Response to reviewer and community comments for the article "Mass changes of the Antarctic Peninsula ice sheet and peripheral glaciers, 2007–2021 "

Dear Editor, Reviewers and Matt King,
We have received two well-informed reviews and one community comment which were overall positive and constructive. The two reviewers and Matt King had some minor comments which we have now thoroughly addressed. The following document details the changes we propose to improve the article, with comments in black and our answers in blue.
Thank you for your consideration of our revised manuscript,
Kind regards,

Maud Bernat, on behalf of the Co-authors

Reviewer 1

This is a well-executed study that makes a genuine contribution to our understanding of Antarctic Peninsula (AP) mass changes. The authors combine SPOT5-HRS DEMs with REMA, vertically adjusted using ICESat and ICESat-2 laser altimetry, to produce high-resolution (30 m) elevation change maps over a 14-year period. The approach is methodologically sound, the uncertainty quantification is thorough, and the spatial detail revealed is a clear advance over previous altimetry-based estimates. I recommend acceptance after minor revisions.

Major comments:

1. The authors report an AP Ice Sheet mass change of -26.5 ± 5.3 Gt/a, which is 4–5 times more negative than the IMBIE altimetry estimate of -5.2 ± 2.3 Gt/a. While the authors correctly attribute this discrepancy to the resolution limitations of satellite altimetry in mountainous terrain, the discussion would benefit from a more explicit spatial breakdown of where this difference originates.

   We agree, and now add this discussion to the article. First, the IMBIE altimetry estimate is available as a time series over the AP, but does not provide spatialized data. The estimate is calculated as the average of several altimetry studies that cover different time periods, which do not match the one from this study. Therefore, a spatial attribution of the differences is not possible when comparing only to the IMBIE figures.
   Second, the study from Smith et al., 2020 provides a dh/dt map using laser altimetry over the

period 2003-2019, which is close to the one from this study (2007-2021). To detail the spatial origin of this difference, we investigated the difference map between the two studies, presented in the figure below (Fig. A12), that we also added in the appendix of the article. Following this, we added a short paragraph in the section *5.2 Mass changes of the AP* to detail this in the discussion (in **bold**) L535-549:

"The differences between the altimetry estimates and the estimates based on DEM analysis, though both belong to the same elevation-differencing group of methods, can be attributed to a variety of sources. Altimetry studies tend to provide less negative mass change estimates (e.g. Schröder et al., 2019, Hassan et al., 2025), except for Smith et al. (2020), who obtained a mass change of -39 ± 5 Gt/a over the period 2003−2019. ***Discrepancies between elevation change maps derived from altimetry and from DEMs differencing highly depend on the area considered. When comparing the maps from Smith et al. (2020) to the results from our study, the mean elevation change rate difference is negligible over the ZWA12 southern regions 24 and 27 (0.002 and 0.008 m/a respectively). Those two regions are the largest of the AP and present a gentle topography that is accurately captured by laser altimetry. However, the elevation change rate difference is much larger over northern regions 25 and 26, reaching -0.10 m/a and -0.14 m/a respectively. Two distinct spatial patterns explain these discrepancies. First, interpolation errors in laser altimetry propagate outliers, which are more likely to remain due to the complex topography. For example, a small part of the northern tip of the Trinity Peninsula alone accounts for an unrealistic mass change of 10 Gt/a in Smith et al. 2020's study. Second, the limited spatial resolution generates a bimodal pattern observed on many intensely thinning glaciers, such as Hektoria, Drygalski and Fleming Glaciers. The most negative dh/dt values at low elevations (0-700 m) are overestimated, i.e. more positive, and the less negative dh/dt values at medium elevations (above 700 m) are underestimated, i.e. more negative, because of signal leakage. This systematically induces an overestimation of mean elevation change rates, up to 1.05 m/a (Crane Glacier) and 2.17 m/a (Hektoria Glacier) from the laser altimetry maps. Thus, if an apparent agreement can exist between laser altimetry and DEM differencing at AP scale, it is most likely coincidental and conceals significant spatial differences related to the methods' limits.****"*

