<u>Author Response to RC1</u>

Review of Shutkin et al. ‘Modeling the impacts of climate trends and lake formation on the retreat of a tropical Andean glacier (1962-2020)’.

Summary

Shutkin et al. have examined the past behaviour of Queshque Glacier, a glacier found in the Cordillera Blanca of Peru. They use observations of the recession of the glacier between 1962 and 2008 and several other datasets relating to glacier volume and dynamics, as well as local climate, to calibrate the Open Global Glacier Model (OGGM), which they then use to simulate contemporary glacier behaviour. They analyse the variability of OGGM simulations in relation to input data to establish model sensitivity to input parameters. Despite their use of a simplified temperature-index model, which the authors acknowledge has well established limitations, their findings show how they are able to replicate characteristic behaviour of Queshque Glacier in its climatologically complex setting. The authors also illustrate the impact of the transition of the glacier from land- to lake-terminating during the study period, suggesting that this process now largely dictates the glaciers mass loss trajectory over climate.

Overall, the paper is well structured and written and the authors have constructed their study in a comprehensive manner. The main findings are of relevance across the field of tropical glaciers and it is good to see that additional work is now being done to incorporate ice loss processes experienced by lake-terminating glaciers, which are prevalent across the Cordillera Blanca and, increasingly, worldwide.

Major comments:

We believe the study currently has one main limitation which we suggest requires revisiting – that is the approach employed to generate one of the two DEMs used to estimate glacier mass balance, and the subsequent treatment of elevation change data. The 1962 DEM produced by the authors has been generated using a manual photogrammetric plotter, which is methodologically dated and has produced a result which is *very* contrasting in data quality to their 2008 lidar derived DEM. Given the availability of software specifically designed to process optical stereo imagery to produce high-quality DEMs (example recommended later in the review) we would suggest reprocessing of the 1962 imagery should be attempted, if the authors have access to the images, to bring that dataset in line with the 2008 lidar DEM. This reprocessing would ensure that any biases associated with DEM difference data derived from methodologically contrasting DEMs (e.g. https://xdem.readthedocs.io/en/stable/) are minimised. Similarly, the subsequent treatment of elevation change data derived from this current DEM pair is lacking rigor, namely consideration of outlier identification and removal and subsequent gap filling, which are both required prior to geodetic mass balance estimation (Piermattei et al., 2024). Finally, the authors make no attempt to calculate the uncertainty associated with their glacier mass balance data, on which the rest of their analyses is based. This needs revisiting.

Considering the above, we recommend that major revisions are needed before the paper can be considered for publication.

Author Response

We thank the review team for their constructive feedback. Please find our detailed responses below, which are also reflected in a revised manuscript and supplement. The reviewers' major comments concerned the 1962 DEM quality, DEM outlier identification, and treatment of uncertainty in the difference of DEMs. We respond to these issues in logical order below before addressing the minor comments.

1. *1962 DEM Quality*

We appreciate the concern that methodological differences in DEM construction may impact their intercomparison and potentially introduce biases into the DEM difference map. Although we no longer have access to all metadata required for using the most state-of-the-art photogrammetric software, we note that the same DEM was used previously for a similar analysis (Mark & Seltzer, 2005). *Nonetheless, we have conducted multiple comparisons indicating that our DEM is of acceptable quality.*

First, we have accessed a 10 m resolution DEM constructed from the same 1962 stereo imagery using ERDAS Leica Photogrammetry Suite version 11. This DEM was used for a similar analysis of Queshque Glacier and others in the region (Huh et al., 2017). *A comparison between Huh et al.'s (2017) DEM and the one used in the present study shows considerable quality differences favoring our choice of DEM. We attribute quality concerns in the latter DEM to extremely low contrast in much of the accumulation zone that hampered the effectiveness of the DEM generation software. This resulted in obviously unnatural terrain artifacts that are absent from our chosen DEM product. On this basis, and on the basis that our data product has already been accepted for publication in reputable journals, we believe that while more state-of-the-art photogrammetry software could improve our DEM, this is not guaranteed.*

Second, we compare differences between our 2008 and 1962 DEMs over stable terrain to quantify the resulting uncertainty in elevation change over the glacier (see below).

