Impact of boundary conditions on the modeled thermal regime of the Antarctic ice sheet
– Response to Review #2 –

In-Woo PARK et al.

September 24, 2023

We would like thank the anonymous reviewer for their insightful and valuable comments, which have helped us to improve our manuscript. We address the remarks below point by point. To clarify the reviewer's comment and its reply, we have used red for the reviewer comment and black for the replying comment (RC).

1 Reviewer #2 (Anonymous Reviewer)

Summary:

The manuscript presents simulations of Antarctica to investigate the sensitivity of the modeled thermal state to the boundary conditions and inversion method using the three-dimensional thermomechanical ice sheet model, ISSM. They focus on the influences caused by differences in existing geothermal heat flux maps and the effect of differences in the ice vertical velocity. Both GHF and vertical velocity are poorly constrained in models but are known to affect the thermal state. The authors provide a new set of model simulations with different combinations of GHF maps and vertical velocity parameterizations to generate 3D temperature fields. By comparing their modeled temperature fields to existing borehole temperature profiles, the authors conclude that vertical velocity has a greater influence on the thermal state than GHF. This new contribution is very compelling, since it implies that vertical velocity is critical to constrain in ice sheet models in order to accurately model the thermal state. However, the authors miss the opportunity for some additional analysis and discussion which will further strengthen their findings and narrative.

Major Issues

The main findings are ice-sheet scale conclusions about the effect of boundary conditions and model initialization on the thermal state, while the boreholes that are analyzed to make these conclusions are largely from the Siple coast, which has a unique thermal configuration (Englehardt 2004, Bougamont et al., 2015, Ng and Conway 2004, etc). The stagnation of Siple Coast ice streams (i.e. Kamb $>$ 150 years ago) exhibits an interesting thermal regime today that must contain memory of the past slow down. However, this would not be the case for most other parts of Antarctica. I think the paper should be strengthened in two ways.

- The authors should add more discussion on the Siple Coast model/observation discrepancies since it is very interesting. I think there is a missed opportunity to elaborate on the convex vs. concave temperature profiles for models vs. observations for the KIS and ER profiles (Fig. 2). I wonder if the difference in shape is an indication that the model's lack of the long-term thermal state memory really matters for this region. I know this shortcoming is hinted at in the last paragraph of the discussion, but I think the authors miss the chance to add interesting discussion about what the discrepancies in model vs observed temperature profiles are telling us about the thermal regime. I'm not expecting new results, but I would like to see some speculation about the effects of boundary conditions vs. initialization approach (inversion with present day conditions, paleo spin-up, thermal steady state approximation, etc) in the discussion.

RC: We appreciate your invaluable comment. Your suggested references, particularly the Kamb Ice Stream (KIS or UpC reference), have been immensely helpful in enhancing our understanding of the ice dynamics processes. Previous studies (Bougamont et al., 2015) successfully replicated the concave shape observed in KIS and ER borehole temperatures, opposite to the typical shape of borehole temperature, by adopting a plastic basal boundary condition for higher order model. A plastic boundary condition is given by

$$\tau_b = -a\ \exp(-be)\frac{\mathbf{u}_b}{\|\mathbf{u}\|}, \tag{1}$$

where a and b are two positive empirical constants, e is void ratio, and $\mathbf{u}_b = (u, v)$ is basal ice velocity, $\|\mathbf{u}\| = \sqrt{u^2 + v^2 + \gamma^2}$, and regularization term with $0 < \gamma \ll (u, v)$. We believe that plastic till deformation approach is a crucial in addressing the issues observed in KIS and ER boreholes.

Consequently, we have incorporated this discussion into the manuscript as follows:

Line 286-294: The deceleration of tributaries at KIS and ER is attributed to water-piracy hypothesis (Alley et al., 1994) or removal of basal water contributing to the loss of lubrication (Tulaczyk et al., 2000; Bougamont et al., 2003). In model experiments, Bougamont et al. (2015) revealed changes in the tributaries at KIS and ER using a plastic till deformation friction law including simple subglacial hydrology model. In contrast, we employ the Budd type friction law and assume the effective pressure fully connected to ocean part, not including changes in the effective pressure. The variation in effective pressures also changed the basal ice velocity in Budd type friction law. In addition, a selection of other types of friction law, including Weertman (Weertman, 1974), Schoof (Schoof, 2005), and Coulomb (Tsai et al., 2015) types, also influences the initialization and future fate of ice (Brondex et al., 2017, 2019). Further investigation is required, such as the application of other types of friction laws or initialization with paleo spin-up, to better understand temperature profiles.

- If the focus of the paper is on the broad scale effects of GHF vs vertical velocity on thermal regime, then there are more borehole temperature profiles, which should be added to the analysis. There are more from East Antarctica such as Dome C, Lake Vostok, Talos Dome, South Pole. There are also more borehole temperature profiles in other parts of West Antarctica such as Kohnen and Byrd (maybe some others I am missing?).

RC: We appreciate the insightful comment from the reviewer and have made additional efforts to address these suggestions. In response to your comment, we have used polynomial functions from Talalay et al. (2020), which provide polynomial functions describing the relationship between temperature and depth from the surface (Table 1). As we have already obtained and utilized data from Dome Fuji and WAIS Divide, we have focused on utilizing borehole temperatures from Byrd, Vostok, Dome C, and Kohnen from Talalay et al. (2020).

Table 1: The polynomial approximation depicting the relationship between borehole temperature $T(^\circ\mathrm{C})$ and vertical depth z (m). The polynomial function is listed on Table 3 in Talalay et al. (2020)

Drill sites	Polynomial
Byrd	$T = -28.343 + 0.8367 \times 10^{-3}z - 6.7651 \times 10^{-6}z^2 + 6.1339 \times 10^{-9}z^3$
Vostok	$T = -56.034 + 2.9889 \times 10^{-3}z + 3.888 \times 10^{-6}z^2 + 0.2419 \times 10^{-9}z^3$
Dome C	$T = -54.316 + 5.2978 \times 10^{-3}z + 4.4141 \times 10^{-6}z^2 - 0.368 \times 10^{-9}z^3$
Kohnen	$T = -44.428 + 1.7384 \times 10^{-3}z + 4.4124 \times 10^{-6}z^2 + 0.184 \times 10^{-9}z^3$

Figure 1 displays modeled and observed borehole temperatures at Byrd, Vostok, Dome C, and Kohnen. Note that we do not adjust the surface temperature to match the observed temperature, which is why the surface temperature from ERA-Interim displays a large offset from top of borehole temperature. For Byrd and Vostok, considering the drilling borehole depth, the observed temperature derived from polynomial approximation displays notably high temperatures below 1700 m and 3000 m. Consequently, we truncate observed temperature below a specific depth (Figure 1). Despite the surface temperature mismatch, the modeled temperature at Vostok, Dome C, and Kohnen somehow captures the linear shape of observed borehole temperature (Figure 1).

