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Re: [Paper ID #egusphere-2025-5381] On the use of radar isochrones to improve paleo-ice-sheet model simulations away from ice divides by J. A. Bodart, et al.

Dear Editor, Reviewers,

We would like to thank both reviewers for very insightful and constructive reviews of our manuscript, as well as the Editor and the editorial team for handling the review process.

We are very pleased to see that both reviewers recognised the importance of our results and how these were presented in our manuscript. Both have provided us with some excellent comments, which have undoubtedly improved the quality of our manuscript.

In the following response letter, we begin by addressing the comments from Reviewer #1, followed by those made by Reviewer #2. We have formatted the comments of each reviewer in italics and have indented our responses in green below each comment. Please note that the line numbers provided in this response letter refer to the updated manuscript (non-tracked version), unless otherwise indicated.

Upon a decision from the Editor, we will re-submit the two versions of our manuscript, one with tracked changes (‘Bodart_et_al_2026_resubmitted_tracked’) and the final updated version with all changes incorporated but not highlighted (‘Bodart_et_al_2026_resubmitted’).

We look forward to hearing your decision and stand-by in the meantime with any queries you might have.

With best wishes,

Dr Julien Bodart (on behalf of all co-authors)

Reviewer #1:

General comments

This manuscript explores the use of internal reflection horizons (IRHs) to calibrate ice-sheet models. To support this goal, the authors trace multiple IRHs in two key regions of East Antarctica and then use these horizons to evaluate model outputs produced under different model setups. Through this work, they demonstrate that incorporating IRHs into ice-sheet models can provide an additional and powerful constraint on simulations of past ice-sheet dynamics, especially away from ice divides.

The paper addresses an important topic. The authors have done a substantial amount of high-quality work, particularly in producing, documenting, and sharing new IRH datasets in a standardized, model-ready form. I especially appreciate the careful uncertainties analysis. The modeling results are clearly presented and thoroughly discussed.

Overall, I believe this is a timely and well-motivated study at the interface of radar stratigraphy, ice-sheet modelling, and data–model integration. I will be happy to see the paper being accepted for publication after addressing the points below.

> We would like to thank Reviewer #1, Dr Shuai Yan, for his positive comments and useful suggestions, which have improved this manuscript.

Major suggestions:

1. Manuscript structure and readability

a. The manuscript presents a substantial amount of new IRH tracing, together with depth and age uncertainty estimates. While I very much appreciate the volume of new data and the thorough description, it is difficult to quickly locate specific information in such a long paper (for example, the uncertainty associated with a particular IRH in a given region). I strongly recommend adding a concise summary table that lists, for each IRH and region, information such as the nominal age, associated uncertainties, etc.

> Thank you for the suggestion, which also corresponds to a similar suggestion from Reviewer 2. We have now added a comprehensive table in the main text (Table 2) which provides this specific information for each IRH across both sectors.
>
> Please also note that the dating of the isochrones over WSB have changed ever so slightly due to an error in the version of AICC (2012 vs 2023) which was noticed during the reviews. The new ages are as follows: 6.6 ± 0.1 ka, 13.1 ± 0.3 ka, 37.7 ± 0.5 ka, 64.1 ± 2.5 ka, 73.1 ± 1.2 ka, 90.8 ± 1.4 ka, 117.0 ± 1.3 ka, 122.8 ± 1.0 ka, and 128.4 ± 3.6 ka. We also updated the labels on all the figures. The isochrone ages over DML remain unchanged.

b. The results and discussion are combined into a single section. This is acceptable in principle, and I appreciate that, given the integrative nature of this study, substantial description and discussion of the IRH data are needed before the model–data comparisons can be presented. However, in the current form this combined section becomes very long and dense, with few natural breaks, which makes it hard to navigate. I suggest either (i) separating Results and Discussion into two sections, or (ii) introducing clearer internal structure and signposting so that readers can more easily follow the progression from data description to modelling results and their implications.

Thank you for this comment, which reflects a similar comment from Reviewer 2. We like the structure of the Results and Discussions section, but we agree that the previous version of the paper lacked some structure, particularly for Section 3.2. We have addressed this comment as follows:

1. We have updated the last paragraph of the introduction which (a) sets the key objectives of the paper early, as suggested by Reviewer 2 to improve clarity; and (b) describes the structure of the paper so that readers are provided with this information form the outset.
2. We have retained but rephrased the first paragraph of the "Results and Discussions" section, which sets the structure of this chapter clearly and points to the different figures and tables that will be discussed.
3. We have added a new sub-section under Section 3.2 "Data-model comparison" titled "3.2.1 Modelling rationale", which now comes before the already existing (but labelled 3.2.1 in the previous version of the paper) "3.2.2 Comparison between the observed and modelled 91-ka isochrone across WSB and DML regions". We provide an additional introductory paragraph under Section 3.2 to guide the reader towards each of sections 3.2.1 and 3.2.2, again pointing to specific figures and tables in these two sections.

   We believe that adding this new Section 3.2.1 to the already existing 3.2.2 significantly improves readability of this Section 3.2 of the paper, and that the individual introductory paragraphs throughout the paper significantly help with "signposting", as recommended by Reviewer 1 and 2. Thank you for this suggestion.

c. The section numbering and nesting are confusing in places. For example, there is a Section 3.2 under which a subsection is labelled 3.2.1, but there is no 3.2.2. I recommend revisiting the section hierarchy and renumbering, and possibly adding a short "roadmap" paragraph at the start of Section 3 to guide the reader through the data description, model setup, and data-model comparison.

Thank you for noticing this and for this useful suggestion. The section numbering has been modified to take into account the Reviewer's previous comment and the comments from Reviewer 2 on the same topic, as stated in our answer to the previous comment. Section 3.2.1 is now a new section describing the model rationale, and 3.2.2 is the modified "3.2.1" section of the previous version of the paper.

2. Limited model setup explanations

The different model experiments are generated by modifying the precipitation-scaling parameter and the till effective pressure parameter. These are important concepts for understanding the subsequent results, yet they are only briefly introduced, and the actual values used in each simulation are not very clearly summarized. I suggest:

a. Providing a short explanation of the physical meaning of the precipitation scaling and till effective pressure parameters, and why these two were chosen as the focus of the sensitivity tests over other possible parameters.

