Response to reviewer 1 (Tobias Zolles)

Long-term firn and mass balance modelling for Abramov glacier, Pamir Alay

Marlene Kronenberg, Ward van Pelt, Horst Machguth, Joel Fiddes, Martin Hoelzle, Felix Pertziger

Dear Reviewer,

We would like to thank you for your attention to our manuscript. We appreciate the interest in our study and thank for your constructive review and suggestions how to improve the quality of the paper. Below, we respond point by point to all comments, and state how we plan to account for them in a revised version of the paper. The responses (normal font style) to the reviewer's comments are written directly into the reviews (displayed in italic font style). Revised/additional figures can be found at the end of this response letter and are labeled with Roman numerals to them from figures in the manuscript.

Marlene Kronenberg,

Fribourg, May 24, 2022

1 Summary

The authors apply an energy and mass balance model for firn and ice to a glacier in High-Mountain Asia (HMA) using the almost 50-year record of meteorological weather station data (AWS) together with down-scaled reanalysis data from ERA5. There is no significant trend in the annual mass balance found, though differences in space and time exist.

The manuscript is quite extensive and technical in the methods, for the main results as well as the abstract focusing on the modeling aspect. In particular, it describes the weather station data treatment quite extensively. These parts of the manuscript would benefit greatly from a shortening. The written part of the results is concise, but there are many figures which are not well included in the results or discussion.

The manuscript describes the measurements, measurement data handling, and treatment in an extensive fashion. Furthermore, throughout the manuscript, the value of this data for the model performance is emphasized. This should be either reduced or also stressed in the abstract and title.

We agree that the manuscript is rather extensive and technical and thank the reviewer for the suggestions of shortening. The data set available for the site is exceptional for this region and allowed for the application of a model of such complexity. This will be further stressed in the abstract. We consider it difficult to capture everything within the title. A slight modification to 'Long-term firn and mass balance modelling for Abramov glacier in the data-scarce Pamir Alay' is suggested.

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2 General suggestions and comments

Shorten and homogenize sections 2.3.1 and 2.3.2 with 2.4.1, which are quite lengthy in comparison to the TopoSCALE and bias correction section which is quite brief. It is also not clear to me, what is done with the precipitation forcing for the AWS time period. For the monthly averages used for the bias correction of the TopoSCALE data mentioned in 2.4.3, is this a monthly average for every January or is it bias corrected for each individual month of the time series? In case of the first, this statement is wrong "Monthly averages of the final cloud fraction time series correspond to observed values for the years for which measurements are available", if the latter, it is a strange way to bias correct, please clarify this.

We will shorten the sections about station data in the main manuscript and provide additional information in the supplementary material. There is no precipitation data available from the AWS (2014-2020) and TopoSCALE ERA5 data is used as a model forcing. We will more clearly state which data set is used for what (as suggested by Reviewer II). The correction was done for a monthly average of every January etc. Indeed, the statement about the cloud time series is wrong (should be "Monthly averages of the final cloud fraction time series correspond to the monthly averages prior to correction"). Will be corrected.

Why is the cloud cover used and not incoming long-wave radiation?

We use cloud cover for several reasons: (i) The EBFM parametrisations use cloud cover to compute incoming shortwave and longwave radiation. Cloud cover is thus a necessary model input. (ii) There are no long-wave incoming radiation measurements available from the original station and thus no such data available for the first years of the study period. (iii) The AWS longwave radiation is affected by a nearby located rock face and there are substantial data gaps. (iv) The TopoSCALE ERA5 long-wave data can thus not be validated/corrected (which would definitely be necessary given the bias corrections needed for the other TopoSCALE ERA5 variables).

Try to shorten 2.5.1 or maybe move the more detailed part to an appendix or supplement.

We will shorten and move details to the supplement.

Some statements are very vague or numbers are missing, try to avoid rather/certain thresholds/relatively/etc.

We will use clearer language and provide numbers.

