (2024) conflates ethnicity with historical linguistic groups, which is best captured in the concluding sentence of his commentary, "…the authors should have discussed the interactions that took place in the NCLR confining themselves to the use of archaeological cultural types such as Urewe/the Early Iron Age and Kansyore, *instead of focusing on specific ethnic groups such as Bantu, Central Sudanic or Eastern Sog* [sic] *speakers* [emphasis ours]." Ironically, we did precisely what he suggests in his commentary: we meticulously discussed interactions among different groups—linguistic groups, *not ethnic groups*—using their material cultural remains. The learning, practice, and collaboration are clear in the material and language evidence.

Schoenbrun helpfully argues that multilingualism is a bridge to our points about social entanglement in the NCLR, poignantly observing that this research "…clarifies the 'muddier' picture' of Bantu language expansions in Grollemund et al. (2023)…. [and] *moves beyond stable, bounded groups…*[emphasis ours]." In other words, ethnicity is moot, it is irrelevant to our construction of the pasts of multiple, entwined groups. The mutability of identity in a social setting such as the NCLR is profoundly acute, most vividly expressed materially in the development of Boudiné ware but also in ritual practices such as topographic placement of burials on western rims of volcanic craters and the envelopment of the dead in ceramic vessels.

One of the most significant contributions that our study makes, we believe, is to date extensively and to characterize the cultural setting and meanings of twelve human burials over the entire spectrum of multilingual interaction in the NCLR as well as during the Bigo period, noted for its dearth of such evidence. Reid comments on the importance of the burials for eastern African archaeology. Otherwise, the burials are curiously side-stepped by the three reviewers. Kessy does challenge our interpretations of the burials, suggesting that we should have ended our characterization with descriptions only. Our response to this *disembodied* approach is that part of the archaeological mission is to use informed analogy, without the assumption of historical continuity, to give life to burial practices in the past. In this instance, we see burial placement on the western rims of volcanic calderas as celestial rituals of renewal, with the rays of the rising sun first striking the western rims—a practice that carried into the

mid-second millennium AD with Bigo communities also burying their dead on western rims and on high remnant cones. We would be remiss not to draw attention to mid-second millennium AD communities in the Great Lakes that practiced rituals of renewal linked to the death/birth cycle of the new moon (Roscoe, 1923; Schmidt, 2017; Schmidt & Arthur, 2018). Celestial renewal is a deeply embedded symbolic practice that begs to be integrated into interpretations in compelling and informed ways, not sidestepped or dismissed. Some of these ritual practices remained vital into the second half of the last century (Schmidt, 2017, 2018).

Kessy and Schoenbrun both focus on Bantu migrations, though we devote only passing mention—suggesting a possible northerly route—to this highly contested issue. Kessy, for example, discusses Bantu migrations at length, including to southern Africa, arguing that the Bantu expanded rapidly, thus "overpopulating…the East African region as well as making an extraordinary rapid migration to South Africa." This discussion and other asides into the rapidity of pastoral vis-à-vis Bantu farming expansion and the anthropological inadequacy of genetic studies that use linguistic and archaeological data are not relevant to our study.

Despite our insignificant treatment of Bantu migrations, Schoenbrun devotes attention to advocating a southern passage via the Sangha River in the DRC (Grollemund et al., 2023; also see Bostoen et al., 2015). But Seidensticker (2024) has conclusively shown that settlement dates in this zone refute any hypothesis for its being the corridor of Bantu population movements towards the south (also see Clist et al., 2023). Our focus was the historical linguistic evidence presented by Schoenbrun (1993, 1998) that some Bantu speakers dwelled near the western foothills of the Rwenzori Mountains beginning about 500 BC. They did not arrive by 500 BC at the Rwenzori Mountains via a southerly route. What is central to our understanding of multilingual communities *is the dwelling time* of Bantu speakers alongside Sudanic speakers and hunter/gatherer communities around the Rwenzori Mountains. Yet, Kessy writes, "The authors painstakingly struggle with the interactions and movements of the Bantu in the NCLR." While we pointedly address Bantu speakers' interactions with Sudanic speakers—key to showing the syncretism manifest in the material record—we say nothing about movements of Bantu speakers within the NCLR. Our interpretation treats the Bantu expansion and its colonialist orientation as an assumed problem reliant on mummified identities and replaces that model with empirical evidence within a comparative regional understanding. We do so purposefully by highlighting this *region* (NCLR) and the immediately surrounding area, without prognosticating about eastern and southern Africa broadly.