Thank you for this suggestion. We have added several sentences on this which address this point and the next point (our answer to this point and the next one are provided together in the below comment "(b)").

b. Adding a small table or a clear paragraph that lists the parameter values for each simulation, instead of showing such information in a figure legend panel, so that readers can easily see how the experiments differ.

Thank you for these two comments. We would like to clarify that this information was already partly provided in Section 3.2.1 or the original manuscript, but modified subsequently to take into account these comments, as follows:

"To best evaluate how well the model reproduces the observed isochrone, we ran three simulations on a regional domains across WSB and DML by slightly modifying the precipitation scaling which governs the relationship between surface temperature change and precipitation change, and the PISM-specific till effective pressure parameter which governs how much of the overburden pressure from the ice load is transferred to the subglacial till to drain excess water from the sediment column (Bueler and van Pelt, 2015), thereby modulating the effective pressure (Fig. 4).

Whilst the precipitation scaling controls Surface Mass Balance (SMB), which in East Antarctica is dominated by net accumulation of snow and minimal surface melt, the till effective pressure influences sliding over the bed via its interaction with the substrate underneath the ice. The selection of these two parameters was thus made to illustrate the imprint of SMB and basal ice flow on the elevations of isochrones via a small (but commonly used) subset of the large parameter space available to ice-sheet models and climate forcing choices (i.e., Albrecht et al., 2020)."

In addition, we recognise that perhaps this paragraph was not as clearly stated and may have been lost in the rest of the text. We have made this paragraph a stand-alone one as part of this section, and have also added a second paragraph in Lines 389-398 (new version of the paper) which additionally addresses parts of comments (a) and (b) of the Reviewer, as follows:

"We tested values for the precipitation scaling ranging from 5 - 11 % and for the till effective pressure from 0.023 - 0.03 (Fig. 4). The first range of values is informed from reconstructions of precipitation scaling at ice cores across Antarctica (particularly the low-end of this range, e.g., Frieler et al. (2015); Fudge et al. (2016)) and testing of values slightly outside of these reconstructions that provide a best-fit in isochrone elevations (i.e., the higher-end of this range). The second parameter is a refined range of values that produces acceptable present-day offsets between modelled and observed surfaces within a much wider range of possible values. We note that many other parameters can also influence the present-day offsets in surface elevations and the mismatch in isochrones; however, mismatches in isochrone elevations across drainage basins influenced both by surface mass balance changes as well as ice dynamics are particularly driven by these two PISM parameters whilst at the same time providing similar surface elevation offsets, thus making them ideal for our experiments."

These clarifications would make the modelling setup more transparent and help readers interpret the model–isochrone misfits.

We believe that the changes made have significantly clarified and improved the description of the parameters used in the model. Thank you.

Line-by-line comments

- *There is mixed usage of "isochrones", "isochronal surfaces", and "IRHs" throughout the manuscript to refer to the same features. I recommend adopting a more consistent terminology. Also, I suggest avoiding "isochronal surfaces," because "surface" is already used frequently in the manuscript to refer to the ice-sheet surface, which may cause confusion.*

  We have removed "isochronal surfaces" from the paper and harmonised the terminology, using mainly the acronym "IRHs" throughout the paper, and occasionally using the term "isochrones" to avoid repetition. We have also removed the word "layer" or "englacial layer -ing" and replaced these with either IRH or isochrone.

- *The impact of interpolated (rather than directly mapped) bed elevation is discussed and highlighted. Could the authors also comment on the potential impact of interpolated IRH depths?*

  Because interpolated IRH depths (which come from the model) are impacted by the underlying interpolated gridded product (surface and bed elevations), any over-fitting by the interpolation algorithm in e.g., Bedmap3 will impact the model geometries and all its components. We believe that this was already mentioned in Lines 380-390 where we mention that the problem with the underlying boundary conditions in the model result in the modelled isochrones to follow the modelled bed and thus cut through the observed bed, which would not happen had accurate boundary conditions where no over-fitting of the interpolation algorithm due to the lack of underlying radar data been used.

- *Line #75: I assume "This" stands for the DML survey?*

  Yes, "This" referred to any surveys that are tailored to acquiring data suitable for ice-sheet modelling applications. We do not think it is necessary to amend this in the text.

- *Line #117, "WSL": do the authors mean WSB here?*

  Yes, thank you for catching this small error. We meant WSB. This has been amended in two locations where this was the case.

- *Line #143, "Where necessary, we also made use of the 3-D capability of the software to find intersecting IRHs that were not visible in the 2-D view": This sounds like an interesting and useful approach. Could the authors elaborate?*

  The software we used for tracing isochrones (i.e., Petrel Schlumberger) offers the ability to view the radar data in three dimensions, which is a great advantage to other geophysics software such as Landmark where only 2D view is available. At times, we made use of this 3-D viewer to view the data in three dimensions which can help to visualise where isochrones from other surveys intersect our radar lines. This can be more informative than simply relying on "flat" 2-D crossovers in the 2D view, as it gives the user the ability to rotate the image around to gain a different viewing angle. We do not think it is necessary to add more information in the text regarding this point.

- *Line #161: is this depth from the ice surface or depth from the radar system?*

  Below the surface. We added this specificity in the modified manuscript.

- *Line #185, "These three sources of uncertainties are then combined by calculating the root-sum square error for each isochrone depth": Do the authors mean the RMS error for each IRH represents the combination of these three sources of uncertainties?*

  Yes, this was amended as follows: "Assuming these uncertainties are not correlated, the total IRH depth uncertainty is calculated as the root-sum-square error of these three uncertainties"

- *Line #191, "It is worth …… into account": This would be a stronger and more useful statement if the authors could provide typical values for relevant model uncertainties together with references.*

  As this is a relatively new approach to constraining ice-sheet models, there are few studies that have done such comparison. However, we have added a pointer to our Section 3.2.1 which discusses the amount of offset that is typical between modelled isochrones and observed ones in our simulations, and also cited Sutter et al. (2021) which previously showed best-fit values for model vs isochrones in East Antarctica, showing that this offset is approximately an order of magnitude greater (in the hundreds of meters) compared with radar-derived depth uncertainties (in the tens of meters).

