Reviewer 2

The manuscript by "Kneib et al." describes and demonstrates the calculation of a Surface Mass Balance (SMB) field for the Argentière Glacier in the Mont Blanc massif using high-resolution dh/dt fields (from Pleiades DEMs) and the calculation of ice flux divergence (using three different ice thickness datasets, a high-resolution velocity field, and two different methods to calculate the divergence). The resulting SMB field is then compared with results from an SMB model and stake observations, with the ultimate goal of obtaining a precipitation correction that better represents avalanche deposits. The impact of this correction on the glacier's future ice volume is subsequently examined using a 3D thermomechanical ice flow model.

The manuscript is very well-written, with a clear sequence of the steps undertaken by the authors. The reader can easily follow the process, which allows to apply a similar setup for other glaciers (highly appreciated). The various figures, both in the text and in the supplementary material, are well-chosen to represent the results obtained. In addition to the very detailed work carried out (including all uncertainties), the result is compelling and demonstrates the significant added value of the method.

In my opinion, this paper is definitely a valuable contribution to the glaciological community and is therefore highly recommended for publication in 'The Cryosphere.'

We would like to thank Dr Van Tricht for his thorough review and his very relevant and constructive comments.

I have only a few textual comments and a couple of minor clarifications which I would like to see in the revised version. The only more substantial work I can see is calculating/presenting a mass balance derivation for each year in the studied period (2012-2021). How well does the mean specific mass balance obtained with the applied method represent the mean specific mass balance from the glaciological method and SMB model? And how does the mass balance of the stakes per year compare with the method used?

Thanks for this suggestion. There were several reasons why we choose this relatively long time period to conduct our SMB inversions, which are mainly due to uncertainty considerations:

- When reducing the observation period to time intervals shorter than 3 years it has been shown that at the glacier scale the density conversion factors could deviate outside the range of 850 +/- 60 kg/m^3 at the glacier scale (Huss, 2013). To stay within reasonable uncertainty bounds, particularly in the accumulation zones, we therefore chose to stick to a longer time series.
- Elevation change and velocity data become very gappy at the annual scale, despite the quality of the Pléiades images. These gaps, especially at the base of the headwalls (which are subject to steep slopes and topographic shadows that affect the velocity and elevation data) need to be filled for our inversion approach and this may lead to artefacts and inconsistencies in the products. Figure S16

show examples of velocity maps for the periods 2012-2015, 2015-2018 and 2018-2021 which indicate a high density of gaps at the base of the headwalls, particularly for the period before 2018. Aggregating these velocity maps is therefore crucial to reduce the uncertainty in the velocity product. Similarly, one can refer to the Figure 2 in Beraud et al. (2023) (see copy below) that indicates the numbers of gaps needing to be interpolated in the Argentière DEMs. While their study also includes winter pairs, this shows that some locations on the glacier are particularly difficult to map, thus the need to use as many DEMs as possible to constrain the elevation change patterns in these locations.
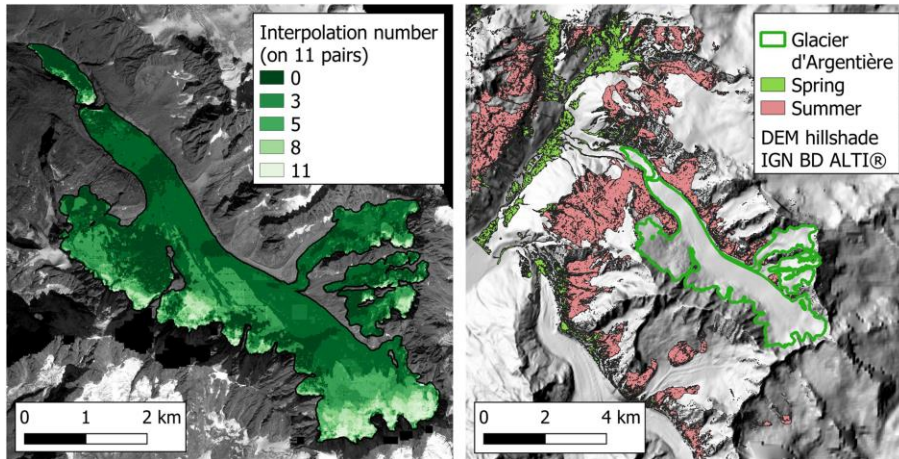
Fig. 2 from Beraud et al. (2023). Left: Map of the Glacier d'Argentière showing the number of interpolations between consecutive DEMs (total 11 pairs). Right: Snow-free stable areas used by three co-registrations or more. Spring areas nearly fully cover summer areas.