Reconsidering your comment on the surface mass balance: The surface mass balance is the result of accumulation (+) and ablation (-) at the surface including precipitation (+), moisture exchange (+/-), mass loss through runoff (-) and refreezing above the previous summer surface (+). What is refreezing? If it is surface melt water, it is not accumulation for the SMB, as the melted snow was above the previous summer surface too before it melts. If it is refreezing of rain then it falls to the category of precipitation. Please clarify this. For example with "including solid precipitation" and "refreezing of rainwater above the previous summer surface".

It is mainly snow melt water (precipitation is predominantly solid) which is refreezing within the seasonal snow pack. Indeed, the statement was misleading, as melt does not automatically mean mass loss in the EBFM. Will be corrected.

Section 3.5 is also quite extensive, try to shorten it, you already have most information in the table anyway

Agreed. We will shorten this description.

For p-values where your statistical analysis tool gave you less than the significant digits change it to "<0.001" instead of = 0.000.

ok.

In the results section there are a lot of figures: Check for each figure if it is referred to in the main text. Do you really need it is as part of the main manuscript, can even more of them be shown in the supplement or removed altogether? The correlation plots and one of 11 and 12 could be potential starting points. Furthermore, check your figure axes, in the case of shared y-axes readability may be harder than expected.

We will move figures to the appendix and more often refer to the remaining ones in the text.

The uncertainty discussion and estimation did not quantify or investigate the influence of any assumptions like basing fresh snow albedo tuning only on the summer month, the bias correction *approach on the input data, parameter choices, etc. What is the influence of the precipitation under catch correction? What is the influence of splitting the cloud cover differently over the day, not conforming o the daily average?*

We will provide a comprehensive sensitivity discussion including the quantification of parameter and forcing (also based on the two different runs) sensitivities. We perform model runs for selected grid points using perturbed parameters and show corresponding results in the appendix (cf. Figs. I and II). Additionally, we will present the sensitivities regarding different cloud cover forcings (please note that there is no average cloud cover data available - average in line 87 was wrong, should be max.). See also answer to specific comment regarding cloud cover below.

How does the correlation between measured and modeled SMB depend on the point and time? Figure 7 a/b does not show us if the model fails for certain time periods or certain point measurements. This could be further investigated. Additionally, basing the quantification of agreement on R^2 is tricky, as this is just about the correlation and not absolute errors, so systematic over- and underestimation are not accounted for. There are multiple ways how to compare measurements with models (Zolles et al. 2019). The choice of comparison method has a direct influence on the evaluation.

We will add a visualization for different periods in the scatter plots and show the annual average bias together with the annual mass balance (current figure 4). R^2 was not used for optimization. We aimed on reducing the bias while keeping an eye on the RMS and regression line to omit compensating effect (e.g. a compensation by an underestimation of melt by an underestimated accumulation).

There were two different simulations conducted, as mentioned in the summary, this could be used more to emphasize the improvement that additional measurement data could provide.

The differences between both simulations will be more comprehensively discussed and pointing out that measurements are necessary to evaluate the model performance and that additional data would be an asset.

During the entire discussion, the uncertainties are all given as the relation to the model and model forcing, though not quantified apart from one alternative run based on a shorter tuning period. The precipitation is here most likely the dominant factor due to uncertainty in climate model and measurements. In addition surface mass balance measurements are also uncertain, Zemp et al. 2013 mention that the related uncertainty of the field measurements at point locations is estimated to be $0.14\,\mathrm{m\,w.e.\,a^{-1}}$. What is the impact of this on the uncertainties? How does this change the confidence intervals?

We will add a comprehensive sensitivity analysis which will also allow to quantify the influence of different parameters. Model runs for perturbed parameters were preformed following and expanding the analysis presented for Morteratsch glacier by Klok and Oerlemans (2002). In addition, we will estimate the uncertainties of point measurements based on Thibert et al. (2008) and show them together with an annual average misfit and annual modelled mass balance (current figure 4) for a better estimate of uncertainties. Please note that the parameters were not tuned for the alternative run (will be clarified).