- *Line #195 "accept that this could amount to several more metres in places": A brief quantitative estimate/statistics, with a pointer to Appendix A, would make this statement clearer and more informative.*

  Unfortunately, it is not known how many meters this could amount to; however, we can get an idea by assessing the crossover errors between intersecting isochrones of the same age. We have reformulated this sentence and the next two, to make this clearer and frame their unknown errors in relation to Appendix A:

  "We do not consider errors associated with our picking as this is difficult to estimate, nor do we account for snow accumulation or snow re-distribution between survey years. Considering previous estimates of picking uncertainties and travel-time-to-depth conversion from past studies, we expect these unquantified uncertainties to be smaller than the range resolution of the radar systems used here for the area and timespan considered. However, to ensure that IRHs are, on average, the same across intersecting transects, we conducted a crossover analysis of IRH depths across both WSB and DML radar lines."

- *Line #508: repeated "the"*

  Removed, thank you.

- *Figure 1: I appreciate that the authors include ice velocity as the background field. However, with the current colormap, the maps are dominated by deep colors, which makes it difficult to see some important features (e.g., survey lines with fewer existing IRHs, ice divides). I also recommend increasing the font size of the lat/lon labels, color bar labels, and place names.*

  We thank the reviewer for his comments regarding our Figure 1. We have increased the font size for the labels and made the colour scheme for some of the labels bolder and brighter to aid visualisation, as requested. We did not modify the colour scale for the ice velocities as we believe that the other lines are clear enough. With regards to the IRHs, they are not depicted in this figure, but in the new Figures 2 (WSB) and 3 (DML) where IRH coverage is shown.

Reviewer #2:

This manuscript primarily has two components, the first is about tracing and dating 16 IRHs across two major East Antarctic regions using newly acquired and some existing data resources. The latter is about demonstrating IRH utility for model calibration using specific 90ka IRH. The authors have put in substantial effort acquiring new radar data, tracing and dating isochrones across two major East Antarctic regions, and demonstrating their utility for the Antarctic modelling community. The datasets being released will be immediately useful, and the modelling insights are valuable. The language is clear and the figures are mostly well made. This work is worthy of publication in The Cryosphere, but after some revisions which are needed to be addressed.

We would like to thank the anonymous reviewer for their time in reviewing our paper and for their positive review which has improved our manuscript.

Major comments:

- *The main issue is that the manuscript is trying to do two things - present the IRH datasets and demonstrate their utility - but only actually uses 2 of those 16 IRHs in the modelling exercise. So, the reader has to go through detailed descriptions and methods for all 16 IRHs (Sections 2.1-2.2, 3.1, plus Figure 2 with 16 panels!) before getting to the modelling study that only uses the ~90 ka isochrone. This makes the manuscript feel a bit more like a data paper at times. I suggest restructuring the manuscript a bit to improve on this aspect. Below is how I suggest restructuring the paper:*

    1. *Introduction*
    2. *Airborne radar data; 2.1 New data at WSB; 2.1 New data at DML*
    3. *Mapping IRHs; 3.1 Isochrone tracing, dating and validation; 3.1.1 IRH tracing; 3.1.2 IRH uncertainty; 3.1.3 IRH ages and age uncertainty; 3.2 Tracked IRHs; 3.2.1 IRH over WSB; 3.2.2 IRH over DML*
    4. *Data-model comparison; 4.1 The 90 Ka isochrone; 4.2 Model Setup; 4.2 Data–model comparison results*
    5. *Conclusions*

    Thank you for this suggestion. We appreciate that the structure of the previous version of the paper was confusing in places, and we thank the reviewer for suggesting a potential solution which would fix these issues. However, we do not think that the suggested changes to the structure of the paper by Reviewer 2 would make it less confusing. This is because the structure proposed by the Reviewer would mix “Data and Methods” with “Results and Discussions”, which would risk confusing the reader further. We believe that the current structure, which places all the methods under Section 2 (data over WSB and DML, IRH tracing and dating, and the modelling set-up) and then proceeds to discuss the results into a combined Section 3 (first describing the isochrones over both sectors, and then discussing their use into the model) is a reasonable approach.

Recognising the points made by Reviewer 1 and 2 regarding the confusion around the structure of the paper, we made several improvements which we believe address the issues raised. Particularly, we (i) added and expanded existing introductory paragraphs throughout the paper (i.e., the "Introduction" and "Results and Discussions" sections) to help with "signposting" the reader to what is to come, and (ii) added a new sub-section under "3.2" (Section 3.2.1, which is then followed by Section 3.2.2) which we believe significantly improves the readability and structure of this part of the paper. We refer the Reviewer to our answer to the second comment from Reviewer 1 on what exactly has been done to improve the structure.

The restructured section 2 and 3 cover the data acquisition/tracking/uncertainty/description while Section 4 contains the modelling exercise. Making this distinction clear to readers will help a lot.

- *The last paragraph of the introduction, should also be updated according to the restructure and prepare the reader for what is to come.*

  Thank you for this suggestion, which is similar to Reviewer 1's suggestion. We have expanded the original introductory paragraph that guides the reader through the structure of the paper and added additional introductory paragraphs throughout the Results and Discussion section to avoid confusion. We have also explicitly stated the objectives of our study in this paragraph, as suggested by Reviewer 2 (see below comment).

- *Also, add a table (or two tables one for each region) listing the IRHs, age, age uncertainty, dated using which ice core, depth at the core, coverage (km), and maybe highlighting the 90 ka IRH.*

  Thank you for the suggestion, which also corresponds to a similar suggestion from Reviewer 1. We have now added a comprehensive table in the main text (Table 2) which provides this specific information for each IRH across both sectors.

  Please also note that the dating of the isochrones over WSB have changed ever so slightly, due to an error in the version of AICC (2012 vs 2023) which was noticed during the reviews. The new ages are as follows: 6.6 ± 0.1 ka, 13.1 ± 0.3 ka, 37.7 ± 0.5 ka, 64.1 ± 2.5 ka, 73.1 ± 1.2 ka, 90.8 ± 1.4 ka, 117.0 ± 1.3 ka, 122.8 ± 1.0 ka, and 128.4 ± 3.6 ka. We also updated the labels on all the figures. The isochrone ages over DML remain unchanged.

