Review of Temme *et al*.: *"Strategies for Regional Modelling of Surface Mass Balance at the Monte Sarmiento Massif, Tierra del Fuego"*, by Enrico Mattea

The study by Temme *et al*. employs models of various complexity level (from degree-day to full energy-balance) to simulate glacier mass balance at the Monte Sarmiento Massif (MSM), Tierra del Fuego. The models are calibrated against geodetic mass balance estimations, testing three different calibration strategies, and evaluated using an objective aggregate score. The Authors conclude that regional geodetic observations are the better calibration target to improve model transferability, compared to single-glacier mass balance; the addition of a snowdrift model increases overall model performance. At the same time, no single model clearly out-performs the others, and comparison to *in situ* ablation measurements shows very poor agreement for all tested approaches.

The research questions addressed by the Authors are relevant and of current interest – especially calibration of physical mass balance models and assessment of the benefits of added complexity compared to parametrized approaches (e.g., Brun *et al*., 2022). The investigated location is important for an improved coverage of diverse climatic and topographic settings in mass balance modeling. Furthermore, I appreciate the Authors honest presentation of the challenges facing model calibration, validation and transferability.

Still, in the current form the study raises methodological concerns about the input datasets processing and the model calibration choices. These could potentially lead to significant differences in the reported results, and need to be discussed by the Authors. Presentation of the methods and results also needs to be improved, both to ensure reproducibility and to better substantiate the conclusions. Thus, the manuscript clearly needs major revisions. My review includes three Major and some Minor comments that should be addressed in the Authors response, and several Technical comments which refer to individual statements, tables and figures.

Major comment 1: model sensitivity and the choice of calibration parameters

One stated focus of the study is the calibration of surface mass balance (SMB) models of various complexity. As such, the choice of which model parameters are subject to calibration (and of the explored ranges of values) is crucial and must be informed by a well-documented sensitivity analysis – all the more so when models are run in a setting with scarce *in situ* observations like the Cordillera Darwin. In fact, sensitivity of physically-based glacier mass balance models like COSIPY is an important topic of current research (e.g., Brun *et al*., 2022, and reviewer comments therein; Mattea *et al*., 2021). Comprehensive sensitivity analyses from diverse glacierized regions are needed to assess the benefits of increased model complexity, which is one of the stated purposes of the present study.

The Authors select some parameters for calibration (Table 1, ll. 268-269), without showing nor discussing the associated sensitivity testing; further on, there is no more discussion of the consequences of leaving other parameters at their default values. Such values are either arbitrarily chosen, or calibrated by previous studies in settings potentially very different from the MSM study area.

In fact, the best-performing model runs all achieve very similar skill scores for each model type (Fig. S1, S2): as such, the choice of a best-performing parameter set can certainly be affected by the values selected for the other parameters (not considered for calibration). In other words, multiple combinations of physically plausible values can produce very similar results for glacier-averaged mass balance. With little *in situ* data available (notably a complete lack of accumulation measurements), the simulation is therefore largely under-constrained; calibration choices made by the Authors must be better discussed.

In particular, the correction of precipitation under-catch is set at 20 % throughout the simulations (l. 137), with no supporting evidence. Such a parameter is known to be highly uncertain and time-dependent (e.g., Sevruk, 1997; Barandun *et al*., 2015; Buisán *et al*., 2017), and exerts a direct control on modeled mass balance – so much that it is the one parameter of choice for model calibration to geodetic mass balance in Huss *et al*. (2009). While the claimed focus of the manuscript is more on calibration of the melt model (l. 14), several sections refer to *SMB* model performance and transferability, clearly including also accumulation (ll. 199-214). Moreover, the snowdrift module used in calibration strategy C is allowed to alter snowfall totals by ± 10 % (l. 320). Given the relatively small 20 % precipitation correction, such a potential bias is significant and should be discussed.

Other parameter choices which should be addressed in the manuscript include atmospheric transmissivity (l. 242); fresh snow albedo in COSIPY (as DDF_{snow} is indeed calibrated in the PDD model); the threshold temperature for solid/liquid precipitation; and the temperature at which melt can occur in the PDD model. For each of these parameters, the Authors should provide supporting evidence for the used values; or at least comment on the consequences of them being somewhat arbitrarily chosen.

