Rebuttal letter – EGU Solid Earth

Preprint egusphere-2023-1711: "Fast uplift in the Southern Patagonian Andes due to long and short term deglaciation and the asthenospheric window underneath"

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Dear Editors from EGU Solid Earth,

We are pleased to receive and reply to the revisions of our article. We are grateful for the many comments of appreciation for our work. Please, find below our detailed answers (in italic) to the reviewers' comments (in regular font). *The revised article with and without track-changes will be provided upon submission. In our replies, line numbers always refer to the new manuscript without track changes. Thank you for your work and for considering our manuscript for publication in your journal.*

Best regards,

On behalf of all co-authors,

Veleda A. P. Muller.

1. Reviewer #1, Federico Davila, Comments:

The manuscript deals with an interesting thermo-mechanical model to estimate uplift values derived from deglaciation in a scenario with temperature anomaly development (slab window). The estimated values were compared to the very high uplift amounts in Patagonia derived from GNSS studies.

I am adding in the EGU system an edited pdf, where authors will be able to read my comments in the appropriate places of their manuscript. But essentially, my main concerns are related to the model setup, associated with the glacial history of any area, which might impact on results and interpretations.

We thank the reviewer for the comments of appreciation for our work. We reply to the reviewer's concerns hereafter.

1.1. First, I wonder how the results would be influenced by an ice sheet that reduces from a leading edge, migrating backward (like a river knickpoint), instead of reducing homogeneously the ice thickness in time.

To address the reviewer's remark, we run a set of simulations in which we reduce 75% of the synthetic icecap removing vertical 'slices' of ice from the borders towards the center of the ice sheet during 20000 years (similar to model set 1; see figures below). As expected, we recognize that the spatial distribution (i.e., pattern) and the magnitude (i.e., maximum value for each timestep) of the uplift rate are different from the reference simulations in which we reduce homogeneously the ice thickness on time. The change in the spatial pattern of uplift rate is readily explained by the different spatial pattern of the unloading imposed (i.e., uniform ice melting vs. lateral inward ice melting). The change in the *magnitude of uplift rate, instead, is due to the activation of local plastic (effectively brittle) strain by the instantaneous removal of 2 km of ice at the edges of the ice sheet. Comparing the modeled uplift rates obtained with these boundary conditions to GNSS data that provide the aseismic regional visco-elastic vertical strain (rather than the plastic deformation related to local seismicity) would be misleading. In addition, we arbitrarily removed the entire column of ice from the edges inward because there is limited information on the net ice mass balance of the Southern Patagonian Icefield since the LGM nor the thickness of the ice sheet edges in time (e.g. Hein et al., 2010; Strelin et al., 2014; Kaplan et al., 2016; Reynhout et al., 2019; Davies et al., 2020), and the information on the extent of the ice sheet through time is insufficient for our model constraints.*

For these reasons, we refrain from adding these tests to the manuscript, and only show it here to release the reviewer's concern. We stress that our preferred simpler approach, in which we simulate the deglaciation by homogeneously reducing the ice density/thickness and compare modeled uplift rates to vertical GNSS data, prevents us from addressing the leading drivers of the pattern of uplift rates, but still enables us to focus on and address those of the magnitude of regional aseismic visco-elastic uplift rates.