- We also expect a high temporal variability in accumulation (especially from avalanching, Hynek et al., 2024), as can be seen in the elevation change patterns shown in Figure 4 of Beraud et al. (2023). While it is definitely very interesting to analyse this annual variability, we were more interested in the average contribution of avalanches to the surface mass balance over a time period long enough to smooth out this variability and analyse the long term signal.

For all these reasons we decided to stick to our relatively long study period of 2012-2021, which uses all the data that was available from the Pléiades observations. We agree that it could have been possible to reduce this period by a few years or cut it in two without increasing the uncertainties too much, but this was not necessary for the purpose of our research questions and therefore felt beyond the scope of this study. We will therefore keep it as it is. These points are already mentioned in the discussion section:

'We computed the mean value for the full 2012-2021 period, but assuming that the flux divergence and firn density remain constant over ~1 decade, these could be refined to yearly or even seasonal time-scales using high-resolution elevation changes from the Pléiades DEMs (Jourdain et al., 2023; Zeller et al., 2023), with some additional uncertainties caused by the DEM differencing over shorter time periods (Beraud et al., 2023).'

Specific comments:

- Line 15: "Particularly" might be removed here

  Will be removed as suggested

- Line 16: High resolution (<-> high quality) as well?

  We will add it as suggested

- Line 18: A bit unclear if the approach to invert only uses three different ice thickness estimates or three different methods. Further, if formulated like now, it seems that the ice thickness setups show a good agreement, but it is the three different inversions that do show the good agreement.

  Agreed. We will change this as follows:

  'Three inversions are conducted using three different ice thickness modelling approaches, two of which are constrained by observations. The inversions all show a very good agreement between inverted surface mass balance and *in situ* measurements (RMSE between 0.50 and 0.96 m w.e. yr^{-1} for the 11-year average).'

- Line 19: After reading, the "consensus F2019 estimate" is not constrained by the ice thickness measurements?

  See response to comment above.

- Line 20: Maybe mention the range of RMSE values?

  Will be added as suggested.

- Line 21: "the" modelling approaches

  Will be modified as suggested.

- Line 23: "Avalanching" -> avalanche deposits

  Will be modified as suggested.

- Line 33: Accumulation zone

  Will be modified as suggested.

- Line 36: and extrapolation?

  Will be added as suggested

- Line 41: Compaction as well?

  We considered compaction to be an 'internal process' but will add it as an example: 'This mismatch is due to internal processes such as compaction'

- Line 45: Remove "and"

  Will be removed.

- Line 52-53: Potentially add a reference to "Turchaninova A.S., Lazarev A.V., Marchenko E.S., Seliverstov Y.G., Sokratov S.A., Petrakov D.A., Barandun M., Kenzhebaev R., Saks T. Methods of snow avalanche nourishment assessment (on the example of three Tian Shan glaciers). Ice and Snow. 2019;59(4):460-474. https://doi.org/10.15356/2076-6734-2019-4-438

Really nice reference, thanks! We will add it here.

- Line 67: "ice flux"

Will be modified as suggested.

- Line 67: Therefore -> subsequently

Will be modified as suggested.

- Line 81: "which" does not have a direct link here. You probably mean that ice thickness and velocity is less constrained in the accumulation zone and therefore the uncertainty of the product is larger, butt this might need to be a bit clearer stated

We will modify the sentence for clarity: 'However, these estimates depend on the quality of the ice thickness, velocity and elevation change data which are less constrained and therefore lead to higher uncertainties in the accumulation area of the glaciers (Miles et al., 2021).'