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3 General suggestions and comments

P2 L27: Wrong Hock reference: I guess: Hock 2005: Progress in Physical Geography 29, 3 (2005) pp.362–391

Will be corrected.

P2 L29: acts→removes

ok.

P2 L34-43: Mention the other studies first, then relate to Pamir Alay

ok.

P2 L48: Remove relatively

ok.

P2 L49: Change to "...mass fluxes over the period from YYYY – YYYY "

ok.

P2:L50: Delete "to our knowledge"

ok.

P3 L58: The mean annual .... add "The"

ok.

P4 L64-70: Could this be moved to the introduction, feels a bit out of place

Will be moved.

P4 L87: Remove sentence starting with "Most recorded "

ok.

P4 L89: Could be misleading as you did correct for undercatch later?

Agreed. Will be clarified.

P5 L98: We assign observed daily minimum cloud cover to the first four time steps and daily average cloud cover for the rest of the day. What is the impact of this assumption, did you test it, could you verify it? It does not conform to the daily average if 4 steps are lower and the rest is the average?

We will present mass balance sensitivity regarding the use of different cloud cover forcing for the morning and the afternoon. Please note that no 'average' cloud cover data is available as only a low and a high (not average) cloud cover values are reported per day reported. (The 'average' in L87 is actually wrong - will be corrected).

P5 L105: What is done for precipitation in this period?

As for the other climate variables, We use debiased TopoSCALE ERA5 data as model forcing. The use of data will be clarified.

P5 L106-109: Remove the entire paragraph

ok. Will move detailed description to supplement.

P5 L110: What does this interquartile range filter do? Is this physically reasonable to remove your so to speak outliers, even more so for the outliers that were not detected? The SMB is non-linear with regard to the forcing, is the curve not smoothed this way and the SMB higher? Please clarify, investigate and add to the discussion.

Upper and lower fences were calculated using the upper (q_{75}, median between 50th percentile and uppermost extreme) and lower quartiles (q_{25}, median between 50th percentile and lowermost extreme of the data set). (upper fence: $q_{75} + 3 \times q_{75}$-q_{25}). The filter was used to identify extreme outliers of the entire data data set (likely caused by malfunctions). More local outliers were than removed using a moving mean method. See also Wilks (2011).

As the thereby corrected AWS data was not used to force the model, there is no consequence on the SMB by this approach. As the AWS data was only marginally used, the whole pre-processing is not that relevant for the study and will be substantially shortened in the main manuscript.

P5 L117: Why 1500W/m^2?

Values above appear as outliers when visually analyzing the instantaneous time series.

P6: Section 2.4.2 If you are using ERA5, why is the incoming long-wave radiation not used directly rather than using a cloud cover, which is then adjusted and strangely distributed over the day, and an empiric parametrization using c1 and c2 which are likely different for HMA as your reference used the model on Svalbard

See answer to general remark (second point) above.

P7 L155: section 2.5.1: Shorten, or put to appendix

ok.

P8 L228: Is the temperature of the snowfall considered when fresh mass is added to the snowpack? If rain's is not.

no.

P8 L230: Remove 2nd mention of "subsurface" in this line.

ok.

P8 L234: What would be the impact of the penetrating short wave radiation with quite thin layers? In addition, as it is mentioned a fresh snow layer in summer often melts extremely fast this might be even more relevant?

No data is available for this or comparable sites. The absorption can affect the snow temperature (Munneke et al., 2009). Results from Dalum et al. (2021) also highlight that this is a relevant process with impacts on the surface mass balance and refreezing of different areas on Greenland ice sheet.

P11 L277: Quantify what the certain threshold is.

ok.

P11 L289ff: Check general comments on refreezing.

ok. Will be clarified.

P12 L 315-331: Is this necessary as full text or is table 2 not enough?