We made efforts to collect additional borehole logging profiles, including RABID project, Whillans, WACSWAIN project described below.

1. RABID project
   - Hot water drilling at Rutford Ice Stream, West Antarctica (Smith, 2005)
   - Measure borehole temperature using thermistor string, which of length is about 300 m (Smith, 2005, p. 20)
2. Whillans Ice Stream Subglacial Access Research Drilling (WISSARD) project
   - How water drilling at Subglacial Lake Whillans.
   - Measure borehole temperature distributed temperature sensing (DTS) (Fisher et al., 2015).
3. WACSWAIN project (Mulvaney et al., 2021)
   - Drilling borehole at Skytrain Ice Rise (see Figure 4 at Mulvaney et al., 2021)

However, we were unable to obtain actual vertical temperature profile data. There might be other drilling projects that we are unaware of.

While the validation of our research would significantly benefit from the inclusion of additional borehole temperature profiles, unfortunately, we were unable to obtain any additional borehole temperature profiles. Actually we invested a significant amount of time in collecting borehole temperature profiles; however, accessing borehole temperatures is often challenging.

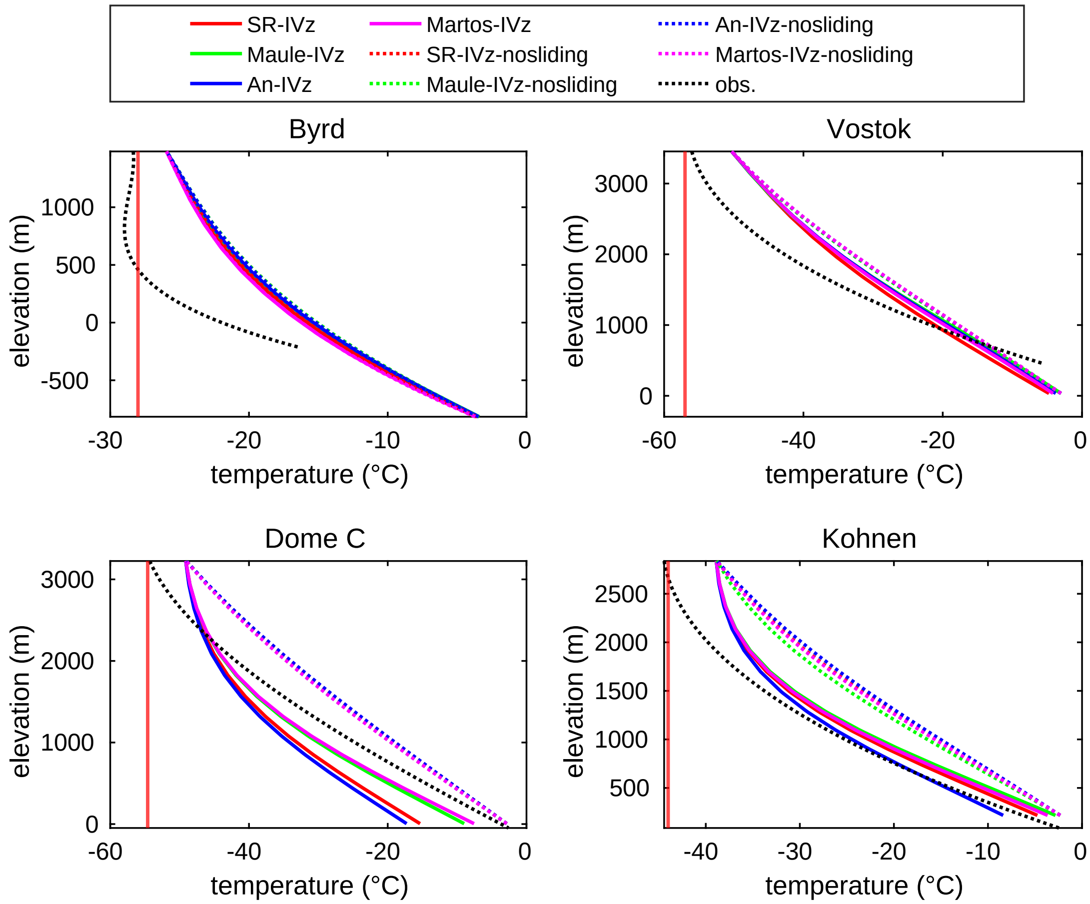

Figure 1: Modeled and observed borehole temperatures at Byrd, Vostok, Dome C, and Kohnen. The observed borehole temperatures are derived from the polynomial approximation from (Talalay et al., 2020)(also see Table 1). A red vertical line indicates the observed surface temperature. For Byrd and Vostok, temperature data is limited to depth of 1700 m and 3000 m because unusual temperature is calculated below these depths with polynomial approximation.

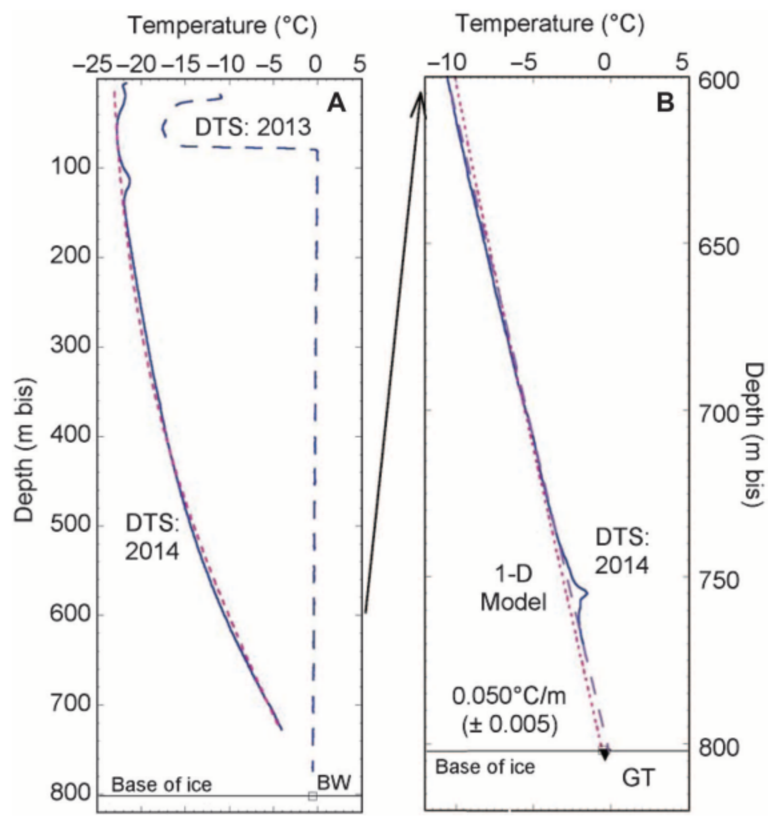

Figure 2: Measuring temperature using distributed temperature sensing (DTS). This graphic is from Figure 3 at Fisher et al. (2015).