2. *Geodetic Mass Balance Uncertainty*

If we safely assume that the 2008 LiDAR is of much higher quality than the 1962 DEM, then differences in the elevation of stable terrain between the two data products can be attributed to artifacts or inaccuracies in the earlier product.

As described in the supplemental material, we aligned the 1962 DEM to that of 2008 using a 3-dimensional coregistration process. We have subsequently compared our methodology to the common Nuth & Kääb algorithm (Nuth & Kääb, 2011). *Comparison of residual error over stable ground indicates that our results are more robust than those accomplished using the methods from Nuth & Kääb. We have updated Fig. S3 to highlight this result and reproduce it here as Fig. 1.*

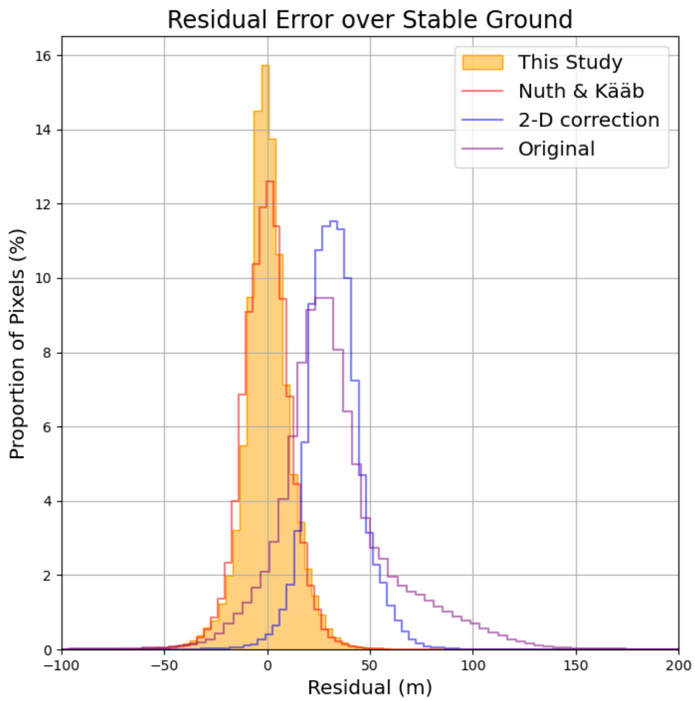

Fig. 1: Difference of DEMs residual error over stable ground at different stages and using different methods of coregistration.

Following the reviewers' recommendation, we use the xDEM python package (xDEM contributors, 2024) *to evaluate the uncertainty of our geodetic mass balance. Having minimized systematic error through DEM coregistration, we follow* Hugonnet et al. (2022) *to evaluate random error over stable terrain then infer uncertainty in elevation change over the glacier. Random error is quantified by considering both the heterscedasticity and spatial correlation of error. Heteroscedasticity is evaluated across gradients of DEM slope and curvature, calculated using methods from* Horn (1981) *and* Zevenbergen & Thorne (1987)*, respectively. The spatial correlation of error is estimated by an empirical variogram using Dowd's estimator* (Dowd, 1984)*. The uncertainty in elevation change within the 2008 glacier boundary is then calculated as the average pairwise product of pixel uncertainties times the spatial correlation of error between each two pixels (Hugonnet et al., 2022 eqs. 17-19).*

Following these methods, we arrive at a mean elevation change of -22.61±0.81 m across the glacier (Fig. 2). Maintaining the density assumption of 900 kg m^{-3} as used throughout the manuscript, this translates to a geodetic mass balance of -442±16 mm w.e. a^{-1}. The change in elevation across the entire DEM including stable and unstable terrain is shown in Fig. 2. We consider the level of uncertainty arrived at using these methods to be acceptable for the purpose of our study. Since most error over stable ground (and therefore elevation change uncertainty) is attributable to artifacts and errors in the 1962 DEM, we also consider the acceptable uncertainty range as testament to the adequacy of our 1962 dataset.

