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

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First and foremost we thank you for your thoughtful comments on our manuscript. Your comments echo many of those made by Referee 1 (the response to whom is posted for review), and certain from Referees 2 and 3 (see responses). We've endeavoured to address each of your questions and comments here. We include your original comments «in brackets» and outline our responses below, clarifying our intent or outlining the planned alterations we will make to the manuscript.

« 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. »

The new data we present come from two Rwenzori valleys, the Nyamugasani and the Bujuku valleys. These data provide the first direct constraints on past ice extents in both valleys throughout the Holocene Epoch. They show a pattern of similar glacial fluctuations in both valleys – specifically rapid Early Holocene recession of ice to within the Late Holocene maximum glacial extents. In addition to this new information regarding Rwenzori glaciation, we make use of local records of paleotemperature and precipitation in order to evaluate the response of Rwenzori glaciers to Holocene climatic changes. Such an analysis cannot be conducted elsewhere in the tropics, as terrestrial temperature records such as those from East Africa (e.g., brGDGT temperature reconstructions from lake sediments) do not yet exist for other regions. We also synthesise existing literature on East African glaciation during the Holocene in order to provide a review of past glaciation in the region. Such a review is a useful, novel addition to the current literature, as much existing work on Holocene glaciation in the African tropics was produced before the advent of surface-exposure dating (see references in manuscript). Moreover, existing, global syntheses of Holocene glaciation do not include many of these African records (see Solomina et al., 2015). Although we make these points in the manuscript, we will adjust the language to make these elements of the manuscript and its novelty clearer.

Regarding the paper ‘in review’, 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.

« 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. »

In the abstract and introduction we outline the many ways that tropical glacial systems play vital roles in the welfare of communities, be it through tourism, as critical parts of

alpine ecosystems, as freshwater reservoirs, or as sources of hydropower. No single tropical glacial system may fall into each of these categories, yet we suggest that understanding a system in one valley or region can have implications for understanding tropical glacial systems elsewhere. We also think it useful to highlight the utility and urgency in understanding tropical glacial systems more broadly.

With this in mind, we did not suggest that Rwenzori glaciers (or African glaciers in general) are vital sources of freshwater to local communities, as they are not. However, Rwenzori glaciers underpin regional tourism and are a primary draw for trekkers and tourists visiting the region, as are the remaining glaciers on Mt. Kenya and Kilimanjaro. As such they are a fundamental aspect of local economies. They are also a critical part of the African alpine ecosystem, and the loss of these glaciers has major potential impacts on surrounding habitats (Oyana and Nakileza, 2016).

« 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. »

We agree that assessing the modern sensitivity of glaciers in the tropics - and elsewhere - to climatic variables is crucial for understanding these systems over time, both in the past and future. However, we would argue that it is not possible to extrapolate fully the dynamics of past glaciation based on modern observations which span only a few decades, particularly when past climate conditions were so different from modern in terms of both global (e.g., greenhouse gas concentrations) and regional (humidity/precipitation, insolation, seasonality, etc.) conditions. While an understanding of modern change is crucial, it can only be made complete, we argue, when contextualised. We will alter the manuscript to make the connection of our study to modern change more explicit. Specifically, we will emphasise our interpretation that temperature played a dominant role in Holocene ice extents despite increases in precipitation. This has implications for future projections of tropical glacial recession in light of precipitation changes.

« 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. »

We agree that more work is needed to assess the potential synchrony (or asynchrony) of Holocene glacial fluctuations in East Africa, particularly on centennial timescales. This highlights a vital avenue for future research across the region, one we will emphasise in the manuscript. However, in the case of the Rwenzori, the data are from two

independent glacial catchments and yield similar results, suggesting that the overall pattern we identify is likely representative of the Rwenzori as a whole.

We also suggest that the available evidence from the region, while limited, supports the hypothesis that glacial fluctuations were broadly similar across the region during the Holocene Epoch. The data presented and summarised in the manuscript also represent the sum of the last ∼40 years of work on East African glacial extents during the Holocene. To be sure, there can be no definitive statement at present, but the available evidence is compatible with a broadly coherent regional pattern of glacial fluctuations on millennial timescales. We will make this nuance clearer in the manuscript.

« 2. The Early and Middle Holocene moraine stages dated in the Rwenzori Mountains (∼11.7 ka), on Mount Kenya (∼10.2 and ∼8.6 ka), and on Kilimanjaro (∼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 variations. How do you interpret the differences? »

A direct comparison of the Holocene moraine data from Kilimanjaro and Mt. Kenya with the Rwenzori chronology requires a full re-calculation of the original 36Cl surface-exposure ages (Shanahan and Zreda, 2000) to incorporate updated production rate and production-rate scaling methodologies. However, this is not possible because the sample data required to perform such a recalculation are not included in the original publication (Shanahan and Zreda, 2000). Although the existing 36Cl chronologies from Kilimanjaro and Mt. Kenya are not necessarily in conflict with the glacial chronology from the Rwenzori, we consider it inappropriate to make specific, centennial-scale interpretations based upon the 36Cl ages. In addition, the scatter in ages from single landforms is too great to permit centennial or millennial-scale correlations with discrete climate events. However we do not want to ignore these data outright and consider them worthy of mention in our broader discussion. We suggest that, similar to the Rwenzori chronology, the data broadly indicate Late Glacial and Early Holocene glacial recession on Kilimanjaro and Mt. Kenya.

