Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-61-AC1, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

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

Margaret S. Jackson et al.

margaret.jackson@nuigalway.ie

Received and published: 1 August 2020

We thank Referee 1 for their thoughtful comments on our manuscript. Although we disagree with certain of the referee's points, we think that their critiques are well-founded and believe that in addressing these concerns our manuscript is much improved.

We have addressed each element of Referee 1's critique in full. In certain cases this entails some revision and modification of the manuscript, but we believe the majority of the Referee's concerns may be addressed through clarifying our intent both here and within the text.

Below we delineate the original referee comments in «brackets» and outline our response below each relevant passage.

« The manuscript "Holocene glaciation in the Rwenzori Mountains, Uganda" by Jackson et al., presents a new set of Holocene cosmogenic dates (n=12) from 2 valleys in Uganda. In my opinion, this study should not be published in the journal Climate of the Past. The primary reasons for this are (1) it is too regional in significance to be appropriate for COP (note the title- does not really reflect an inference about climate); (2) in my opinion it has a very limited ability to provide any concrete climate inferences, not because of the study design, but because of the number of samples, and the quality of the data that can be provided from the limited chronological constraint provided by these dates.; »

We appreciate that Referee 1 feels this manuscript may be a poor fit for Climate of the Past. Indeed, journal 'fit' is never cut-and-dried, and different readers can (and do!) come away with different views on how a contribution fits the overall mission of a publication. We think that these data and the resultant information to be gleaned from them about past glaciation and tropical climate in East Africa are appropriate for Climate of the Past. This is particularly so in consideration of (a) the fact that there exist so few records of past terrestrial temperature from the tropics writ large and (b) our novel coupling of Holocene glacial chronologies with local records of temperature and precipitation. We are also encouraged by the comments from Referees 2 and 3, who support publication of the manuscript in Climate of the Past (pending revision).

The type of direct comparison between past glacial fluctuations and paleoclimate conditions that we present is not possible to undertake elsewhere in the tropics, as similar such terrestrial temperature records (brGDGT geochemical temperature reconstructions) are not available outside of East Africa. Thus, the manuscript presents the first comparison of past tropical glacial fluctuations with nearby temperature records for the Holocene. The interpretations that we are able to make from the Rwenzori data presented here may aid in understanding past glacial chronologies from the wider tropics where such temperature records are not available. In addition, the Rwenzori chronology is particularly useful because it constrains glacial extents for the whole of the Holocene in multiple catchments, which is not the case for existing East African glacial chronologies. At Kilimanjaro, surface-exposure data do not bear directly on Holocene ice extents (Shanahan and Zreda, 2000). At Mt. Kenya, although there are Holocene-age surface-exposure data for the Teleki valley catchment, these are not replicated in any other catchments. The available surface-exposure data are also scattered (Shanahan and Zreda, 2000), and so are not useful for correlating with other records on millennial or centennial timescales.

We respectfully disagree with the Referee's comment that the dataset presented here is too small to provide any concrete inferences. Although modest in size, the Rwenzori glacial chronology presented here documents glacial extents for the entire Holocene Epoch. We argue that the combined detailed field mapping and lack of samples is itself important and noteworthy, as there are no moraines to be dated in either the Nyamugasani or Bujuku valley between the Early Holocene moraines (∼11 ka) and the dated and/or observed Late Holocene ice extents. This, in addition to the dated perched boulders and other geomorphic evidence discussed, shows rapid Early Holocene glacial retreat and evidence that glaciers remained near or inboard of their Late Holocene/Historical maxima throughout the Holocene Epoch.

Regarding the title of the manuscript, we will revise this to reflect the true scope and novelty of the work. Our suggested/edited title is "Holocene glaciation and climate conditions in East Africa".

« (3) the main conclusions, while theoretically plausible, are not unambiguously supported by the data, and this is not clear from the abstract or conclusions;»

We agree that the language within the manuscript was too definitive in tone and lacked an emphasis on the unknowns yet to be explored. We will modify the language to make explicit the remaining uncertainties and will refocus the latter portion of the discussion to emphasise the Holocene history of glaciation in East Africa. This is also noted below and in our responses to Referees 2 and 3.

