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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.

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We have addressed each element of Referee 2's comments below. We note where comments from Referee 2 overlap with comments from Referee 1 or 3. We highlight the original referee comments in «brackets» and give our responses below each relevant passage.

« 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

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the paper is ready for publication. Below I have provided my line number edits and comments. »

We thank Referee 2 for their thoughtful comments and suggestions, which will improve the final manuscript. Below we address each comment individually and include suggested alterations to the text and figures.

« Abstract: - While I agree that understanding tropical glaciation in the past is a worthwhile 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 referring by "little" so the reader can better understand their argument. »

We agree that our statement here is blunt and needs nuance. With this statement we are referring only to the relative paucity of data on Holocene tropical glacial fluctuations relative to what is known for higher-latitude glacial fluctuations. Figure 2 in Solomina et al. (2015) provides an illustration of this point. The 'low-latitudes' in this case are 22 data entries on Holocene glacial fluctuations, including one from Papua New Guinea, three from East Africa (one at Kilimanjaro and two from Mt. Kenya), and 18 from South America. Although this is by no means "little" data for the tropics, it is much less than higher-latitude regions. For example, the same data compilation includes eight studies from Spitsbergen and 15 entries from the monsoon-influenced Himalaya (Solomina et al., 2015). Figure 2 in Solomina et al. (2015) also highlights a fundamental element of many tropical glacial chronologies, namely that many of these entries for tropical glacial fluctuations do not provide information about glacial fluctuations throughout the Holocene, but rather more limited time slices. We think tropical glacial histories are of particular interest due to the relative lack of data from the tropics (and tropical Africa in particular), a point we will clarify in the revised version of the manuscript.

« 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 unpublished 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. »

(Here we provide the same response as given to a similar comment by Referee 1): We agree that citing a paper not yet available to the public (Jackson et al, in review) at the time of submission was not ideal. This paper is now accepted for publication in Quaternary Science Reviews and will be cited as Jackson et al. (2020). We provide a web link to the published journal article here [https://www.sciencedirect.com/science/article/pii/S0277379120304170].

The paper referred to (i.e., Jackson et al., 2020) reports and interprets a Rwenzori glacial chronology for late-glacial time (∼16-11 ka). We intentionally split off the data in the CP manuscript because it deals with a Rwenzori glacial chronology for the Holocene. We felt that the late-glacial and Holocene data required quite different backgrounds and understanding of regional and global climate conditions and dynamics, and the implications of these datasets were different in geographic and climatic scope. As mentioned above, the number of new 10Be ages presented in the CP manuscript, while small, still greatly increases what is known about Rwenzori glaciation during the Holocene and is an important contribution to existing East African records.

« 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. »

We will provide additional annotation in this figure and will outline the moraine crest in Figure 2c as well as sample locations in Figure 2b.

« 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? »

This is a photo of the Speke moraine (a view of the right lateral, taken from the left-lateral ridge). The scale in the photo is perhaps misleading, as the 'trees' here are no more than 1-2 m high. We do not consider it necessary to correct for any impact of this vegetation because the shrubs/stalks persist primarily below the ridge crest itself (as visible in Figure 2c). The boulders and cobbles on the ridge crest feature some moss cover. However, based upon the density and thickness of the moss, and using shielding parameters as described by Plug et al. (2007), a persistent moss cover on this moraine ridge for the full duration of exposure would alter the ultimate exposure ages by only ∼2% at most, or ∼10 years. As we think it unlikely that moss was a persistent feature for the full duration of exposure (in terms of ecological succession and moss thickness), we do not correct the 10Be ages for any such vegetation effects. We will clearly explain this reasoning in the text.

« 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. »

This information (blank 10/9Be ratio) is given in Table 2. We will add the name of the carrier used for each sample group to Table 2.

« 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. »

We choose to utilise time-invariant "St" scaling because existing high-elevation, low-latitude production rate calibrations are most robust (lowest uncertainty by total scatter) when determined using time-invariant scaling (as evaluated using v3 of the online cosmogenic nuclide exposure-age calculator described by Balco et al. (2008) and subsequently updated). We suggest, however, that although "St" scaling is the most appropriate choice as based on this metric, the use of an alternative time-variant scaling scheme (such as “LSDn”) would not fundamentally alter our conclusions as the difference in 10Be ages calculated with these schemes does not fall beyond the 2-sigma error threshold for either calculation. Moreover, we do not attempt to make any centennial-scale inferences regarding these data. The largest age offsets between scaling methodologies occur in the bedrock 10Be exposure-age equivalents (∼400 years difference when using “St” versus “LSDn” scaling) and we do not make climatic interpretations using these data.

« 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. »

We initially reported these data as ratios rather than as ‘exposure ages’ in order to prevent readers from perhaps misinterpreting the data when reviewing the figures. We note in the text that it is inadvisable to treat these bedrock ages as ‘simple’ exposure ages of single duration. However, we understand the need for clarity in the figure, and will change these show the ‘exposure age’ of these bedrock samples. We will mark these samples in the legend as ‘exposure-age equivalent’ rather than ‘yr BP’.