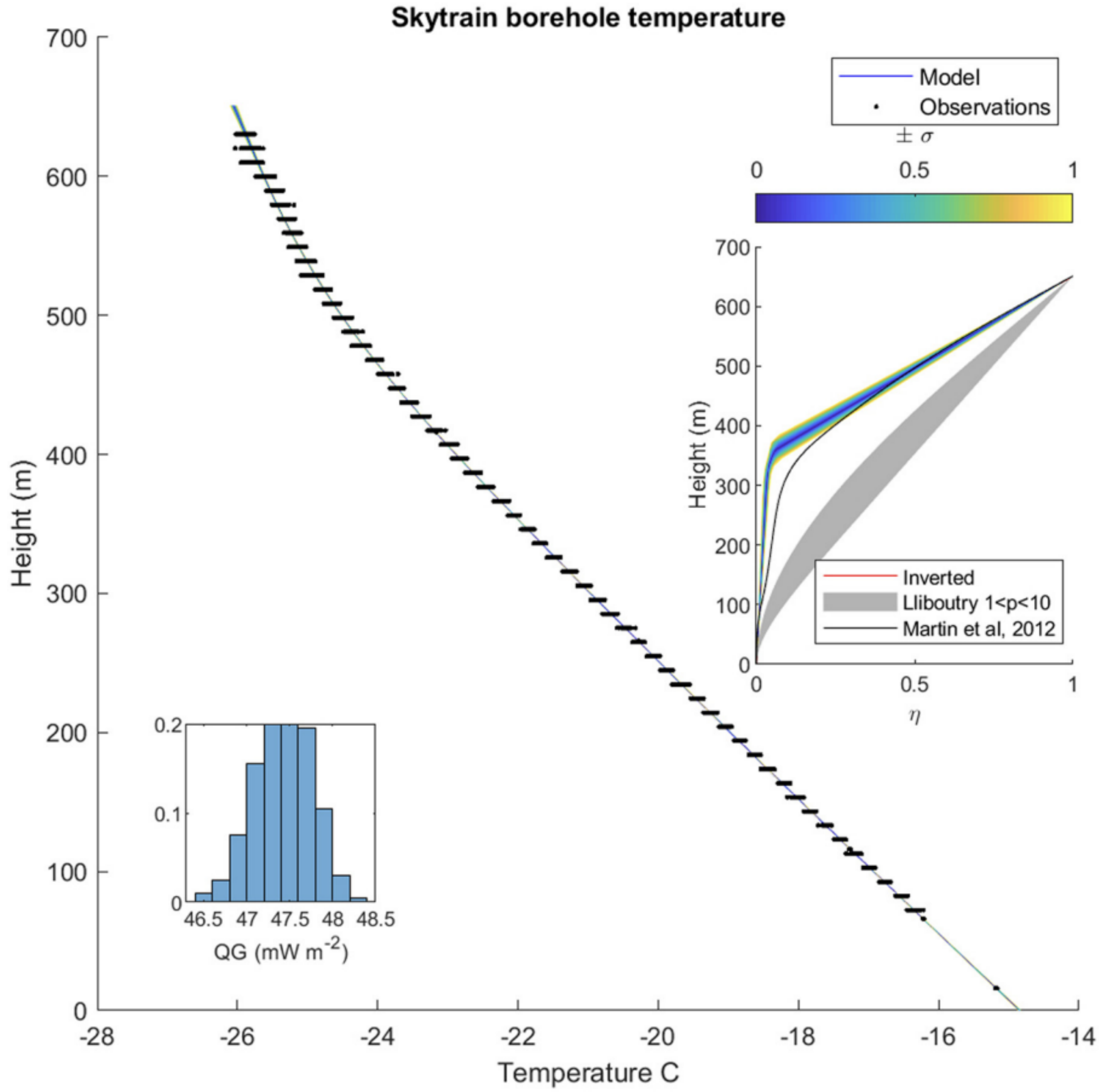

Figure 3: Observed and modeled borehole temperature profiles at Skytrain Ice Rise (Figure 12 from Mulvaney et al., 2021).

Regarding the naming of ”fast flow” and ”slow flow” regions, somewhere early on in the manuscript, it should say that it isn’t possible to drill into most really fast flowing regions because deformation etc. prevent drilling. In the paper I would say “fast flow” only defines a unique subset of ice streams (WIS and BIS) where the flow regime supports drilling, so there are borehole temperature profiles for only those regions. Because of this, the “fast flow” conclusions may not apply for other parts of Antarctica. I might even recommend renaming “fast flow” to “Siple coast fast flow” or something like that throughout the manuscript to clarify this point.

RC: Thank you for your comment. As suggested by the reviewer, we have modified notation of ”fast flow” to ”Siple coast fast flow” in revised manuscript.

Modified phrases:

> Line 163-165: Note that AIS/WIS-1991-1, AIS/WIS-1995-4,7, and BIS-1998-4,5 ~~located in fast flow regions ,~~ are located in regions with comparatively high ice velocity compared to other boreholes and have concave temperature profiles. To clearly define this specific fast flow region, we refer to AIS/WIS and BIS as Siple coast fast flow region.

There are known difference in 2m air temperature amongst reanalysis products such as ERA-interim, ERA5, RACMO, MERRA, MAR. Why do the authors choose ERA-Interim? The author’s test the effect of the basal boundary condition by changing GHF maps, while the surface boundary condition is never tested. It would be helpful to see additional simulations using different 2m air temperature products to see its effect on the vertical temperature profiles, even if this effect is less significant.

RC: We appreciate you pointing out the aspect we overlooked. We have conducted additional analysis to address your question.