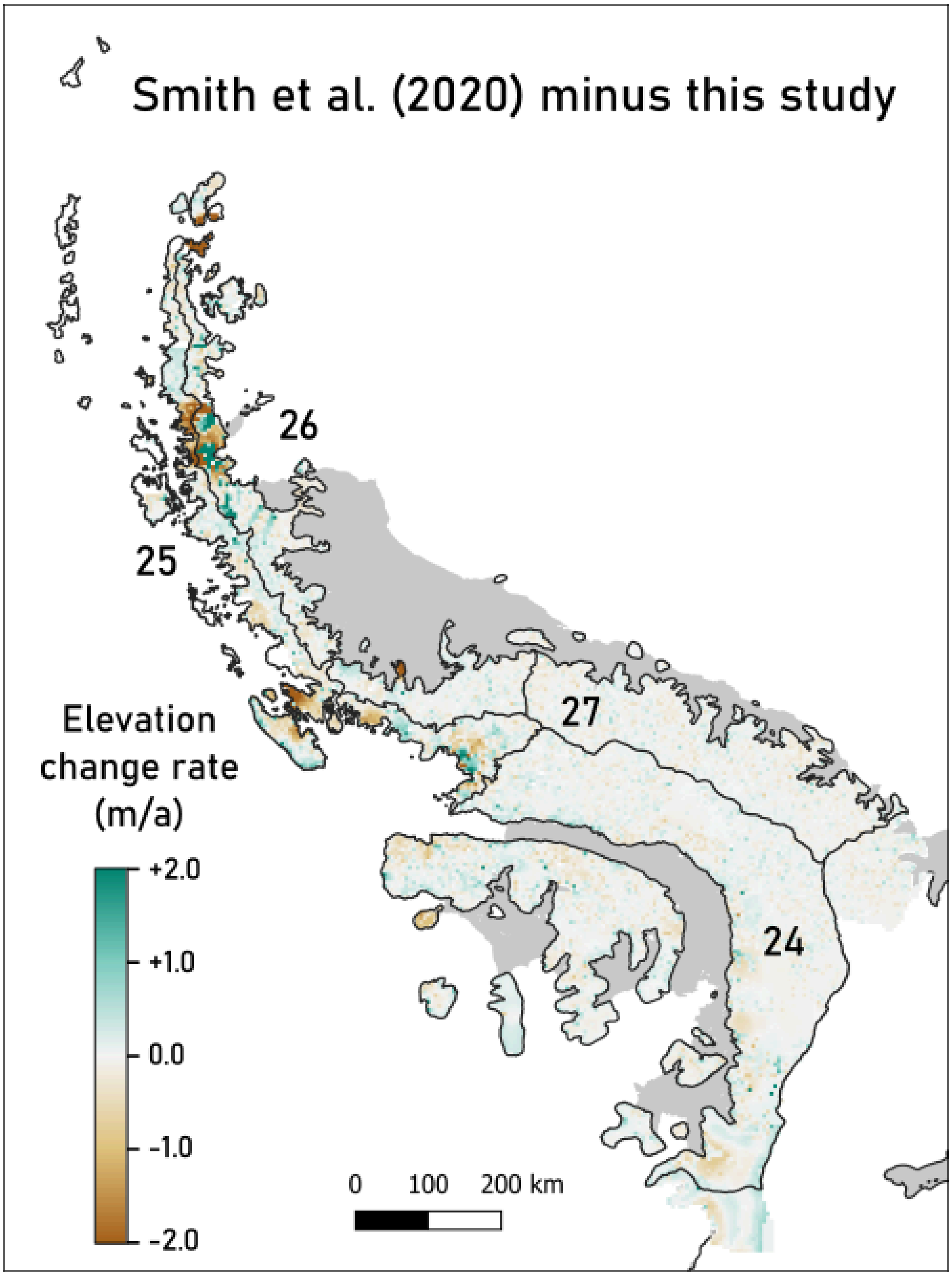

Figure A12: **Elevation change rate difference between the elevation change map from Smith et al 2020 and the one from this study.** The ZWA12 outlines are plotted in black and the numbers correspond to the associated subregion number (24 to 27). The RGI outlines are plotted in black.

2. The FAC correction is identified as the largest source of uncertainty, yet the discussion of inter-model divergence remains brief. Since the authors already computed mass change outputs for all four FDMs individually, a short dedicated discussion of why these models diverge would be a valuable addition. In particular, are certain models known to perform better over the Peninsula, given its strong west–east climate gradient?

The models indeed present several differences, which are apparent in the appendix figure A6 (elevation changes due to FAC for each model). Regarding the East-West gradient, all the firn models considered (IMAU, MAR, GSFC and GEMB) do produce contrasted spatial patterns of FAC. Additionally, half of the models (GSFC-FDM and MAR-FDM) present contrasted spatial patterns of FAC change. Still, we note that even though the FAC maps should reflect this East-West gradient characteristic of the AP, it is not necessarily the case for the FAC change. The spatial resolution of the models also impacts the spatial size of the patterns that can be modeled. More precisely, MAR has a resolution of 27.5 km, IMAU of 27 km, GSFC of 12.5 km and GEMB of 10 km. The northern part of the AP, especially along the Trinity Peninsula, can be as narrow as 35 km, which is hardly resolved by the coarser models (MAR, IMAU) and frequently results in no data.

The decision we made in this study to use several FAC models instead of one chosen arbitrarily is also to report their spread and use it to quantify the uncertainties. Investigating in detail the origin of the firn models' divergences would require a study on its own, and goes beyond the scope of our article. Moreover, the FAC (and its temporal change) is poorly known at the AP scale and defining a best performing model is difficult considering the very limited amount of validation data.