Will be shortened.

P13 L344: See my general comment for how to compare measurements and model results, which objective did you use? Bias, MAD, RMSD, etc (Zolles et al. 2019)

We mainly aimed on reducing the bias (see also answer above).

P13 Table 1: What is the impact of calibrating fresh snow albedo only in summer?

The fresh snow albedo is mainly relevant during the melt season. It was calibrated with measurements from the early melt season. The sensitivity regarding perturbations of the fresh snow albedo will be discussed.

P14 Table 2: The fresh snow density is huge if compared to what is measured. I have used the same value before, but did you try different values?

No, we haven't. This would indeed be interesting. However, the current value produces reasonable subsurface densities as visible from figure 13.

Page 17: Figure 5: The left subplot is not readable, with your choice of colors for the different time periods, you cannot see the values if there is a larger area at a later time for previous times, this is clear for 4000-4300m and maybe at the top (could also be non-changing area there). This has to be changed. The shared Y-axis may be a bit too far off from the other subplots, reduce the white space in between the panels or add the Y elevation axis to each.

Colors in plot a will be improved and axes references will be added.

Figure 6: As mentioned above, the different stakes/stake locations or time could be highlighted here, this has the possibility to show more information, else remove.

Will add more information and move the figure to the supplement.

L 360: If that is the only sentence about figure 9, remove figure 9.

Will move the figure to the supplement.

P 18: Figure 7 same as for figure 6

Will add more information and move the figure to the supplement.

Figure 8: Maybe go for multiple colors. The red frame overlaps with baseline at 0, you hardly can see Qlat. Maybe do not make it a full rectangle but just up from zero but no overlap with x-axis/baseline.

Will be improved as suggested.

P19 Table 4: p-values 0.000→ <0.001

ok.

P20 Figure 9: Shared Y-axis on the left panel not right, remove or supplement.

ok.

P23: L421: Mention your value at "an overall mass loss" so the comparison to the other studies works, this might also give the word "somewhat" in L423 a meaning, else remove it.

Will add values.

P24 L435: related→ correlated

Will be changed.

P24 L44ff: Is the unit for this not simple m w.e. m-1

Units will be corrected.