Figure 4 displays the climatological mean 2-m air temperature of ERA-Interim (Dee et al., 2011) (hereafter ERAI), ERA5 (Hersbach et al., 2023), RACMO2.3p2 forced with ERA5 (hereafter RACMO23p2-ERA5) (van Wessem et al., 2023), and MERRA2 (Global Modeling and Assimilation Office (GMAO), 2015). The 2-m air temperature data from ERAI shows less variability than the others, and its smoothed nature might compromise accuracy (Figure 4). While ERA5, RACMO23p2-ERA5, and MERRA2 are relatively recent datasets, they exhibit some differences in the climatological mean 2-m air temperatures when compared to the observed surface temperatures at each borehole (Figure 5). Consequently, it is apparent that surface ice temperature corrections are also necessary for ERA5, MERRA2, RACMO23p2-ERA5. Moreover, through a simple exponential decay correction considering differences between the reanlayses and observations (Figure 4), it’s evident that the corrected ERAI and ERA5 data results closely align with the observed values, surpassing their initial counterparts. Therefore, we have selected ERA5 from these reanalyses and conducted additional experiments following the same experimental design.

The additional experiments conducted with 2-m air temperature from ERA5 display no significant differences compared to experiments with ERAI (Figure 6). However, in case of SD, RR, and AIS/WIS (only applicable in the case of nosliding), there are slight discrepancies in surface temperature leading to shifts in the modeled temperature profiles when using ERA5. In fact, ERA5 corrections at these specific locations have improved results, making them more similar to obser-vaiton.

An important conclusion drawn from this series of additional experiments is that surface ice temperature, or the accuracy of its correction, significantly influences the simulated ice temperature profiles within the model, and this information has been incorporated into the discussion section.

> **Discussion**
> Line 350: The surface temperature of ice would be one of factors to consider the boundary condition of thermal model. While ERA5 (Hersbach et al., 2023), RACMO2.3p2 forced with ERA5 (van Wessem et al., 2023), and MERRA2 (Global Modeling and Assimilation Office (GMAO), 2015) are the recent reanalysis datasets, they display some discrepancies between the climatological mean 2-m air temperature (1980-2018) and observed surface temperature at each borehole (Figure S6). For the comparison with different version of ECMWF (European Centre for Medium-Range Weather Forecasts) reanalysis data, we perform experiments using the same manner, utilizing 2-m air temperature from ERA5. These results display no significant differences compared to experiments using ERA-Interim (Figure S7). However, in case of SD, RR, and AIS/WIS (only for the IVz-nosliding case), they display slight discrepancies in surface temperature leading to shifts in the modeled temperature profiles. In fact, the improvement in surface temperature and the accurate correction would bring the modeled temperatures into closer agreement with observations.

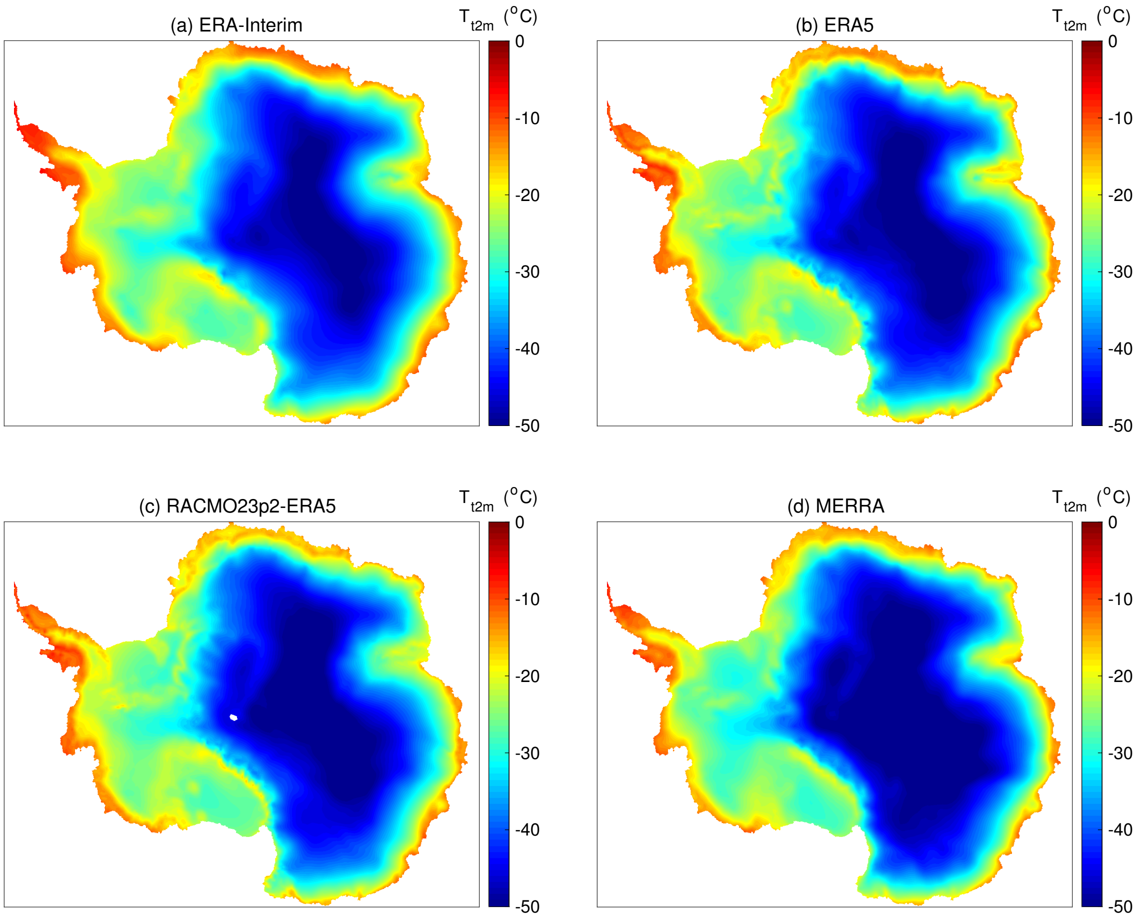

Figure 4: Climatological mean 2-m air temperature for (a) ERA-Interim, (b) ERA5, (c) RACMO23p2-ERA5, and (d) MERRA2 during 1979-2018.

Table 2: Velocity misfit between modeled and observed surface ice velocity and total grounded ice melting volume for IVz and IVz-nosliding forced with ERA5.