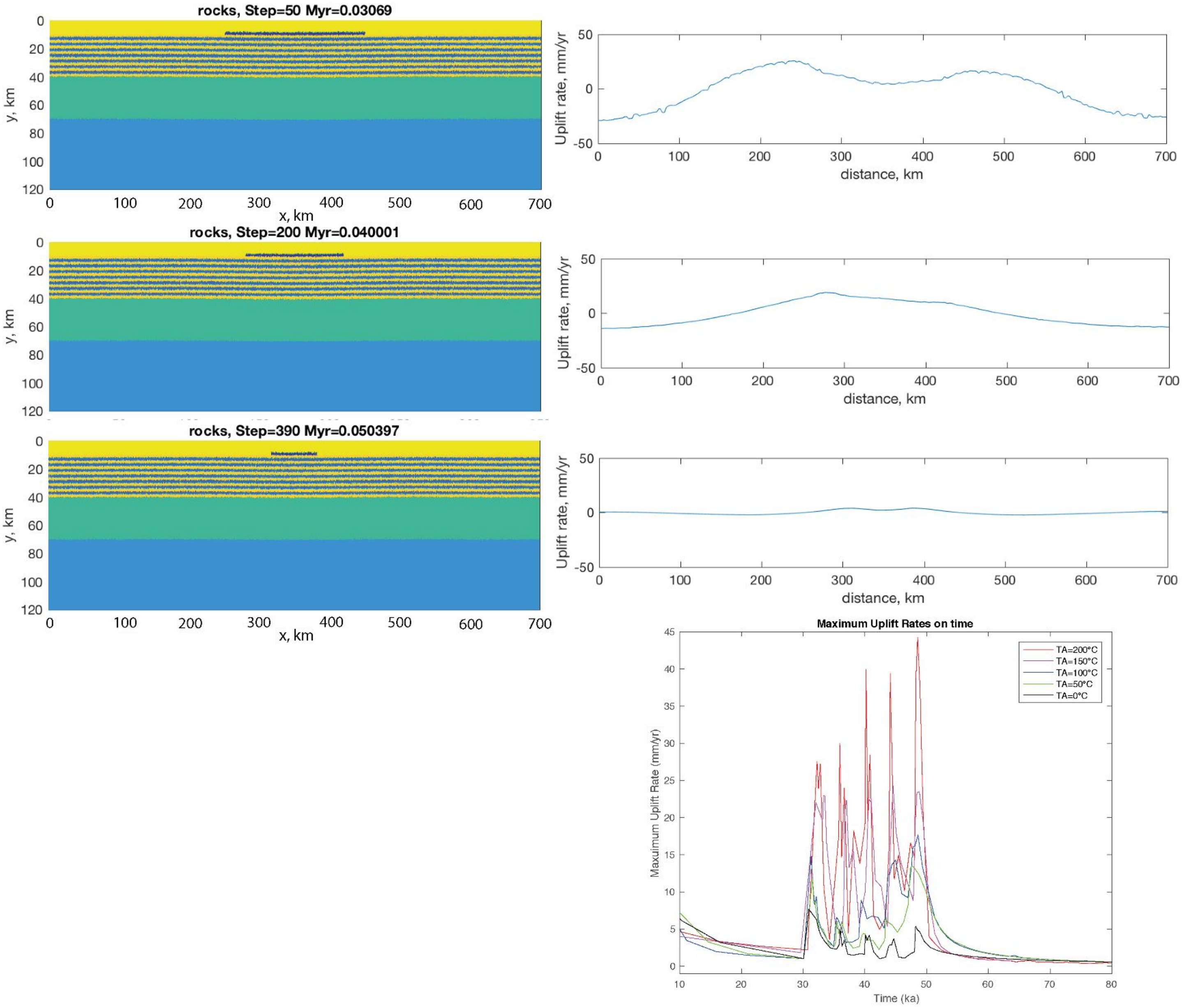

Figure 1. Synthetic icecap lateral retreat (left panels) and respective uplift rates in space (right panels) during 20000 years of deglaciation; and maximum uplift rates vs. time for the different tested thermal anomalies, deglaciation starts at 30 ka in the simulations.

1.2. Second, the authors disregard the influence of tectonics because of the analyzed time lapse.

Yes, we assume that the tectonic strain is negligible given that we focus on time scales shorter than 20000 years. According to tectonic reconstructions, the background tectonic stresses around the Southern Patagonian Icefield are stable since 8 Ma (Eagles and Scott, 2014) (lines 273-275 and 374-377).

1.3. However, I wonder how erosion might influence uplift considering glacial settings show strong exhumation. This was demonstrated using thermochronology (Thomson et al., 2010, Davies et al., 2020; among others).

Measurements of regional erosion rates since the LGM and the LIA are scarce, but we now report the ones published in Fernandez et al. (2016) based on the volume of sediments accumulated in millennial time-scales in fjords and bays across Southern Patagonia and the Antarctic Peninsula (lines 345-346). We also bring a discussion about the erosion rates in million years time-scales based on low-temperature thermochronology (e.g., Fosdick et al., 2013; Herman and Brandon, 2015; Fernandez et al., 2016; Muller et al., 2023, Thomson et al., 2010; Davies et al., 2020, lines 348-353). Given the short time interval considered here, however, it seems reasonable to assume that the eroded material is still in the transport zone and therefore does not significantly contribute to unloading the surface of the orogeny. Even if long term erosion rates contribute to present-day uplift rate, since measured long term erosion rates are comparable to those of e.g. the Alps, we assume that such contribution may explain a similar fraction of uplift rates (i.e., generally a fraction of a mm/yr; Sternai et al., 2019).