Focusing on the COSIPY model, as acknowledged at l. 549, the best performing set of calibrated parameters lies on the margin of the tested ranges – for all three parameters (Table 1, Fig. S2c). I commend the effort by the Authors to not introduce physically implausible values in the simulations (l. 550); nonetheless, such a result confirms that the value of one or more other parameters (not considered for calibration) is not optimal. This should be discussed, since the purpose of calibration is usually to find a local maximum of model skill within the tested parameter ranges – not outside. In particular, the best parameter set appears to minimize energy inputs to the glacier (highest albedo, slowest albedo decay, smallest roughness length in a warm and moist setting). A well-documented examination of the simulated energy fluxes may yield some insights into the causes of the observed model behavior.

While all models achieve quite a low RMS error compared to the glacier-wide geodetic estimations (Table 2), agreement with the *in situ* ablation measurements is very poor (Table 3) and should be better discussed. Importantly, model biases appear to persist (for a given observation period) across stake locations (Fig. S4). The presence of large, spatially coherent biases should be investigated. It could indicate an enduring model miscalibration (Mattea *et al*., 2021), or the input meteorological series could include biases or major outliers – although the effect of the latter could be partly mitigated by the use of downscaled reanalysis data. Some questions that could be addressed include: if stakes are "spread over the ablation area" (l. 144), why are melt amounts almost the same at all stake locations according to the PDD model (Fig. S4)? Is the drop in modeled melt over 2016 (Apr-Oct) supported by a drop in PDDs? If yes, what is then the role of incoming radiation? (2016 Apr-Oct is notably the only instance in Fig. S4 where the SEB_Gpot simulates more ablation than SEB_G). It would also be interesting to calculate the cumulative sum of positive degree-days estimated at each stake location over each period shown in Fig. S4, and also to compare it against the value computed from AWS data only.

Finally, the aggregate model score (l. 334) is suitable for model ranking, but the actual sensitivity (i.e., the impact of a parameter change on modeled mass balance) is arguably more interesting for inter-model comparisons, uncertainty assessments and the design of future studies. It should therefore be briefly summarized for each parameter, and possibly reported in extended form in the supplementary materials.

Major comment 2: reference-surface mass balance compared to geodetic mass balance

SMB in the four models is computed over 2000-2022 (and sub-periods) using the constant glacier outlines of Barcaza *et al.* (2017) and presumably a constant digital elevation model (DEM). This approach is commonly referred to as the reference-surface mass balance (RSMB; Elsberg *et al.*, 2001), as opposed to the so-called conventional mass balance (CoMB), which is calculated taking into account the temporal evolution of glacier extent and hypsometry (Huss *et al*., 2012).

Glacier retreat – taking place mostly at the terminus, where specific mass balance is more negative – provides a stabilizing (negative) feedback, which reduces mass losses. As such, over the years the cumulative CoMB of a retreating glacier will accumulate an increasingly positive bias compared to the RSMB (Fig. I). The magnitude of such a bias is related to the extent deglacierized during the study period, especially increasing (on a retreating glacier) if the reference surface is measured at the start (Elsberg *et al*., 2001; Mukherjee *et al*., 2022). A larger bias is also possible on glaciers with steep mass balance gradients, as in Tierra del Fuego.

The RSMB is arguably more useful than the CoMB for climatic interpretations (e.g. Harrison *et al*., 2005); but unlike the geodetic mass balance it does not simply reflect mass change at the considered glaciers (Thomson *et al*., 2017). As such, the two values are not directly comparable for model calibration.

The order of magnitude of the discrepancy can be roughly quantified, using the land-terminating Pagels glacier (Fig. 1) as an example. Reported glacier-wide RSMB is -0.49 m w.e. yr^{-1} (Table 2, PDD model, Strategy C), over an area of 18.59 km^2 (Table 2). The 2004-2019 area loss (as per the 2022 inventory: https://dga.mop.gob.cl/estudiospublicaciones/mapoteca/Documents/IPG2022.zip) is 0.67 km^2, in a region with strongly negative specific mass balance (Fig. 5). If the average SMB over the 2004-2019 deglacierized area is e.g. -6 m w.e. yr^{-1}, glacier-wide SMB (modeled over the 2004 area) could then be decomposed in the following area-weighted average:

$$-0.49 \cdot 18.59 = X \cdot (18.59 - 0.67) + (-6) \cdot 0.67$$

X being the average mass balance over the 2019 glacier extent.

The result is X = -0.28 m w.e. yr^{-1}, which is 0.21 m w.e. yr^{-1} less negative than the reported value of -0.49 m w.e. yr^{-1}.