« (4) I am concerned that given the reliance on ages that are presented and under review elsewhere (Jackson et al., under review), for the interpretations that are being made in this paper, that really the two papers should have been combined and splitting the manuscripts in two seems unjustified. I explain in more detail below. The paper presents only 12 dates – a small number to constrain any sort of cosmogenic glacial history, particularly since the authors have several other papers published or currently in review from the same sites. Without seeing the other papers, the apparent justification is that this paper is focused on Holocene variability. The authors thus present some older deglacial ages (e.g., 10-12 kyr BP) that appear to be from the other Jackson et al., in press paper, some latest Holocene ages (300-500 yr BP, n=4), in another valley 5 boulders on bedrock – not associated with glacial moraines (11-12 kyr (n=2), ∼4 kyr BP n=3) and samples of bedrock in the uppermost cirque from this valley (5-6 kyr, n=3). »

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.

« The way I would interpret this data is that it does seem that you had an early Holocene deglacial retreat at 11-10 kyr BP. And there is evidence from one site that there was a small standstill or readvance during the Little Ice Age (note that this readvance is seen in other African localities, including Mt Kenya and Kilimajaro, I think. The interpretation of the other boulder ages is ambiguous. The upvalley cirque ages of 5 kyr are the same as the valley boulders. So how do we interpret these boulder ages? They are not associated with any geomorphic features, so they may just reflect material deposited during retreat of the ice, and their age may not have any real meaning (ie perhaps they are simply inherited cosmogenic nuclides. Alternatively, the 5kyr ages in the uppermost cirque may suggest that the valleys were basically ice free by 5 kyr. »

We are in agreement with Referee 1 that the data presented suggest rapid deglaciation after ∼11 ka, and apparent readvance or re-nucleation of ice during the Late Holocene (a similar Late Holocene moraine is dated on Mt. Kenya (Shanahan and Zreda, 2000) and discussed in the text). However, we do not consider the older/lower-elevation perched boulder ages (RZ-15-09, RZ-15-10, RZ-15-11) from the Nyamugasani valley to be ambiguous, and suggest that these reflect continued Early Holocene glacial recession (∼11-10 ka). The two other perched boulder ages (RZ-15-07, RZ-15-08) are ambiguous, because (a) the ages are similar to the 'equivalent' exposure ages of the nearby bedrock, and (b) the boulders themselves were covered by ice and/or snow during the early to middle 20th century. We acknowledge the uncertain interpretations of these latter two boulder ages in the text and refrain from basing paleoclimatic interpretations on them. We note that these boulder ages may support a Middle Holocene ice advance to a position near or inboard of the Late Holocene/historical ice extent.

« From this data they make several inferences: 1- glaciers did not readvance beyond their late Holocene maxima during the early or mid Holocene. This is possible, but given that the record is inherently erosive, it is hard to say anything from the absence of evidence. The old ages in the upper cirque bedrock do seem to support this inference (that ice was gone by 5 kyr) but then the authors also say this: Line 355 "Although the timing of ice recession and re-nucleation within the cirque cannot be established

with the data presented here, the bedrock 10Be concentrations suggest that the cirque remained ice-free for a significant portion of the Holocene Epoch." »

We think that this apparent uncertainty from Referee 1 likely reflects our failure to make clear the potential history recorded by bedrock surface-exposure age equivalents, and the way in which we explicitly treat these bedrock data differently than we do exposure ages as measured from perched boulders or moraines. Although we agree it is difficult to prove a negative, we suggest that the lack of evidence for significant glacial expansion outboard of Late Holocene maxima in the Rwenzori is indeed a sufficient, and in fact robust, basis for inferring this glacial history. The absence of additional moraines outboard of the ∼250-450 yr old Speke moraine in the Bujuku valley, in a catchment that provides ample material for moraine formation, is one indication. In the Nyamugasani valley, the perched boulders dated to ∼11-10 ka were, we suggest, deposited by ice retreat/thinning during the Early Holocene. It is unlikely that these boulders were re-covered by a readvance of ice during the Holocene, as a glacier extensive enough to reach this location would have presumably entrained these boulders as it flowed down valley rather than leaving them unaltered. Such downslope transport would have also presumably rotated the boulders such that the exposure ages would not show the similarity they do (∼12-11 ka). Moreover, glacial cover for any appreciable period of time would have reduced the exposure ages of these samples, a possibility which seems unlikely in light of their similarity with samples farther down valley in the catchment.

The bedrock itself yields 10Be concentrations equivalent to ∼5 ka of total exposure. As we note in the text, we hesitate to infer that these measurements reflect a single duration of exposure and so leave open the possibility that ice may have ablated away completely from the cirque and re-nucleated once or many times during the Holocene. Whereas boulders that are entrained during sub- or englacial transport are presumed to undergo erosion sufficient to remove any surface material containing already accumulated 10Be, bedrock surfaces may not always be abraided sufficiently by flowing ice to remove such 'inherited' 10Be (Dunai, 2010).

