Answer to Reviewer 2

In the following, we provide a point-by-point Author Response (AR) to any of the Reviewer Comments (RC) obtained for the manuscript that was under discussion. Suggestions for how the manuscript text could be revised are presented in *italics*.

Major Comments:

RC1: Temperature lapse rates: There are several issues regarding the treatment of temperature lapse rates in this study.

a) Initial lapse rate calculation: The authors calculate the initial lapse rates from the temperature and elevation of the cells next to the glacier (L170, L215). I am concerned that if the neighboring grid cells are not ice-covered but instead rock, forest, or other surfaces, their microclimate and hence surface temperature differs from that of glaciers. Consequently, the derived lapse rates may be inaccurate. I recommend calculating lapse rates directly from pressure levels at glacier grid cells, as for example done in Draeger et al. (2024) or Marzeion et al. (2012). Additionally, the manuscript does not report the initial lapse rates used. Please provide a range and compare them to commonly used lapse rates in global glacier modeling (e.g., the constant value of - 6.5 K km^{-1} in Schuster et al., 2023).

b) Lapse rate adjustments: The authors introduce a new third calibration step, where temperature lapse rates are adjusted to further optimize the model's reproduction of observed SCAF (L211–212), which is new compared to Cremona et al. (2025). It is unclear to me why the remaining bias in the third calibration step is attributed to the lapse rate rather than to near-surface temperature values. Previous glacier models have often applied a separate temperature bias correction in addition to lapse-rate correction (e.g., Rounce et al., 2020). To correctly separate the sources of bias, I recommend calculating more accurate lapse rates from pressure levels and framing the adjustment as an additive temperature bias correction instead of a temperature lapse rate adjustment. Conceptually, this distinction is important: lapse rate bias represents the bias in the vertical temperature gradient, while temperature bias reflects errors in near-surface temperature. This reframing would not change results, as the lapse rate correction is also applied additively (see minor comment on assumed typo in Equation (4)), but it would clearly disentangle the two effects and improve conceptual clarity.

AR1: We thank the Reviewer for the review of our manuscript and for the time invested in checking our submission. a) For what the specific comment is concerned, we entirely understand that this approach would be optimal and is often used in regional-scale glacier studies. However, in our case it is both not possible, and -– in our opinion -– not necessary. We elaborate our argumentation below in a new paragraph in the Discussion section to address the constraints and limitations with respect to the choice of air temperature lapse rates.

The calculation of the initial temperature lapse rate, based on the grid cells around the glacier's centroid (see Sect. 3.2), does not consider the surface type of the cells, which may lead to inaccurate estimates. Our temperature lapse rates range from –6.5 to –4.4 K km^{-1} with an average value of – 6 K km^{-1}, i.e. slightly lower than the value commonly used in global glacier modelling studies of –6.5 K km^{-1} (Schuster et al., 2023). While for the large glaciers, the surface type of most grid cells around the glacier centroid corresponds to ice, for small glaciers the surface type may be different, i.e. mostly rock. Thus, the different microclimate could affect the resulting temperature lapse rate. To overcome this challenge, other studies calculated lapse rates directly from pressure levels of gridded climate re-analysis datasets (e.g. Marzeion et al., 2012; Draeger et al., 2024). However, the high-resolution meteorological dataset for Switzerland does not provide the respective information. Also, gridded air temperature in the available data set is not derived from physically-informed modelling that distinguishes between glacier and non-glacier grid cells but is based solely on terrain elevation and interpolation of surrounding weather station data (MeteoSwiss, 2021). As a result, since most *meteorological stations are located below 2500 m a.s.l., air temperature lapse rates in high mountain areas are inherently poorly constrained. We thus adjust lapse rates during calibration, and we argue that applying a more sophisticated method to estimate their initial values would only have a minor impact.*

b) As described above, we now specifically discuss the constraints and limitations due to the choice of air temperature lapse rates in a new paragraph of the Discussion section. We also fully understand the concern raised by the reviewer here. Indeed, adjusting air temperature (in addition to the precipitation correction) is a common approach in global glacier modelling (e.g. Huss and Hock, 2015; Rounce et al., 2023; Schuster et al., 2023). We considered implementing a similar strategy here as well, however, we realized that it is not effective enough to improve model performance in both matching geodetic mass change rates and observations of the Snow-Covered Area Fraction (SCAF). The air temperature lapse rates in high-mountain areas are inherently poorly constrained. This is also mentioned by the review in the first comment. Although lapse rates are measured and well represented in the high-resolution meteorological data set used in our study below ca. 2500 m a.s.l. they are basically extrapolated above this elevation, which is most relevant for glaciers. We thus argue that air temperature lapse rates are actually a poorly constrained model parameter that can be used to optimize agreement with all available data sets. These explanations are now included in the additional Discussion paragraph (see above).