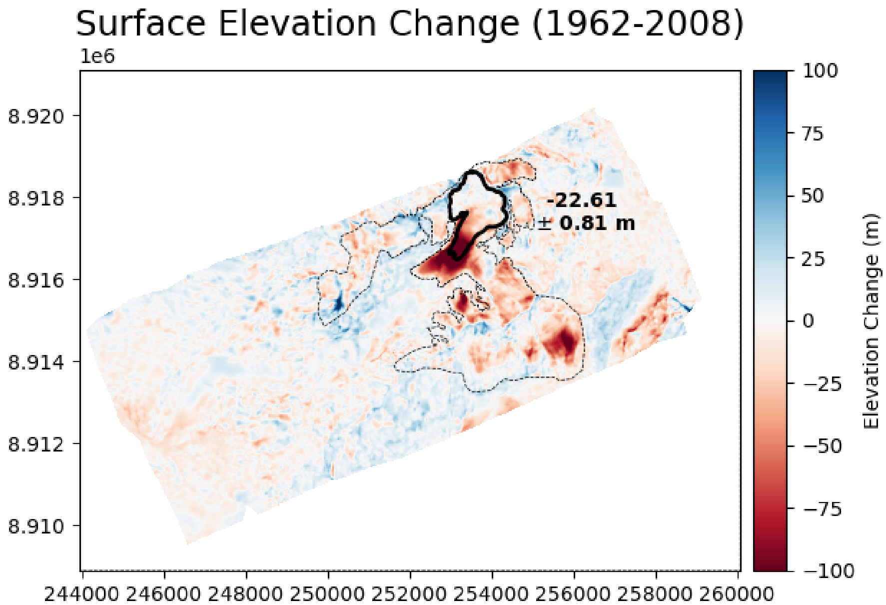

Fig 2: Difference of DEMs (1962-2008) shows significant surface height change across the glacier ablation zone. The 2008 glacier boundary is outlined in bold and unstable terrain (including the 1962 glacier boundary) is delineated by a dashed line. Note that additional terrain above 5000 m in 2008 was also considered to be unstable.

3. *DEM Outlier Identification*

We have assessed the presence of outliers (95th percentile) in the difference of DEMs between 1962 and 2008. These results are compared to the DEM difference used in the original submission.

The outlier detection procedure is as follows:

1. *The data were binned according to their positioning in 50 altitude bins (~16 m) according to the 2008 DEM.*
2. *Pixel values for surface height change were compared to the mean value of each altitude bin. Pixel values with z-score absolute values greater than 1.96 (two-tailed 95th percentile) were considered to be outliers resulting from DEM or coregistration errors and were replaced by the mean value from the appropriate altitude bin.*
3. *The resulting map of change in surface height was used to recalculate the specific (area averaged) mass balance across the entire glacier surface. These results were compared to the original value used.*

After removing outliers, the new SMB was calculated to be -435 mm w.e. per year, a 1.5% positive change from the originally published figure. Maps depicting the original DEM difference, altitude-binned averages, and detected outliers are included below (Fig. 3).

We note that the difference in specific mass balance is within the uncertainty window estimated in the previous step and that it is difficult to distinguish between outliers caused by map artifacts *versus extreme natural phenomena. Furthermore, previous glaciological studies using outlier detection and gap filling have operated with very different data constraints. For example,* Piermattei et al. (2024) *use ASTER and TanDEM-X data, both of which are known to contain artifacts and data voids in mountainous regions. Geodetic mass balance estimation using these global datasets therefore may require outlier correction. In our case using local datasets without issues such as cloud cover, this requirement is less apparent. Given this ambiguity and the negligible impact of outlier correction on the resulting geodetic mass balance estimation when compared to the overall uncertainty, we believe that our original figure is of sufficient quality. We thank the reviewers for their methodological recommendations which have allowed us to more rigorously defend this claim.*