We make the assumption that the bedrock was sufficiently eroded during the last glacial period to remove any accumulated 10Be in the surface and thus to 'reset' the surface-exposure clock. Although the nature of single-nuclide dating leaves determining the absolute history of this bedrock impossible, we suggest multiple scenarios in the text that may explain the measured 10Be concentrations. These concentrations may, as Referee 1 points out, be the results of a single period of ∼5 ka exposure during the Early, Middle, or Late Holocene. Alternatively, ice may have ablated away and later re-nucleated once or multiple times over the course of the Holocene. We suggest that ice ablated away completely at least once (otherwise the 10Be concentration would be significantly lower) and later renucleated. The fact that ice covered the site in the early and middle 20th century (as observed by Osmaston and Pasteur, 1972, and documented by Mary Meader in 1937) before ablating away indicates this scenario occurred at least once in the Holocene.

« 2- Line 436 "our comparison. . . indicates that ice masses did not respond linearly to temperature." From the really limited data here, I don't see how you can say this. The delay may be simply a lag in the response rather than a nonlinear adjustment. »

We agree that a lagged response may be a contributing factor to the inferred pattern of ice-extent fluctuations and note this in Line 444 of the manuscript.

« 3- Line 552 conclusion: "Based on a comparison of tropical East African glacial fluctuations with regional climate records, we suggest that temperature acted as the primary control on glacial fluctuations throughout the Holocene". I have two concerns about this statement. First is that it is hard to establish the relationship between the glacial chronology and temperature reconstructions given the limited constraints. Essentially what we have is that ice retreat started in the early Holocene and was likely complete by 5 kyr (unless there is cosmogenic inheritance in the upper cirque bedrock). The African lake temperature records do show a signal that is consistent with this but it is not the only explanation. For example, several of the precipitation records also show declining values from 11-12 kyr and could be partially responsible for the glacier retreat. Essen-

tially the problem is that we have very little we can say about what happened during the Holocene from these samples. Furthermore, there is no apparent cooling during the Little Ice Age in the temperature records which could account for the glacial readvance at that time. »

We agree that there are many factors that influence glacial mass balance. As we note in the text, glaciers are sensitive not only to temperature, but to a number of geographic and hydroclimatic variables (Lines 399-404). Relatively low precipitation amounts during the Younger Dryas (∼12.8-11.7 ka) may have contributed to a negative mass balance and glacial retreat, but we note that the onset of the African Humid Period at ∼11.6 ka marked a rapid transition to more moist conditions in the region, and all precipitation records we highlight show rapid precipitation rise underway by ∼11.4 ka. The Holocene temperature compilation of Ivory et al. (2017) suggests that regional temperatures roughly plateaued between ∼11.5 and 9.5 ka, as precipitation first increased and then remained elevated. However there is no evidence that glaciers in either catchment readvanced with the onset of elevated precipitation during the period of sustained, consistent temperatures. In this case, and elsewhere as we highlight in the text, we suggest that although precipitation affected mass balance, at no point in the record were precipitation levels sufficient to overcome the impacts of changing temperature.

Regarding the Little Ice Age, the compilation of regional temperature records produced by Ivory et al. (2017) does show cooling on the order of ∼0.5°C between ∼700-250 yrs BP. We would argue that this may align with the glacial readvance (and the timing of subsequent recession) recorded in the Rwenzori and at Mt. Kenya ∼250 yrs BP.

« Its not clear that the South American comparison is all that convincing of a tropics-wide temperature mechanism. Both Africa and South America chronologies suggest that retreat started early and the most extensive subsequent advance was in the latest Holocene. But South American records show evidence of much greater dynamics (presumably associated with rapid retreat) after 5 kyr, whereas at Ruwenzori, the ice was already nearly gone by that time. »

We agree that there is much more work to be done assessing the possible centennial-scale synchrony of Holocene glacial fluctuations across the tropics. We will change the tone and text to address the existing uncertainties in the comparison. However, the broad similarities that Referee 1 highlights (i.e., Early Holocene retreat, most extensive subsequent advance during the Late Holocene) are exactly what we focus on in the manuscript. We think these similarities are worth noting.