RC2: Extrapolation: The extrapolation relies only on topographic features and does not incorporate climatological variables, which could in my view introduce substantial uncertainty. L383–385 states: "Considering that the Swiss-wide average precipitation correction factor is 1.8 and the winter mass balance 1.27 m w.e., uncertainties in winter mass balance introduced during the extrapolation correspond to 20–25% on average." This is considerable, and it would be worth testing whether including climatological predictors per glacier (e.g., average mean summer temperature, average total winter precipitation, average temperature range, etc.) could improve the performance. Moreover, since SCAF observations are mostly available for larger glaciers (L59), it would be very valuable to include an analysis of extrapolation performance for smaller glaciers. As the training set is biased toward large glaciers, there is a risk that mass loss from smaller glaciers is not correctly modeled (underestimated?) when extrapolating parameters. For instance, you could compare the test-set performance between small and large glaciers.

AR2: This is a valid comment and we agree that testing additional variables can be beneficial. In response to this suggestion, we included more predictors in the machine-learning scheme for mass balance extrapolation and evaluated whether the results improve compared to those presented in the submitted version. In fact, we found that including factors such as the average mean summer temperature, average total winter precipitation and average temperature range does not deliver significant differences in the resulting mass balances. Therefore, we decided not to rely on these additional predictors to keep the number of variables entering the machine-learning approach as parsimonious as possible. This was added to the Method section as follows:

We also tested including additional predictors, namely the average mean summer temperature, the average total winter precipitation, and the average temperature range, which are related to the climatological conditions of each glacier and could thus potentially improve model performance. However, since these variables did not lead to a significant improvement of the results, we chose not to include them in order to maintain a parsimonious set of predictors for the machine-learning approach.

Regarding the second part of the comment, we added a paragraph to the Discussion section, discussing the limitations due to the small training data set which is biased towards large glaciers as follows:

Another limitation is associated with the machine-learning extrapolation approach. In fact, the *training data set for the machine-learning model is (i) relatively small, and (ii) biased towards large glaciers. This may lead to a bias in inferred mass balances, especially for small glaciers. Despite these limitations, the validation, which also includes small glaciers (about 40% of considered glaciers are smaller than 1 km^2) indicated that the extrapolation approach is also reliable for that size class (see Sect. 3.3). No bias in the resulting mass balance for small glaciers was detected. This confirms the method's reliability, but we suggest keeping in mind such limitations if the presented method is to be applied elsewhere.*

RC3: Evaluation: I am wondering why the authors have not compared their estimates against the seminal dataset by Hugonnet et al. (2021), which provides geodetic mass balance for all glaciers worldwide, including the Swiss Alps, for 2000–2019. This dataset is currently the most widely used for calibrating regional and global mass balance models. It would be valuable for the glacier modeling community to compare the mass balance results in this study with theirs and to discuss differences in the treatment of digital elevation models, etc.

AR3: We are aware of this global-scale data set, but we do not consider it directly comparable in the frame of the present study for the following reasons: (1) Time periods covered vary. A comparison may be possible using a sub-set of Hugonnet's pseudo-annual data over the common period, but it is known that the annual resolution of the product does not fully resolve actual mass balance variability. (2) There is a clear mismatch in the level of detail between the Hugonnet et al. (2021) data set and our digital elevation models and volume changes: Our data are based on high-resolution (25-50cm) orthophotos and locally referenced photogrammetry, whereas the global-scale data set is much coarser. This is especially relevant for the many small glaciers in the study region. (3) There is an inconsistency in the inventory data: Although Hugonnet relies on the Randolph Glacier Inventory from 2003 in the study region, we use the local Swiss inventory from 2016 (center of our study period) as a baseline. That inventory has a much higher level of detail and also resolves small and debris-covered glaciers.