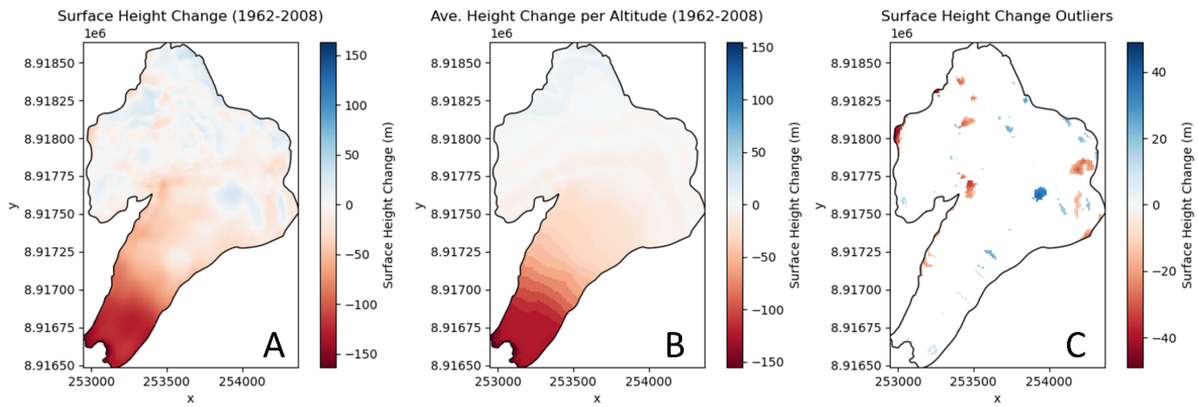

Fig 3: Outlier detection began from the map of elevation difference (a), which was then binned into 50 elevation bands (b). Pixel values exceeding 2σ deviation from the elevation band mean were considered to be outliers (c).

More minor suggested amendments:

L57: perhaps ‘dictate’ rather than ‘direct’ retreat patterns?

Accepted

L95: Refer to Fig 1 here.

Accepted

L95: It would be easy and useful to visualise this increased recession after the transition of the terminus type from land- to lake-terminating. Repeat mapping of terminus position from optical imagery at timesteps a few years apart should yield a nice set of ice front position estimates and the distance between them should show the increase in recession rate related to terminus type transition? Landsat 7 and 8 images would capture this well.

We have conducted the suggested analysis using Landsat 8 and Sentinel 2 images and will include an additional supplemental figure demonstrating accelerated retreat during the period of lake calving (Fig. 4). It must be noted, however, that the absence of annual imagery earlier in the timeseries precludes an assessment of normal variability in the actual retreat rate. Furthermore, our decision to use the elevation band flowline approach in OGGM limits the model’s accuracy *when it comes to glacier length. The linear retreat rate is therefore an inferior validation metric, but does still yield the useful insight that retreat greatly accelerated as frontal ablation developed.*