- *Fig 2 is very busy. And only 2 of the 16 are relevant for modelling section. I suggest splitting it into two figures (2 and 3) one for each region. For each of the regions, make the 90ka subfigure significantly larger than the rest and label the ice cores (similar to figure 1). The coverage of the regions has slightly changed from figure 1, so please include an inset showing the coverage.*

Thank you for this suggestion. We agree that the figure was very busy and hard to read in its previous format. We have taken onboard the suggestion from Reviewer 2 to increase the size of the 91-ka isochrone coverage for both WSB and DML (see below for a copy of the updated figures). We have also split the original figure (Figure 2 a-p) into two figures and placed each figure in its appropriate section (WSB figure in Section 3.1.1, DML figure in Section 3.1.2). With regards to adding a new inset to the figures, we believe that this is not necessary given the fact these insets are already provided in Figure 1, and the coverage is not different, just the axes are more zoomed in than in Figure 1. We believe that the changes made have significantly improved the figure, so we thank the reviewer for their suggestion.

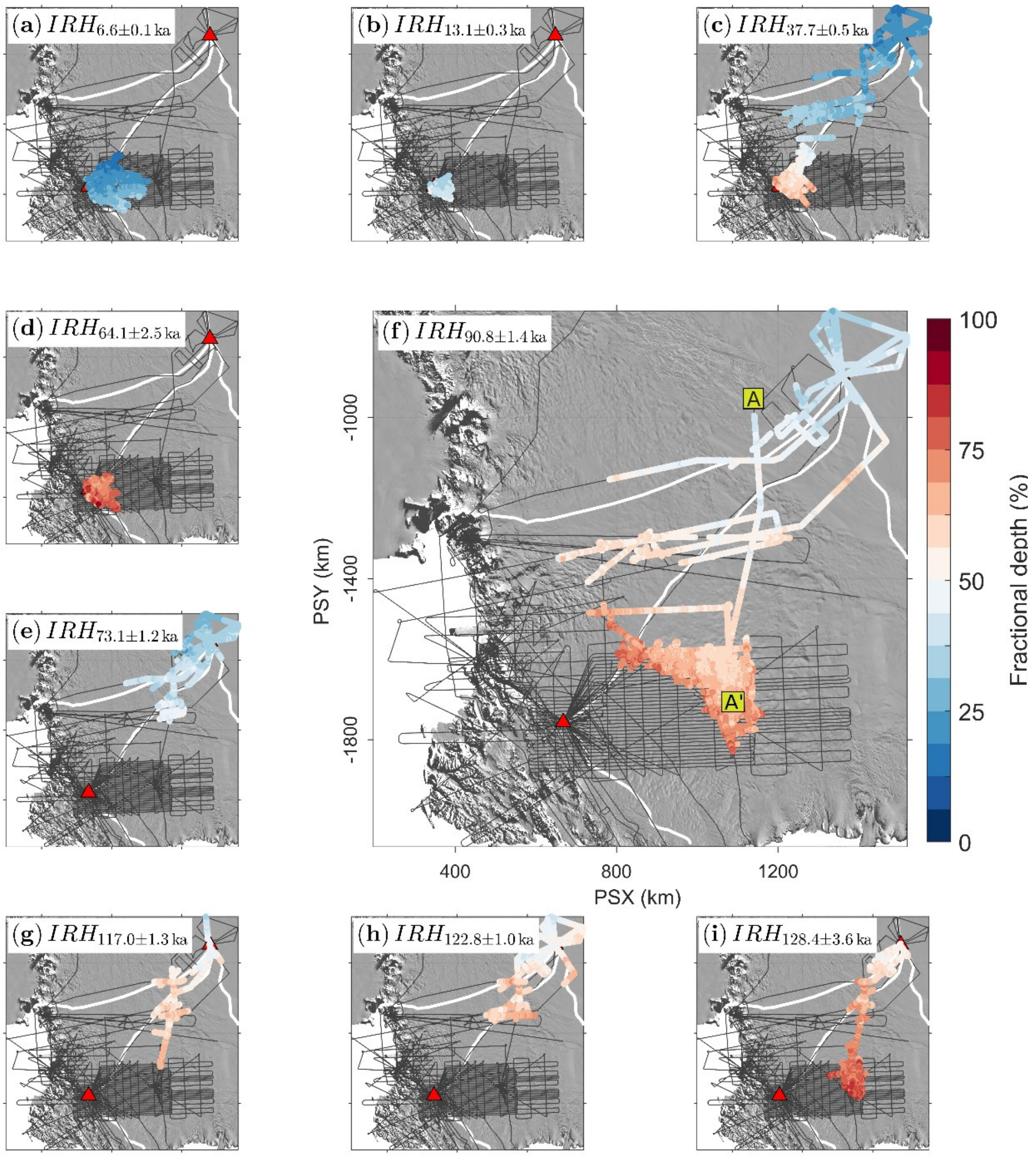

Fig 1. Updated Figure 2 (a-i) in the text showing the coverage of the WSB isochrones only, with the 91-ka isochrone shown more prominently. Further information is provided in the figure caption in the main text.