- Figure 1: The colour scale is a bit large for the values of the SMB (especially the positive ones). Maybe limit this to +3 m w.e. yr-1 so that you can see a bit more the variations? The circles of the GPR locations are too large so that you cannot locate the individual points (it now seems to be more a line of GPR). Panel b, is it an option to zoom in somehow (e.g., to the Mont Blanc massif)?

Thanks for these suggestions. We will use an asymmetrical color scale to constrain it to [0; 3] m w.e. yr-1 for the positive values. The GPR acquisitions are very dense so they really are more lines than points, we will change the symbol in the legend. We will also zoom panel b to the Mt. Blanc massif.

- Line 132: Which software is used to compute the surface displacement, velocity?

The processing chain used is the same as the one described in Millan et al. (2019) and more recently in Mouginot et al. (2023), which uses Python 3.7 and Fortran. We will specify this in the text:

'The velocity fields were obtained using normalised cross-correlation and the images were co-registered using the median velocity of the off-glacier terrain using the workflow described by Millan et al., (2019) and Mouginot et al. (2023).'

Mouginot, J., Rabatel, A., Ducasse, E., & Millan, R. (2023). Optimization of Cross Correlation Algorithm for Annual Mapping of Alpine Glacier Flow Velocities; Application to Sentinel-2. *IEEE Transactions on Geoscience and Remote Sensing, 61*, 1–12. https://doi.org/10.1109/TGRS.2022.3223259

- Line 141: You mention later that the F2019 did not use the GPR measurement, so it is a bit strange to mention here that the three methods use in situ data

We will modify the sentence as follows to clarify this point: 'We used distributed ice thicknesses obtained from three different modelling approaches, two of which are constrained by *in situ* ice thickness observations.'

- Line 184: There is some error between ice thickness (H) change (dH/dt) and surface elevation (h) change (dh/dt).

We will correct this for equations (2) and (3).

- Line 184: Formulating H_2O makes the equation for me a bit confusing. Consider writing just r_w and r_i.

  We will change $rH2O$ to rw

- Line 185: Any evidence that internal and basal mass balance are negligible from previous studies?

  The good agreement between geodetic mass balance and surface mass balance measurements shown in Beraud et al. (2023) seems to comfort this hypothesis. Basal ablation from geothermal heat is generally very limited on temperate alpine glaciers (Alexander et al., 2011). Neglecting the internal mass balance is a stronger hypothesis, which is discussed in more details in the discussion section:

  'In our processing we assumed that there is no significant change in firn compaction rates, and as such the influence of the density uncertainties on the final uncertainty are of secondary concern (Fig. S9, S10). This assumption may not hold for other glaciers and other time periods, for which changing firn densification may lead to surface lowering with little influence on the surface mass balance or flux divergence (Belart et al., 2017; Pelto et al., 2019; Réveillet et al., 2021; Vincent et al., 2020; Zeller et al., 2023). Firn compaction could partly explain the low SMB values relative to the Tour Noir GLACIOCLIM measurements in all three modelling approaches. To give an order of magnitude, assuming a 20 m thick firn layer, a 100 kg m^{-3} increase in mean density over the study period would explain a 0.2 m w.e. yr^{-1} SMB difference, which is however insufficient to explain the differences between observed and SIA or F2019 SMB.'

  We will add the two references mentioned above to the sentence to justify this choice.

  Alexander D, Shulmeister J, Davies T. High basal melting rates within high-precipitation temperate glaciers. *Journal of Glaciology*. 2011;57(205):789-795. doi:10.3189/002214311798043726

- Line 188: Here you refer to ice thickness as H

  Apologies for this, we will change it in equation 3.

- Line 211: Any estimate for y based on observations or modelling?