To sum up the considerations mentioned above, we added the following sentences in the discussion L664-672: *"Given the short time period, which allows for limited densification and therefore limits the impact of the model densification choices, the likely sources of differences in FAC change are the forcing (as described in Mottram et al., 2021), surface parameterizations (e.g., surface snow density), and potentially the treatment of liquid water from melt and rain. The spatial resolution, ranging from 10 km (GEMB-FDM) to 27.5 km (MAR-FDM) may also affect the ability of the models to resolve small-scale spatial patterns, such as the East-West gradient across the AP. This is particularly visible in the northern AP, where the standard deviation between the models reaches its maximum (Fig. A6). The AP is a difficult place to validate climate and FDM models due to the scarcity of in situ measurements."*

We also modified the appendix figure A6, adding the map of the standard deviation between the models, which facilitates identifying diverging areas.

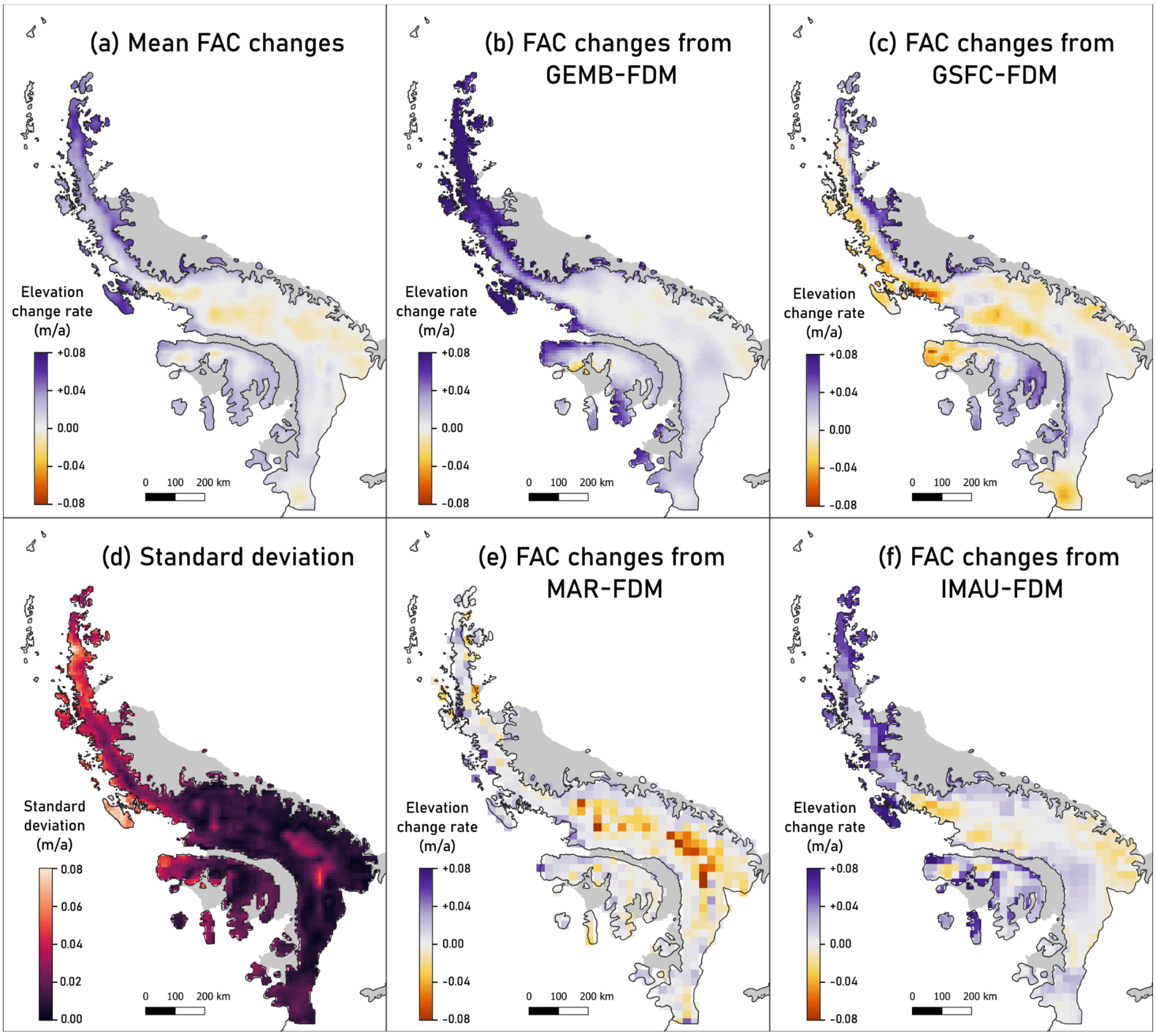

Figure A6. **FAC elevation change rates from firn densification models (2007−2021).** The mean FAC elevation change rate is shown on (a), the standard deviation on (d) and the FAC elevation change rates coming from the different FDMs are plotted on (b), (c), (e) and (f).

