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LETTER TO THE EDITOR

Lake-level changes and hominin occupations in the arid Turkana basin during volcanic closure of the Omo River outflows to the Indian Ocean – comment on the published paper by Boës et al. Quaternary Research (2018), Vol. 91.2, 892–909

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In a recent paper, Boës et al. (2018) propose a reconstruction of paleolake water-level changes in the Turkana Depression from ca. 2.4 to 1.7 Ma. Their approach, based on the geochemical analysis of major elements, is promising, although only a very minor interval (2.26–2.19 Ma; table 2, fig. 7 and 8 in Boës et al., 2018) was actually analyzed. Additionally, Boës et al. (2018, p. 4) indicate that, "for the stratigraphic interval of 15 m below and 24 m above the [Kay Behrensmeyer Site (KBS) Tuff], our cross section is completed by that of Nutz et al. (2017)"; this cross section records the time interval between ca. 1.95 and 1.72 Ma. We failed, however, to identify any methodically presented geological and/or geochemical data for either the 2.40–2.26 Ma period or the 2.19–1.95 Ma interval. From our point of view, this is a critical point that seriously weakens the credibility of this study.

That being said, we limit our comment to Boës et al. (2018) by focusing on the portion of their work that directly overlaps with our published data (Nutz et al., 2017). An examination of their paper shows that our data and conclusions are neither reproduced accurately nor summarized properly. Our main criticism is that major changes to the original interpretations have not been explained. As such, we wish only to avoid the diffusion of any erroneous messages that could be associated with our initial publication. Hence, via this comment, we take the opportunity to point out and discuss the main differences between our original work and its transcription into Boës et al. (2018). This focused comment does not mean that we agree with the rest of this paper.

First, a prominent sandy bed isolated within the offshore lacustrine muds and located at ∼96 m on their stratigraphic column (fig. 3 in Boës et al., 2018) is marked as "Foreshore" by Boës et al. (2018). This sandy bed (∼10 m below the KBS Tuff), however, appears to be one of the interpreted turbiditic beds in the original work (Nutz et al. 2017). This modified interpretation has wide implications for paleolake reconstruction, although this is neither mentioned nor supported by any geological evidence. By altering the original interpretation, Boës et al. (2018) delineate a new regressive-transgressive cycle that does not exist in Nutz et al. (2017). Indeed, a vertical transition from offshore muds to foreshore sands and then back to offshore muds would require a significant and abrupt lake-level fall and subsequent rise. In the absence of any supporting evidence, we argue that our original interpretation (i.e., an offshore turbidite bed) remains the only supported interpretation. Consequently, the "pre-Lorenyang" and "Lorenyang" phases (*sensu* Boës et al., 2018) are not separated by a lowstand episode, but rather they represent a single continuous lacustrine highstand.

Second, the stratigraphic column (fig. 3 in Boës et al., 2018) has not been complemented accurately with sections from Nutz et al. (2017). Boës et al. (2018) ended the column at ∼140 m (i.e., ∼30 m above the KBS Tuff). They attribute the ∼30-m-thick clastic/bioclastic interval above the KBS

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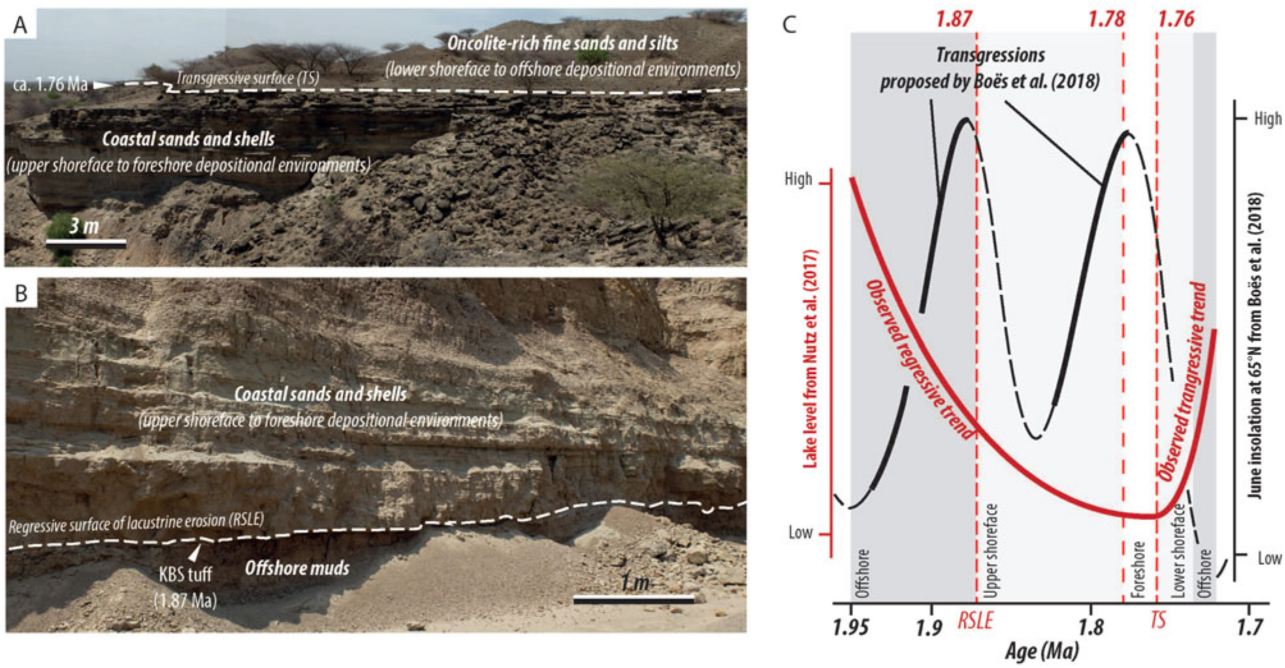

