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Interactive
comment

Interactive comment on "Simulating the internal structure of the Antarctic Ice Sheet – towards a spatio-temporal calibration for ice-sheet modelling" *by* Johannes Sutter et al.

Anonymous Referee #1

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The manuscript by Sutter et al. (2020) shows how the ice sheet internal layer structure can be exploited to understand and diagnose ice sheet model output. The authors present a clear case for the utility of comparing isochrones derived from observations and ice sheet model simulations to determine ice sheet model performance, particularly highlighting where we need:

1. Better constraints on boundary conditions (e.g. bed topography; geothermal heat flux);

2. Better constraints on climate forcings (e.g. spatial variation in paleo accumulation rates);

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Discussion paper

3. Better constraints on ice sheet model parameters (e.g. basal drag over marine sectors of the ice sheet);

4. Long term simulations to adequately represent 3D flow fields and ice sheet geometries.

The diagnostic method presented in this manuscript (i.e. use of particle tracer method) is freely available and can be readily applied to any ice sheet model output, making this diagnostic tool accessible for ice sheet modellers.

The manuscript addresses a highly relevant scientific question, especially with the work of the AntArchitecture project. To the best of my knowledge the concept is novel, and the scope of the model simulations and comparison with observations is appropriate to support the interpretations and conclusions, and to demonstrate broad applicability of the method to the ice sheet modelling community. Overall, this is a worthwhile study that is certainly within the scope of TC.

I have a few main comments that should be addressed before publication, mostly related to finessing the structure and flow of the manuscript for improved readability, and on how generalizable the results are for the whole ice sheet. I also have included a list of specific comments that should be addressed.

Main comments

1. The title

   The title should be tightened to be more specific. E.g. "Simulating the internal structure" could be qualified, especially given that the isochrones were derived from simulated velocity and geometry fields, rather than calculated online in the ice sheet model. Suggest something like: "Investigating the internal structure of the Antarctic Ice Sheet: the utility of isochrones for spatio-temporal ice sheet model calibration" OR "On the use of isochrones as a novel diagnostic for ice sheet model performance"

2. Structure and readability of sections 3-5:

I found the structure and flow of sections 3-5 difficult to follow, and because of this the findings of the study – particularly with respect to clearly identifying the sources of mismatches between observed and simulated isochrone depths – and their significance were weakened. For example, L195-206 introduces the main goal and three main potential sources of model-obs mismatches. While the sections that follow contain discussion of each of these sources, it is not always clear which source is being considered (e.g. with subsection headers), and there is some repetition (particularly sections 3.2 and 4). It'd be good to see a restructuring through sections 3 and 4, with distinct subsections to systematically address each source of mismatch, first with respect to Dome C (section 3), then with respect to the broader EAIS (section 4).

In each section, the aim should be named early in the paragraph so that it's clear why certain experiments are being assessed, and the significance of the results. E.g. for L253-276, evaluating the model parameterisation, the aim of that paragraph is stated on L262-263, but should be moved to the start. This would help in interpreting the results.

In section 4 on P16 L341-376, the presentation of the results/physical conditions is mixed up with the discussion of the processes that could contribute to obs-model mismatches and how they contrast regionally. It'd be good to first clearly discuss the Dome C and Dome Fuji results. E.g. "for DML-VIII23, the bedrock elevation is above sea level and relatively flat. For this transect, the obs-model mismatch..." Then in a new paragraph, contrast the obs-model mismatch between each of the transects (DML-VIII23, CEA-10, DML-IV24) to highlight the sources/processes that could contribute to the mismatch. It'll then be easier to come away with a clear picture of what is causing what on the larger scale.

It'd be helpful for readability of the conclusions section to restructure into 4 paragraphs:

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- Summarise how well the paleo model outputs match the main observed features of the internal ice sheet structure and capture broad-scale SMB patterns.
- Summarise how the method helps to understand the processes under-lying/sources of mismatches between observed and simulated isochrone depths, with specific reference to the four areas identified in comment 1. above: (a) boundary conditions; (b) forcing time series; (c) initial conditions/parameters; and (d) simulation time.
- Summarise where more efforts are needed: (a) constraints on the spatial variability in paleo accumulation rates; (b) constraints on basal drag in marine sectors.
- Discuss the model intercomparison

3. Reporting of basal drag and methodology:

   One of the main findings is that the basal drag can play a large role in the mismatch between observed and simulated isochrones. I.e., the large mismatch in some regions is due to an overestimation of vertical velocities where there is low basal friction. However, the actual basal drag is not reported in this paper for the simulations. This makes it very difficult to ascertain whether the aims of the study that relate to basal drag have been adequately addressed, and the degree to which the discussion around the accuracy and constraints on the basal drag applies. The basal drag coefficient should be reported for each of the simulations as a separate figure in the main body of the manuscript. It'd also be good to see the drag coefficient as a separate panel on figure 9. I recommend adding supplementary material that describes how the basal drag was calculated for each of the simulations.