For these reasons, a direct comparison would not help. We have nevertheless taken up this suggestion and now include a comparison of our annual resolution to the results presented by The GlaMBIE Team (2025) providing annual mass change at the level of the European Alps based on a combination of multiple methods, among others the Hugonnet et al. (2021) data set. The results are now described and indicate that:

Additionally, we compared our mass balance estimates with the one from The GLAMBIE Team (2025), derived for the European Alps. Despite the fact that the two datasets do not compare the exact same region, the results are consistent (Fig. 12b). The findings agree with the comparison with local data sets based on upscaling (GLAMOS, 2024; van Tiel et al., 2025), indicating that the mass balances generally compare well, with the main discrepancies observed in individual years, especially the extreme ones such as 2022 and 2023.

RC4: Area changes: It is unclear how exactly the area changes are calculated. The authors update the area-elevation distribution of their model domain "by attributing relative annual area changes equally to all bands below the mean equilibrium line altitude" (L201). Does this mean that each elevation band below the mean ELA changes by the same relative area? If so, this may not reflect reality, as lower-elevation bands near the terminus typically lose mass and area fastest. I am wondering how glacier retreat occurs in your model under this assumption? Applying uniform relative area changes could artificially retain lower bands, potentially exaggerating glacier-wide negative mass balance and contributing to the reported bias of -0.05 m w.e. (L253). Additionally, Cremona et al. (2025) report a 0.04 m w.e. difference in seasonal mass balance for a 0.5% yearly glacier area change (L377–379), which may explain part of the observed bias.

Regarding the "mean equilibrium line altitude" (L201), are you assuming a constant ELA throughout the study period? How was it calculated – for example, using median elevation as a proxy or derived from yearly mass balance values per band? Please clarify the rationale for assuming a constant ELA. I also suggest considering a sensitivity study to assess how changes in ELA would influence glacier-wide