  To our knowledge, no study has focused on distributed estimates of '*y*' for Argentière Glacier, which is why we derived it from the IGM simulations (Fig. S7). There is a preprint in EGUsphere that has looked at ice deformation using borehole data in the lower part of Argentière Glacier (Roldan-Blasco et al., 2024), finding a yearly mean vertical velocity of 38 m/yr for a surface velocity of 43 m/yr, so a 0.88 y ratio. However, these are local measurements in the lower part of the ablation area and likely not representative of the entire glacier. We therefore considered a uniform distribution of y between 0.8 and 1 to quantify its uncertainties in a conservative way (Cuffey & Paterson, 2010).

- Line 215: If formulated like this, it seems you calculate the ice flux divergence in three different ways. But as far as I understand, this is not the case? Three different ice thickness estimates are used, and two different approaches to determine the ice flux divergence.

Yes, good point, we will make this more explicit: 'We computed the flux divergence using three different ice thickness estimates and two different approaches, leading to three SMB estimates (Eq. 2), the **SIA, F2019** and **IGM** estimates.'

- Line 223 and 224: Some repetition here of the ranges for the uncertainty (with lines 208-213).

We will remove this sentence to avoid repetition.

- Figure 2: Like for F1, is it possible to limit the range of the colour scale for the SMB?

We will use an asymmetrical color scale to constrain it to [0; 3] m yr-1 for the positive values.

- Line 243: I do not completely get this 10% best SMB estimates. Can you clarify this a bit?

Yes, apologies if that was not clear. The idea here was to take the best 10% scenarios (relative to in situ measurements) to be used as a reference to quantify the spatial variability and contribution from avalanches. We will clarify it at the end of the sentence:

'We compared the inverted distributed SMB with the *in situ* SMB at the GLACIOCLIM stake locations and for each modelling approach selected the best 10% SMB estimates that minimised the weighted quadratic sum of the RMSE of the Argentière and Tour Noir stakes to be used as reference scenarios for the quantification of the avalanche contribution.'

This complements the details given in 2.9:

'Here, we re-evaluated this over the Rognons, Tour Noir and Améthystes tributaries and in the accumulation area using the mean of the best 10% inverted SMB patterns of each modelling approach'

- Line 273: How does the exponentially decay of the albedo is determined? Which time scale is used?

This exponential decay is based on the parametrization presented in Hock and Holmgren (2005), using the same parameters as in their study, with a decay of -0.1 day-1. We will add this reference here:

'Where α is the local surface albedo (=alpha_ice or =alpha_snow, which decreases exponentially with the age of the surface snow; Hock and Holmgren, 2005), '

- Line 341: Again, here you mean dh/dt I guess? The difference between dH/dt and dh/dt is not clear throughout the manuscript. I guess because bedrock elevation is considered to be stable, both are the same, but you should state this somewhere and from then on work always with dH/dt

We will change this to dh/dt.

- Line 343: The median of the 2012-2021 period for every grid cell?

Correct, we will specify this here.

- Figure 3: Both for the elevation change and the surface velocity, the colour scales could be optimized (wider for velocity, smaller for elevation change).

We will modify this as suggested.

- Line 348: Median or mean velocity (<-> line 343)?

  Good catch, this is the median, we will correct it in the caption.

- Figure 5: You state different modelling approaches, but in fact it concerns two different ice flux modelling approaches and three different ice thickness estimates?

  For clarity we will change this sentence to: '(a) SIA, (b) F2019 and (c) IGM SMB estimates.'

- How well can you invert the annual surface mass balance? There is only a focus on the multi-year average

  Please refer to our answer to the general comment above.

- Line 385-389: I do not completely get this sentence (which I find too long).