The actual numbers will depend on the spatial distribution of specific mass balance and on the spatial patterns of glacier retreat, but clearly the mass balance discrepancy has the same order of magnitude as the reported RMSE values (Table 2). As with the model parameter choices, this can certainly affect the best parameter combinations which are computed by calibration. As such, the results of Table 2 – including the relative performance of models and calibration strategies on individual glaciers – may be inaccurate, and statements such as l. 409 (“further tuning is neither required nor justifiable”) and l. 521 (“Going from a single-glacier calibration (Strategy A) to a regional calibration (Strategy B), only the TLR needs changing”) may no longer hold true. The rough calculation shown above refers to a single glacier (Pagels); still, the argument is readily transferable to all glaciers in the MSM, which are undergoing rapid (but uneven) area changes at their termini.

In order to properly compare model output to geodetic mass change, the models should be run on up-to-date input grids for each year (Barandun *et al*., 2015). Alternatively, the CoMB could be computed from the RSMB with the methods of Elsberg *et al*. (2001), or in a post-processing stage as in Kronenberg *et al*. (2022).

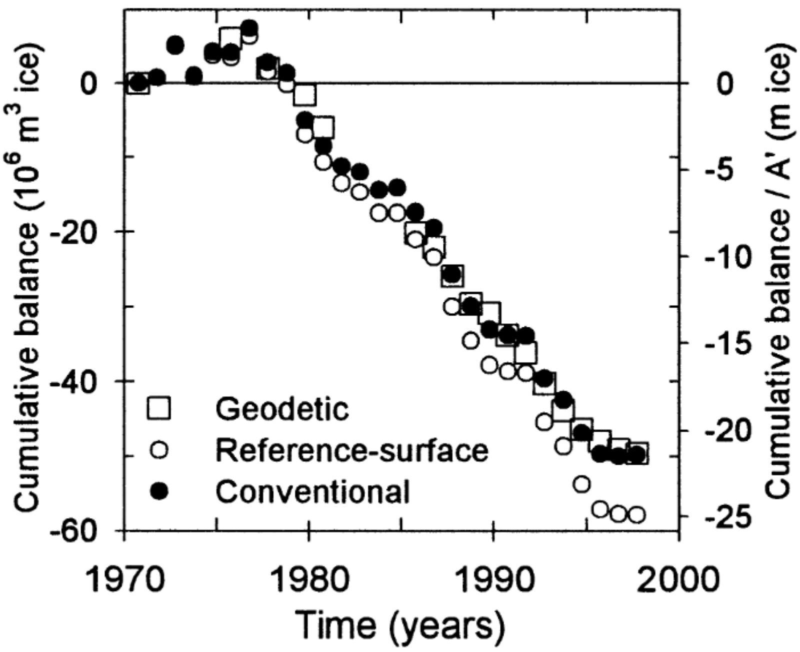


Figure I: cumulative geodetic, reference-surface and conventional mass balance of South Cascade Glacier (USA). Figure from Elsberg et al. (2001).

Major comment 3: geodetic data processing

I tried to reproduce the computed geodetic mass balances (Fig. 2), from the glacier outlines of Barcaza *et al*. (2017) and the grids of surface elevation change of Braun *et al.* (2019), downloaded respectively from https://dga.mop.gob.cl/estudiospublicaciones/mapoteca/Documents/Glaciares.zip and https://doi.pangaea.de/10.1594/PANGAEA.893611.

The elevation change grids contain patches of large absolute values near the edges of the glaciers (Fig. II), which are likely outliers and can significantly alter geodetic mass balance estimations. Moreover, large data voids are visible in the accumulation areas of several glaciers.

Indeed, recomputed geodetic mass balance (Fig. IIIa) does not match the result in Fig. 2 of the manuscript. Filtering out the 2nd and 98th percentiles of elevation changes (as mentioned by Braun *et al*., 2019) yields a closer result but not quite a match (Fig. IIIb); if anything, it shows that the study results can again be very different following relatively minor methodological choices. Since geodetic mass balances are a key input of the present study, it is important to detail all processing steps (filtering, gap-filling, etc.) applied to the initial datasets – possibly in an appendix or supplement.

Moreover, uncertainties in the geodetic mass balances (quickly mentioned at l. 174) must be shown, both per-glacier and for the entire study area (in Fig. 2 and/or Table 2).