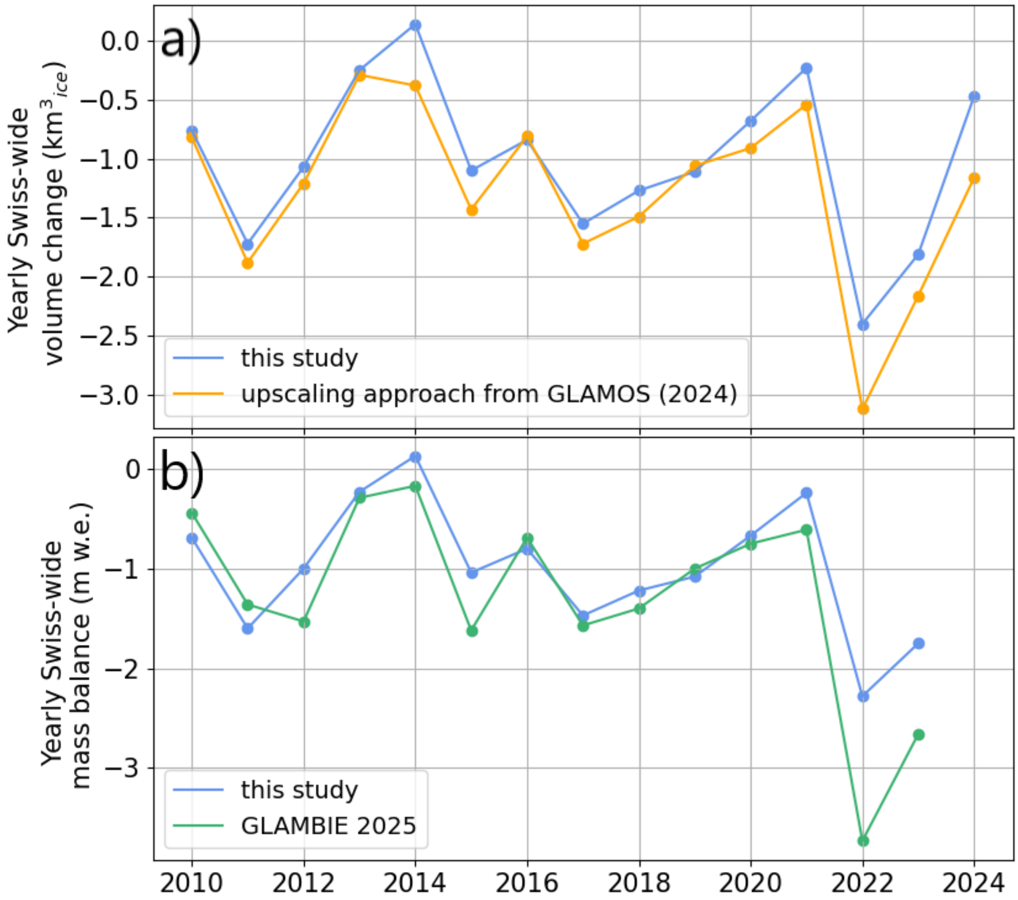

Figure 1: Comparison with existing approaches. a) Comparison of the yearly Swiss-wide ice volume change between this study (blue) and the upscaling of local mass balance measurements (GLAMOS, 2024c; van Tiel et al., 2025) (orange). b) Comparison of the yearly Swiss-wide mass balance from this study (blue) and the yearly mass balance for the European Alps from The Glambie Team (2025) (green).

mass balance.

AR4: We realize that the description of our procedure for estimating the changes in glacier area and their elevation-distribution was not clear enough and we have updated the text accordingly, thus also answering the concerns of the reviewer:

Since the area and volume for the inventory date are given, this year serves as our starting point for the glacier area updating forward (up to 2024) and backwards in time (to 2010). This is achieved by first calculating the cumulative yearly mass balance with Eqs. (1)-(3), which allows deriving the ΔV_y*, and subsequently calculating the glacier area with Eq. (6) for the next time step (see also van Tiel et al., 2025). To update the area-elevation distribution of our model domain, it is assumed that all changes in glacier area occur in the ablation zone, i.e. below the median glacier elevation according to the geometry of the SGI 2016. We attribute computed relative annual area changes equally to all bands below this mean equilibrium line altitude, corresponding to the center of the study period. This means that each elevation band in the lower part of the glacier shows the same annual area changes in relative terms. Since this is a considerable simplification of actual retreat dynamics, the associated limitations are discussed in Sect. 4.4.*

As mentioned in the revised text above, we agree with the reviewer that the simple parameterizations to account for area changes and debris cover effects are first-order approximations that may not fully reproduce reality. However, they permit deriving reasonable regional estimates, which is the focus of the study. We added the following paragraph in the discussion section to address this point:

Another limitation is associated with the rather simplistic parameterizations employed to account for changes in glacier area and the effect of supraglacial debris. Both parameterizations are first-order approximations to account for these effects for the presented regional-scale assessment. We thus do not expect these approaches to exactly match actual processes everywhere, but assume them to be reliable on average. As a consequence, for example, elevation bands near the terminus are likely to lose mass and area with a somewhat lower rate than it is modelled. Nevertheless, since the study focuses on the regional scale, and bearing in mind the relatively short study period considered, we consider these first-order approximations to be reasonable assumptions.

RC5: Albedo parameterization: In general, net radiation, particularly shortwave radiation, is the dominant term in the surface energy balance of glaciers (Hock, 2005). Therefore, the albedo parameterization is critical. The authors use the parameterization by Brock et al. (2000), which is based on a single glacier in Switzerland with data from 1993–1994. However, albedo can vary significantly between glaciers, even within the same mountain range, and also shows strong temporal variability (Williamson et al., 2025). Moreover, the authors state in L309–311: "The high correlations [of winter precipitation anomaly with winter mass balance, and summer temperature anomaly with summer mass balance] confirm the predominant influence of temperature and precipitation on determining seasonal mass balance variations." While temperature and precipitation are indeed important, the authors have not analyzed other drivers, such as albedo variations, even though they acknowledge their significance (L313). For example, Williamson et al. (2025) found that 31–41% of increased melt in Western North America and the Canadian Arctic is attributable to albedo decline. I therefore suggest: (1) Including a sensitivity study on the influence of the chosen albedo parameterization on mass balance and (2) analyzing correlations between albedo variations and mass balance to assess its potential impact. This would provide a more balanced assessment of the factors driving seasonal and annual glacier mass balance and avoid over-attributing effects to temperature and precipitation alone.

