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

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.

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.

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

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

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

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”.

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

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

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.

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?

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.

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.

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 m w.e. a−1. What is the impact of this on the uncertainties? How does this change the confidence intervals?

Specific comments:

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

P2 L29: acts → remove s

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

P2 L48: Remove relatively

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

P2:L50: Delete "to our knowledge"

P3 L58: The mean annual …. add "The"

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

P4 L87: Remove sentence starting with "Most recorded "

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

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?

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

P5 L106-109: Remove the entire paragraph

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.

P5 L117: Why 1500W/m²?

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

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

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

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

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?

P11 L277: Quantify what the certain threshold is.

P11 L289ff: Check general comments on refreezing.

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

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)

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

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?

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.

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.

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

P 18: Figure 7 same as for figure 6

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.

P19 Table 4: p-values 0.000 → <0.001

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

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.

P24 L435: related → correlated

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

References:

Hock 2005 other paper: Progress in Physical Geography 29, 3 (2005) pp. 362–391

Zemp, M., Thibert, E., Huss, M., Stumm, D., Rolstad Denby, C., Nuth, C., Nussbaumer, S. U., Moholdt, G., Mercer, A., Mayer, C., Joerg, P. C., Jansson, P., Hynek, B., Fischer, A., Escher-Vetter, H., Elvehøy, H., and Andreassen, L. M.: Reanalysing glacier mass balance measurement series, The Cryosphere, 7, 1227–1245, https://doi.org/10.5194/tc-7-1227-2013, 2013. a, b

Zolles, T.; Maussion, F.; Galos, S. P.; Gurgiser, W.; Nicholson, L. (2019): Robust uncertainty assessment of the spatio-temporal transferability of glacier mass and energy balance models. In: The Cryosphere 13, pp. 469 - 489