We added these considerations to the text at lines 345-365 and thank the reviewer for making this point.

PDF comments:

1.4. Below you will find some comments, but please take special care with some controls you have not considered as erosion. Particularly in glacial scenarios, where it is very important.

See point 1.3.

1.5. l. 56. deformation reduces and the evolution of thrusting is null to very minor

Corrected in the text (lines 70-71).

1.6. l. 58-60. Please check Avila and Davila 2020 in J. Geodynamics, Avila et al 2023, and Ding et al 2023, who also estimated uplift across southern Patagonia using different approaches.

Thanks for the references. The estimates in these studies were added to the text and this paragraph was rewritten to better explain dynamic and isostatic uplift (lines 62-96).

1.7. l. 77-82. I wonder how older ice fields, millions of years, affected the results considering the response to flexural rebound is partly viscous-elastic.

Reasonable mantle relaxation times should vary between a few thousands of years and up to a few tens of thousand of year (e.g., equation (11), line 438-444). Thus, the effects of ice fields older than should not be seen in present-day uplift rate measurements.

1.8. l. 85. How much?

We now specify these values in lines 129-130 based on Fig. 1 and Lange et al. (2014).

1.9. l.94. This agrees with estimations from Avila and Davila 2020 (J Geodyn), which calculated the thermal lithospheric thickness and uplift rates from a reduction during the Neogene.

Thanks for the reference that was added to the sentence (line 137).

1.10. l. 97. ref please.

References are the same as in lines 136-138, and we now specify "viscosity estimates from these previous studies".

1.11. l. 100. ref please.

References added (line 143).

1.12. l. 106-108. I wonder if the erosional history could not reproduce the same pattern and magnitudes. Please discuss here and justify the hypothesis.

See points 1.1 and 1.3 and discussion lines 329-377.

1.13. l. 159. Why?

The lateral extent of the model domain is scaled on the study region (the South American continent are latitudes of the SPI is between 400 and 700 km wide) and large enough to avoid boundary effects in the numerical results (210-213). The depth is set to account for realistic thickness of the lithosphere and the asthenospheric mantle (van der Meijde et al., 2013; Ávila and Dávila, 2018, 2020) and to avoid boundary effects in the numerical results (lines 213-217).

1.14. l. 163. Avila and Davila, 2020 estimated the lithosphere structure in the area, combining different data.

We added this reference (line 217).

1.15. l. 166-168. A recent inverse modeling computed the best fit Te in the region (Avila et l 2023: tectonophysics).

Added at lines 225-227.

1.16. l. 183-184. Why?

Based on LIA and LGM maximum extents of the glaciers in Southern Patagonia. We added this information in the text (lines 257-262).

1.17. l. 199-203. I am not really sure if the magnitude of the uplift is not affected by the extent of the icecap. If you produce a retreating the uplift starts affecting the leading edge of the cap, and moving backward. Similar to knick points in rivers.

See point 1.1.

1.18. l. 221-223. You mean asthenospheric mantle?

Yes, corrected throughout the manuscript.

1.19. l. 276-278. You should also consider the erosion driven by glaciation-deglaciation cycles, which produce a lot of unroofing and, then, rebound. Please take a look to Avila et al 2023 and Ding et al 2023 models, which consider erosion.

See points 1.3 and 1.12.

1.20. l. 290-292. Please revise the sentence.

Corrected at lines 399-401.

1.21. 298-302. Note please that you did not model erosion.

See points 1.3 and 1.12.

2. Reviewer #2, anonymous, Comments:

The manuscript is well written accompanied by appropriate figures and tables. It fits to EGUsphere. I recommend its publication subject to some minor polishing. I hope the authors will find my remarks, listed below, helpful.

We thank the reviewers for the appreciation of our work. We address the reviewer's concerns hereafter.

2.1. GIA: Add a few more words about this phenomenon. You could add in the Sec."Introduction" some lines explaining the GIA in Patagonia and why this process is important to this study.

We added more details about GIA (lines 43-51), and our modeling approach in Patagonia (lines 57-61) in the first paragraph of the Introduction.

