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Climate of the Past Discussions

Interactive comment on "Holocene glaciation in the Rwenzori Mountains, Uganda" *by* Margaret S. Jackson et al.

Anonymous Referee #3

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This short manuscript is clearly written and well-organized, and reports a small but interesting new set of twelve Be-10 ages from glacial moraines, boulders, and bedrock surfaces in the Rwenzori Mountains. The authors present some reasonable interpretations of the Holocene glacial history at their field sites based on their data and field observations. These results are then compared with regional climate proxies and other glacial records in East Africa, and also to tropical glacier records in South America. Overall, I think these newly reported findings from the Rwenzori are valuable and add important knowledge to the glacial and climate history of tropical East Africa. However, the number of new exposure ages is quite modest, and as such, it is difficult to extrapolate from these to make strong arguments about commonalities with Holocene glacier records in South America and pan-tropical climate forcings. I support the publication of

these results after appropriate revisions, but I urge the authors to be more cautious and realistic about the limitations of inferring global-scale correlations and climate forcing mechanisms from a small data set.

More specific comments and critiques are listed below. I hope the authors find these constructive, and I encourage them to address and resolve these in order to improve their manuscript.

Lines 27-28: That's probably not a fair statement these days, at least outside Africa, as there have been a number of studies and reviews of tropical glaciation in recent years.

Line 68: List in chronological order. It's also curious that the Late Holocene is not regarded here as a time period of interest - especially in light of statements about this work's relevance to modern/future climate change.

Line 82 / Figure 1: The satellite image in panel b is not an acceptable substitute for a proper glacial-geomorphic map. The moraines and overall topography are very hard to see. I suggest replacing with a DEM or contour map if available, overlain by a more detailed map indicating glacial features and other relevant landforms in these valleys. Without a well-labeled glacial-geomorphic map, the text descriptions of these field areas are very hard to follow.

Line 93: That is very inclusive. What kind of crystalline rock, exactly?

Line 111: This is the first of many citations to an in-review manuscript that is not currently accessible to reviewers. Because many interpretations here are reliant on context and support from the results in the unavailable in-review manuscript, it is not really possible to properly assess this new manuscript. In fact, the frequent references to the in-press manuscript and the importance of those findings to the interpretation of the new ages reported here raises the question of why these data were not all reported together in a single paper.

Line 123: I assume these calendar ages are recalibrated from the original radiocarbon

data using updated calibration curves, but the details of the calendar age estimation need to be explained here.

Line 190: How is the landslide dated? How reliable is that age?

Line 229 / Table 1: Density and erosion columns can be eliminated since the values are uniform for all samples. Instead, just note these values in a footnote. Also, how close are the three boulders (RZ-12-22, 24, 25) with indistinguishable latitude-longitude coordinates? A field photo would help show the field relations.

Lines 241-244: It's an odd choice to show Be-10 concentrations instead of apparent ages for the bedrock surfaces on Figure 4, even if there's a suspicion of complex exposure scenarios. This forces readers to find the ages in Table 3 (where they are reported) to gain some sense of the exposure durations and how they fit in with the other ages on the map. Also, if isotope inheritance is the concern, then that same issue could also potentially apply to the boulder surfaces - as acknowledged in the discussion.

Line 246 / Table 2: Why are the isotope ratios in a different table than the concentrations (and the ages, for that matter)? I suggest some consolidation of the three tables, ideally into one table if possible. The first three columns are identical in all of them, other columns can be eliminated (as noted earlier), and it's inconvenient to have to retrieve data from individual samples spread across three different tables. Also, given that the sample ratios are just over one order of magnitude above the blanks, it is important to consider how well these blank values are known. If they are all prepared from the same spike, it appears they vary quite a bit - and are therefore known with far less certainty than implied by the analytical uncertainties on individual blanks. This is a potentially important source of uncertainty for the youngest samples that's not being properly represented.

Lines 256-257: This looks to be a vague way of saying the boulder surfaces show few signs of erosion, and does not provide any useful information about the condition of

the surfaces. Is it true erosion is not evident? I'm skeptical, as the sentence after this implies there may in fact be considerable erosion. Please provide more detailed descriptions of the quality and appearance of the sampled surfaces in the first paragraph of this section Also, please add some photos of the sampled boulders and surfaces - I would say at least a couple boulder/bedrock photos are required in order to show readers the sample sites.

Line 276 / Figure 3: What is the vertical exaggeration in this figure? Assuming there's none, the Speke moraine would appear to be on a very steep and unstable location right beneath big cliffs that are prone to rockfall. In other words, it looks to be a risky place for exposure dating. This might not be as bad as it looks if the VE (if there is any) was turned down.

Line 301 / Figure 4: See earlier comment. It's very odd to show isotope concentrations rather than apparent ages for the bedrock in this figure. Please show the ages instead.

Lines 324-327: Not sure I agree with this interpretation. Steep ice-contact proximal slopes and more gentle ice-distal slopes are very typical of young / recently abandoned moraines, including those found in locations only minimally or not affected by rockfall. There's no evidence presented here ruling out the possibility of large volumes of debris transported sub- and englacially to the glacier margin as the moraine was being constructed.

Lines 328-329: See earlier comment. How is it known that the sampled boulders were deposited by the glacier, rather than coming from rockfall from the upslope cliffs that came to rest in post-glacial times?

Line 426: Rather than "dominate" consider replacing with "result in negative"

Lines 434-436: You had said earlier that you would not use these two ages in any subsequent interpretations. If that's the intention, this speculation should be omitted here.

Line 445: Replace "fact" with "interpretation"

Lines 511-513: This is a very far-reaching statement to support based on the modest number of new ages presented in this manuscript. The data are especially sparse for the Late Holocene; only 4 ages on one moraine segment are leaned on as being representative of the timing of Late Holocene glaciation in the East African tropics, which is a big extrapolation. And while tempting, it's an even bigger jump to then suggest these ages support a common pan-tropical climate forcing. Apart from the sparse chronology issue, there's also the uncertainty of what specific climate controls are dominating glacier mass balances in various tropical regions on separate continents and over a range of scales from regional to single-valley. The authors favor temperature as the main driver but acknowledge some major untested assumptions, hence a lingering enigma. I encourage the authors to dial it back here, and not go much further than to say their ages hint at similarities in Holocene glacial fluctuations in tropical South America and East Africa, but that a lot more age control (and more modeling, as they suggest) is needed to explore this further.

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