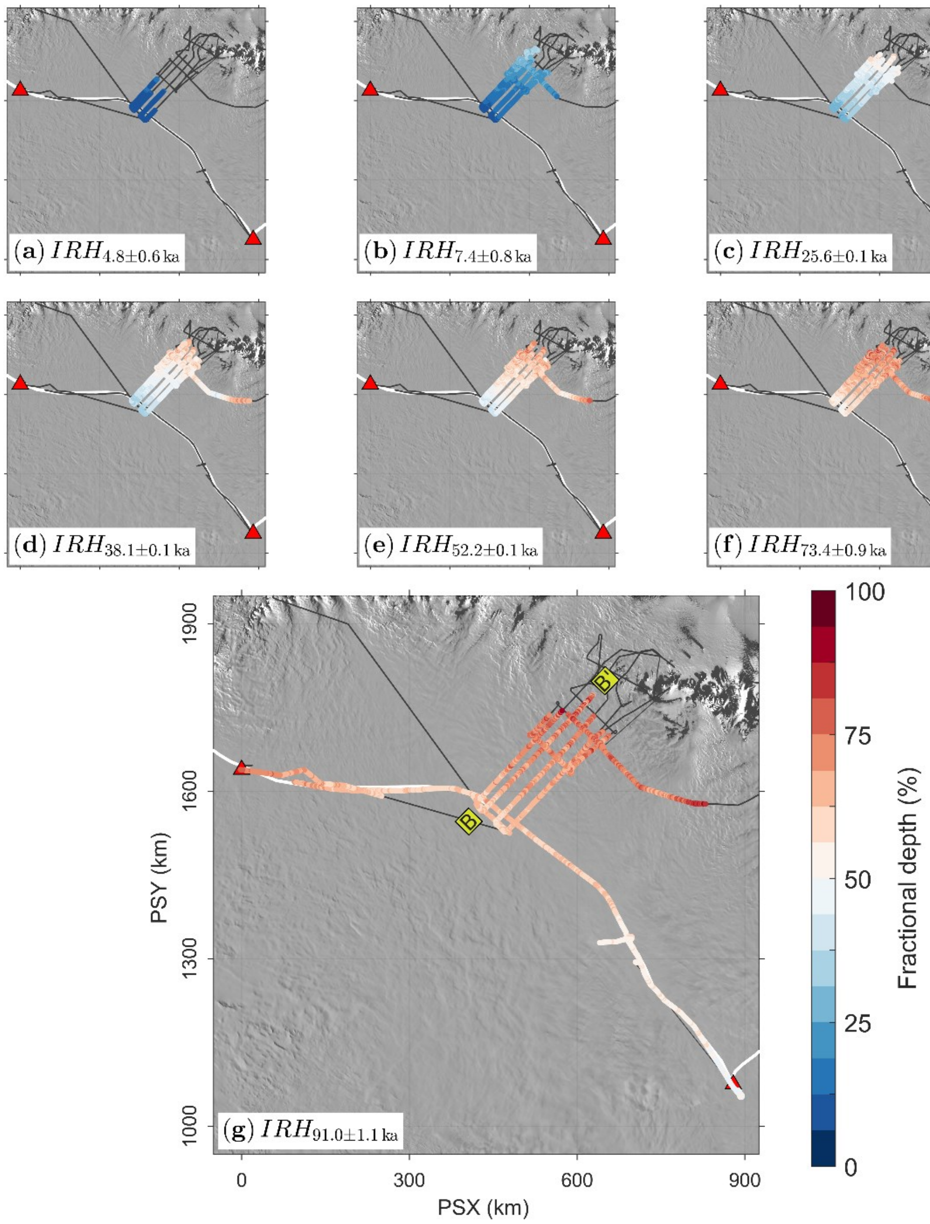

Fig 2. Updated Figure 2 (now Figure 3a-g) in the text showing the coverage of the DML isochrones only, with the 91-ka isochrone shown more prominently. Further information is provided in the figure caption in the main text.

- *Fig1: The caption clarifies that "..throughout the paper, grid-west, grid-east, and grid-south are defined relative to the map grid orientation" but it is still difficult to follow the text. For e.g. in L268: "central grid-west to grid-east portion", where is the centre of the WSB grid? Is it the centre of the map as the caption says the directions are relative to the map? Please consider updating the text and/or the figure, to make this read better. Maybe putting in the Lat Lon coordinates in parenthesis in the text will help guide the reader.*

  Thank you for this suggestion. We have added a blue star in Figure 1 which depicts the centre of the "WSB grid" mentioned throughout the paper, and modified the caption of the figure to make the references to "grid east, west and south" clearer, as follows:

"The blue star in (a) represents the centre of the main radar grid over WSB (coordinates: 71.65◦S, 148.65◦E), which is referred throughout the text as the "WSB grid". From this centre point, grid-west, grid-east, and grid-south are relative to the map grid orientation (i.e., right, left, down respectively from this blue star)."

We now refer to this blue star in the text as well. We believe that this will help guide the reader for what is to come in the main text. Thank you for this suggestion.

- *In section 2.1.2, the paper emphasizes the importance of survey design for ice-sheet modeling, noting that DML was specifically designed for modeling applications while WSB was acquired primarily for ice thickness mapping. However, it doesn't discuss if the newly acquired DML data work better for model constraints than WSB, given the differences. If one looks at the RMSE values, it seems that DML actually performed worse than WSB? Is this all because of the missing mountain in BEDMAP3? I think survey design implications should also be discussed towards the end of the paper.*

  Thank you for this point. Please note we have significantly re-worked the first and second paragraphs of Section 2.1.2 to reflect several comments from Reviewer 2 (this comment, but also the one directly below this one and an additional one in the "minor comments" section). We provide the modified version of these paragraphs in our response to the related "minor comment" from Reviewer 2 below.

  We hope that this significantly improves and clarifies the key difference between WSB and DML; why we need direct along-track radar data for isochrone comparisons; and why we expect the WSB data to still provide better results compared with the DML data, despite suggesting that the DML data should theoretically provide better mismatches in isochrone elevations. We have also taken the opportunity in this paragraph to add a few more sentences about survey designs for ice-sheet modelling applications focusing on isochrones.

  Responding to this comment specifically here, it is true that the RMSEs between modelled and observed isochrones over the DML profile (B-B') are greater than over WSB (in other words, the model performs poorer over DML than WSB). We discussed this in the text (Sections 3.2.1 in the previous version), which is supported by the Appendix Table C1, although admittedly this point could have been made more obvious in the text, which has now been done in Section 2.1.2.

  The reason for the greater mismatches over DML is indeed primarily due to the bed topography which is not well constrained even in the latest iteration of the Bedmap/BedMachine products (see Appendix Figure B). Indeed, this lack of data was one of the main motivations for two field seasons in 2023-24 and 2024-25 in which the DML data were acquired. However, the Bedmap3 gridded data was produced at the same time as our new data were being acquired, thus there was no time to include these into the new iteration of the gridded product (see below). Nevertheless, one can see that the model still produces acceptable offsets between modelled and observed surfaces over DML. On the other hand, one of the main reasons for the model performing better over WSB is that the underlying radar data over this sector were included in the Bedmap3 gridded product which we use to model the catchment and compare with isochrones. This is not the case for DML, as highlighted above. This points to the fact that non-flow aligned data are better than no data at all; the next step being that we would have flow-aligned information (i.e., our new DML lines) integrated into the gridded product which would then be used to run the model.

Certainly, the model would perform better had the new CHARISO bed data been integrated into Bedmap3 and then used to run the model. One could integrate this new bed data into the gridded product, and run the model, but this is no simple task given the tailored processing and interpolation techniques required to re-produce the gridded product. Interestingly the CHARISO radar data and model comparison presented here highlight at times stark offsets behind real world topography (and stratigraphy) and the model world. Typical modelling approaches based on present-day surface elevation offsets would provide a false sense of validity of the modelling results as they would not allow for an assessment of the internal flow and imprint of incomplete boundary conditions.