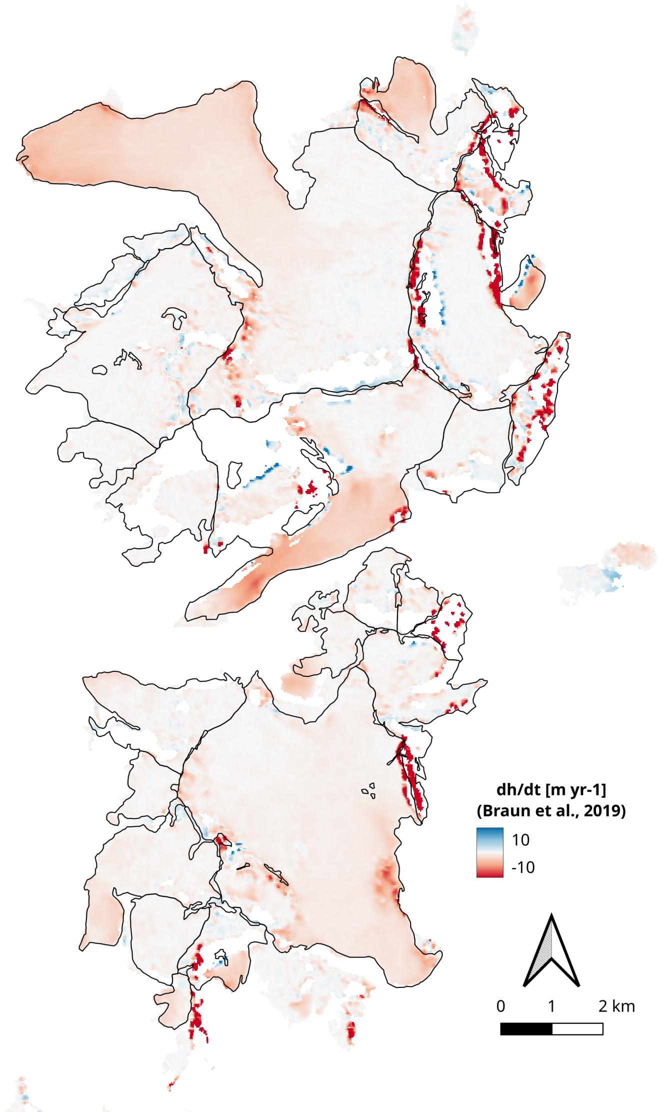
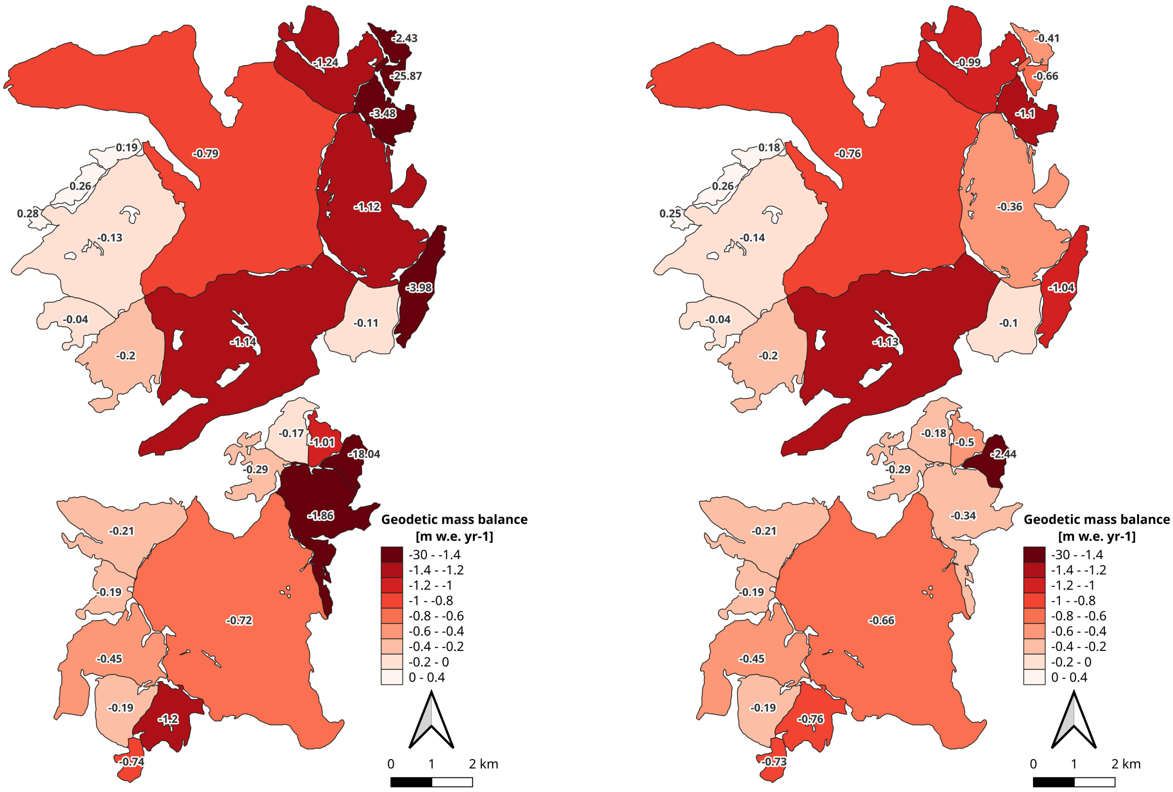