Minor comments

- The abstract states the AP Ice Sheet lost -27 ± 9 Gt/a, while Table 1 reports -28.2 ± 5.9 Gt/a for the SIL20 (APIS) outline. Please clarify this apparent inconsistency.
  Thanks for pointing out this inconsistency. We corrected the value to -28 ± 6 Gt/a.
- Figure 1: "Prince Gustav chanel" should read "Prince Gustav Channel."
  We updated the figure and also corrected the capital letters of Islands and Glaciers.
- Line 416: Trooz Glacier is cited with a thickening signal of +0.32 ± 0.26 m/a in the text, yet Figure 7 reports +0.06 ± 0.08 m/a. Please clarify whether these values refer to different spatial extents or time periods.
  This difference between the values is caused by the elevation interval restriction in the sentence L419-421. To clarify, we modified the text as follows: *"For example, considering only elevations below 800 m, Trooz Glacier (Region 25, Fig. 4g) presents a mean ± 2-σ ice elevation change rate of +0.32 ± 0.26* m_{ice}*/a. This value, more positive than the mean ice elevation change*

rate over all elevations of the catchment (+0.06 ± 0.8 m_{ice}/a, Fig. 7), highlights the low elevation thickening."

- Line 547: "Eembayment" should read "Embayment."
  We corrected the typo.
- Line 659: "in a many areas" should read "in many areas."
  We corrected the typo.
- Line 711: "personnal communication" should read "personal communication."
  We corrected the typo.

References:

Mottram, R., Hansen, N., Kittel, C., van Wessem, J. M., Agosta, C., Amory, C., Boberg, F., van de Berg, W. J., Fettweis, X., Gossart, A., van Lipzig, N. P. M., van Meijgaard, E., Orr, A., Phillips, T., Webster, S., Simonsen, S. B., and Souverijns, N.: What is the surface mass balance of Antarctica? An intercomparison of regional climate model estimates, The Cryosphere, 15, 3751–3784, https://doi.org/10.5194/tc-15-3751-2021, 2021.

Rignot, E., G.Casassa, P.Gogineni, W.Krabill, A.Rivera, and R.Thomas (2004), Accelerated ice discharge from the Antarctic Peninsula following the collapse of Larsen B ice shelf, Geophys. Res. Lett., 31, L18401, doi:10.1029/2004GL020697.

Rott H, Rack W, Skvarca P, Angelis HD. Northern Larsen Ice Shelf, Antarctica: further retreat after collapse. *Annals of Glaciology*. 2002;34:277-282. doi:10.3189/172756402781817716

Scambos, T. A., J. A.Bohlander, C. A.Shuman, and P.Skvarca (2004), Glacier acceleration and thinning after ice shelf collapse in the Larsen B embayment, Antarctica, Geophys. Res. Lett., 31, L18402, doi:10.1029/2004GL020670.

Reviewer 2

The paper describes elevation and mass change observations on the Antarctic Peninsula and surrounding islands (AP). The region is a difficult setup for mass change assessment, since all measuring methods show limitations so far. Radar altimetry has a rather coarse footprint or the swath mode data is limited by the steep topography, specifically on the northern part of the AP. Laser altimetry is limited by clouds and the sparse tracks. DEM differencing from optical imagery was limited by frequent clouds and oversaturation while bi-static radar interferometry by the short observation times, challenges for phase unwrapping and co-registration on sparse stable ground. Direct measurements from gravimetry are limited due to the narrow mountain range and a cross orbit track setting. The input-output method is subject to uncertain ice thickness estimations and measurements as previous papers revealed and what forms the base for the current SCAR RINGS AP initiative. Hence, this new data sets based on DEMs from the SPOT5-HRS and REMA is of high relevance, since it overcomes some of the deficiencies mentioned above for optical DEM differencing, specifically the difficulties with over-saturation in bright areas. Besides the DEM differencing, the paper includes an analysis on firn compaction models and GIA and elastic rebound. The paper is very well written and advances our understanding of mass changes for this region. The methods are very extensively described which is very much appreciated. I strongly recommend acceptance of the paper after some minor corrections.

Specific comments:

L12: I agree that your estimate is 4-5 times more negative as IMBIE, however, this value range is not completely new. A previous DEM change analysis by Seehaus et al. (2023) also referred to later in this paper reported comparable values (24 +- 2.8 Gt) although for a shorter time period (2013-2017). Hence, at least an indication, that such a previous study exist should be mentioned also in the abstract.
We fully agree that the radar DEM differencing study from Seehaus et al. (2023) already reported mass changes more negative than the IMBIE altimetry average. However, a direct comparison between our study and these results is not possible as the coverage differs strongly. Seehaus et al. cover only the northern part of the Antarctic Peninsula, whereas our study covers the entire AP (like IMBIE). Also, as mentioned by the reviewer, the periods substantially differ (2013-2017 vs 2007-2021), which makes a comparison even more tedious. Hence, we prefer to mention the study from Seehaus et al. (2023) later in the introduction.

We also added a sentence in the discussion to emphasize the earlier findings of Seehaus et al. L529-530: *"Seehaus et al. (2023) also reported more negative mass change rates than those provided by the altimetry in a radar DEM study over the northern AP between 2013 and 2017."*

L21: "… responding dramatically" is a kind of not clearly defined expression. The authors might consider choosing a different wording.
The reviewer might have misread the text as the original sentence is *"In the context of climate change,*

the AP ice masses are responding dynamically."

L23: I suggest to refer to the original literature that discovered this reaction to ice shelf collapse and not to one of the many subsequent studies. Specifically, as this reaction has not been discovered by altimetry.