4 Figures

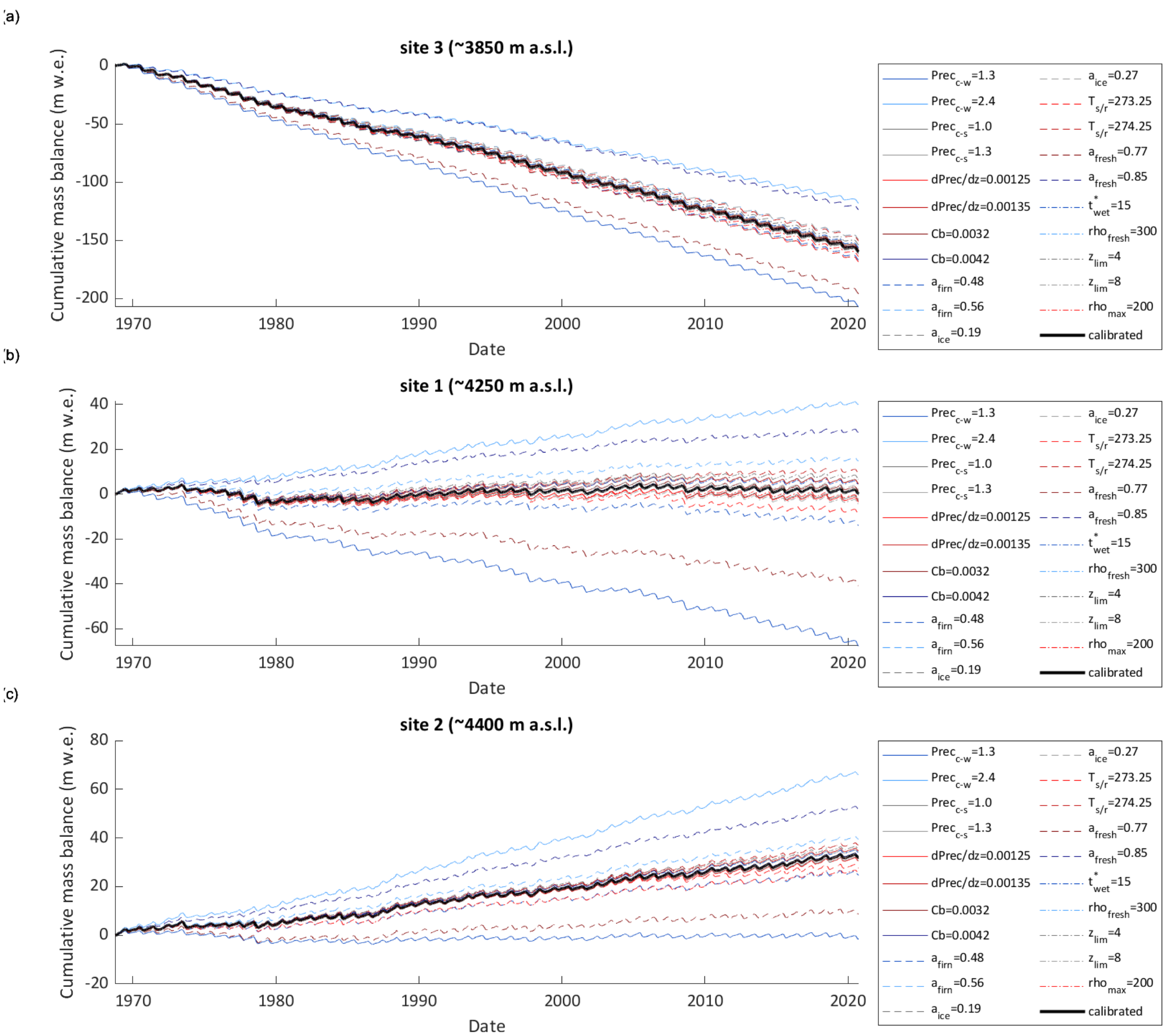

Figure I: Cumulative mass balance evolution for disturbed model parameters using ranges from literature. The cumulative mass balance is shown for three selected points (ablation area (a), lower accumulation area (b) and accumulation area (c)).