- *L388: The manuscript uses the newly acquired radar data over DML to identify mismatches between modelled and observed isochrones, attributing these discrepancies to inaccurate Bedmap3 bed interpolation. However, this raises two questions: (1) Since the new radar data also includes bed picks, couldn't the bed elevation mismatch be identified more directly by comparing the new bed data with Bedmap3, rather than inferring it indirectly through IRH-model mismatches? (2) If newly acquired IRH data is being used to constrain the model, shouldn't the bed elevation boundary conditions also be updated with bed data from the same radar survey? Using new IRH data while retaining outdated/interpolated bed elevations creates an internal inconsistency. I think the primary takeaway from this exercise is that we need more radar data. In most cases, wherever we have such deep IRHs we will also have accompanying bed elevation data.*

  Thank you for this comment. We did not intend to suggest in the paper that the mismatch between the modelled and observed isochrones is solely due to the inaccuracy in the Bedmap3 bed interpolation over the whole profile, and indeed, generally, isochrones do follow the bed profile relatively well.

  The overarching modelling goal is to improve the transient paleo-evolution of the ice sheet which can be achieved by fitting multiple isochronal horizons. This allows for tailored tuning of, for example, SMB, sliding, or internal deformation, as these different processes affect the elevation of isochrones to different degrees. The goal is not to identify bed elevation mismatches. In fact, areas with strong differences in bed elevation (such as the tail end of the DML transect) cannot be used to calibrate the model as one would overfit model parameters to compensate for the bedrock mismatches. On both accounts we agree with the reviewer: there is an urgent need for more radar data, and we can nicely employ the existing radar data which already are included in the most recent bed compilations. However, for most of these data, isochrones have not been traced and dated. This manuscript partly fills this gap by providing isochrones for WSB and DML.

  We agree with the second point the reviewer raises, which they referred to as an "*internal inconsistency"* regarding the fact the newly acquired DML data is not embedded into the Bedmap3 gridded product which we use to run the model. However, one of the key points of our paper and this analysis is to show that isochrones are essential when differentiating between sets of simulations that provide similar offsets in surface elevations while differing in their internal structure. We show here that without these radar data (of which we need much more, as suggested by the reviewer here), and their by product (i.e., isochrones), we would not be able to differentiate which simulation provides both the best surface elevation offsets but also the most realistic transient paleo-evolution. We believe that both the WSB and DML experiments shown in Figure 4 illustrate this point.

The re-working of the first and second paragraph at the start of Section 2.1.2, modified as a result of a comment from Reviewer 2 in the "minor comments section below" hopefully addresses this point further and clarifies early on the expectations of a better match over WSB than DML due to our newly acquired radar data not yet being part of the Bedmap3 product used to run the model.

Minor comments:

- *Title: The title is super generic. It also misses out on mentioning the data release.*

  Thank you for this comment. We agree that the previous title was perhaps too generic. We have amended it as follows: "Radar isochrones as constraints on paleo–ice-sheet model simulations in two off-divide regions of East Antarctica"

- *L4: where englacial layering is more disrupted: add "typically" or similar after "is"*

  Removed, thank you.

- *L9: "imprint of" should be followed by "on" at some later part of the sentence. After reading the manuscript I understand the intention, but it's not very easy to grasp with this sentence. Try rephrasing.*
  Agreed. We replaced "imprint" with "[…] influence […], which can lead to […]"

- *L13: The last sentence of the abstract is unnecessary considering its location. It is describing the nature of the study, which someone who has already read the abstract must have understood. Either move it to an early part of the abstract or remove it or replace it with a more conclusive/forward looking statement.*

  Agreed. We rephrased the last sentence as follows: "This study demonstrates that achieving a good present-day match in ice-sheet geometries in off-divide areas does not necessarily translate to an appropriate transient ice-sheet evolution in the model and thus emphasises the need to incorporate isochrones as boundary conditions in paleo-ice-sheet model simulations."

- *L31: Could possibly also refer to Cavitte et al., 2023*

  Agreed and added.

- *L34: You could also mention that this bias in studies towards the divide is because the ice divides are typically better suited for drilling ice cores. And we need ice cores to use IRH as isochrones. Obvious but worth mentioning.*

Agreed. We added "primarily because of the stable ice-sheet conditions which lead to surface-conformable layering, and the nearby presence of ice cores used for dating these isochrones."

- *L45: "yet to be fully evaluated" So they have been evaluated but not "fully"? Confusing. Please rephrase it.*

  What we meant here is that very few continental ice-sheet models have been used to simulate englacial layers and use these for model calibration. Only PISM in Antarctica (Sutter et al., 2021) and Yelmo in Greenland (Born and Robinson, 2021) have done so at time of writing.

  Perhaps, the words "fully evaluated" were a poor choice of words, as we will never "fully evaluate" models given the complexity and sheer scale of the problem at hand. We have slightly modified the corresponding paragraph as follows:

  "Continental-scale ice-sheet models employed for sea-level projections are usually not calibrated or constrained to match the paleo-evolution of ice streams and entire drainage sectors where ice-dynamical processes and boundary conditions are less well constrained (Sutter et al., 2021). However, this evaluation is critical for strengthening projections of future sea-level change, which are often subject to large uncertainties arising not only from climate scenarios but also to a large part from model initialisation and parametric uncertainties (Golledge et al., 2015; DeConto and Pollard, 2016; Seroussi et al., 2020; Siegert et al., 2020; DeConto et al., 2021; IPCC, 2023; Morlighem et al., 2024)."

- *L47: remove "our study". It seems odd here as nothing has been mentioned about the study at this point (except that it lies at the intersections of…).*

  Agreed. We replaced "in our study" with "here".

- *L54: Suggest starting with "IRHs offer a ...." for a more clear stance.*

  Agreed. Thank you.

- *L58: "can complement" sounds weak. Maybe say "can further constrain" instead.*

  We believe that "complement" is appropriate here, so have not made this suggested change.

- *L60: Better replace this opening with the objectives stated on the L329.*

  Agreed and modified accordingly.

- *L102: surface pick "and elevation data".*

  We are not sure what the reviewer means here, so have left this as is.

- *L107: Here by "obtaining" do you mean "accessing" or "acquiring"?*

  Thank you for spotting this. We should have used the word "acquiring". This has been modified.