We present evidence that departs significantly from the paradigm of Urewe ceramics and iron production, replacing that paradigm—within the NCLR—with Transitional Urewe *without* iron production. This aspect of our empirical study concerns Kessy: "The difficulty in identifying the cultures was also increased by the lack of evidence for the association between basic iron production and classic Urewe ware during the Early Iron Age in the Ndali Crater Lakes Region." This remark captures how deeply engrained the Classic Urewe/iron production package is in thinking about Late Holocene archaeology in Africa. In fact, the absence of classic Urewe and iron did not complicate our identification of different cultures but instead provided a much-welcomed difference that added considerable interactive nuances to our study and freed us from the silo of the usual type-culture.

Schoenbrun speculates that Urewe ware may have its origins with people associated with Sudanic languages—a position that is impossible to sustain with archaeological evidence, as is the notion that Urewe ware came from the north, a speculation derived from Van Grunderbeek's (e.g., 1988) unsupported hypothesis. It was outside the scope of our study to

address the origin of Urewe ware, but since its origins are raised, we must highlight that there is a community in eastern DCR that made Urewe-like ceramics recently (Soper, 1971). These observations point to the need for much deeper research into the ancient history of the eastern DCR as well as northern Uganda—as Reid importantly suggests, as well as to an enduring tradition of pottery-making among Bantu speakers. What little research has addressed ceramics (e.g., Mercader et al., 2000) in the eastern DCR is limited to a rock shelter without a deep ceramic chronology.

We enthusiastically subscribe to Schoenbrun's idea of multilingualism, but we do not hold to the idea that one multilingual process began in the last millennium BC with the making of classic Urewe, iron smelting, and later, Boudiné. We assume that Schoenbrun's reference is to Rwanda, where Boudine is found with classic Urewe (e.g., Giblin, 2013; Hiernaux & Maquet, 1960; Nenquin, 1967). The chronological evidence for Boudiné's development is insufficient to sustain this hypothesis (see Giblin, 2013). In the NCLR, classic Urewe and iron smelting are absent and Boudiné is not linked to either. The process we identify is linked to producers of Transitional Urewe (TU) and the genesis of Boudiné in the presence of those making Kansyore ware. We mention in our article that there is a possibility that Kansyore ceramics were documented in Rwanda and named as Type C and then renamed C Ware (see Nenquin, 1967). If so, this may suggest similar multilingual interactions also occurred there. But iron working has no role in this syncretic process. The propositions proffered by Schoenbrun, however, suggest an important vein of speculative thinking that needs deeper exploration as we continue to inquire into this important frontier of interaction.

Two commentators paid little attention to the meticulous descriptions of material culture that underwrite our diagnostic characterizations and extensive dating of Kansyore, Transitional Urewe, Boudiné, and Bigo period settlements and substance. Understandably, short commentaries on lengthy published works are by nature limited and highlight specific items. Given that AAR is an archaeology journal, it is unfortunate that two reviewers do not engage much of the remarkable material evidence in depth. Kessy and Schoenbrun also do not address the documented, long-term environmental changes in sediment cores collected during the project nor perhaps the most unique cultural findings from the NCLR: settlement patterns and landscape use, especially during the first millennium AD. The latter comprehensive treatment, intricately linked to the documentation of environmental change, allows us to precisely sort out climate and anthropocentric causes for environmental change (also see Schmidt et al., 2024a, 2024b). For decades environmental scientists have struggled to explain non-climate-induced changes but have flailed in their attempts to marshal substantive evidence to support human-induced changes. This study importantly does that at a regional scale.

Reid importantly addresses some of our study's material evidence, pointing out the significance of burials and the development and dating of Boudiné ware. As well, he reacts to the notion that populations were once attracted to the NCLR by drought conditions during the late thirteenth century AD. He maintains that there is no evidence for a disruption of occupation or abandonment of grasslands to the East of the NCLR. We suggest, though, that the paucity of comprehensive regional research along the Katongo River and elsewhere in the region denies this claim. We readily agree that this is a question that needs to be built into future research. But there is little question that the *profound drought* conditions (Saulnier-Talbot et al., 2014, 2018) of the latter half of the thirteen century would have stimulated the search for more stable environmental conditions found in the NCLR. Moreover, to argue for the stability of the region based on contemporary climate conditions in the Mawogola grasslands is an untenable analogy for six hundred years ago. Reid's notes, correctly, that Bigo dates relatively later [than Ntuusi].

The populations that departed the Mawogola grasslands in the fourteenth and fifteenth centuries AD, however, need not have come from Ntuusi, Munsa, or Bigo nor any other earthwork site in this vast region. Finally, our use of "Bigo" is a generic nomenclature that is meant to represent later, mid-2nd millennium populations from that vast eastern region through which the Katonga River runs.