   It'd also be worth reiterating some of the salient features of the model setup/methodology, particularly those aspects that are relied upon for interpretation of the results. For example, how was the model tuned at Dome C? This is

key in understanding point (i): the impact of the paleoclimate climate forcing on the model-obs mismatch (see specific point below).

4. Generalisation of results to the large-scale:

   I'm slightly concerned that the abstract (and then the conclusions) oversells the results by use of some general terms e.g. "Antarctic Ice Sheet", "the interior", and "subglacial basins". The results for the isochrones that are analysed certainly support the interpretations; however, in reality, these isochrones only cover very small parts of the East Antarctic Ice Sheet. How representative are the results for these particular observational isochrones for the large-scale ice sheet? Please clarify this, and be more specific in the abstract and conclusion to reflect this. Please also consider discussing the generalisability of the results in more detail in section 4.

5. Grammar and spelling:

   I have noted some grammatical and spelling points below, but not all of them – others should be able to be corrected fairly easily with a standard LaTeX spelling checker or online e.g. grammarly.com. Some of the sentences are also long and a little bit wordy – it would be worth shortening the longer sentences for improved readability.

Specific comments

P1 L6. "We simulate observed isochrone elevations within the AIS via passive Lagrangian tracers" >> "We calculate isochrone elevations from simulated AIS geometries and velocities via passive Lagrangian tracers".

P2 L31. Check citation style. Also on P6 L152-153, P12 L283, P18 L367.

P2 L45. What does "in scope of" mean here? "Relevant to"?

Table 1. I couldn't see the analysis of a number of these isochrones (e.g. CEA-7,8,12,13). Were these reported within the figures and text?

P6 L134. Sentences starting from "We use" to a new paragraph.

P6 L136-139. The sentence starting with "Based on the analysis" was a bit confusing. I took it to mean the following: (a) that the observed isochrone data used in this study (derived from Winter et al. 2017) are assumed to have a maximum age uncertainty of 1 ka; (b) that the observed isochrone data are all above or at 2000 m depth; (c) that the age uncertainty nonlinearly increases with depth; (d) that the age uncertainty of the observed isochrone data is always lower than the uncertainty in the simulated data. Is this what is meant? This sentence could be reworded for improved clarity.

P6 L138. "2/3 or" $>>$ "2/3 of"

P6 L154. "relatively coarse resolution" $>>$ "relatively coarse resolution model grid"? Does the mesh size evolve over time with grounding line migration or is it static? Some more details on the model experiments would be helpful here.

P8 L175. How is "accurate enough" determined? How do we know that the misfits between the radar isochrones and the simulated isochrones using this method and the ISM are not sensitive to the temporal and spatial resolution of the ISM output and the Lagrangian particle tracking algorithm? It would be good to see a sensitivity analysis or uncertainty quantification here.

P8 L184 "We also tested other seeding strategies..." What was the outcome of these tests (i.e. was there any sensitivity to seeding mask)? A section quantifying the uncertainty in the tracer method would be appropriate in a supplementary material document.

P8 L201. "the model ensemble was tuned". What does this mean? Please provide specific details of what fields were tuned and to what data.

P8 L201-202. "To assess the impact of model" $>>$ "To assess the impact of (ii) model"

Figure 3. For this figure and for figures 4, 6, 8, and 9, please make the figure bigger (e.g. textwidth) and increase the font size. The blue lines in panel C are difficult to see - perhaps use black instead - and it'd be great to label the transects in panels B and C (see also comment on figure 4). In the caption, I wasn't sure what this sentence meant: "Due to the small mismatch... are discernible." Does this mean that the background colour is relatively uniform? Consider modifying the colour bar to zoom to the relevant colour range represented in the figure. Also, "strong" $>>$ "large" in the second last sentence of the caption.

P9 L214. "To evaluate the validity of the forcing approach in Sutter et al. (2019).." Earlier, it is mentioned that the model ensemble is tuned to match the regional configuration near Dome C. For what paleoclimate forcing was this tuning carried out? Please comment on whether/how this tuning might impact the capacity to assess the validity of the forcing approach.

Figure 4. The transect lines and labels are difficult to see on panel A - please make the lines thicker and more contrasting with the background. It's difficult to determine which line is which on panel B (the brown/purple/red colours are similar - perhaps choose more contrasting colours for the bed elevation). Please also make the lines thicker and include a legend on panel B?

P10 L219. "DC-57a" is not marked in bold in figure 4A (that's DC-X45a). Which transect is referred to here? Suggest labelling all transects more clearly on the figures.