- *L108 Replace "been about obtaining information" with something like "aimed towards mapping the ice thickness."*

  Thank you. We have replaced "obtaining" with "mapping".

- *L108 This sentence is way too long (4 lines!). I would suggest rephrasing/splitting.*

  Agreed. We have split this sentence into two. Thank you.

- *L113: What is "ice streaming"?*

  Thank you. We simply meant fast but constrained ice flow. We have removed or rephrased this.

- *L113: The discussion of what models need is confusing. The terms "non-interpolated bed data" and "englacial layering along specific radar transects" suggest that gridded surveys are inadequate for modelling, but this is misleading as gridded surveys can also provide better along/across flow bed topography and englacial surfaces. Maybe first clarify, what exactly are the requirements for this specific type of modelling? Then how does a gridded isochronal/bed elevation data won't meet these expectations? How a more targeted survey as per the model requirements could be better. Please clarify this distinction and explain why flow-oriented surveys may be preferred for ice-sheet model calibration, despite gridded surveys offering better spatial interpolation.*

  Thank you for this comment and for offering us the opportunity to expand on this both here and in the paper. This comment is related to two major comments from Reviewer 2 above, which we have responded already. However, we take the opportunity here to combine our actionable answer to these three comments in one modified section of our text. We have modified the first and second paragraph of Section 2.1.2, as follows:

  "Gridded (i.e., interpolated) bed-products such as Bedmachine or Bedmap (Morlighem et al., 2020; Pritchard et al., 2025) form the backbone of any ice sheet modelling efforts providing the necessary boundary conditions. However, acquiring radar data that is tailored to specific ice-sheet modelling needs (i.e., calibrating models with isochrones) is relatively rare in Antarctica, primarily because the main motivation of radar surveying in Antarctica has historically been about mapping ice thickness and bedrock topography in an efficient manner. This has typically been characterised by surveying an entire catchment on a regular grid-like pattern, thus also facilitating optimized airborne geophysical data acquisition for gravimetry and magnetics (i.e., Fig. 1a). Because the geometry of deeper isochrones is heavily influenced by ice-dynamical processes such as ice flow in off-divide areas (Catania et al., 2005, 2006; Karlsson et al., 2009), models employed away from stable ice divides also need tailored information oriented perpendicular or parallel to the direction of flow to capture the imprint of fast ice flow often preserved in the geometry of isochrones.

Additionally, ice-sheet models benefit from high-resolution bedrock data (ideally non-interpolated), which allow for direct evaluation along radar transects of how well the model reproduces observed features such as the surface, bed, and isochrones.

Where the WSB and DML regions differ is precisely in the motivation of acquisition; the WSB data was acquired to obtain ice-thickness and bedrock topography over large scales, rather than being explicitly guided by surface-flow directions, whereas the DML lines were specifically designed to support modern ice-sheet modelling by capturing drainage pathways from the stable ice-sheet divide to the grounding line. Admittedly, modelling results over the WSB region are expected to show higher accuracy both spatially (across the catchment) but also directly along radar transects when compared to observations, because the WISE-ISODYN bed-elevation data (alongside many more detailed surveys in the area) are already included in the gridded product used to run the model. This means the model is better constrained because the boundary conditions are better known. However, because this information was not provided with a particular consideration to ice streams direction, it is not straight forward to evaluate the model's ability to reproduce complex isochronal geometries directly along and across fast-flowing ice streams using the existing radar transects. The WSB data largely compensate for this by providing a relatively dense radar grid covering a large area in contrast to the few individual flight lines acquired in the vicinity of the new CHARISO lines (Fig. B1). Ideally, dense radar grids accompanied by individual along flow transects would perfectly address current modelling needs. Across DML where data coverage is generally poorer than over WSB (Fig. B1), we expect the surface, bed, and isochronal elevations to show a poorer match since the DML profiles were not included in the latest iteration of the Bedmap3 gridded product which is used to run the model. Nonetheless, this provides an opportunity to assess how well the model deals with drainage sectors in the absence of this underlying tailored radar data, a problem that is often overlooked when modelling at large scales in areas with poor data coverage."

- *L121: Remove "providing…modelling". It is implicit considering the context.*

  Agreed and removed.

- *L126: "?" missing reference?*

  Thank you for catching this. The missing reference was the "Rodriguez-Morales et al., 2013" IEEE paper, which we added to the reference list.

- *L129: replace "further" with "additional"*

  Added. Thank you.

- *L131: remove "both"*

  Done. Thank you.

- *L174: If I understood correctly, the 2% error in the firn correction is representative of the TALDICE ice core site. However with the assumption of spatially invariant firn correction the uncertainty should be higher accounting for the spatial variability in surface density over the*

1000s of kms covered by the IRH. The difference in the firn correction values at the EDC and TALDICE core also suggests this point.

Thank you for this point. The 2% error is applied to the density values at TALDICE due to the lack of published errors in depth-density curve there, but this 2% is not specific to the TALDICE site. Rather, it is a value that has been suggested for EDC and Vostok ice cores (see the text for more details on this). We agree that the firn corrections for TALDICE, EDC, Vostok (and for all firn corrections applied in Antarctica in previous isochrone papers) are likely not representative across large spatial scales, but in the absence of seismic refraction measurements or firn cores across, for example, the divide, which would enable us to quantify the spatial uncertainty better, we do not currently have any means of assessing this across scales efficiently. We do note that the main source of uncertainty when converting to depths in metres is the change in electromagnetic speed in ice, rather than the firn correction uncertainty, so it is likely that any small increase in this 2% error at TALDICE will still fall within the uncertainties from the electromagnetic speed in ice alone, particularly for deeper isochrones like the ones discussed here. Importantly, this uncertainty can easily be re-calculated in the future with better uncertainty estimates, using our raw data to re-calculate depths and depths uncertainty, as we point out in the “Codes and Data Availability” section of the paper. Finally, this uncertainty only applies to those isochrones dated at TALDICE. As shown in Figure 2, these only represent small but dense patches of 3 isochrones around the TALDICE ice core (and thus not as spatially spread out as for the other isochrones that connect EDC with the main WSB radar grid).