2.2. I would like to see somewhat more "discernible" conclusions. Consider splitting the "Discussion" section from the "Conclusion" section.

We follow the reviewer's suggestion and now distinguish these sections (Conclusions in lines 456-466).

2.3. The manuscript reevaluates the GIA contribution to vertical land motion in Patagonia, but there is no mention of the ice model that was used to calculate the displacements. Furthermore, in the manuscript there is no reference to where the rates, due to the GIA, come from. How were they calculated? What code did you use? You wrote it? Did you get it from literature? Explain better, perhaps expanding the Numerical model section.

We now added more detail in the Methods section about the observed uplift rates used for comparison in our study, published in Lange et al. (2014) (lines 156-160).

The pseudo-icecap in our models is a simplistic layer with an initial thickness of 2000 m that is reduced throughout the deglaciation, as explained in detail at lines 238-262. We now also include more detail on how the uplift rates are calculated in our forward models (lines 263-272).

Furthermore, section 2.1 is entirely dedicated to the numerical model we used and also provide useful references where the numerical method and code are described in detail.

2.4. LGM starts from 26000 years Before Present, not 20000.

We corrected this at line 105-106, but we clarify that 20000 years BP was used as the onset of LGM deglaciation in Patagonia, dated between 21 and 18000 years BP (e.g., Hein et al., 2010; Bendle et al., 2017; Davies et al., 2020) (lines 106-107). Also the global LGM is normally referred as at 21000 years BP (lines 49-51, Peltier, 2004), but we acknowledge that the age of deglaciation is variable worldwide (e.g., Hughes et al., 2013).

2.5. Also, how did you calculate the yield strength ($\Delta\sigma$)? What code did you use? How does it work? Do lithospheric strength values depend on strain rates? Or the thermal regime? Or even by the thickness of the crust? Explain better.

We use a fully coupled numerical thermo-mechanical model, see also point 2.3 and *section 2.1 for more detail and references. The deviatoric yielding stress* $\Delta\sigma = (\sigma 1 - \sigma 3)$ *depends on the strain rate, temperature, composition and rheological parameters of the considered material (please refer to Gerya, 2019 and/or Sternai et al., 2021, for more detail). The yield strength profile shown in Fig. 2 was calculated for a low Te (elastic lithospheric thickness), as expected for this region (lines 220-227), and* σ *is given by equations 2 and 5-9.*

2.6. Fig. 2. ---> explain yield strength profile in Discussion section.

We now discuss this at lines 378-389.

2.7. l293-l294: the regional asthenosphere viscosity was estimated..... and satellite observations. What does this phrase mean? Consider rephrasing this sentence.

Rephrased in lines 402-404.

Minor style picks:

2.8. l17:~20000---->~26000 years ago. It is appropriate to write BP (Before Present) when talking about large temporal periods (Think of it as a hint).

Corrected throughout the manuscript.

2.9. l66:38 to 55 S ---->38°to 55°S

Corrected in line 98.

2.10. l78: What does "approximately" mean? You need to be more specific

See point 2.4. We improved the phrasing (lines 105-107).

2.11. l303: ~20000 ----> ~26000 years ago (check all parts carefully where is written "20000").

In this case we did not added BP because we are talking about a period of time not necessarily before present day, note "lasting" in line 413.

3. Reviewer #3, Joelle Nicolas, Comments:

3.1. This paper is dealing with GIA and LIA modelling to explain the Earth deformation observed in South Patagonia. The paper is written in good English and illustrated with appropriate Figures and Tables. The authors performed a lot of computations and expose interesting results addressing a current scientific question. Nevertheless, some essential points are missing. Therefore, I recommend the publication of this paper in EGUSphere after major corrections in line with my suggestions in the comments below. I'm sure that the authors would be able to do it properly and it would certainly add value to the paper and to the reliability of the results. Despite the paper is well written and the results are interesting, some key points are not explained or even mentioned. These are essential to be considered to increase the reliability of the results and convince the reader.

We thank the reviewers for the appreciation of our work. We address the reviewer's concerns hereafter.

3.2. I wonder why the two effects LIA and GIA are never added before the comparison between model and observations, as well as why the actual ice melting is not considered. Indeed, the actual uplift signal observed is the combination of visco-elastic response of the crust to GIA, LIA and actual ice melting. What about the current ice melting signal (and acceleration) in the study as the corresponding elastic deformation is not negligeable at all?

