Climate
of the Past

Discussions

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

Anonymous Referee #2

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The paper by Jackson et al. provide twelve new 10Be based surface exposure ages from the Rwenzori Mountains of Uganda to place new constraints on late Pleistocene and Holocene glacial changes. Overall, I find the paper easy to read and think the results could be of interest to a broad audience such as the readership of Climate of the Past. However, I think the authors need to address several shortfalls before the paper is ready for publication. Below I have provided my line number edits and comments.

Abstract: - While I agree that understanding tropical glaciation in the past is a worth-while endeavor, I think the authors have overstated how much we do not know about tropical glaciation over the Holocene. Certainly, there is more than "relatively little" known in my mind. I suggest trying to be more quantitative about what they are refer-

ring by "little" so the reader can better understand their argument.

Line 171, 199, etc.: The authors cite the work of Jackson et al. in review in multiple places within the paper. I am not sure about CPs policy on citing in review or un-published peer review work but from a reader's perspective this seems unusual and unhelpful since I cannot reference back to the paper to understand what is being cited. I also think the authors are relying quite a bit on this other data and wonder why the two papers have been separated from one another given these data from the in review paper are somewhat critical to the author's arguments.

Figure 2: It would be useful in Figure 2c if the authors provided some indication of the moraine crest for the reader – perhaps some arrows.

Figure 2: In Figure 2c there appear to be many trees within the photo. I'm confused by this photo given that the authors state on lines 250-252 that vegetation is sparse above 4000 m and that vegetation cover was not used to correct the 10Be dates. Some explanation seems warranted here since there appears to be clear signs of heavy vegetation cover in this region. Is this the ∼300-400 year Speke moraine, or is it an older moraine?

Lines 231: The process blank 9/10Be ratio should be provided as well as the carrier name or stock number. This is important for historical documentation.

Lines 238-240: The authors should say why they choose one over the other and they should say why it does not alter their conclusions. This would help the reader more fully understand their position on the matter.

Line 240-243: While I understand why presenting the ratios might be justified, I think it makes the several parts in the manuscript confusing for the reader doing it this way. Especially, when the authors then use the calibrated ages later on in the study (see the Discussion). I suggest providing the calculated ages and simply explaining why they are likely complex ages related to prior exposure.

Line 248-250: Provide a citation for the daytime temperatures and solar radiation that are being referenced. As-is, I find the snow correction argument weak and it needs more justification which I think a few references could help with.

Line 277: In the methods, the authors state that they use the LSDn scaling (Line 239) but throughout the text and starting here they seem to be using the St scaling. This needs to be corrected in the paper and/or table.

Line 288: It would be useful to the reader if the authors provided some photos of the boulder that they are referring or some dimensional information about the boulders. This is important information to convey to the reader since the classification of boulder is large (i.e. > 256 mm).

Figure 4: - I recommend providing ages instead of ratios for the three bedrock samples - In the legend and the boxes, it is hard to differentiate between the colors. Either makes these lines thicker and/or make the colors standout more between each other. - The sample ID provided in the boxes do not match the samples numbers from the tables. I'm therefore not sure what the samples numbers in the boxes represent and would suggest matching them to the tables for reader. - The authors provide data from an in review paper. I find these data quite helpful for their argument but unfortunately because it is in another paper I cannot reasonably evaluate the data. Again, I wonder why the authors have split the data between the papers but think until the in review paper is published it makes these data less convincing to the author's arguments.

Line 313: When the authors say "glacial deposits" do they also mean till? Is it bare bedrock?

Line 314: It would be useful to see a more zoomed out view of the Bujuku valley so the ~11 ka moraine vs. the Speke moraine can be seen.

Line 315: Cavagnaro, 2017 is a undergraduate thesis. I'm not sure if it is appropriate to cite work that is not peer reviewed.

Line 317: While the landslide is not disturbed, is it possible it occurred onto the glacier and then melted out? Without more information about the landslide and how it was dated, it is hard as a reader to evaluate if this is true without having a photo of the landslide or more information beyond the unpublished thesis that I was not able to access.

Line 322-325: Here I strongly disagree with the authors. Steep ice-contact slopes and more gentle distal slopes are the norm for moraines and especially true for young moraines that are ice cored and late Holocene in age. Therefore, I don't understand how the authors conclude the moraine was related to rock fall. Is the moraine highly sorted? Is it possible the "moraine" is in fact a protalus rampart (e.g. Ballntyne and Kirkbride, 1986)?

Line 326: Based on the imagery I cannot see the fan-like slopes. Is it possible to provide some zoomed in images of what is being referred?

Line 327: If the moraine is in fact a debris slide onto the slope, then how can the ages be interpreted as the timing of moraine abandonment? It seems like the boulder ages should be predating the timing of the glacier retreat. This needs to be explained.

Line 339: The ages are now being presented for the bedrock ages. The authors need to decide if they are going to present the ages or the ratios. I suggest the former.

Line 340: What evidence is this based on (photos, documentation, etc.)?

Line 357-358: This argument is reasonable but the assumption that LGM ice core erased all prior exposure needs to be restate here for the reader. Also, it would be worth providing some justification about why this assumption is more reasonable than inheritance being pervasive.

Lines 367: Again, some boulder photos would go a long way.

Line 373: The author might comment on why there is no late Holocene moraine at both locations? Is this an elevation or aspect issue?

Line 402: Again, it is hard to evaluate new and prior work that is not published yet.

Line 437: It is not clear what the authors mean. Presumably the glaciers respond to temperature in the same way – more temperature means more ablation and vice versa. I think other factors come into play here (hypsometry, winter precipitation, energy balance) and therefore it isn't that they respond non-linearly it is that other factors are important and that temperature is not the only driver of ice mass position.

Line 538-540: If the glaciers are similar tropic wide, then I would expect other climate archives to also reflect this. What about the PAGES 2k reconstructions or the Marcott et al. 2013 stacks? Do they show a similar pattern as suggested by the glacial data? These needs further justification.