Figure II: 2000-2011/15 elevation change rate at MSM. Data by Braun et al. (2019), outlines by Barcaza et al. (2017).



(a) (b)

Figure III: (a) geodetic mass balance computed from the original data of Barcaza et al. (2017) and Braun et al. (2019). The grids of surface elevation change (Fig. II) are simply averaged over each polygon, then the result is converted into a mass change by multiplication by the density factor of 900 kg m^{-3}. (b) Same as (a), after filtering the elevation change grids at the 2nd and 98th percentiles.

Minor comments

1. Presentation of mass balance models
   Introduction and description of mass balance models should be improved. At ll. 64-70, the text needs to cover previous work on temperature index models, with more references than just Six *et al.* (2009) and Gabbi *et al.* (2014). Such models are mentioned here for the first time – not just in the Methods section; thus the relevant references should also appear here.
   Not all empirical models simply assume a linear relationship between temperature and melt rates (l. 65) – the most relevant variants and enhancements should be briefly mentioned. As the paper is about calibration strategies, it would be useful to also cite (and possibly compare in the discussion) other approaches at calibration of PDD models, like the use of snow line positions of Barandun *et al*. (2021). Calibration of full energy-balance models has also been extensively tackled in previous work, which should be appropriately referenced (e.g., van Pelt *et al*., 2012; Gilbert *et al.*, 2014; Mattea *et al*., 2021; and references therein).

2. Presentation of the input data
   All data mentioned in Sect. 2 should be shown in greater detail. Specifically, an ablation stake network is mentioned – it should be displayed on a map (possibly an inset of Fig. 1). The same applies to the automatic ablation sensor and the location of ground-penetrating radar tracks. The final meteorological series is also a key input, as such it should be either made publicly available, or shown in a figure (possibly in the supplementary materials).

3. Description of the methods
   The methods should be presented in enough detail to enable reproducibility of the study. Below, I list some instances where more information is needed.
   - Numerical model setup: some information on the actual model setup is missing, such as the elevation grid used and the grid cell resolution. Did the Authors re-implement their own version of a PDD model? If yes, it would be good (for reproducibility) to make the code publicly accessible online. Moreover, does the time resolution listed for the PDD model (24 / 8 = 3 hours, l. 230) apply also to the other models used? COSIPY also has several parameters related to the vertical subsurface layers (l. 253) – were these left at their default values? Recent evidence indicates potentially large impacts of the numerical setup on computed melt amounts (Brun *et al*., 2022, and reviewer comments therein). For reproducibility and future comparisons, it would be beneficial to add a table (possibly in the supplementary material) of the main parameter values used in the models setup.
   - The accumulation model should be better explained. Specifically, how is precipitation partitioned into solid and liquid components? How are the AWS measurements used to “inform the statistical downscaling” (l. 143) of precipitation? In the orographic precipitation model, the “timescales of hydrometeors” should be briefly explained (since they are explicitly referred to). The sensitivity tests mentioned at l. 211 should be better explained – what is the “optimal relative humidity threshold”? Optimal in relation to what, according to which metric?
   - The snowdrift model described (Eq. 6) does not match the cited Warscher *et al*. (2013, Eq. 10) – there is an additional factor U giving linear dependence of accumulation on wind speed. If this is indeed the case, the change is major and should be explained.

- I could not find which glaciers exactly contribute to the B_{MSMnc} (massif-wide mass balance used for calibration). Are these all the glaciers of Table 2 except all the lake terminating ones? It should be made more clear in the table caption.
- At ll. 319-320, it should be made clear how the Authors "defin[e] the regional massif-wide amount of accumulation". Is it simply the output of the Orographic Precipitation Model, partitioned into solid and liquid precipitation according to local air temperature?
- At l. 437, the Authors claim that snow line altitudes from satellite observations support their computed spatial patterns of Equilibrium Line Altitude (ELA). While I believe the Authors, I still suggest to either remove the statement or show supporting evidence.
- At l. 515, it is not clear how the Authors "calculate a rough estimate of τ from ERA5 data". The method should be described (possibly in the supplementary material), or a reference should be provided.