As regards differences between the Kilimanjaro and Mt. Kenya chronologies, at Mt. Kenya the ages mentioned (∼10.2 and 8.6 ka) come from the Teleki Valley, and older, pre-Holocene ages come from the separate Gorges Valley (Shanahan and Zreda, 2000). Shanahan and Zreda (2000) did not date additional Holocene or Late Glacial landforms farther down valley in the Teleki Valley, nor did they date landforms farther up valley in the Gorges Valley. At Kilimanjaro, the age mentioned (∼13.8 ka) is pre-Holocene, and so does not bear directly on Holocene ice fluctuations. Although there are likely some differences in glacial chronologies between these two mountains for a number of regions (e.g., valley hypsometries, alpine versus plateau glaciation, etc.) whether there is truly a mismatch may only be determined by deliberate sampling and dating of the full suite of landforms in and between valley systems.

« 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). »

We absolutely agree that temperature alone cannot account for the fluctuations of tropical glaciers in the Rwenzori or elsewhere, either now or in the past. It is not our intent to argue that temperature is the sole control on glacial mass balance. Indeed, all glaciers are sensitive to a litany of factors, including hypsometry, insolation, and precipitation. We concur that our language within the text was too definitive in this regard, and will alter our text to make the nuance of our argument more clear. However,

we suggest that there are marked similarities between glacial fluctuations across the humid tropics, specifically a marked Early Holocene retreat after ∼11 ka, and apparent re-advance or renucleation of ice during the Late Holocene. Our aim in this text is not to argue for a true synchrony in tropical glacial fluctuations over the course of the Holocene, as indeed there is much work to be done to determine the millennial or centennial-scale fluctuations of glaciers at all sites. Instead we highlight the broad pattern of glacial fluctuations in the tropics that appears more similar than dissimilar over millennial timescales, which may be an intriguing focus for future research.

In our response to Referee 1, we note: "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), broad similarities are worth noting when glaciers elsewhere in the world display such a markedly different history."

« 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 0C 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 ongoing 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. »

In our response to Referee 2, we wrote: “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 in time 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.”

We agree that temperature alone cannot account for the fluctuations of tropical glaciers in the Rwenzori and elsewhere. It is not our intent to argue that temperature is the sole control on glacial mass balance (see comment above). We suggest that, over millennial timescales, Rwenzori glacial fluctuations reflect a pattern of growth and decay that does not align with reconstructed regional precipitation during the Holocene, nor would it appear that glaciers readvanced during periods of elevated or rising precipitation.

The studies by Mölg et al. are concerned with recent, decadal-scale changes in glacial extents. Although these are important works which we will be sure to include in our revised manuscript, our chronology cannot speak to decadal-scale changes over the Holocene - just as these other studies mentioned cannot address millennial-scale change.

Taylor et al. (2006) suggests that Rwenzori glacial melt is dominated by changes in atmospheric temperature. Although disputed by other studies (Molg et al., 2003, 2006), the work by Taylor et al. (2006) does not stand alone in its suggestion that recent glacial melt in the Rwenzori is a temperature-dominated signal. For example, Russell et al. (2009) use sedimentary analysis of Rwenzori lakes to infer the onset of recent (near-historical) Rwenzori deglaciation was underway before start of regional drying ∼1880 AD. This analysis suggests that glaciers retreated in response to a forcing beyond aridity. To be sure, regional drying after ∼1880 AD would have impacted glacial mass balance and likely encouraged further recession. However, whether it is appropriate to make direct comparisons between modern decadal-scale climatology and the conditions of the Early or Middle Holocene, when global boundary conditions were markedly different, is another matter for discussion.

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

Yes, we will add coordinates to Figure 1b within the revised figure.

« 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. »

We will update Figure 1b within the text in order to make the terrain and geomorphic context clearer. We plan to insert a hill-shaded contour map of the area of interest in place of the satellite image.

« 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? »

Yes, we will provide photos of the cirque bedrock and of perched erratic boulders within the upper Nyamugasani valley, and will include these in the revised manuscript.

« 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. »

We regret not highlighting the important work of Ossendörf et al. (2019) within our overview of East African glaciation in our original draft. In emphasising the record from Lake Garba Guracha we intended only to utilise a record that explicitly referenced the Holocene period within its analysis. The work of Ossendörf et al. (2019) detailed glacial fluctuations throughout the last glacial period in the Bale Mountains, but did not include data on potential Holocene fluctuations. We will update the manuscript to include this contextual data from the Ethiopian Highlands.