« 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. »

Weather station data collected by Lentini et al. (2011) reflects temperature and precipitation variability on Mt. Stanley in the central Rwenzori at an elevation of 4,750 m from the year 2006 to 2009. The Rwenzori glaciers do not experience a seasonal ‘winter’ snowfall, but instead have a seasonality of precipitation as the ITCZ passes over head twice each year, with greater diurnal temperature changes than any seasonal changes.

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Rain falls on the high Rwenzori peaks year round, including during the ‘dry’ seasons of boreal winter and summer. Although there is some difference in air temperature during the wet and dry seasons, mean daily high air temperatures range between ~5.5 and 4°C throughout the year. Measured daily low temperatures are between ~2 and 0°C. In addition, average relative humidity is constant across the seasons (between ~95-85%). We note that the elevation of the weather station is higher than any of the sample locations we report and describe in our manuscript, and so daytime temperatures at each site should generally reach above 4°C during the day year round. In addition, night time temperatures would rarely be below freezing

« 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. »

In lines 235-238, we state that we utilise “St” scaling in all figures (and report these in Table 3). We include “LSDn” scaling results in Table 3 for comparison and, while we note that 10Be ages calculated using both scaling methods are similar, we utilise “St” throughout the discussion (see response above). We will clarify this in a revised manuscript.

« 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). »

We will provide information regarding boulder dimensions and additional field sample photos in a revised manuscript.

« 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. »

As mentioned above, we initially reported these data as ratios rather than as 'exposure ages' in order to prevent readers from perhaps misinterpreting the data when reviewing the figures. We will change this figure to show the 'exposure age' of these bedrock samples. We will mark these samples in the legend as 'exposure-age equivalent' rather than 'yr BP' and will make the colors/lines more distinct to make the distinction between sample types clearer.

The Sample ID numbers (RZ-XX-XX) are separate from the Map ID number (included in the figures) and refer instead to the original field sample ID and designation as used throughout fieldwork and later laboratory processing. We will make this clearer in the Table captions.

As noted above, the paper we cite as 'in review' is now accepted for publication in Quaternary Science Reviews and will be cited as Jackson et al. (2020). We provide a web link to the published journal article here [https://www.sciencedirect.com/science/article/pii/S0277379120304170].

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

We do not mean till here, necessarily, as the surface of the valley floor has been infilled by wetland (and so is not bare bedrock). Nearer the valley walls (and in some cases transiting the valley floor) colluvial deposits spill into the catchment. In other valleys at similar elevation to the upper Bujuku (3900-4000 m asl), such as in the Nyamugasani valley, although wetland has infilled areas of the valley floor there are moraines clearly visible above the wetland surfaces. In some cases these moraines dam lakes. We make our observation here about the Bujuku valley based upon the paucity of any such moraines, as well as the absence of glacial till/moraines on the valley walls above the wetlands (in areas not affected by colluvium). We will expand the discussion to make our geomorphic observations more explicit.

« 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. »

We will update all map figures that include the existing satellite views, to be paired with map-view hill-shaded contour maps of the areas of interest. For the Bujuku valley we will include the ∼11.7 ka moraine and the ∼11 ka landslide.

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

We consider this thesis to be appropriate to cite because it is available online through the Dartmouth College library. However, we will instead cite Jackson et al. (2020), which also includes the landslide data.

« 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. »

We do not think that the landslide occurred atop the glacier, as there is no indication of slope change or disturbance. If the landslide had melted out/settled after initial deposition, we would not expect the sampled landslide boulders to yield similar 10Be ages. Because two samples yield identical 10Be ages, we suggest that any post-depositional movement or rotation of the slide and its sediments is unlikely. We will expand the discussion to make our geomorphic observations more explicit.

« 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)? »

Here we clarify our description of the Speke moraine. The moraine ridges have sharp crests, with steep ice-contact slopes and more gentle ice-distal slopes. We suggest that the moraine ridges themselves were constructed along the margins of the former Speke glacier at least in part by sediments derived from rockfall onto the glacier surface, although we do not consider this to be the exclusive mechanism of formation. The very steep mountain slopes above the former glacier position would likely have provided material for later deposition via rockfall onto the glacier surface. This is supported in part by the fact that some of the clasts that comprise the Speke moraine are angular rather than molded in form, suggesting they did not undergo extensive subglacial erosion or entrainment. Other clasts, however, show evidence of glacial abrasion, suggesting that this feature is not a protalus rampart. In addition, Ballantyne and Kirkbride (1986) note that the ice-proximal slopes of pro-talus ramparts are generally less steep than ice-distal slopes, which is not the case for the Speke moraine. We will add additional images of this feature to Figure 3 and expand our description to make the context and geometry of the landform clearer.

« 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? »

We will include additional imagery (satellite and field photos) of the Speke moraine in Figure 3.

« 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. »

Here we would suggest that the 'older' Speke moraine ages (∼400-460 yrs BP) may reflect the fact that some moraine material was sourced via rockfall onto the glacier surface and so was minimally eroded. These ages would therefore contain 'inherited' 10Be. In this case the 'younger' ages (∼270-360 yrs BP) would be more representative of the timing of moraine abandonment. However, due to the scatter of ages on this moraine we do not attempt to correlate the timing of moraine abandonment with any discrete climatic forcing, and rather note only that following Early Holocene recession the maximum extent of ice on Mt. Speke occurred during the Late Holocene, within the last ∼400 years.