AR5: We fully agree with the reviewer that albedo variations are indeed important for glacier melt modelling. As suggested, we have now included a sensitivity study to assess the impact of the applied albedo parameterization on the results and found that the sensitivity is low. This is not surprising given that the model is tightly constrained with various observational data, hence leaving little room for the albedo parameterization to strongly affect trends in our results. Moreover, we also note that data for incorporating actual daily changes in regional-scale albedo (e.g. due to Saharan dust deposition) are not available and including such time-varying processes in our regional model would be beyond the scope of this study.

To test the sensitivity to the modelled glacier surface albedo, we tested two additional scenarios, i.e. by systematically varying the albedo by +25% and by −25%. The sensitivity of the resulting mass balance is low, i.e. with a standard deviation between the two scenarios of just 2% for both winter and annual mass balance. This is not surprising given that the model is tightly constrained with various observational data, thus leaving little room for the albedo parameterization to strongly affect trends in our results.

RC6: Description of data and methods: In the current draft, the data and methods sections are intermixed. For example, important methodological details, such as volume–area scaling, are presented in the Data section, which makes it difficult to follow and locate information. For better readability, I suggest clearly separating the Data and Methods sections.

AR6: We thank the reviewer for this comment and refer to AR4 of Reviewer Response #1.

Minor Comments:

RC7: Abstract: It would be good to mention that the authors are using a simplified surface energy balance melt model.

AR7: We added the information according to the reviewer's suggestion as follows:

To do so, we leverage two simplified surface energy-balance models and remote sensing observations, i.e. geodetic volume changes and observations of the snow-covered area fraction (SCAF) of glaciers during summer, together with machine-learning techniques for extrapolation purposes.

RC8: L55: The authors state that "the last complete assessment of geodetic mass balance covering all glaciers of the Swiss Alps spans the period 1980–2010 (Fischer et al., 2015)". However, the dataset by Hugonnet et al. (2021) is not mentioned. I recommend that the authors acknowledge and discuss this dataset (see Major Comment 3).

AR8: Please also refer to AR3. Besides Hugonnet et al. (2021) there were also other regional-scale assessments of European glacier mass change (e.g. Sommer et al., 2020). However, these large-scale data sets are not comparable in terms of their resolution and the inventory baseline-data used. We have nevertheless added a note to refer to these additional studies:

Note that there are additional recent regional to global-scale assessments of the geodetic change of glacier mass on the scale of the European Alps (e.g., Hugonnet et al., 2021; Sommer et al., 2020), but these datasets are not comparable in terms of spatial resolution or the baseline inventory data used.

RC9: L64: Typo

AR9: Thank you, we corrected accordingly.

RC10: L 68: Typo: should read "Fig. 2."

AR10: Thank you, we changed accordingly.

RC11: L 102: The manuscript uses the term "validation." Model validation usually refers to assessing model structure and assumptions during the development stage, whereas "evaluation" refers to assessing model performance afterwards. I argue that what is performed here is "evaluation," and suggest using this term consistently throughout the paper.

AR11: Thanks for this useful comment. This was changed accordingly.

RC12: L129-130: Typos

AR12: Thank you, we corrected the mistake.

RC13: L168: Equation (4) seems to contain a typo. I assume you mean T(z) = T + (z - z_{ref}) * deltaT/deltaz instead of multiplication.

AR13: Thanks for catching this! We corrected it.

RC14: L169-170: Please clarify how the temperature lapse rate was calculated, including the method used (linear regression?), the number of neighboring grid cells considered, the elevation range they cover, and whether extrapolation beyond this range was applied to higher or lower elevation bands.

AR14: Apologies for the missing information. We added it to the manuscript accordingly:

..., where T is the temperature at the reference elevation z_{ref} of the grid cell in the meteorological product closest to the glacier centroid, and dT/dz is the temperature lapse rate derived with linear regression from temperature values of the five surrounding cells.

RC15: L177-178: The manuscript states: "For glaciers larger than 2 km^2, we use the melt factor provided by Rounce et al. (2021)." It is unclear what is meant by "melt factor." Are you referring to the sub-debris melt enhancement factors from Rounce et al. (2021)? If so, please use the correct terminology consistently throughout the manuscript to avoid confusion with the melt factor (MF) in your model.

AR15: We apologize for the confusion caused by our wording. Yes, it is the sub-debris melt enhancement factor. We reworded as suggested.

RC16: L 179: "At each elevation band, the average debris melt factor is calculated." Please provide more detail on this calculation. The description of the methodology for dealing with debris cover is currently vague and would benefit from clarification.