GHF	Velocity misfit (m yr^{-1})		Total grounded ice melting volume (Gt yr^{-1})	
	IVz	IVz-nosliding	IVz	IVz-noslding
Shapiro	13.01	18.96	21.29	24.18
Fox	13.48	19.28	25.22	28.54
An	12.95	19.93	16.87	19.39
Martos	13.01	18.01	25.32	28.18

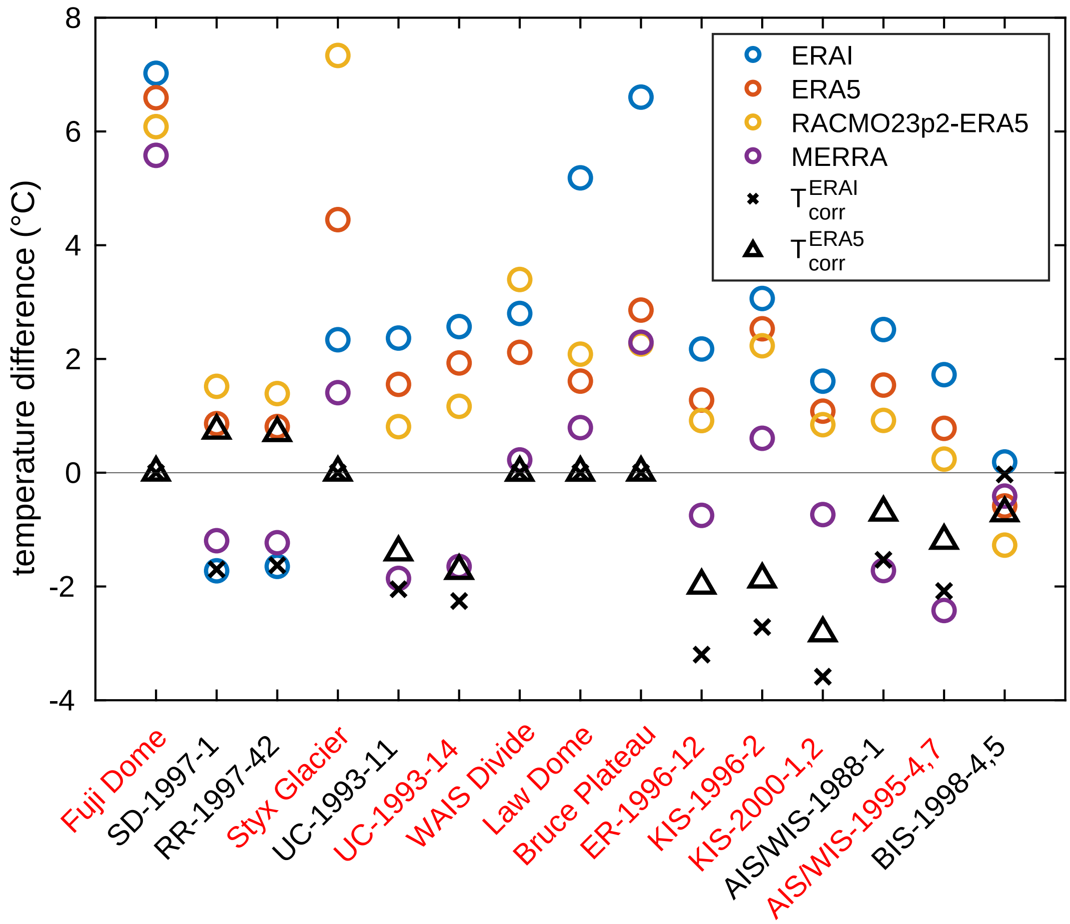

Figure 5: Difference between the climatological mean 2-m air temperature and observed surface temperature at each borehole. The climatological means for ERAI, ERA5, RACMO23p2-ERA5, and MERRA2 are from the period 1979-2018. Borehole names highlighted in red indicate where surface ice temperature is corrected using exponential decay. T_{corr} indicates the difference between the corrected and observed temperatures for ERAI and ERA5.

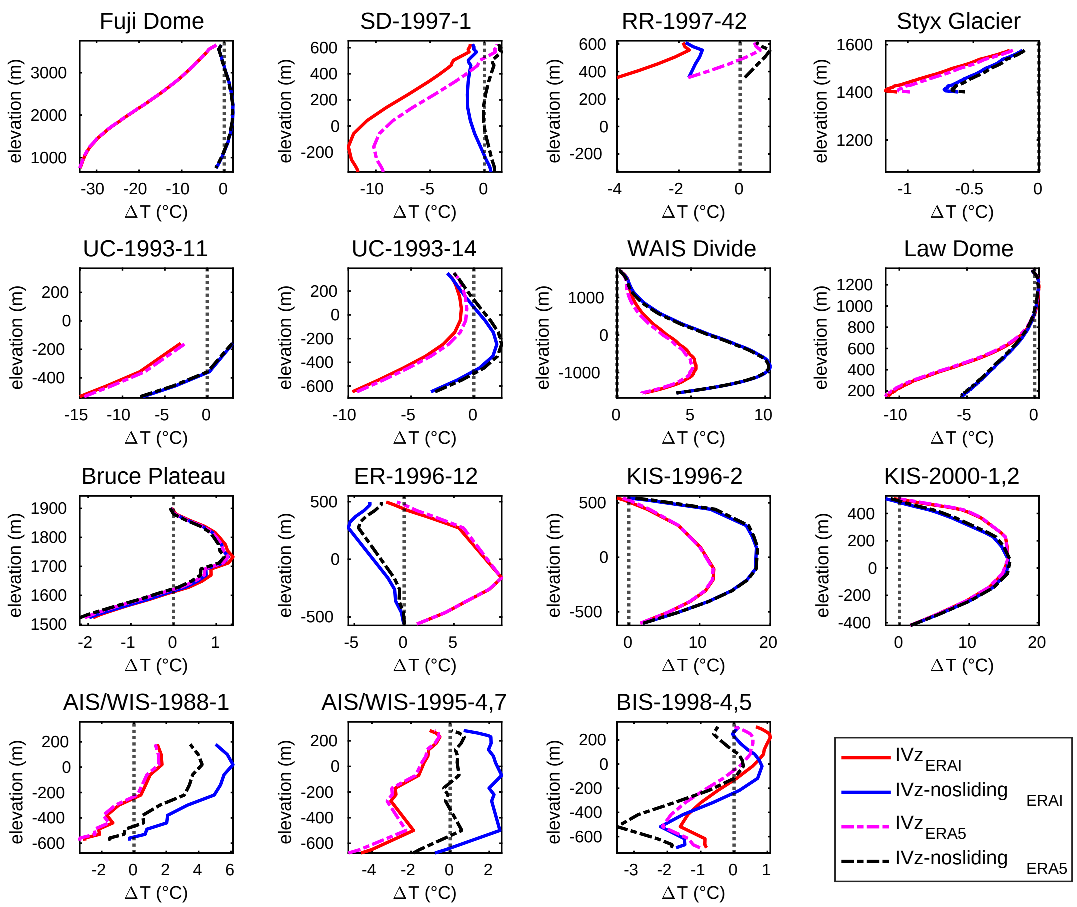

Figure 6: Differences between observed and the mean modeled vertical temperature profiles depend on each experiment group. A black vertical dashed line indicates where the misfit is zero.