Please see points 2.1 and 2.3 regarding the comparison between observed and modeled uplift rate as well as the modeling approach.

We now include information about recent ice melting in the introduction and discussion (lines 119-124, 330-344). While we are currently working on a similar study that specifically addresses the elastic deformation due to recent ice melting, the role of faster deglaciation and current ice melting is beyond the scope of the present manuscript, but we add a phrase regarding it in lines 444-448.

3.3. Please explain how the GNSS and remote sensing velocities were estimated as well as for which period and in which reference frame they are expressed. This is extremely important because, depending on the duration of the time series analysed and on the parameters, used to process the observations, the vertical velocity results obtained may vary considerably. And then, the comparison won't have the same relevance with respect to the models computed in this paper. Indicate also what remote sensing satellite and data were used.

We added a paragraph at lines 156-160 (section 2) in order to provide the reader with the most adequate reference regarding the GNSS data, which we remark are taken from the bibliography (Lange et al., 2014).

3.4. I'm surprised that the authors don't mention any comparison with GRACE observations. Which ice model was used?

We now include information and comparison with GRACE and other ice loss rate studies (See point 3.2, lines 119-124), and they were used to estimate ice thickness variations during LIA numerical simulations (lines 253-257).

Please note that, as we state at lines 53-59, this study differs from usual GIA investigations in that it does not use of a pre-constructed ice model, but rather a deglacial history that is constrained by literature data. Section 2.2. provides extensive detail on the modeling approach, parameters, and boundary conditions. See also points 2.3 and 2.5.

3.5. Explain why it is relevant to consider only one LIA model. Would it be possible to add some error bars on this model?

The LIA glacier extent is good estimated and because of this we use only one LIA model (lines 253-262).

See point 3.4. Note that this is forward modeling, and error bars of the model estimates may only be assessed by the parametric study, which here focuses on the asthenospheric mantle potential temperature (as we mention at lines 162-164, and 275-280).

3.6. Explain why the post-LGM model set 2 is flat in the second part and no small linear trend was considered and why the two models doesn't reach the same ice thickness at time 0.

We wanted to simulate two post-LGM deglaciation scenarios, Model set 1 is conservative and decreases the ice thickness by 75% linearly with time, whereas Model set 2 decreases the ice thickness in 95% in the first 10000 years (lines 245-253). This is because some authors argue that the Patagonian Ice Sheet retreated to its approximate current position in the first 10000 years of deglaciation (McCulloch et al., 2000; Hulton et al., 2002; Boex et al., 2013; Bendle et al., 2017; Davies et al., 2020), which would be the case of Model set 2 (lines 110-114). However, geochronological and stratigraphy studies show several and variable periods of glacial "maxima" and "minimum" since at least 20000 years BP (Glasser et al., 2004, 2008, 2011; Davies and Glasser, 2012; Strelin et al., 2014; Kaplan et al., 2016; Reynhout et al., 2019), thus justifying our Model set 1 (lines 114-117).

Note that the final ice thickness of the two Model sets is not meant to be the same.

3.7. It is unclear how the deformation resulting from the unloading were computed. Did the authors used Green's function and Love numbers or other methods?

We use forward modeling based on a previously developed, thoroughly tested and largely applied numerical thermo-mechanical geodynamic model. Sections 2.1. and 2.2 are entirely devoted to the description of the numerical model and modeling approach. We also provide the reader with references to the full description of the numerical techniques (e.g., Gerya, 2019).

3.8. It would be interesting to explain why the maximum rate of uplift occurs at 300-400 km (Fig. 4-6). It would also be helpful to show the same plots with the cumulatve effect of GIA and LIA as well as the differences between the two GIA models.

Figures 1-4 give an idea of the rebound under a uniformly thinning synthetic ice-cap with initial dimensions estimated in base of past glacier wideness (lines 258-262), the distribution of the uplift is in agreement with the theory of lithosphere flexure (Turcotte and Schubert, 2002) and we now include how the topography is calculated for each timestep (lines 268-272). We, however, focus on the magnitude rather than in the spatial pattern of uplift, as we mention explicitly at lines 149-153, and 162-164 (see point 1.1).