AR16: We rephrased the subsection "Debris cover" as follows to promote clarity:

Supraglacial debris cover has a significant effect on melt enhancement or reduction for the ice underneath (Nicholson and Benn, 2012; Rounce et al., 2021). To account for this effect, the modelled melt resulting from Eqs. (2) and (3), corresponding to those of clean ice, is scaled with a sub-debris melt enhancement factor. A sub-debris melt enhancement factor below 1 indicates that the debris protects the ice, leading to reduced melt compared to clean ice. This is often the case for a thick layer of debris with a significant isolation effect on the underlying ice. Vice versa, a sub-debris melt enhancement factor above 1 indicates enhanced melt compared to clean ice, which is typical for a thin layer of debris that offers virtually no isolation effect but decreases the surface albedo. For glaciers larger than 2 km^2, we use the distributed sub-debris melt enhancement factor provided by Rounce et al. (2021), which is derived by relying on Landsat-8 images spanning 2013-2018 combined with modelling. At each elevation band, the average sub-debris melt enhancement factor from Rounce et al. (2021) ($\bar{E}_d$) is multiplied with the modelled melt from Eqs. (2) and (3) as follows:

$$a_{sfc,deb} = a_{sfc} * \bar{E}_d \quad (1)$$

Note that this occurs only for snow-free elevation bands, i.e. bands with a modelled snow depth larger than zero (see also Sect. 3.2). For glaciers smaller than 2 km^2, Rounce et al. (2021) did not calculate the sub-debris melt enhancement factor. For these glaciers, we derive the sub-debris melt enhancement factor for each elevation band by using the debris-covered area provided by the SGI2016 and assuming a debris melt factor of 0.63 over this area, i.e. the mean value found by Rounce et al. (2021). In this approach, the debris factor remains constant over time for each elevation band, i.e. debris thickness and characteristics are assumed to remain unchanged over the study period, under consideration of the geometry corresponding to the year of the SGI2016.

RC17: L180: "Only if the elevation band is snow-free." Please explain how you distinguish between snow-covered and snow-free conditions.

AR17: We refer to AR19, where we clarify how snow cover on the glacier ice is modelled.

RC18: L195: Consider rephrasing "define c" as "calculate c".

AR18: We re-worded as suggested.

RC19: L 203-226: It is unclear how SCAF modeling is performed. Please provide a brief overview of the methodology in addition to referencing Cremona et al. (2025).

AR19: Thanks for raising the important aspect. We added a brief description of how we model snow cover as follows:

To model the evolution of snow cover on top of the glacier ice, we assume an initial snow depth of zero across the entire glacier at the start of the study period. The snow water equivalent of each elevation band is subsequently updated according to the modelled climatic mass balance, i.e. the daily accumulation and melt. At each time step, the modelled snow-covered area fraction is derived by summing the area of all elevation bands with a snow depth greater than zero with respect to the beginning of each hydrological year, and dividing this total by the glacier area.

RC20: L208: Please provide a reference for this statement.

AR20: We added the following reference as requested to support the statement: Menounos et al. (2025).

RC21: L239: In addition to absolute error measures, please also provide relative error metrics for the machine learning model (e.g., mean absolute percentage error). Also, please specify the sizes of the training, validation, and test sets.

AR21: We agree with the reviewer that the manuscript would benefit from this information. For changes to the text, we refer to AR2 of Reviewer Response #1.

RC22: L247: The phrase "To address the uncertainty" would be better phrased as "To evaluate the model," since the uncertainty itself is not being addressed here.

AR22: We agree with the reviewer and re-worded accordingly.

RC23: L384: The authors mention the average precipitation correction factor. It would be helpful to also report the mean and ranges of the other calibrated parameters in the manuscript.