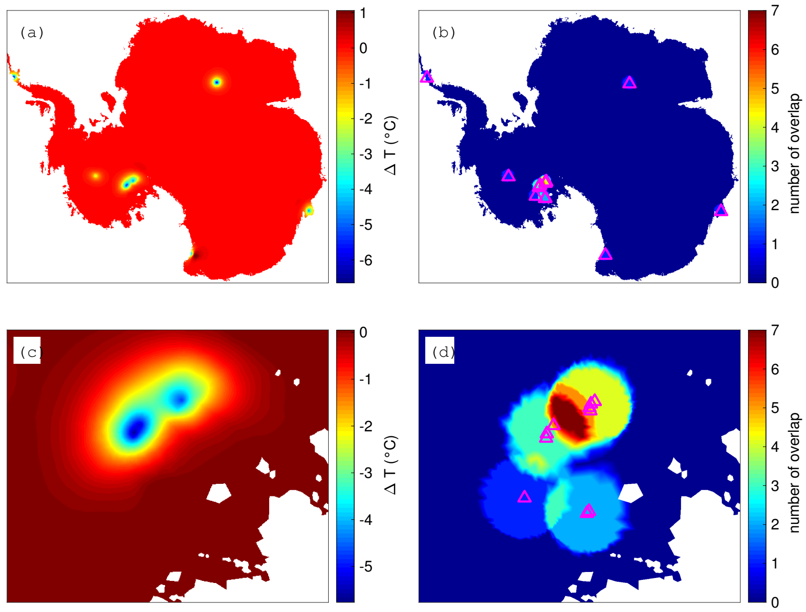

Figure 7: (a,c) Temperature difference between corrected and original. (b,d) Number of overlapping region where a interference radius is set to 10 km. Magenta triangle indicates borehole locations used in this study. Region at (c,d) is zoomed into the WAIS discharging to Ross Ice Shelf, where most boreholes are located.

Also on the topic of surface temperature, in Fig. 2, it looks like the surface temperatures between the ISSM simulations and observation are not a great match for some boreholes (e.g. KIS, WIS, ER, Bruce, UC, ER, SD). I see in the text it says that the model surface temperatures are adjusted using an exponential decay function to better match the observations so I would like to know why there is this miss match. Would a different 2m air temperature map provide a better match to observations needing less correction (see comment above)?

RC: The correction for surface temperature is effective at Fuji Dome, Styx Glacier, WAIS Divide, and Law Dome (Figure 5). As the boreholes at UC, ER, SD, and AIS are in close proximity (Figure 7c,d), each corrected temperature can influence the others. In the exponential decay correction, a 10 km radius results in approximately 80% differences ($\exp(-10/50) \approx 0.81$), and impacts nearby boreholes. Therefore, instead of applying the correction to all boreholes, we have selected specific ones (highlighted in red in Figure 5).

How was 10m/yr threshold for surface velocity chosen for the IVz-nosliding experiment? Are the results sensitive to nudging this threshold? I am not necessarily asking for more model simulations here, but I would like to better understand the choice and its likely effect on the results.

RC: We would like to clarify that we based our experiments on an ice velocity threshold of approximately 10 m/yr, as most boreholes in the slow flow regions observed ice velocities below this value.

In response to the reviewer's comments, we have conducted additional experiments in the IVz-nosliding group, where we set the nosliding slow flow region velocities to 15, 18, and 20 m/yr, respectively. To maintain clarity in naming each experiment, we followed a consistent format. For instance, we have denoted IVz-nosliding with velocities less than 15 m/yr as IVz-nosliding15, and similarly for the other experiments.

Figure 8 displays discrepancies between the mean modeled and observed temperatures. In comparison to IVz-nosliding10, the additional experiments generally exhibit similar trends in most regions, albeit not replicating its values accurately (Figure 10). This can be attributed to the fact that the absence of sliding in specific regions affects the stress equilibrium of the ice and changes in ice velocity also affects the thermal regime. The new experiments slightly improve results in Law Dome, ER, and KIS regions, prompting a need for deeper investigation into the causes. Nonetheless, the more significant finding is that the analysis of additional experiments indicates that there is little change in the total grounded ice volume as the nosliding slow flow regions expand within IVz-nosliding (Figure 10).

These additional experiment results confirm that changing the criteria for nosliding slow flow regions does not significantly impact the key findings. In addition, these experiments reveals that higher nosliding slow flow region boundary results in higher misfit in initialized surface ice velocity. Therefore, using the existing results with a threshold of 10 m/yr to minimize misfit is a valid approach and does not pose significant issues, especially given the minimal differences observed.