4. Quantification

   Throughout the manuscript, several statements should receive quantitative support. Some examples:
   - l. 436, "Equilibrium line altitudes tend to be lower in the east of the massif" – by how much, and what is the spread? The ELA is one of the fundamental quantities in mass balance studies, and its spatial patterns are certainly of interest for comparisons and future studies.
   - l. 494, "the differences between both models are overall minor" – it would be good to mention here the relevant values from Table 2, such as the global mass balance and RMSE differences.
   - l. 644, "surface velocities of around 402 m yr^{-1}" – 402 is quite a specific number, which suggests an uncertainty (and/or variability) affecting only the units place, all across the glacier calving front. Is this the case? If not, could the Authors provide an estimation of the spatio-temporal variability and uncertainty of the values? Else, the number should be given as an order of magnitude only.
   - ll. 653-654, "the uncertainty in the observed elevation change rate is large […] we assume an increased uncertainty [...]" – the Authors mention estimating these uncertainties (l. 174); the numbers should be provided, to support the given explanation of the mass balance discrepancy (is the uncertainty at glacier 138 20 % times larger than for the other glaciers? Or 100 times larger?).

5. Benefit of increasing the complexity level

   Research question Q3 (l. 99) states: "Can the performance of the SMB model be improved by increasing the complexity level regarding included processes?". The inclusion of a snowdrift module is indeed shown to reduce the overall model error. But at the same time, the addition of a physical model for incoming radiation (SEB_G, l. 244) also represents an increase in the complexity level; and the Authors observe (l. 495) that it does not improve the performance of the SMB model. As such, the conclusion at l. 694 should be revised to reflect these contrasting results.

Technical comments

- ll. 31 and 53: “2000-2011/14” is not fully clear, please explain the date range.
- l. 33: I suggest adding *in situ* to “scarce observations of glacier MB”, as remote sensing observations appear to be plentiful.
- l. 49: please specify the time range of the Little Ice Age – is it the same period as commonly understood in the European Alps?
- ll. 53-54: the two estimates of annual thinning rates appear to be in stark contrast. It would be useful to mention whether they have been reconciled, or they refer to different areas, or the more recent study has superseded the previous results.
- ll. 58-63 are a description of the study site, partially repeated from line 107 in section “Study site and data”.
- ll. 391 and 395: if I understand correctly, mass balance in Strategy B is calibrated solely to the regional value (l. 385). As such, it is not surprising that the value of B_{MSMnc} is reproduced perfectly and the bias is no longer discernible – it is the only expected outcome of a successful single-target calibration. If that is the case, I would then suggest rephrasing these sentences.
- l. 411: this is a methodological choice which should be mentioned already in the methods.
- l. 442: summer and winter together amount to 46 % of snow accumulation – then at least one other season should contribute the single largest amount over the year. Could the Authors please provide some information on the occurrence of the other 54% of snowfall?
- l. 456: this appears to be an exact repetition of l. 352.
- ll. 467-468: it is not immediately clear what is a negative range of uncertainty, please explain.
- Table 3: here the BIAS (mean signed difference) should be shown alongside the RMSE. Moreover, the simple (unweighted) arithmetic average of RMSE at multiple stakes and at one automatic ablation sensor does not appear to be a very relevant metric.
- l. 563: winter ablation is mentioned here (in the Discussion) for the first time. Its quantification is a result and should appear already in the corresponding section if it is to be compared to previous studies.
- ll. 595-604: these are objective results, I would recommend moving them to Sect. 4.
- l. 613: could the Authors formulate here a hypothesis as to why the exclusion of Schiaparelli Glacier from the results significantly alters the relative performance of the models? This would be beneficial for a deeper understanding of the models intercomparison and applicability to other geographic settings.
- l. 701: the PDD approach is by now well established and known to produce robust results, “surprisingly good” may not be the best wording here.
- Fig. S1a/b/c: add white crosses as in Fig. S1d/e.
- Fig. S1e: it is quite hard to compare the different values. I would suggest placing the two *DDF* values on different axes, to see if a more readable (smoother) result can be achieved.
- Fig. S5: the Y axis is likely wrongly labeled – ablation rates are too low compared to e.g. Table 3.

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