« 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. »

(Here we provide the same response as given to a similar comment by Referee 2 above): We initially reported these data as ratios rather than as 'exposure ages' in order to prevent readers from perhaps misinterpreting the data when reviewing the figures. We note in the text that it is inadvisable to treat these bedrock ages as 'simple' exposure ages of single duration. However, we understand the need for clarity in the figure, and will change these show the 'exposure age' of these bedrock samples. We will mark these samples in the legend as 'exposure-age equivalent' rather than 'yr BP'.

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

As cited in the text, this is based on direct observations by Whittow (1963) and H. Osmaston (Osmaston and Pasteur, 1972). In addition, Mary Meader (1937) photographed the peak during her aerial survey of the Rwenzori, photos of which are available for viewing online. A link to these photos is included in the works cited [https://collections.lib.uwm.edu/digital/collection/agsafrica/id/358/rec/102].

« 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. »

We will re-state this point here for the reader. We will also mention that, although we consider it unlikely, we cannot prove that all bedrock surfaces were sufficiently eroded during the LGM to remove any inherited 10Be. Regardless, we do not rely on the bedrock 'exposure-age equivalents' for any aspect of the discussion beyond the comparison with 20th century ice-cover observations. Thus, any potential inherited 10Be in the bedrock samples/ages does not affect our interpretations and discussion.

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

We will add additional field and sample photos to the revised manuscript.

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

This is likely an elevation issue first and foremost. Mt. Speke (4,890 m asl) is higher than Mt. Weisman (4,620 m asl). In addition, the Speke moraine (∼4,050 m asl) represents a much greater catchment area (and sediment delivery area) than does the Thomson cirque (∼4,525). If ice on Mt. Weisman was not effectively erosive, it would take a much longer time to produce a moraine ridge on Mt. Weisman than on Mt. Speke.

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

(Here we provide the same response as given to a similar comment by Referee 2, above): We agree that citing a paper not yet available to the public (Jackson et al, in review) at the time of submission was not ideal. This paper is now accepted for publication in Quaternary Science Reviews (QSR) and will be cited as Jackson et al. (2020). We provide a web link to the published journal article here [https://www.sciencedirect.com/science/article/pii/S0277379120304170].

« 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. »

We agree that temperature is not the sole control on glacial mass balance in the Rwenzori or elsewhere, and did not intend for that to be implied. Precipitation, hypsometry, humidity, aspect, all have a role to play in mass balance change, and disentangling each variable from the others is difficult even with high-resolution models. However, we do want to clarify what we mean in this portion of the discussion.

In using the term 'non-linear' to describe the inferred pattern of glacial fluctuations in the Rwenzori, we wanted to highlight the apparent threshold response of ice in the Bujuku and Nyamugasani valleys to Early Holocene warming. Although precipitation remained elevated from ∼11.7-5 ka, this elevated precipitation regime was not sufficient to maintain positive mass balance - or indeed to allow glaciers to persist. In addition, the apparent expansion of glaciers in the Late Holocene coincides with more recent apparent cooling with persistent dry conditions. Although it may be inappropriate to draw direct correlations between recent decadal changes and millennial-scale changes during the Holocene (as boundary conditions such as greenhouse gas levels and insolation were different), these records suggest that temperature acted (and may yet act) as the primary control on mass balance, and that any future increase in regional precipitation would be insufficient to induce glacial regrowth or readvance in light of still warming temperatures.

« 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?

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These needs further justification. »

In the revised text we will step back from our suggestion that glaciers in the African and South American tropics experienced synchronous fluctuations during the Holocene. We acknowledge that the data are not yet robust enough in both regions to make this statement. However, Referee 2's comment does bring up an interesting point regarding the existence and availability of tropical temperature records beyond tropical Africa.

The Marcott et al. (2013) data stacks show relatively little change in Holocene temperatures in the low latitudes. This reconstruction suggests that Early Holocene temperatures were only ~0.4°C cooler relative to the Holocene average, and that a Middle Holocene climatic optimum centred at ~5 ka was only ~0.15° C warmer than average. In contrast, the regional African temperature stack of Ivory et al. (2017) indicates temperature changes of ~2.5°C between ~11 and 5 ka. The overall pattern of temperature changes reported by Marcott et al. (2013) and Ivory et al. (2017) is similar, but the magnitude and variability of the former is much less than that of the latter. This may be due to the predominance of marine proxies used by Marcott et al. (2013). In contrast, the record of Ivory et al. (2017) is based entirely on terrestrial proxies and may indicate the influence of lapse rate changes on terrestrial (and higher elevation) temperatures over time relative to marine temperature changes. In any case, the differences between these proxy types are an important avenue for future study. The similarity of sign, if not magnitude, between the Marcott et al. (2013) stack and the Ivory et al. (2017) compilation may point toward a broader similarity in wider tropical temperatures, but more work is needed to assess this possibility – particularly in the terrestrial tropics.

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