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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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Jackson et al. provide new evidence for Holocene glacier fluctuations in tropical Eastern Africa based on 10Be surface exposure dating of moraine boulders (n = 4), unassigned boulders (n = 5), and bedrock (n = 3). Glacial deposits are a valuable proxy for past climatic and environmental changes and often constitute the only possibility for the direct dating and reconstruction of these changes, especially at high elevations. To better understand the impact of past climatic changes on the afro-alpine environment and, vice versa, to infer potential implications of past glacier fluctuations for the tropical palaeoclimate, new insights into the Holocene glacial history of Eastern Africa are most welcome. However, I think it would be worthwhile to specify the aim of the study and reconsider some of the general conclusions, as I will further outline below.

General comments:

Following previous publications (Kelly et al., 2014; Jackson et al., 2019; Jackson et al., in review), this manuscript is the fourth one presenting glacial chronological data from more or less similar sites in the Rwenzori Mountains. Splitting a glacial chronology into several papers might be reasonable to have more space for discussing individual aspects/events in detail, but the added value of this manuscript remains unclear after first reading. Most of the exposure ages from the Nyamugasani Valley (Fig. 4) that determine the glacier extent at ~12-11 ka (before the onset of deglaciation) stem apparently from the other manuscript in review (Jackson et al., in review). From my understanding, the "only" new finding based on the additional ages from the upper part of the valley is that the "Thomson cirque" (and maybe Mount Weisman too?) was probably ice-free by ~5 ka or at least for a longer period during the Holocene. Since the reader has no insight into the other manuscript in review, it would be important to elucidate the novelty or new aspect of the contribution presented here.

In the abstract, the authors propose that "understanding how tropical glaciers responded to past periods of warming is crucial for predicting and adapting to future climate change [...]" (lines 26-27). They state further in the introduction that "tropical glaciers are a primary source of freshwater and are a fundamental component of regional economies [...]" (lines 52-53) and that "determining when and how glaciers in the African tropics fluctuated during past warm periods provides crucial information for assessing whether, or how long, tropical glaciers may persist under future warming scenarios" (lines 70-72). Although the ongoing glacial melting in the tropics and most of the mountains worldwide is of great concern, the contribution of the meltwater from the relatively small glaciers in equatorial Eastern Africa to the alpine runoff is negligible (e.g. Kaser et al., 2004; Taylor et al., 2009) and thus do not seem to play a major role for the regional economy and freshwater supply. Moreover, the authors do not explain how limited information on past glacial fluctuations could help to better project the future evolution of tropical glaciers in response to global climate change. Reconstructed glacier extents and established glacial chronologies provide without doubt important information on past glacier dynamics, but I think the palaeoclimatic, -environmental, and -glacial data are too uncertain to draw meaningful conclusions about "[. . .] the sensitivity of tropical glaciers to future climate change" (lines 545-546). I would even argue the other way round that modern observations and investigations regarding the climate sensitivity of tropical glaciers in Africa are inevitable for a reliable interpretation of past glacier fluctuations in the region (e.g. Mölg et al., 2003; Mölg et al., 2004; Mölg et al., 2008; Mölg et al., 2009; Nicholson et al., 2013). It is a bit surprising that the authors do not pick up the topic again in the discussion and do not emphasize the claimed relevance of their findings for the future evolution of tropical glaciers. I would therefore recommend that the authors rather stress the palaeoclimatic and -environmental relevance of their study in the abstract and introduction.

The two main conclusions of the manuscript are that (1) Holocene glacier fluctuations were similar across the tropics and based on the consideration of regional climate records that (2) "[. . .] temperature acted as the primary control on glacial fluctuations throughout the Holocene" (lines 553-554). I do not agree with these statements for the following reasons:

1. The 10Be exposure ages from the Holocene moraine stages in the Rwenzori Mountains (Nyamugasani Valley), on Mount Kenya (Teleki Valley), and on Kilimanjaro (Kibo Peak) originate more or less from one valley/locality (Shanahan and Zreda, 2000). Whether the respective ages are representative for the entire mountain range can neither be confirmed nor refuted. I think without further evidence it remains hypothetical whether glaciers in tropical Eastern Africa responded synchronously to Holocene climate changes or not.

2. The Early and Middle Holocene moraine stages dated in the Rwenzori Mountains ($\sim$11.7 ka), on Mount Kenya ($\sim$10.2 and $\sim$8.6 ka), and on Kilimanjaro ($\sim$13.8 ka) show by no means a similar pattern, apart from a general warming trend after the last glacial period. The differences could be explained by dating uncertainties, but also by climatic

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variations. How do you interpret the differences?