  We will cut it into two different sentences for clarity and simplify the second part: 'There is a good agreement with the SMB measurements on Argentière for both the SIA and F2019 thickness with RMSE values lower than 0.67 m w.e. yr^{-1} (Fig. 6d-e). Uncertainties of the F2019 and SIA estimates reach up to +/- 1.2 m w.e. yr^{-1} (F2019) and +/- 0.8 m w.e. yr^{-1} (SIA) at the stake locations and higher at the margins of the glacier, particularly over the Rognons tributary (Fig. S6), where they locally reach up to +/- 6-8 m w.e. yr^{-1}'

- Figure 9: Very cool to see the impact of taking into account the avalanches. I wonder, however, if it is possible to show the flowlines. My first guess would be that the ice at the location of the avalanche deposits does not flow to the central trunk but moves along the headwall. But this must be different because the central trunk maintains more ice?

  Indeed, according to our simulations with Elmer/Ice, initially the ice at the location of the avalanche deposits does not flow to the central trunk but moves along the headwall (Fig. R1). However, as the glacier retreats the geometry changes and the flow from the avalanche cones does converge into the main glacier trunk.
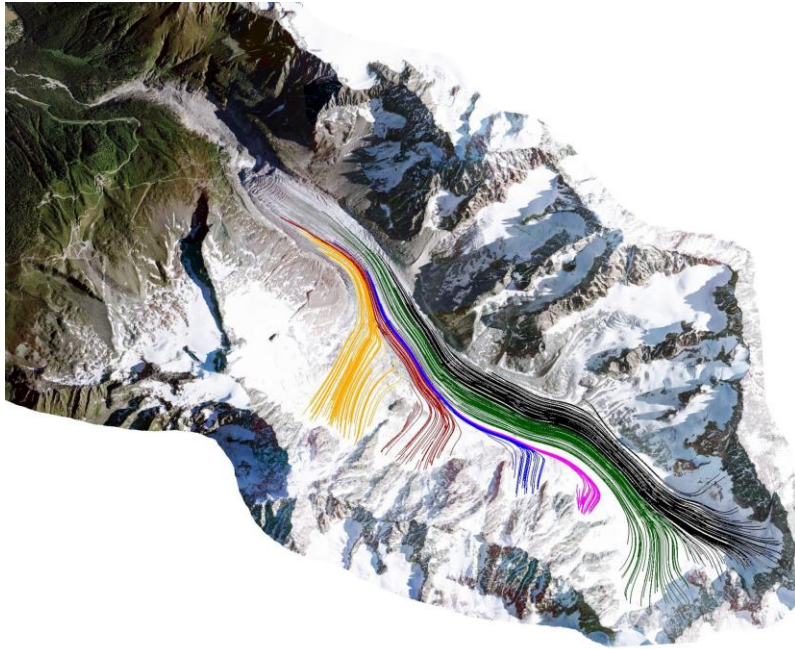
Figure R1: Glacier flowlines in 2021 for the corrected scenario.

- Line 558: Is shading not included in the simplified energy balance model? By modifying the incoming radiation?

  We did not include shading in our simplified energy balance model to reduce the computation cost. This decision is based on preliminary tests that we conducted and that showed that including shading had a limited effect on the surface mass balance as: 1/ in winter the melt is very limited even in the non-shaded locations and 2/ in summer the sun is high enough in the sky that the large majority of the glacier surface receives the same amount of SW radiation. We do agree however that in some cases topographic shading could lead to strong mass balance gradients, and that it will be worthwhile to account for this effect in glacier models.

- Line 603: I guess also the ice thickness is crucial when the approach would be applied on larger scales? As you show in the sensitivity analysis

  It is true that in our sensitivity analysis (Fig. S9-S10) ice thickness is responsible for most of the uncertainty. However, this is a particular case as we have very high-quality velocity data for our study. In the end all three ice thickness scenarios seem to perform relatively well, but when we changed to a coarser resolution velocity product (Fig. S15) this really had a strong negative impact on the results of the SMB inversion. This is why we have stated that velocity seems to be the main current limitation. But of course, this would need to be tested at other sites. We will also mention ice thickness here: 'with the main current limitations being the quality of the surface velocity observations, especially in the accumulation zones, followed by the availability of ice thickness measurements to constrain distributed ice thickness estimates.'

- Line 606: cf line 16 high-resolution vs high-quality (both are true…)

  Good point, we will add 'high quality' here.