- *L238: The manuscript compares the horizontal resolution (8 km) to Sutter et al. (2021) but not the vertical resolution (201 levels). Please clarify: 1) How do the 201 vertical levels compare to Sutter et al. (2021)? 2) Given the reported IRH uncertainties, what model resolution (vertical and horizontal) is appropriate for these constraints? 3) At what resolution would the model become too fine-scale for the IRH data to provide effective calibration? This discussion would help justify the resolution choices and guide future modeling strategies.*

This is a good point, thank you for raising this. Sutter et al. (2021) used 81 vertical levels, so 2.5x less than used here. This was added in the text. However, we note that 81 vertical levels is already a very high number, with some paleo simulations only computing 10-20 layers (e.g., Born and Ronbinson, 2021) and up to 100 levels (ISMIP6, Seroussi et al., 2020). Whilst more vertical levels are beneficial, they come at a heavy computational cost (see below), without drastically improving the simulation results. It would be an interesting study to assess the point at which increasing vertical levels to obtain higher resolution would no longer by beneficial from a computational power point-of-view; however this is beyond the scope of our paper.

Regarding horizontal model resolutions and the model’s ability to reproduce isochrone elevations, the key issue is the computational power required to run such high-resolution simulations. Currently, we can run simulations as fine as 4 kms over such large areas using PISM, but much higher resolution is difficult to achieve at catchment levels. An important point which we made in Lines 190-194 is that models will likely not be able to reproduce a match in isochrone elevations that falls within the dating uncertainty of observed isochrones (i.e., several hundreds of years) due to internal uncertainties (i.e. parametrisation, uncertainties in our current understanding of real-world ice-sheet processes, uncertainties associated with boundary conditions, etc.) in current generations of ice-sheet models. These typically amount to several hundreds or thousands of years, and thus we treat isochrones as ground truth. Models can never be too fine scale, but the computational power required to run sub-km simulations is simply not realistic at present.

- *L251: Rephrase/remove: "To showcase the usefulness". The use of "demonstrate the usefulness" in L228 is still quite fresh.*

  We have removed this from the sentence.

- *L263: Hard to follow on the map. See comment for Fig 1 above.*

  Thank you. We believe that this is now solved with improvements to the Figure and its caption, as per the major comment from the reviewer above.

- *L268: rephrase "points to important lessons learned".*

  We have replaced this with the following: "and points to the need for future surveys to provide enough crossovers in the radar data to ensure isochrones can be adequately traced between the divide and the fast-flowing sectors of the ice sheet."

- *L272: Maybe expand a bit on how close the match is.*

  We specified previously in the text that this match is "[..] when accounting for age uncertainties from this study". We believe that this is now even clearer from the statistics provided in the new Table (Table 2), which now contains the age and age uncertainties for all isochrones traced and dated in this paper, as requested by Reviewer 1 and 2. We have therefore made no further change to this sentence.

- *"an important finding" - I also agree, and this important result could be highlighted elsewhere (conclusions) as well. This will also help balance the conclusions section.*

  Thank you for this suggestion. We have added the following sentence in the Conclusion: "Of the 16 isochrones traced here, 13 correspond — within uncertainties — to previously traced and dated isochrones across both East and West Antarctica. Notably, despite the two sectors being at opposite ends of East Antarctica and separated by ~2,000 km, three common isochrones (∼38, ∼73, and ∼91 ka) are identified across both regions, with the ∼91-ka isochrone exhibiting the greatest spatial extent over both WSB and DML."

- *L312: What does the ice core data used to date this couplet tell us about this suspected volcanic eruption?*

  Over the WAIS, this couplet was found to correspond to strong peaks in acidity concentrations in the WAIS Divide Ice Core by Bodart et al. (2021). We have added this point. Thank you.

- *L329: This point should be made clear to the reader early on. I suggest moving these objectives to the introduction.*

This has now been done and can be found at the start of the last paragraph in the introduction. Thank you for this suggestion.

- *L342: Split the sentence as it's too long. First state the two objectives: (1)use of isochrones to constrain the model in off-divide regions (2) to differentiate between climate and ice-dynamical processes. Then explain how.*

  This has been done. Thank you.

- *L362: What is an "ice-dynamical region"? Better say, "two ice-dynamically different regions".*

  We meant the ice divide vs the fast-flowing ice streams. We have replaced this wording with "two ice-dynamically diverse regions".

- *L367: "will be" -> "are"*

  Done.

- *L369: insert "calibration" after "This".*

  Done.

- *L376: "considerably" -> "considerable"*

  Well spotted. Thank you.

- *L378: How do we know its "over tuning" and not "under tuning"? This manuscript says there is a lack of paleo constraints, and we should use isochrones to further constrain/tune the models.*

  The reviewer probably refers to the fact that every modelling study insufficiently explores the parameter space (i.e., the term "under-tuning" referred to in their comment). By "over-tuning" we refer to the (unfortunate) fact that by adjusting the basal friction (amongst others) to fit the present-day ice thickness or surface velocities, we "over-tune" the model as uncertainties in bedrock topography and geothermal heat flux (amongst others) mean that this parameter is tuned to yield the right surface conditions despite having the wrong boundary conditions. This is what we refer to when we mention "over-tuning" in the text. Another example is tuning isochrone geometry by adjusting surface mass balance, which would likely produce better match in isochrone elevations (particularly near the surface) but likely poorer results further down. In the presence of large bedrock uncertainties or, for example, for deep isochrones which are more responsive to basal conditions and fast ice-flow, over-tuning the model would mean modifying SMB to overcompensate for unknowns in the boundary conditions. Ultimately, there are many examples of over-tuning, and we suggest that employing isochrones at least partly reduces this problem.

- *L380: insert "only" before "relying"*

Inserting “only” here would imply that we have an alternative, but for regional or continental-scale ice-sheet modelling in 3D, we do not. The point that we are trying to make here is that whilst they are essential tools for ice-sheet modelling, and are for the most part very representative, they can be treated often as a ground-truth which may not always be accurate, as we show in our study.

- *The interchangeable use of “profile” and “line” is confusing throughout the manuscript.*

The term “profile” in this section refers to profiles A-A’ and B-B’, which are described in the Figure caption. In other sections of the paper, “profile” might refer to “age-depth” or “density”. We believe that this is a common use of the term “profile” to describe those cases but agree that this was maybe used mistakenly at times. We also think that “Line” (either flight line or radar line in the text) should only refer to the airborne radar flightlines, across WSB or DML. We have gone through the paper made sure the right words were used appropriately.