3. In view of a lacking robust Holocene glacial chronology for Eastern Africa, the dynamic Holocene glacier fluctuations in South America, and the non-consideration of other tropical glacial chronologies, claiming "[similar] Holocene glacial fluctuations across the tropics" (lines 38-39) seems rather speculative than evidence-based. Moreover, this assumption underrates the complex regional response of alpine glacier to climatic changes (e.g. variations in temperature, precipitation, cloudiness, insolation, and moisture) in general. Mountain height, terrain, hypsometry, debris cover, glacier size, and many other geological, geomorphological, glaciological, and climatic parameters control the magnitude and rate of glacier fluctuations, as the regional variations in the response of alpine glaciers to recent global warming underline (e.g. Zemp et al., 2019).

4. A key assumption for the author's hypothesis that " [. . .] tropical glaciers responded to a common, pan-tropical forcing mechanism during the Holocene" (lines 534-535) is that tropical glaciers are highly sensitive to changes in temperature (lines 399-404; see also Jackson et al., 2019). As a reference for the Rwenzori Mountains, the authors quote a controversial study (Taylor et al., 2006a; Taylor et al., 2006b) which claims that rising temperatures are the dominant factor for recent glacier melting in the Rwenzori Mountains. However, the detailed comment on this study by Mölg et al. (2006), which elaborates the importance of other climate variables for the energy and mass balance of tropical glaciers, is neglected in the discussion. Multiple studies from Kilimanjaro and the Rwenzori Mountains stress that climate variables related to air moisture (e.g. specific humidity affecting sublimation, cloudiness affecting incoming solar radiation, precipitation affecting glacier surface albedo and mass gain) play an important role in the present surface energy balance of tropical glaciers in Eastern Africa, especially at high elevations above the 0 °C isotherm (Mölg et al., 2003; Mölg et al., 2004; Mölg et al., 2006; Mölg et al., 2008; Mölg et al., 2009; Nicholson et al., 2013). Since the sensitivity of tropical glaciers in Eastern Africa to different climate variables is an ongo-

ing and very important debate that is crucial for the hypothesis and conclusions of the presented manuscript, the controversial arguments should find more attention in the discussion. In view of the modern observations, I doubt that past glacial fluctuations in Eastern Africa can and should be explained by temperature variations alone.

Specific comments:

Fig. 1b: Would it be possible to add geographic coordinates to the map of the central Rwenzori massif?

Fig. 1b: For me as a reader who is not familiar with the region, it is difficult to interpret the terrain on the Worldview-1 satellite image. Replacing the image by a combination of DEM and hillshade (including the shapes of the lakes) might be an alternative.

Fig. 2: Could you include at least one photo of a sampled boulder and bedrock surface so that the reader gets a better impression of the investigated landforms?

Lines 135-138 and 425-427: The authors rely solely on radiocarbon ages from lake Garba Guracha in the Bale Mountains to discuss the potential timing of deglaciation in the southern Ethiopian Highlands, although direct 36Cl surface exposure ages of 21 moraine boulders from two valleys in the Bale Mountains are published (see Fig. 1 and S6-8 in Ossendorf et al., 2019). The inner-most moraines in the two valleys show that deglaciation in the Bale Mountains began after ~15-14 ka and suggest (not necessarily imply) that the southern Ethiopian Highlands were ice-free before the Pleistocene-Holocene transition.

Lines 254-256.: Did you conduct a simple sensitivity analysis (assuming e.g. two or three plausible erosion rates) to assess the age uncertainty related to erosion?

Table 1: Content-wise, the columns with the 10Be concentrations in Table 1 would fit better in Table 2. Information of sample lithology could be included in Table 1 if available.

Table 3: Could you outline how you define the “internal” (probably analytical) and “ex-

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ternal" error?

Fig. 4: I understand why you report 10Be concentrations instead of exposure ages for the bedrock samples (RZ-15-01, RZ-15-02, RZ-15-03), but they are difficult to interpret and compare with the other results. Therefore, I would recommend to report the exposure ages (instead of concentrations) in the map and note in the legend that they indicate the net duration of bedrock exposure, as you outlined in the text.

Fig. 4 and Table 3: Considering the general uncertainties associated with surface exposure dating (analytical errors, unknown erosion rates, etc.), I don't see justification to report ages in a way (e.g. 11,020 $\pm$ 280 years) that implies the method is precise enough to date events to a specific decade. I would recommend to round the ages and report them in kiloyears (e.g. 11.0 $\pm$ 0.3 ka).

Section 6.3: What is the rationale to explicitly discuss the glacial fluctuations in tropical South America here, although no new or recalculated ages are presented? The aim/motivation for the exclusive comparison between the Holocene ages from the Rwenzori Mountains and Andes is not clear from the abstract and introduction. Glacial chronological data also exist from other locations across the tropics.

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