« 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? »

We did not include an erosion-sensitivity analysis, as prior work in the Rwenzori suggests that raised quartz veins and bulk boulder surfaces yield statistically indistinguishable ages (Jackson et al., 2019). However, we will include such an analysis and discussion in a revised manuscript and describe the results of such a sensitivity test here:

For each sample, we calculated 10Be ages as determined with the following rates of erosion using version 3 of the online calculator as described by Balco et al. (2008 and subsequently updated): 0.00, 0.0001, and 0.0003 cm/yr (i.e., between 0 and 3 cm erosion per 10,000 years).

Results of this analysis indicate that for the Speke moraine samples, no erosion rate was capable of altering the calculated exposure age from the zero-erosion scenario. In the case of the Thomson cirque bedrock, the maximum erosion rate scenario utilised yielded exposure ages only ∼1% older than the zero-erosion scenario (∼80 years).

For the Nyamugasani perched boulder transect, the maximum erosion scenario altered ages by ∼2-3% (∼70-300 years), with ‘older’ calculated exposure ages affected more by potential erosion effects. The more moderate scenario (0.0001 cm/10 kyr) yielded age offsets of ∼110 years, less than 1% of the total exposure age. We note that in each case, the impacts of erosion would not alter our interpretations within the manuscript. Due to the lack of information on and uncertainty surrounding erosion rates in the Rwenzori, we refrain from explicitly including these calculations within the manuscript, but would gladly make note of these values within the Methods section in order to make explicit the negligible impact of potential erosion on the Rwenzori chronology.

« 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. »

We present our reported data in three Tables with a view toward their easy re-use. Table 1 includes all of the information required to immediately ‘cut and paste’ into the online calculator used for age calculation (v2 and v3 of the online calculator as described by Balco et al. (2008) and subsequently updated), whereas Table 2 delineates the laboratory processing data required for determining the 10Be concentration used for calculation. We are hesitant to blend these tables, but propose to do so for inclusion in the manuscript text and to provide a secondary, 'cut and paste' version of these same data for download online. See also our replies to Referee 2 regarding table formatting.

« Table 3: Could you outline how you define the "internal" (probably analytical) and "external" error? »

Internal error is the analytical uncertainty attached to a given measurement. External uncertainty includes the uncertainties associated with the chosen nuclide production rate and scaling scheme used to calculate the resultant exposure ages, as well as the uncertainty associated with sample-specific variables such as topographic shielding. We will make this distinction explicit within the table caption.

« 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. »

(See our response below to Referees 2 and 3 regarding a similar comment): "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 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'."

« 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 ± 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 ± 0.3 ka). »

We agree that uncertainties inherent to surface-exposure dating do not enable decadal certainty in age calculation for reporting, and throughout the Discussion we round these ages to kilo years. However, we believe it is important to report individual ages and associated error, without rounding, within the Results, figures, and data tables in order to highlight the analytical agreement (or disagreement) between discrete samples or landforms. Reporting ages in this way also ensures transparency for those readers who may wish to re-calculate exposure ages or to compare ages as calculated using different production rate scaling schemes. Rounded 'kilo year' ages may obscure the differences between methodologies, and can make later re-interpretation of published results more difficult.

« 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. »

There are indeed glacial chronological data from locations elsewhere in the tropics, however we choose to focus on data from low-latitude South America for the following reasons:

1) The aim of this comparison is to assess, if possible, the potential similarities/differences in glacial fluctuations from different regions of the low latitudes. Any identifiable difference or similarity has direct bearing on the potential mechanisms that controlled glacial mass balance in the tropics, in East Africa and elsewhere, and thus in reconstructing wider tropical paleoclimate. As noted in response to referee comments, we agree that our original language in this section of the discussion was too certain in tone, and suggest re-focusing this portion of the manuscript to emphasise what is known (and unknown) regarding the African chronologies.

2) The climatic setting of the Rwenzori, in the humid 'inner' tropics (Kaser and Os-

maston, 2002) is more similar to the setting of the low-latitude Andes than to other tropical regions which experience a more monsoonal or arid climate (such as the Indian subcontinent or the subtropical Andes). The marked lack of thermal seasonality - and relatively muted seasonality in precipitation - in the humid tropics is a key factor in considering the controls on glacial mass balance (e.g., Sagredo et al., 2014; Rupper and Roe, 2008).

3) We likewise chose to focus our analysis on glaciation in the tropical Andes as this is the region where the majority of prior work on Holocene glaciation has been conducted, and so is the region which provides the most information on past mass balance change as a whole. There is little chronologic control on glacial deposits in Papua New Guinea, and so while we can certainly add mention glaciers at these sites in our revised manuscript, they are of limited use for drawing wider comparisons.

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