We removed the reference to Davidson et al., 2023 and added the following references at the end of the sentence L23: *"these changes are often linked to the thinning or breakup of their ice-shelves, e.g. Drygalski, Hektoria, and Crane Glaciers* ***(Rott et al., 2002; Rignot et al., 2004; Scambos et al., 2004)****"*

L68: If I understand this sentence correctly, the ICESat-1/2 elevations are used not necessarily from the same year, but from the same season. Maybe, this could be expressed more clearly since the subsequent description in the method sections reads like only data from the same year/season had been considered (e.g. L206, L244).

Thanks for highlighting this misunderstanding, we were probably not clear enough in the data description. The laser altimetry data (ICESat and ICESat-2) used to vertically adjust the stereoscopic data (HRS and REMA DEMs) do come from the same year. In other words, each DEM is vertically corrected with altimetry data from +- 2 months. However, the years considered for each epoch span over 3 years. HRS DEMs were acquired during the austral summers 2006, 2007 and 2008 and the REMA DEMs during the austral summers 2020, 2021 and 2022. The paragraph mentioned L68 intended to state that a "seasonal" interval was defined when comparing DEMs between the two epochs. We agree that the sentence is not clear so we removed it as it is not necessary to have it here. The question of the seasonality is already addressed in section 3.2 and 3.3.

L231-233, we added the sentence (in **bold**): *"For each HRS segment, we identified the overlapping REMA DEMs, acquired in the austral summers 2020, 2021 and 2022 (Fig. 2). We limited the day-of-year difference to ± 62 days (2 months) to avoid seasonal errors.* ***For example, a 12-10-2007 SPOT5-HRS DEM would not be compared to a 12-01-2021 SPOT5-HRS DEM as the seasonal difference (3 months) is out of the 2-month maximum interval.****"*

L416: The values in the text and Fig.7 for Trooz Glacier are different. Please clarify if this is caused by the elevation interval restriction to 0-800m.

This difference between the values is indeed caused by the elevation interval restriction in the sentence. To clarify, we modified the text as follows L419-421: *"For example, restricting the elevation interval to 0-800 m, Trooz Glacier (Region 25, Fig. 4g) presents a mean ± 2-σ ice elevation change rate of +0.32 ± 0.26* m_{ice}*/a. This value, more positive than the mean ice elevation change rate over all elevations of the catchment (+0.06 ± 0.8* m_{ice}*/a, Fig. 7), highlights low elevation thickening."*

Fig. 7: it would be interesting to read more including an explanation for the very heterogeneous dh/dt values on Anvers Island, e.g Iliad Glacier and surroundings. It is shown in the map, but I could not find a corresponding section in the main text (L579 and following). Fig.4 shows a quite limited coverage whereas Fig.7 provides values for each glacier catchment. Do those heterogeneous values partially

come from the interpolation?

Thanks for pointing out this area which indeed presents interesting and contrasting spatial patterns. As the reviewer correctly observed, Anvers Island was partially covered by the dh/dt observations (30% of the total area of the island) and thus a substantial portion of it was filled with interpolation before calculating the mean elevation change per catchment (presented in Fig. 7). A zoom on this precise area is presented in Fig. S1 below. For example, the elevation changes of glaciers labeled A, B and C are poorly constrained and thus might not correctly reflect the real changes. Therefore, the apparent contrast between Iliad Glacier and Glaciers A/B/C mean ice elevation changes (Fig. 7) is very uncertain.

Nevertheless, some of the contrasting spatial patterns of dh/dt values in Anvers Island are not the result of interpolation. In particular, Glaciers D and E present very different dh/dt values from Iliad Glacier, which primarily come from observations and not interpolation. When averaged over the catchment, these differences in dh/dt lead to a notable difference in mean elevation changes (Fig. 7). We also note that many other areas in the AP present this kind of contrasted dh/dt patterns, in many cases not induced by the interpolation (e.g. the two neighbouring glaciers Stefan Ice Piedmont and Peter, or Cadman Glacier and its neighbors Fig. 7). This is also a consequence of the high spatial resolution that enables us to observe changes at glacier scale.

Last, this area is also of interest as it shows the dependency of the hypsometric interpolation method on the reference DEM used. Some artefacts in the TanDEM-X DEM appear clearly in the catchment West to Iliad Glacier. However, a sensibility analysis (comparison between the hypsometric interpolations obtained using the TanDEM-X DEM and the REMA DEM as a reference) proved the very limited impact of the reference DEM on the catchment mean elevation change.