We appreciate Reid's attention to the archaeological and environmental substance of our research, but we disagree with several of his specific observations: (1) the discussion about our analysis of different roulette decorative applications requires the need to correct the notion that we attempted to distinguish Middle Iron Age from Late Iron Age roulette wares. We *did not* make such a distinction. Instead, we explained in detail that we distinguished different *contemporary* Middle Iron Age rouletted applications *within the same community*, suggesting different points of origin for neighbors; (2) the observation that we use roulette terminology that does not draw on Soper's (1985) typology captures our purposeful use of terms that better describe the rouletting process, as per field experiments and observations by Schmidt (1974). Plaited grass (the common medium using flat grass blades) is more precise than "knotted" and twisted grass is a more precise descriptor than "twisted cord" for this and nearby regions; (3) as Reid observes, lipid analysis established the presence of ruminant adipose tissue in a Kansyore vessel at the KA-1 site, but not the KA-4 site where the second sample is not Kansyore. It is from a Boudiné burial urn, showing the use of ruminants in funerary rites; (4) we do acknowledge the role of NMK researchers and include one as a co-author in our article. We question any casting of doubt about their capacity to distinguish between *Bos taurus* and the African Forest Buffalo (*Syncerus caffer nanus*). It is not surprising that the African Forest Buffalo is not documented archaeologically in the Mawogola grasslands. This species of buffalo does not populate grasslands and prefers a humid forested environment such as that found around the Rwenzori Mountains and in the Congo Basin; (5) we inform readers that our samples of faunal materials were small, but the modest samples do not diminish the significant trends across sites and through time: domestic animals are dominant and wild species play a supplementary roll. Critically, the types of wild animals hunted during the mid-2nd millennium changed significantly from the mid-first millennium AD to larger mammals requiring cooperative hunts, for example, African Forest Buffalo and Giant Forest Hog (*Hylochoerus meinertzhageni)* (Besigye et al., 2024)—an indisputably important change that accompanies human-induced modifications of the physical environment. Linguistic evidence amplifies our interpretation of the presence of herd animals during the first millennium AD. Thus, we object to the rejection of the Kansyore and Transitional Urewe peoples of the NCLR as herding domestic animals. Multiple sources of evidence affirm their presence, including widespread forest clearance for pastureland. Nor did we claim they were "pastoralists." Rather, they were agropastoralists; (6) we accept the comment that burning of bones may be associated with a variety of causes. However, overlooked are our cross-site observations for consistent burning of the same body parts, not usually associated with ritual contexts nor with activities other than consumption. Burned bones appear to mark a cultural preference as they occur almost exclusively during the mid-second millennium at Bigo period sites.

Reid's commentary mentions that the development of archaeological research in Uganda was impacted by political vicissitudes. There are now contrary trends. The engagement of the Uganda Museum in this research has involved investment of personnel and financial resources—both auspicious for future research. We share Reid's optimism for Ugandan-led research in the future, yet we depart from his supposition that linguistic models are passé and will be ignored by future scholars more interested in national heritage issues or those who see historical linguistic studies as reifying colonial concepts of tribalism (as per Kessy). Historical

scholars once held great hope for articulating archaeological evidence with historical linguistic studies (e.g., Ehret, 1998; Ehret and Posnansky, 1982; Schoenbrun, 1998).

Reid's comment nonetheless does capture a trend moving away from that mission because of competing foci followed *by all nationalities* engaged in archaeological research in Africa (with notable recent exceptions (e.g., Bostoen, 2020; de Luna & Fleisher, 2018; McMaster, 2005). Hopefully, by showing the significant promise of a revitalized practice, this research demonstrates the potential such a remarriage has for building more complex histories and also for curricular enrichment to counter notions that linguistic histories somehow concretize tribal identities. Schoenbrun's comments are particularly fruitful, presenting lines of inquiry that promise to join archaeology with historical linguistic studies in a manner that moves beyond the discourse of the 1970s to 1990s. The NCLR archaeological data present an extraordinarily exciting fit with the historical linguistics of the Albertine Rift, leading to even greater hope that amplified regional research will build on our research foundation.

The deep time societal power relations among communities (first-ness instead of supremacy) and the colonial and postcolonial politics of these relations are subjects that may be engaged in the future as the result of the evidence we present in our article and other forthcoming scholarship, such as preliminary ancient DNA evidence that addresses the mixture of linguistic, cultural, and genetic traits in the NCLR and greater Rwenzori region (Brielle et al., 2024). We want to emphasize that the specific material traits selected for borrowing across ceramic traditions were not the *raison d'être* of our research. The borrowings (such as flat and decorated urn bases and ceramic technology found in Boudiné ceramics) only became known during our analysis, illustrating how archaeology may change the way scholars think about people and deep time histories in the NLCR. The recognition of both environmental and cultural mosaics initiates a fertile springboard for further re-representations rooted in African linguistic and material innovation, which our colleague Shoenbrun emphasizes as a fundamental contribution. This regional scale research takes the first important step to rewrite the history of people and innovation through an Africanist approach that recognizes social entanglement, sharing, and innovation as characteristics of Uganda's NLCR during the first millennium AD and beyond.

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