AR23: We agree with the reviewer and added the ranges of the parameters and their average of the investigated period as follows:

Across Swiss glaciers, MF ranges from 0 to 9 $\mathrm{m\,w.e\,K^{-1}\,d^{-1}}$*, with a median of 1.79* $\mathrm{m\,w.e\,K^{-1}\,d^{-1}}$*,* $a_{snow/ice}$ *ranges from 0 to 0.6* $\mathrm{m\,w.e\,m^{-2}d^{-1}\,W^{-1}\,K^{-1}}$*, with a median of 0.016* $\mathrm{m\,w.e\,m^{-2}d^{-1}\,W^{-1}\,K^{-1}}$ *for the temperature-index model. For the simplified energy-balance models, TF ranges from 0.4 to 8* $\mathrm{m\,w.e\,K^{-1}\,d^{-1}}$*, with a median of 2.90* $\mathrm{m\,w.e\,K^{-1}\,d^{-1}}$*, and SRF ranges from 0 to 2.4* $\mathrm{m^3\,d^{-1}\,W^{-1}}$*, with a median of 0.06* $\mathrm{m^3\,d^{-1}\,W^{-1}}$*.*

RC24: Figure 4: Please explain all abbreviations (ALE, RHO, etc.), as these have not been introduced in the manuscript.

AR24: We agree with the reviewer and now use the glacier names in the figure instead of the abbreviations (see below).

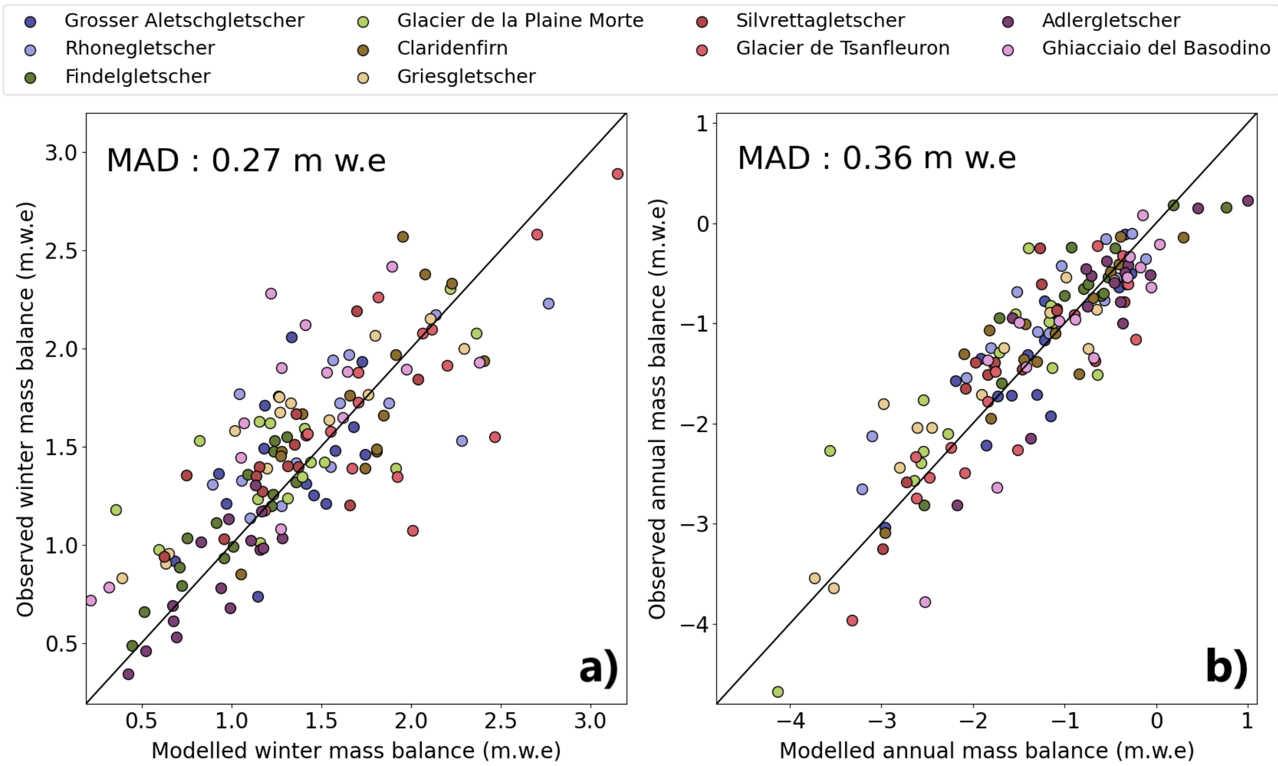

Figure 2: Validation of seasonal glacier-wide mass balance. a) Comparison between modelled and observed winter mass balance. b) Comparison between modelled and observed annual mass balance.

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