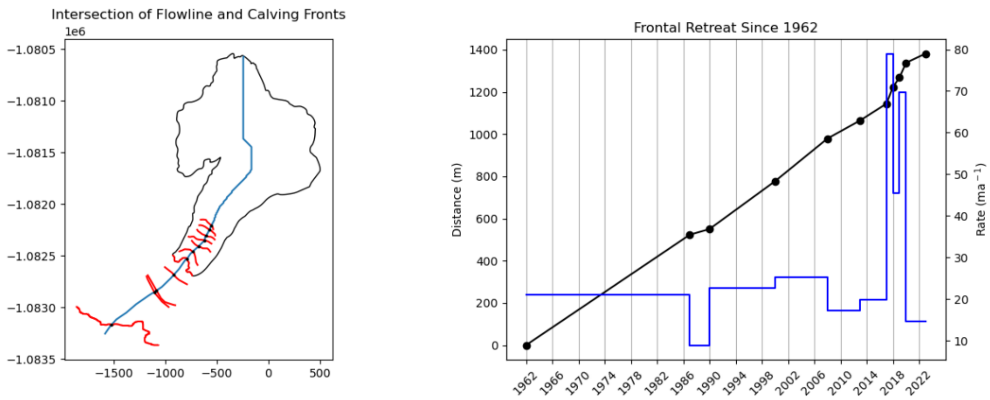

Fig 4: (left) Positions of the glacier terminus mapped fromaerial, Landsat 8, and Sentinel 2 images from 1962-2023. (right) The cumulative retreat of Queshque Glacier (black) with mapped years plotted as points. The retreat rate, or slope of the black line, is plotted in blue.

3.1 We have a few suggestions here:

-We'd really encourage the authors to provide an illustration of the elevation change data they have derived from their respective DEMs across the full study area. This is needed to provide the reader with an indication of the overall quality of the DEMs and the presence or absence of any biases within the derived elevation change data. It'd also provide a powerful illustration of the changes the glacier has experienced.

We provide a map showing the residual differences on stable and unstable terrain between the 1962 and 2008 DEMs with the 1962 and 2008 glacier boundaries outlined in black:

Stable ground was considered to exist off glacier and at altitudes below 5000 m. This second exclusion accounted for resolution differences that produced some large errors over steep ridgelines. As seen in Fig. 1, residuals over stable ground are normally distributed with a mean of approximately 0 m and a standard deviation of about 12 m. Systematic (e.g., aspect-related) bias is minimized, though may be evident locally near the southeast corner of the model domain. Note that this localized issue was persistent across coregistration methods, including when using the Nuth and Kääb (2011) algorithm in the xdem Python package. The large positive residual situated off glacier is due to an artifact in the 1962 DEM wherein a peak is represented as a plateau. This peak is located above 5000 m, however, so did not bias the DEM coregistration process.

-I'd ask the authors to consider the impact of outliers in the elevation change data, which they don't currently mention. As the two DEMs have been generated using very different approaches, there will be outliers which do not represent real elevation change. These should be removed considering the statistical characteristics of elevation change data within a similar altitudinal band (e.g. Gardelle et al., 2013). Over glacier surfaces, values more typical of the elevation change experienced by the glacier within the same altitudinal band should then be used to fill the resulting gaps (e.g. using https://xdem.readthedocs.io/en/stable/), prior to mass balance calculation.

See response in "Major Comments" section above.

-If the authors have access to the 1962 images in their original form, I'd encourage them to explore the possibility of generating a DEM using photogrammetric software now readily available online (e.g. CATALYST https://catalyst.earth/, user-friendly tutorials are available online and a fully functional 7-day trial can be acquired online). The techniques used to generate the two DEMs used to calculate elevation change couldn't currently be more contrasting and various local and broad scale biases could be present as a result, which the reader cant currently see without the data being shown.

See response in "Major Comments" section above.

-The approach to DEM coregistration seems logical and robust and the figures in the supplement suggest good agreement between the DEMs, but a map of elevation change over the glacier and surrounding areas really is needed to confirm this.

See response in "Major Comments" section above.

-The uncertainty associated with the mass balance estimate on which the rest of the modelling is based does not seem to have been considered at all. There are multiple sources of error in the technique the authors have employed (Hugonnet et al., 2022) which can bias the mass balance towards higher/lower overall ice loss. This certainly needs to be estimated to reassure the reader that the mass balance signal is realistic and beyond the level of uncertainty.

See response in "Major Comments" section above.

L134: How many of these point measurements were used to evaluate error? Where were they located? How does the difference between derived and point based measurements vary spatially?