P10 L221. Why use 3, 5, 6, and 8

P10 L225. "completely reproduces". What does this mean? The red line in panel B is sometimes outside of the 5-6

P10 L227. "at least for the interior of the East Antarctic Ice Sheet". The results show that is valid for Dome C (and given that the model is tuned for this region). It's not clear that this conclusion can be extended to the interior of the EAIS in general. Surely this

depends on the degree to which the climate forcing is appropriate in other regions? (which is indeed addressed in section 4).

P10 L230-231. This result might be related to the fact that the method employed in Martos et al. (2017) is not physically realistic. For a description and an updated GHF product for all of Antarctica, see: Stål, T, et al. "Antarctic geothermal heat flow model: Aq1." Geochemistry, Geophysics, Geosystems: e2020GC009428, https://doi.org/10.1029/2020GC009428.

P11 L242-252. I found this paragraph difficult to follow. First, the discussion of the 0.5 cm/a difference between observations (Stenni et al., 2016) and the models simulations + RACMO could be clearer. Is there a reason the simulations match RACMO but not obs? Second, for clarity please reference panel B in the text (e.g. "When we compare (figure 5B) our simplified...") and describe the bias correction that is used. Consider restructuring the paragraph for clarity, and add labels for the different curves in figure 5A.

P11 L251. "along ice divides" $>>$ "along the ice divide near EPICA Dome C"

Figure 5. What are the semi transparent solid lines in panel A? Bias corrected values from the simulation? Please describe, along with the dashed lines, in the figure 5 caption.

P12 L253. "where" » "were"

P12 L256. Here 5

P12 L262-276. Should "pal", "pd-pal", and "pd-pd" be in italics here and through this paragraph?

P12 L267-276. This is a really neat and interesting result. It's also interesting because presumably unknown parameters (e.g. the basal friction coefficient) are also somewhat uncertain in the initialisation of the paleo simulation, but the pal run yields isochrone depths that are within a few

P12 L278-279. "are small" $>>$ "are generally small". E.g. the difference between obs and sim isochrones for 38 ka along DC-Y77 between ~15-25 km is much larger than for any other isochrone in this portion of the transect.

P12 L286. "antarctic" $>>$ "Antarctic". Here and elsewhere.

P13 L293. "60 and 90 ka" $>>$ "90 and 60 ka". How much uncertainty does the shorter spin-up time for 90 and 60 ka introduce?

P13 L297-298. A bit of a jumbled sentence. Suggest: "Areas with sparse radar observations may have bed elevation estimates that differ from high-res radar data by several hundred metres."

P13 L304. "differences in the observed and modelled bedrock relief" $>>$ "differences in the observed and modelled bedrock relief (e.g. see section XX)"

FIgure 7. Final line: "The coloured bars on the bottom of" $>>$ "The coloured bars at the bottom"

P14 L310. "focus of an upcoming paleo-ice-sheet model intercomparison". Great idea.

Figure 8. In the second last line of the caption, "normalised with" $>>$ "normalised by"

P15 L322. "The northern half of the transect bottom part in Figure 9". It's not hugely clear which segment of the transect this refers to - perhaps add demarcation on panels A and B of figure 9.

P16 L334. "The basal friction in the model is a function of bedrock elevation". Please provide the equation and description of the basal friction calculation, perhaps in supplementary material.

P16 L347. "isorchrone" $>>$ "isochrone"

P16 L349. "We limit ourselves to..." New paragraph.

P16 L356-358. Move "which encompasses...Wilkes Subglacial Basin" to earlier where

CEA-10 results are first introduced. Reword remaining part of sentence: "The comparison between DML-VIII23 and CEA-10 potentially highlights a methodological deficiency (leading to unrealistic internal flow and basal sliding) as the isochrone mismatch in CEA-10 cannot be remedied by a surface mass balance correction."

P16 L365. "Dome C". Should this be Dome F or Dome C here and also on page 18? I'm not sure I understand this argument if it's Dome C.

Figure 9. Consider a vertical line on panel B indicating the Dome C location so that we can compare GHF here to the northern portion of the transect. Consider also demarcating (e.g. with a coloured vertical line) the "northern portion/bottom half" of transect in panel B, and perhaps with two "x" the same region on panel A. For panel C, here and in figure 8, I suggest using a different colour map for the basal melt rate - the blue-white-red transition is usually used for differences (where white is 0). In the second line of the caption: "beige" looks more like dark red to me.

P18 L379-380. It would be better to reword as follows: "We are able to reconstruct most large-scale englacial layer features of the observed isochrones..."

P18 L394. "A model intercomparison". New paragraph

Figure 10. Should "DMLVIII 22" on panel A be "DMLVIII 23"? It's hard to see the dashed lines on panel B – please make the lines thicker.

P20 L405. "This would facilitate" $>>$ "This would facilitate the evaluation of"

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-349, 2020.