Although it is beyond the scope of this work, we note that these sorts of broad similarities in regional patterns of deglaciation have been used to compare and contrast glacial records from the Northern and Southern Hemispheres (e.g., Putnam et al., 2012). Glacial chronologies from the European Alps generally indicate rapid Early Holocene retreat and subsequent Middle or Late Holocene re-nucleation/advance. In contrast, glaciers in New Zealand retreated in more stepwise fashion throughout the Holocene. Although suggesting that all Northern Hemisphere glaciers fluctuated synchronously is not possible (nor accurate), the broad similarities are worth noting when glaciers elsewhere in the world display such a markedly different history. To be clear, we agree that our suggestion of a common pan-tropical forcing is likely too speculative and will remove this from the revised text. However, we want to state clearly our interpretations of the existing data from tropical regions, and outline where we may have a different view from Referee 1.

« As a final point, I note that the authors did not appropriately reference the literature in making some of their statements. For example:

Abstract Line27: I think it is incorrect to say that “little is known about the response of tropical glaciers”. There is literature from South America for certain, and though I am less familiar, probably in Asia as well. »

We agree that the abstract line highlighted here is too blunt and requires more nuance (we address this also in our responses to Referees 2 and 3). We hope that the edits

and alterations we suggest making to the rest of the manuscript, in addition to our responses from Referees 2 and 3, have reduced Referee 1’s concerns in this regard. We include below our response to Referee 2’s comment on this passage below:

Response to Referee 2: “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 portion of the 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.”

« L411 – Garcin 2007 is not the appropriate reference for the African Humid Period. I suggest referencing some of the early primary literature by Francoise Gasse and other leaders in the field. »

We did not mean to suggest that Garcin et al. (2007) marks the initial recognition of the African Humid Period, nor would we wish to imply such understanding with our readers. The citation included for the African Humid Period (Garcin et al., 2007) is one we chose in order to define the timing and duration of the event based on recent chronological methods and assessments. We propose to alter the relevant text to include earlier work (e.g., Gasse, 2000) to mark the establishment of the African Humid Period in the literature, while also citing Garcin et al. (2007) as a more recent (temporal) definition of the period that we utilise in our discussion.

References:

Dunai, T.J.: Cosmogenic Nuclides: Principles, concepts and applications in the Earth surface sciences. Cambridge University Press. 2010.

Garcin, Y., Vincens, A., Williamson, D., Buchet, G., and Guiot, J.: Abrupt resumption of the African Monsoon at the Younger Dryas-Holocene climatic transition. Quaternary Science Reviews 26, 690-704. 2007.

Gasse, F. Hydrological changes in the African tropics since the Last Glacial Maximum. Quaternary Science Reviews, 19(1-5), 189-211. 2000.

Ivory, S.J. and Russell, J.: Lowland forest collapse and early human impacts at the end of the African Humid Period at Lake Edward, equatorial East Africa. Quaternary Research 89, 7-20. 2017.

Jackson, M.S., Kelly, M.A., Russell, J.M., Doughty, A.M., Howley, J.A., Cavagnaro, D.B., Zimmerman, S.R.H., and Nakileza, B.: Glacial fluctuations in tropical Africa during the last glacial termination and implications for tropical climate following the Last Glacial Maximum. Quaternary Science Reviews 243, 106455. 2020.

Jomelli, V., Favier, V., Vuille, M., Braucher, R., Martin, L., Blard, P.-H., Colose, C., Brunstein, D., He, F., Khodri, M., BourleÌĂs, D.L., Leanni, L., Rinterknecht, V., Grancher, D., Francou, B., Ceballos, J.L., Fonseca, H., Liu, Z., and Otto-Bleisner, L.: A major advance of tropical Andean glaciers during the Antarctic cold reversal. Nature 513, 224-228. 2014.

Putnam, A.E., Schaefer, J.M., Denton, G.H., Barrell, D.J., Finkel, R.C., Andersen, B.G., Schwartz, R., Chinn, T.J. and Doughty, A.M., 2012. Regional climate control of glaciers in New Zealand and Europe during the pre-industrial Holocene. Nature Geoscience, 5(9), 627-630. 2012.

Rodbell, D.T., Seltzer, G.O., Mark, B.G., Smith, J.A., and Abbott, M.B.: Clastic sediment flux to tropical Andean lakes: records of glaciation and soil erosion. Quaternary Science Reviews 27, 1612–1626. 2008.

Shanahan, T. and Zreda, M.: Chronology of Quaternary glaciations in East Africa. Earth and Planetary Science Letters 177, 23-42. 2000.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-61, 2020.