Only 17 georeferenced point measurements were provided by the 2009 GPR survey report. As elaborated upon in the main text, the points show general consistency with the subsequent 2014 survey (Fig. 5a). The 2009 GPR points span from the bottom of the glacier in the southwest towards the center of the glacier in the northeast of Fig. 5b. The points are located approximately along the centerline of the glacier and are each in proximity to multiple measurements from the subsequent GPR survey. There is no apparent relationship between the mean distance from the 2009 data points to their respective nearest neighbors and the resulting difference between measured and derived thickness. There does, however, appear to be a slight spatial bias, with derived thicknesses being more likely to underestimate the 2009 measurements at lower elevations. The significant outlier where the derived thickness is approximately 28 m thinner than observed occurs at a discontinuity in the 2009 survey, suggesting that the technician may have needed to navigate an obstacle which may have produced abnormalities in the ice thickness profile or potentially caused an error in measurement or radargram interpretation.

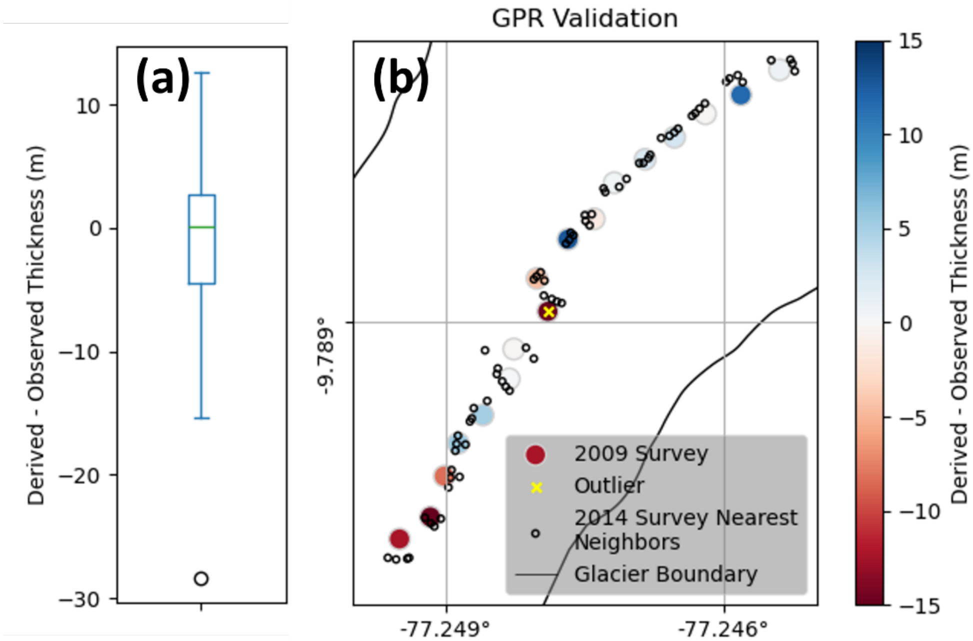

Fig. 5: (a) Box and whiskers plot showing the distribution of error between the derived and observed thickness values, including the position of the single negative outlier. (b) A map of the 2009 GPR survey showing the error calculated at each point.

Line 156: It might be useful to provide a summary figure of the 'average' of the climate data used as input to the temperature index model, as much of the discussion is focused on seasonality (or lack thereof) later in the paper. It would help the reader relate the simulated accumulation and ablation (Fig. 3) to the climate the glacier experiences.

We agree that this would be useful for the reader. We provide Fig. 6 below, which will be included in a revised manuscript.