Figure 1. Reconstructed first-order lake-level variation in the Turkana Depression between ca. 1.95 and 1.72 Ma: a sedimentary geology-based curve (Nutz et al., 2017) versus the inferred insolation curve (Boës et al., 2018). (A) Exposure showing "coastal sands and shells" (Nutz et al., 2017) separated from "oncolite-rich fine sands and silts" by a transgressive surface revealing a transgressive trend starting at ca. 1.76 Ma following a relative lowstand estimated between 1.78 and 1.76 Ma. (B) Offshore muds overlain by coastal sands and shells that reflect the regressive trend identified between 1.95 and 1.78 Ma. (C) The red curve shows the interpreted first-order trend of paleolake Turkana levels between1.95 and 1.72 Ma from Nutz et al. (2017). Note that second-order modulations of paleo-lake levels related to precessional cycles are not represented here. In black, the insolation curve reported from Boës et al. (2018) and their inferred evolution of lake level (from fig. 9B in Boës et al., 2018). In their study, Boës et al. argue that Lake Turkana lake levels paralleled the 100-ka insolation curve in response to greater orbitally forced monsoon-related inputs from the Omo River. Lake-level reconstruction based on the observed geological facts between 1.95 and 1.72 Ma clearly contradicts such a hypothesis, as first- order lake-level evolution does not fit with the 65°N insolation cycles. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Tuff to sedimentation related to the foreshore followed by backshore depositional environments. The original interpretation of this section by Nutz et al. (2017) is much different. Above the KBS Tuff, we identified ~20 m of upper shoreface and then foreshore deposits (referred to as "coastal sands and shells") overlain by ~8 m of lower shoreface grading to upper offshore deposits (referred to as "oncolite-rich silts and sands") that record the onset of a new lake-level rise. More generally, our sequence analysis of the 1.95–1.72 Ma interval demonstrates a first-order regressive trend from ca. 1.95 to 1.78 Ma and a subsequent first-order transgressive trend from ca. 1.76 to 1.72 Ma, thereby revealing a lowstand between ~1.78 and ~1.76 Ma (Fig. 1). Boës et al. (2018) propose two general transgressions, the first ca. 1.93–1.88 Ma and a second ca. 1.82–1.78 Ma (fig. 6B and 9B in Boës et al., 2018). These are separated by two general regressions from ca. 1.88–1.82 Ma and again from ca. 1.78–1.72 Ma. As such, they propose lowstands centered on ca. 1.82 and ca. 1.72 Ma and two highstands centered on ca. 1.88 and ca. 1.78 Ma (Fig. 1). This interpretation differs markedly from our original work. In the absence of any new data and explanation, we do not understand the origin of such a significant modification of our original interpretation. We understand that their unexplained modification allows for a convenient fitting of their hypothesized lake-level curve and their theory of a direct relationship between highstands and the 100-ka insolation cycles at 65°N. Geological facts, however, contradict such statements (Fig. 1A and B).

Third, the transformation of our original geological section raises suspicions and fosters a more critical view of their hypothesized lake-level curve (fig. 6A, 6B, and 9B in Boës et al., 2018). At least for the time interval between 1.95 and 1.72 Ma, the highstand does not coincide with an insolation maximum at 65°N; a perfect example is the 1.78 Ma insolation peak that rather coincides with a lowstand (Fig. 1). In this case, a lowstand separated by two relative highstands is difficult to attribute to any forcing other than climate (see the discussion in Nutz et al., 2017). Thus, we seriously question the direct correlation between 100-ka high insolation peaks at 65°N and paleolake highstands. We further question the use of the absence of correlation between 100-ka high insolation peaks at 65°N and paleolake highstands as evidence of non-climatic forcing.

In conclusion, once published, we believe fully that our results are free to be reused by colleagues; thus, we acknowledge Boës et al. (2018) for their confidence in incorporating our work. Our main concern, however, remains that our data were not properly transcribed. As such, we argue that the lake-level variation curve of Boës et al. (2018) is questionable, and as such, their ideas regarding the controlling factors of lake-level changes likely need to be reexamined.

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