To sum up these remarks, we added the following sentences (in **bold**) in the discussion L684-687: *"The local hypsometric interpolation performs well in smooth high altitude areas but struggles more reproducing local patterns of intense thinning at lower elevations (e.g. Drygalski or Fleming Glaciers).* ***If some of the contrasting dh/dh spatial patterns do reflect observations (e.g. Stefan Ice Piedmont and Peter Glaciers, Cadman Glacier, Fig. 7), the interpolation can artificially generate contrasted values, as in the south of Anvers Island (Fig. 7). Glaciers' mean ice elevation changes must therefore be interpreted with caution, especially when dh/dt observations are scarce."***

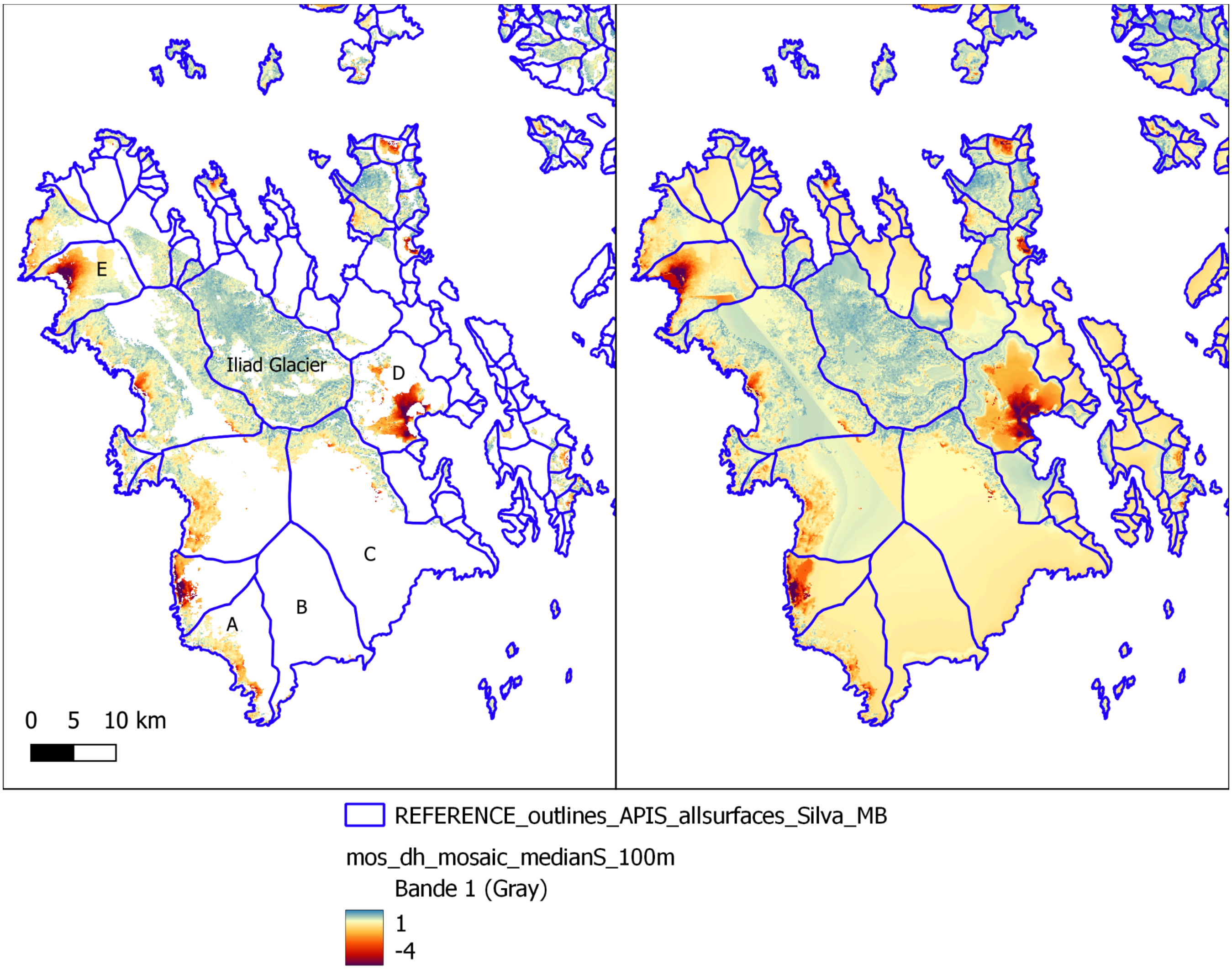

Figure S1: **dh/dt observations (left) and dh/dt complete map filled with interpolation (right) over Anvers Island.** The SIL20 outlines are plotted in blue.

L599: This paragraph mainly contains information from previous work without a real own contribution. Could perhaps be shortened and other parts extended as mentioned before e.g. on Anvers Island glaciers. It is understood, that this is mainly an observational paper, however, an attempt for a glaciological/climatological explanation of those close by differences in advance/retreat would be of interest. This does not come out from the quite general and known statements on DCW influence (L612f). Same for Jason Peninsula.