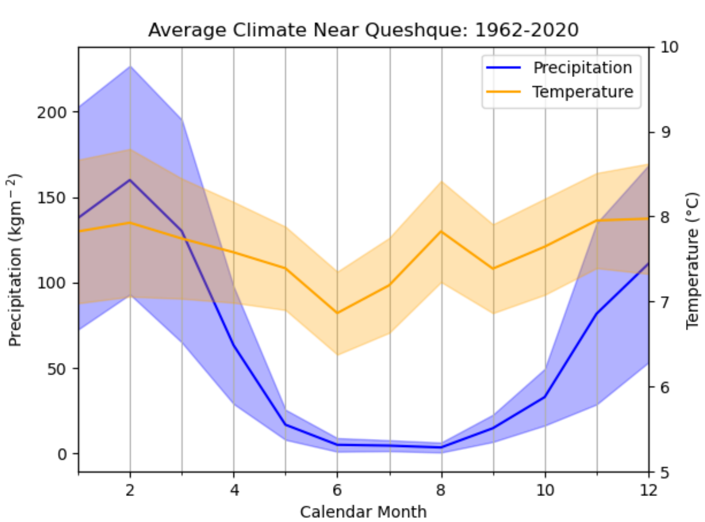

Fig. 6: Climatology near Queshque Glacier during the period 1962 through 2020. Mean temperature (2 m) and precipitation are depicted with 1σ bounds as shaded regions. Climatology data combine CRU (Harris et al., 2014; New et al., 2002) *and PISCO* (Aybar et al., 2020; Huerta et al., 2018) *products. Note that the values shown here are subsequently adjusted by the precipitation factor and temperature bias within the mass balance model.*

Line 163: 'lapse rate of -6.5℃', while this is the global average, was there any testing on using a different value? The tropical Andes can have lapse rates can be lower then -6.5℃, maybe as low

as -3.5℃ (Navarro-Serrano et al., 2020). This could change the temperature index substantially I suspect.

Lapse rates in the tropical Andes are a critical, yet highly uncertain parameter in tropical glaciological studies. Our group's sensor network in the Cordillera Blanca, as well as atmospheric modeling using the WRF, show that regional lapse rates are seasonally variable, increasing in magnitude during the dry season. Measured lapse rates vary from ~-9.1˚C km^{-1} to ~-6.0˚C km^{-1} between seasons, while modeled lapse rates varied from ~-7.5˚C km^{-1} to ~-5.8˚C km^{-1} (Hellström et al., 2017). *One limitation of our model is that it cannot incorporate seasonal lapse rate variability during the mass balance model calibration, despite this playing a potentially crucial role in the tropical Andes. A compromise between the measured and modeled seasonal extremes was therefore selected and we opted for the conventional -6.5˚C km^{-1} for the sake of consistency and comparability with other studies.*

We will clarify this point in the text and address it further in a new limitations section.

Table 1: For the water level, the source could not be Sentinel 2 (optical imagery). The source would be the DEM you used, which needs specifying.

We identified the position of the lake boundary using Sentinel 2 imagery then adopted a water level from the 2008 DEM using this position. This can be clarified in the text and in Table 1.

L236: This description of how the climate data were analysed belongs more in section 3.3 I feel. If it is moved up, it should also be bolstered by citation to appropriate literature to confirm that this is a standard approach to processing these datasets.

We accept moving this description into section 3.3. The detrending method used is a common practice for removing the multidecadal trend from a climatological dataset and converting the timeseries into anomalies from this trend (Wu et al., 2007). *This is a necessary step for comparing the climatology to detrended indices like the SOI or ONI. We further standardize the data using the common standard anomaly approach by dividing absolute anomalies by the timeseries standard deviation* (e.g., Dabernig et al., 2017). *This facilitates comparison between datasets that oscillate on varying orders of magnitude.*

Table 2: The authors state 'low sensitivity' and 'high sensitivity' here, but it is only briefly mentioned in the text. The authors could be more specific on why you have assigned certain model runs as low/high sensitivity. This sensitivity could also be added in the table caption. The mention of the sensitivity from the model runs is only sparsely mentioned in the discussion.

Furthermore, if the authors are not varying the Precipitation Factor, is it necessary to place it in Table 2? Would it have been better placed in table 1 as a 'constant'?

Here "sensitivity" refers to the magnitude of the temperature sensitivity parameter, which dictates the ablation response to a unit change in temperature. We will clarify this point throughout the text and discuss this parameter's relation to the temperature bias more thoroughly in the discussion.

We agree that the precipitation factor should be moved to Table 1.

Table 3: The authors have used different names for their 'sensitivity' models. First, they were 'low – high sensitivity,' now they are 'least – most climate sensitive'. If these contrastingly named model runs are actually the same, the naming needs to be consistent, or if they are different, a section explaining how the experiments were conducted would be useful.