3.9. In the discussion section, the period corresponding to results are not clearly indicated. The authors should specify to which period their uplift values refer.

We modified the discussion accordingly.

3.10. Separate discussion and conclusion sections, with a more complete discussion with at least a more detailed comparison with actual uplift observations and how the results are consistent with other studies considering other period of times, other areas? The authors can have a look to the publicaDons of Hilary R. Martens et al. (e.g. https://doi.org/10.1093/gji/ggw087), A. Richter et al. (e.g. https://doi.org/10.1016/j.epsl.2016.07.042), H. Lau (e.g. https://doi.org/10.1016/j.epsl.2018.12.009) or Yan Hu and Jeffrey Freymueller (e.g. https://doi.org/10.1029/2018JB017028).

We have separated the Discussion and Conclusion sections accordingly, showing the consistency with other studies. We thanks for the references, we incorporate Richter et al. (2016) and Yan et al. (2022) that are very pertinent for this work, but the tidal geophysical studies were not added since they go beyond the means and aims of our study.

3.11. l. 42: I suggest adding the corresponding ice mass involved.

More details about ice mass involved are given in paragraph in lines 97-127.

3.12. l. 74-75: How were estimated the surfaces and volumes?

Based on literature data for present day dimensions of the Patagonian Icefields (Millan et al., 2019).

3.13. l. 83: Explain GNSS when first used.

See point 3.3.

3.14 Section 2.2: Explain the relevance of the different values retained (e.g. l. 159, 160, 182, 184).

The section was modified accordingly (see points 1.13 and 1.16).

3.15 l. 173: Explain what x and y directions correspond to.

Fixed (line 229).

3.16. Section discussion: It is not easy to follow the discussion and to understand which column of Table 2 the reader needs to refer.

The clarity of the discussion section was modified accordingly. Columns of Table 2 are organized by interesting timesteps of deglaciation, for the post-LGM in intervals of 1000 or 5000 years, and for the post-LIA in intervals of 100 years. The caption of Table 2 now clarifies it.

3.17. l. 281: Is it really “a few mm/yr” as in Table 2 values range from 2 to 19 mm/yr?

When TA< 100 °C, as mentioned at line 390, it is really a few mm/yr. Values and Tables are correct.

3.18. l. 290-292: Reformulate the sentence.

The sentence was reformulated (line 399-401).

3.19. l. 294: Which satellite?

The sentence was modified (lines 402-403).

3.20. l. 333: It would be nice to put a more recent value.

We agree, but the sentence is just meant as a general reference. More references regarding ice mass balance in the recent years are given in the introduction and discussion (lines 119-124 and 334-340).

3.21. Figure 1b: What data were used for the background color? How were obtained the GPS vertical velocities?

Details can be found in Lange et al., 2014, as mentioned at lines 156-160.

3.22. Figure 3: Explain how the velocity vectors were obtained and indicate the corresponding error bars. What about Model set 2?

The velocity vectors are computed by the numerical model based on the theory described in section 2 and the associated references. Regarding error bars, see point 3.5. The figure accounts for both Model set 1 and 2, as now mentioned in the caption.

3.23. Fig. 4: What does the distance refer to? To the center of the ice?

Horizontal distance, i.e., model length.

3.24. Fig. 7: Why not use the same scale for both sets? So, it is not easy to compare the two model sets of post-LGM. It would be very useful to add a vertical bar corresponding to the present-day epoch. It is important to be able to compare to the actual geodetic and remote sensing observations. It is not clear which way the time is going. Is it since the glacial maximum or in relation to the present day? Please clarify it.

Please note that, since the different Model sets account for different timing, using the same x-axis would compromise the visualization of the results. Details about the beginning of the deglaciation and the x-axis are now given in the caption of Fig. 7. Also, these are theoretical simulations so referring to an actual 'present day' would be misleading. See also point 3.6.

3.25. Table 1: I suggest starting with the ice and arranging the rows from surface to deepest. Adding information on the depths concerned would be a good idea.

For consistency, we prefer using the usual format of rheological tables for the numerical model used here (e.g., Gerya, 2019; Sternai et al., 2021).

3.26. Table 2: Review the number of significant digits. Clearly indicate where the current period is in the table, so that it is easy to know which values to compare the observations with.

Table 2 was improved and now limit values to the second digits. Thank you for making this point. Regarding the current period, please see point 3.24.

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