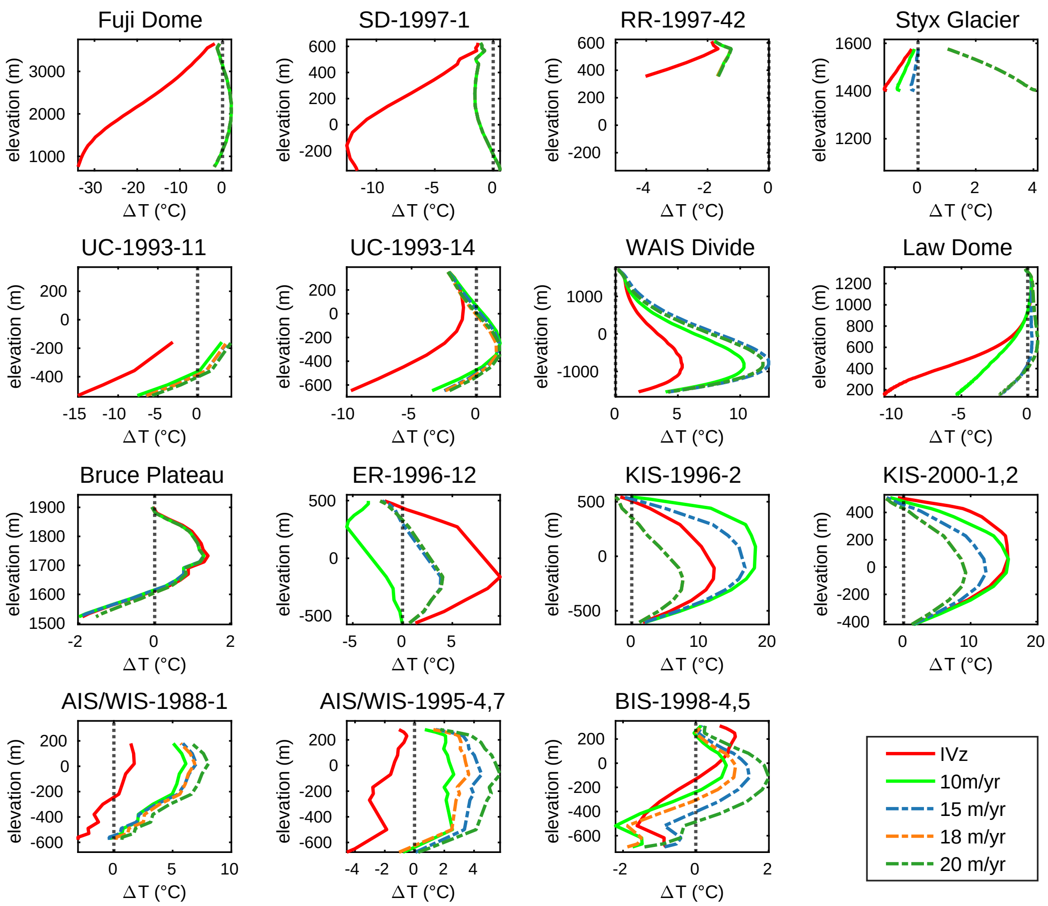

Figure 8: Differences between observed and the mean modeled vertical temperature profiles depend on velocity boundary for IVz-nosliding. Solid and dash line indicate original and additional experiments, respectively. Vertical dash line indicates zero misfit line.

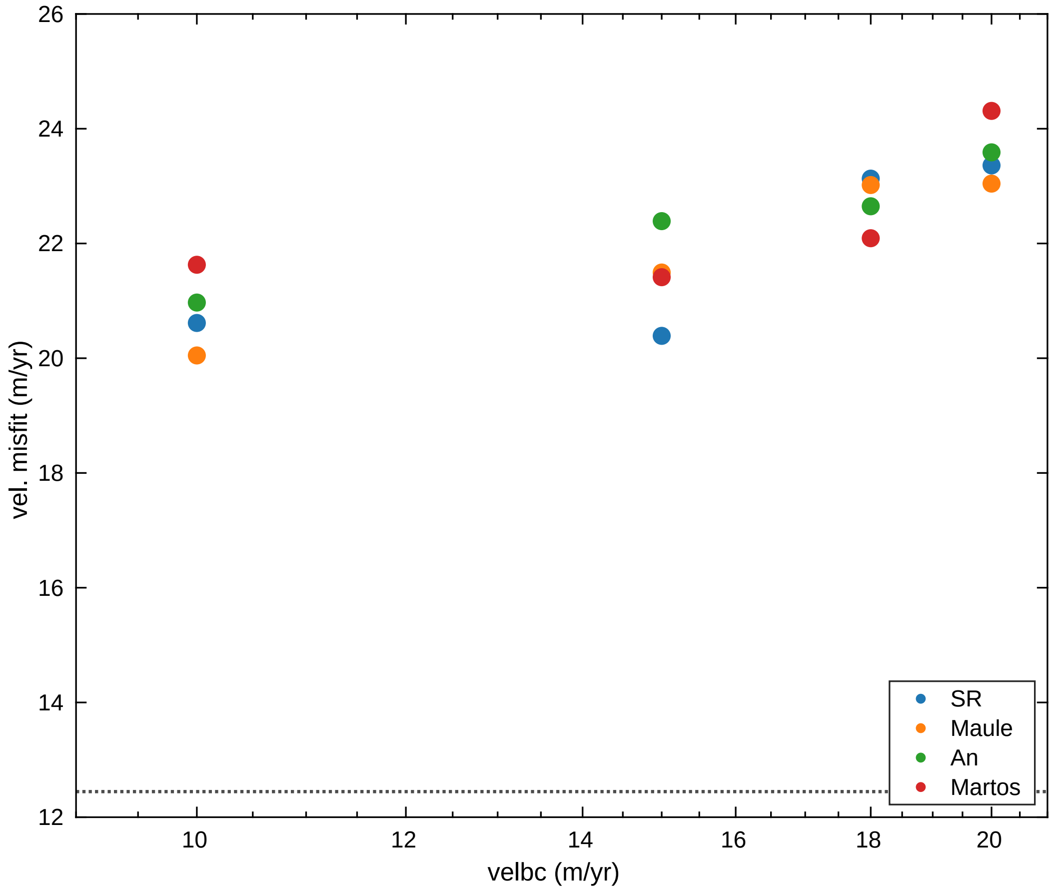

Figure 9: Velocity misfit for (a) whole domain and (b) region where ice velocity is over 50 m yr^{-1}. Horizontal dot line indicates mean velocity misfit of IVz group. Triangle marker indicates original IVz-nosliding experiment.