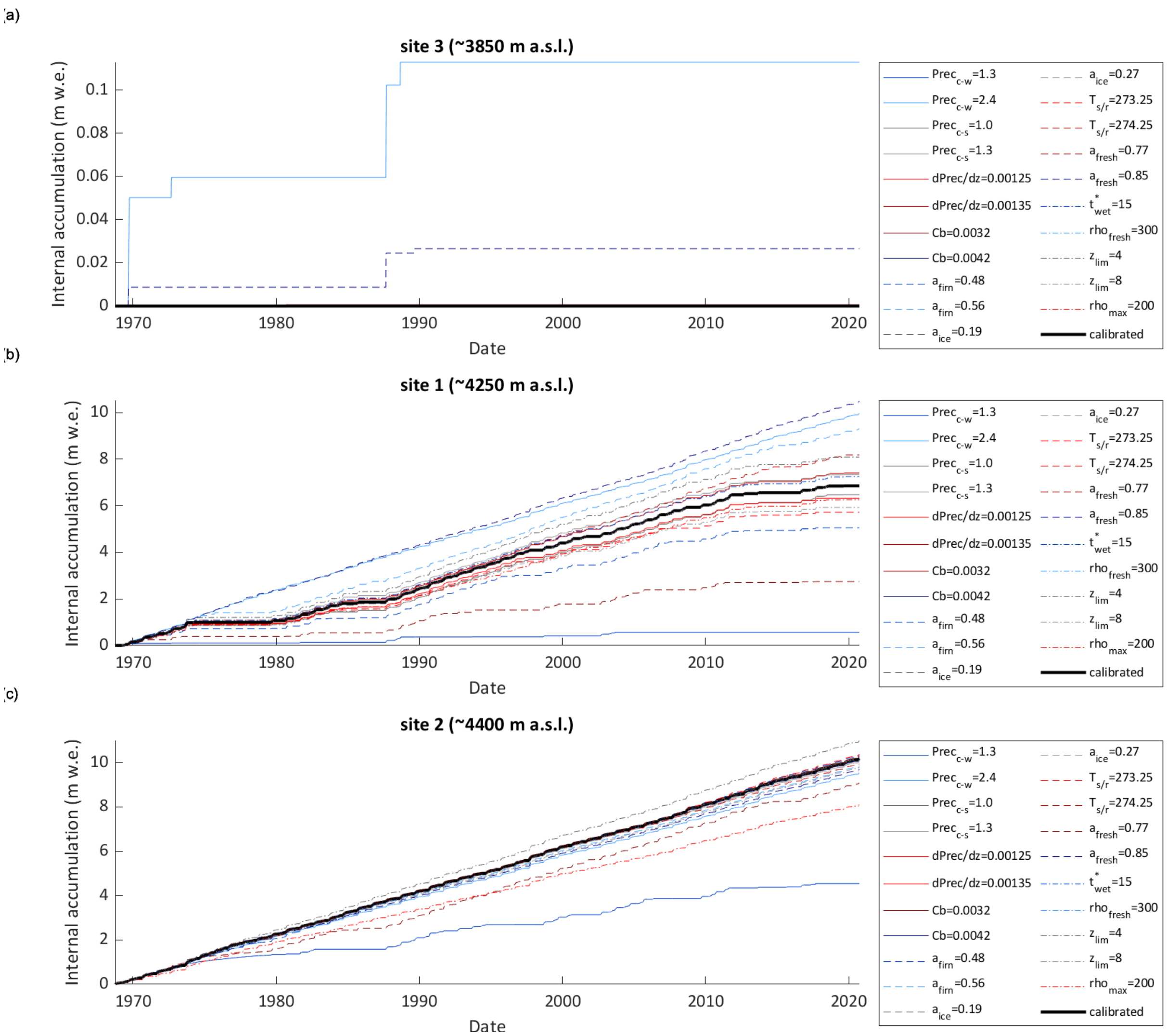

Figure II: Cumulative internal accumulation evolution for disturbed model parameters using ranges from literature. The cumulative internal accumulation is shown for three selected points (ablation area (a), lower accumulation area (b) and accumulation area (c)).

References

Dalum, C. T. V., Berg, W. J. V. D., and Broeke, M. R. V. D.: Impact of updated radiative transfer scheme in snow and ice in RACMO2.3p3 on the surface mass and energy budget of the Greenland ice sheet, The Cryosphere, 15, 1823–1844, https://doi.org/10.5194/tc-15-1823-2021, 2021.

Klok, E. J. and Oerlemans, J.: Model study of the spatial distribution of the energy and mass balance of Morteratschgletscher, Switzerland, Journal of Glaciology, 48, 505–518, https://doi.org/10.3189/172756502781831133, 2002.

Munneke, P. K., Broeke, M. R. V. D., Reijmer, C. H., Helsen, M. M., Boot, W., Schneebeli, M., and Steffen, K.: The role of radiation penetration in the energy budget of the snowpack at Summit, Greenland, The Cryosphere, 3, 155–165, 2009.

Thibert, E., Blanc, R., Vincent, C., and Eckert, N.: Glaciological and Volumetric Mass Balance Measurements: Error Analysis over 51 years for the Sarennes glacier, French Alps, Journal of Glaciology, 54, 522–532, 2008.

Wilks, D. S.: Statistical Methods in the Atmospheric Sciences, Academic Press, 3 edn., 2011.