As suggested, we shortened the paragraph removing too detailed description of previous studies. We also added a few sentences (in **bold**) on Anvers Island and Jason Peninsula cases L624-631:

“Unlike Cadman Glacier, the Funk and Lever neighboring glaciers have shown constant velocities and stable terminus over the past decades (Wallis et al., 2023). We confirm that between 2007 and 2021, a much more negative elevation change was obtained for Cadman Glacier (-0.89 ±0.12 m_{ice}/a) than for Funk and Lever Glaciers (-0.06 ±0.08 m_{ice}/a and -0.13 ±0.10 m_{ice}/a, respectively). Moreover, several

nearby glaciers presented thickening glacier tongues, including Trooz and Funk Glaciers, which participate in the slightly positive or close-to-zero mean elevation change obtained for these glaciers. Wallis et al. (2023) postulated that the disparities between the recent dynamic evolution of the Cadman Glacier and the neighboring glaciers are related to the different bathymetry configurations. ***Following this, the seabed dataset provided by Lavoie et al. (2015), based on swath multibeam data sets from five national programs, provides some insight into the elevation changes of several glaciers of the northern AP. Shallow trough bathymetry is often associated with low glacier flux in this area (Davison et al., 2024). For example, the basin in front of Iliad Glacier, presenting a mildly positive elevation change (+0.22 ±0.12*** m_{ice}***/a) is notably shallow (<300m). Similarly, the positive elevation changes observed on other glaciers of Anvers Island could be related to the shallow bathymetry near the island's eastern coast. The area just offshore of the Jason Peninsula is also very shallow, but glaciers at the very tip of the Peninsula tend to present slightly negative elevation changes. The positive elevation changes of the other glaciers of the Jason Peninsula are likely explained by buttressing from SCAR Inlet Ice Shelf.***

~~Cadman Glacier was exposed to warm subsurface currents due to a deep frontal fjord, which could have weakened the ice shelf and triggered the glacier's increased ice discharge. On the contrary, Funk and Lever Glaciers were protected from warm oceanic intrusions thanks to their shallow sills. Davison et al. (2024) generalize this relationship between glacier ice discharge and bathymetry to the entire northwestern AP. Between 2017 and 2022, they reported a widespread acceleration and increase in ice discharge of glaciers from this region. They attributed these dramatic changes to the influence of warmer ocean temperatures from the CDW, primarily controlled by the topography of the sills."~~

L704: The doi and location of the elevation and mass change product should be published with the paper.

We created a Zenodo repository containing the elevation and mass changes results from the study, accessible here: https://doi.org/10.5281/zenodo.19593987

Seehaus, T., Sommer, C., Dethinne, T., and Malz, P.: Mass changes of the northern Antarctic Peninsula Ice Sheet derived from repeat bi-static synthetic aperture radar acquisitions for the period 2013–2017, The Cryosphere, 17, 4629–4644, https://doi.org/10.5194/tc-17-4629-2023, 2023.

References:

Lavoie, C., Domack, E. W., Pettit, E. C., Scambos, T. A., Larter, R. D., Schenke, H.-W., Yoo, K. C., Gutt, J., Wellner, J., Canals, M., Anderson, J. B., and Amblas, D.: Configuration of the Northern Antarctic Peninsula Ice Sheet at LGM based on a new synthesis of seabed imagery, The Cryosphere, 9, 613–629, https://doi.org/10.5194/tc-9-613-2015, 2015.

Matt King’s comment on the open discussion

A brief comment on the adopted GIA models on what looks like a really nice paper. The millennial-scale GIA models are well known to be inaccurate in the Peninsula. I provide the references below. I think this is a secondary issue, but it does possibly need some quantification of the effect of this mismodelling, given that it is a systematic error.

Nield et al. 2014 https://www.sciencedirect.com/science/article/pii/S0012821X14002519 (see Figure 4 where uplift rates are compared to ICE5G and IJ05R2). ICE6G would have similarly low agreement with the GPS uplifts.

Samrat et al. 2020 https://academic.oup.com/gji/article/222/2/1013/5835685?login=true

I look forward to seeing the finalised paper and data!

Thanks for this interesting comment on crustal deformation. To clarify the crustal deformation processes that exist, we listed them in the table below. The two components that we included in our study are the **Glacial Isostatic Adjustment** (GIA) and the **Elastic Rebound** (ELR). Note that the expression “Glacial Isostatic Adjustment” should be used for the total viscoelastic part of the table, but is commonly used for the long term component as the other components are not modeled. We also clarified this in the text L148-150: *“The term GIA is here used to describe the long term crustal viscoelastic response, i.e. related to the millennial-scale deglaciation, and does not include medium and short term evolutions such as since the Little Ice Age.”*

Viscoelastic			Elastic
Long term (thousands of years) = Deglaciation	**Medium term** (hundreds of years) = Little Ice Age	**Short term** (tens of years) = Climate change	**Present term** (current) = Present ice unloading
V **Glacial Isostatic Adjustment (GIA)**	**X** **(A)**	**X** **(B)**	**V** **Elastic Rebound (ELR)**