The "low" and "high" sensitivities indicated in Fig. 3 (main text) represent models number 2 and 5 from Table 3, whereas models 1 and 6 are indeed the least and most temperature sensitive. We recognize the confusion this may have caused and will be sure to clarify our nomenclature in the revised manuscript.

Section 5.1 The authors may be able to bolster this section by comparing their modelled glacier mass balance evolution against available measurements of glacier mass balance over the period 2000-2019 (Hugonnet et al., 2021). According to the dataset of Hugonnet et al. (2021), the mass balance of RGI 16.02060 (Queshque Glacier) was -0.59 m w.e.a-1 from 2000-05, -0.73 m w.e.a-1 from 2005-10, -0.83 m w.e.a-1 from 2010-15 and -0.88 m w.e.a-1 from 2015-2019. These estimates provide a point of comparison for the authors modelled results and could also be discussed alongside the climate variables the authors have analysed.

We have compared our ensemble mean specific mass balance (SMB) model outputs for each of the 42 epochs provided in the Hugonnet et al. (2021) study. We find that averaged across the years of a given epoch, our ensemble consistently overestimates mass loss as compared to the Hugonnet et al. study. In other words, our model results suggest that Queshque Glacier is retreating faster than the best estimate from the global geodetic mass balance study.

With this said, our results are within the uncertainty bounds provided by Hugonnet et al. during all but 3 of the 42 epochs. All three of these epochs consider change as of 2020 including 2000-2020, 2015-2020, and 2016-2020. This suggests that a systematic bias exists either in the 2020 data used in taking the geodetic mass balance or in our simulation occurring around that time. Other epochs up to 10 years in duration (the second longest duration behind the single 20-year measurement) show agreement between Hugonnet et al.'s and our own data. In summary, this comprehensive comparison indicates general agreement between the two datasets, bolstering confidence in our mass balance simulation.

Figure 5: It might be good to add a second panel to this figure to illustrate where the centreline (assuming these are centreline velocities) of the glacier runs in these modelled velocity profiles.

The elevation band flowline approach used in this study represents glaciological variables as mean altitude-binned values. The modeled and observed velocities from Fig. 5 (main text) each represent such mean values.

We will clarify this point in the figure caption.

Section beginning L334: A good section acknowledging the limitations of the applied approaches.

We appreciate this feedback and intend to expand our limitations section considering the other points made by the reviewers.

L405: It may be worth considering the findings of Malles et al. (2023) in the discussion of the impact of lake development on the studied glacier. Malles et al. (2023) also establish changes in the sensitivity of glaciers to climate following the introduction of glacier-lake interactions to the same model (OGGM), so their findings may well support the inferences made by the authors here.

Malles et al. (2023) presents an interesting study on the effect that considering frontal ablation during OGGM's mass balance calibration has on projected glacier meltwater contributions to sea level rise. The authors find that considering this additional process reduces the overall projected contributions of tidewater glaciers to 21st century sea level rise. While on first glance this appears contrary to our conclusions and those of various studies of lake terminating glaciers (e.g., King et al., 2018)*, this is not the case. As stated in Malles et al. (2023), the modeled reduction in mass loss above sea level when calving is included "is due to the lowering of the sensitivity to atmospheric temperatures…" which is the logical outcome of calibrating a temperature-index model using mass balance data that are already to some extent decoupled from climate. If one assumes that all observed mass loss relates to changes in the climatic mass balance, then any mass loss due to complex calving processes will be falsely attributed to a change in temperature within the OGGM framework. Consequently, projections under warming conditions will overestimate mass loss, as Malles et al. (2023) observe.*

In the case of our study, we calibrate OGGM using observations spanning a long period throughout most of which the glacier was not calving. We can therefore be more confident that our calibration data relate directly to the climatology. We therefore feel it would be inappropriate to draw comparisons with the Malles et al. study.

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