The two articles mentioned highlight the importance of considering the recent visco-elastic response, induced from both medium term (**A**) and from short term (**B**) changes to reproduce the observed GNSS changes. In other words, they show that considering only the GIA + ELR is not sufficient to match the GNSS observations. However, modeling the recent viscoelastic response is not a straightforward task. To our knowledge (and we welcome direct exchange with Maat King if we are wrong), there is no dataset available that provides such results over the entire Antarctic Peninsula for the period considered (2007-2021). More generally, Nield et al. (2014) report a maximum uplift rate of 14.9 ± 2.7 mm/yr at Foyn Point (FONP) between 2009 and 2012 while Samrat et al., (2022) report that *“The* *maximum observed uplift rate of ~12.4 ± 0.2 mm* yr^{-1} *was recorded at the DUPT station between 2009 and 2010.* " Those two stations, FONP and DUPT, are located in the northern AP. This region (corresponding to Regions 25 and 26 from Zwally et al., 2012 outlines) concentrates the most negative elevation changes (Fig. 6). The uncertainty largely comes from the FAC (Fig. A12) as models particularly diverge in this narrow part of the AP. The standard deviation of FAC models ranges between 3 to 6 cm/a in the northern AP. The maximum uplift rate is therefore three times lower than the firn related uncertainty in this region. Regarding the other regions (Regions 24 and 27 from Zwally et al., 2012 outlines), ice changes are less intense but the viscoelastic studies do not cover them, so it is complicated to derive any conclusion.

Therefore, we modified the appendix figure on GIA models (Fig. A7) to show the map of the spread between the models and enable to visualize spatial discrepancies. We also added the following sentences (in **bold**) in the discussion to more clearly mention the limits of the crustal deformation correction L651-656: *"The choice of a GIA or elastic rebound model has minor implications for the final volume changes. The associated corrections systematically represent less than 4% of the signal, and fall within the observational uncertainty.* ***However, the crustal deformation corrections applied in the study (GIA and ELR) do not account for recent viscoelastic response, which might be required to fit with observed bedrock uplift rates (Nield et al., 2014; Samrat et al., 2022). The misfit induced could exceed the GIA and ELR signals in the northern Peninsula. Even if the signal amplitude remains lower than the firn-related uncertainty in this region (Fig. A6), future studies should consider applying a viscoelastic correction at the AP scale in order to account more accurately for crustal deformation.***"

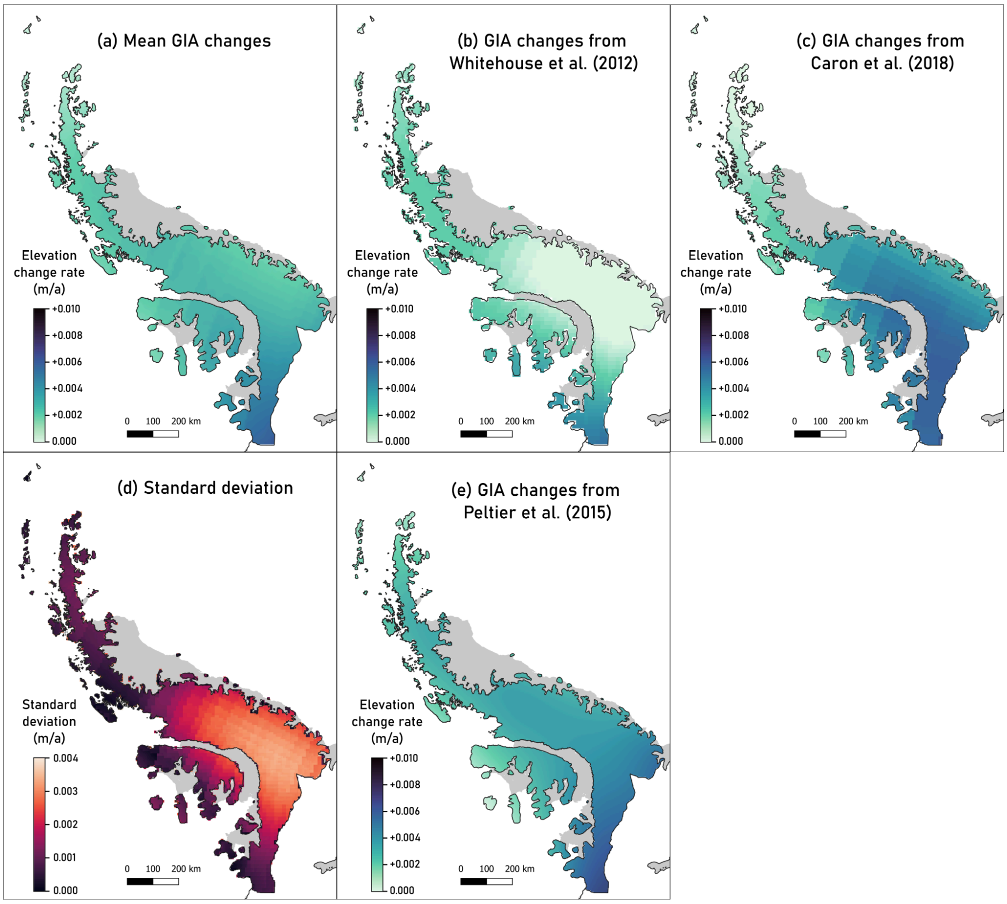

Figure A7: **Elevation change rates from GIA models (2007-2021)**. The mean GIA elevation change rate is shown on the panel (a), the standard deviation on panel (d) and the elevation change rates coming from the different models are plotted on the panels (b), (c) and (e).