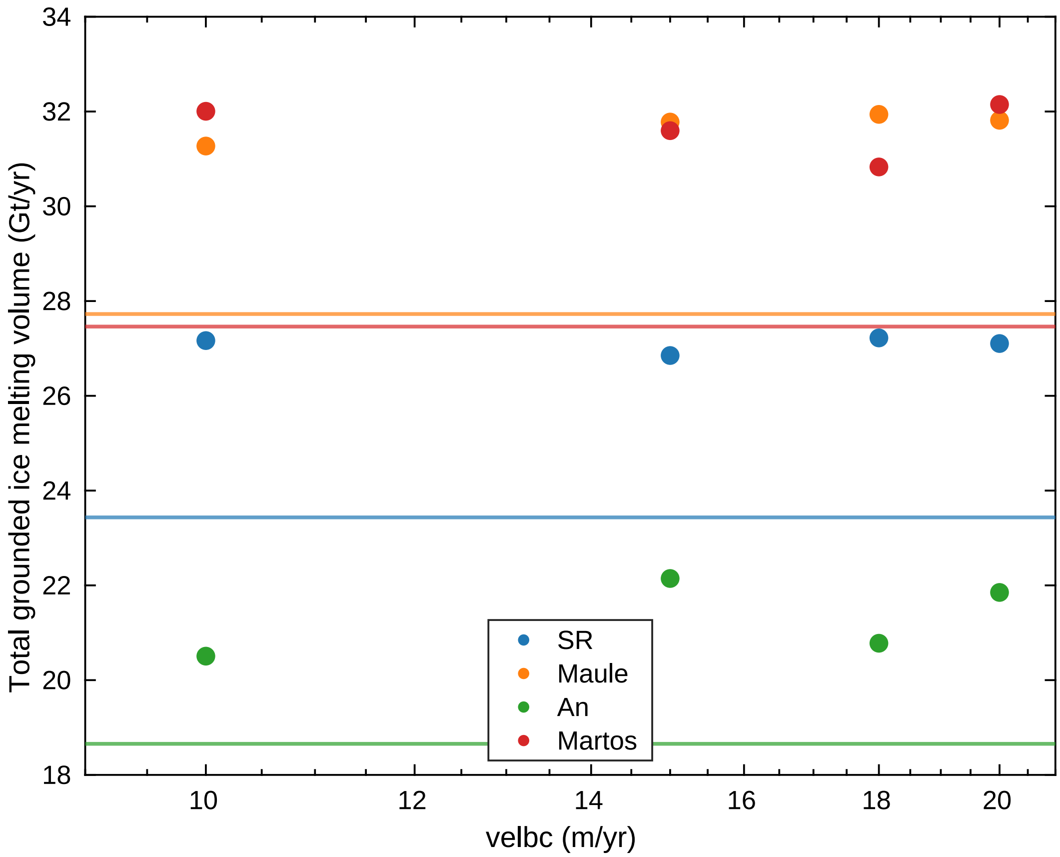

Figure 10: Total grounded ice melting volume depending on IVz-nosliding experiment with different slow flow region boundary. Horizontal line indicates IVz experiment.

Minor Issues

In the abstract, fast flowing has a velocity threshold definition but not slow flowing. This should also be provided. Is it slower than 50 m/yr or something else? The reasoning behind these choices should also be explained somewhere near the beginning of the manuscript. For example, Dawson et al., 2022 uses 100 m/yr to define fast flowing regions. Are these thresholds a result of the velocities seen at the boreholes and some natural separation in the velocities/profiles?

RC: Thank you for pointing out the aspect we overlooked. To define the fast vs. slow flow region, we relied on the critera outlined in Seroussi et al. (2011). According to Seroussi et al. (2011), the regions with ice velocity > 50 m yr^{-1} displays that a ratio between depth-averaged ice velocity and surface ice velocity ($=\|\boldsymbol{u}\|/\|\boldsymbol{u}_s\|$) is approximately 99 % for Higher-Order (HO) model. Therefore, we designated regions with ice velocity more than 50 m/yr as fast flow region, where sliding dominates over internal deformation. In abstract, in accordance with your suggestion, we have added the definition that we consider areas with ice velocities below 50m/yr as slow flow regions.

It would be useful to see observed surface velocities at the boreholes reported in Table 1 so that the reader could see what borehole sites are within the model prescribed no sliding regions (as well as the fast and slow flow groupings). It's hard to get this information from Fig. 1 right now... perhaps if a 10m/yr contour was drawn on the map then that could also work.

RC: Revised as reviewer's comment. We have added white dot contour line indicating where ice velocity is 10 m/yr (Figure 11, see also Figure 1 in revised manuscript).

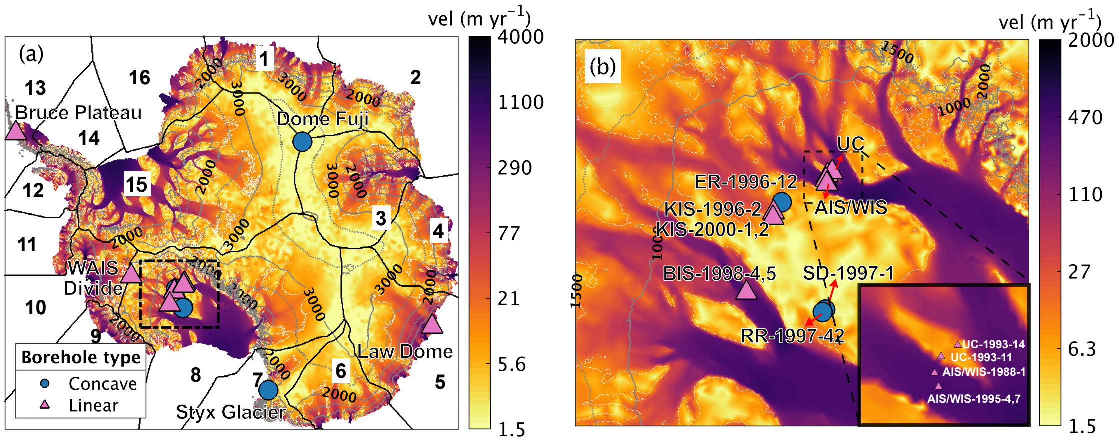

Figure 11: (a) Borehole locations with temperature measurements overlaid over ice velocity (Rignot, 2017). The black dashed box shows the location of (b). The black solid box in (a) indicates each basin from Jourdain et al. (2020), and each number indicates each basin number. We use different symbols for each borehole based on the shape of their temperature profile (triangle and cross red dots indicate concave and linear profiles, respectively). The gray contours indicate surface elevations, with dash lines for every 500 m and solid lines for every 1000 m. The white dot contours indicate regions where ice velocity is 10 m yr^{-1}. (b) Enlargement of borehole locations at West Antarctica overlain over the ice velocity. The borehole names are abbreviated: WIS, Whillans Ice Stream; BIS, Bindschadler Ice Stream; ER, Engelhardt Ridge; KIS, Kamb Ice Stream; RR, Raymond Ridge; UC, Unicorn; AIS, Alley Ice Stream; SD, Siple Dome.

The organization of the subplots in Fig. 2 is somewhat confusing to me. I think the first row are the “linear” borehole profiles and then the rest are the more “concave” profiles. It also took me a while to see that the bottom row of profiles are the ones from the fast flowing regions. I think it would be helpful to see the profiles boxed into a slow flowing group (where IVZ-nosliding fits the observations better) and a fast flow group (where IVZ fits better), like the subtle separation in Table 1.

RC: Revised as reviewer’s comment. To make it more reader-friendly, we have added blue and red boxes indicating slow and Siple coast fast flow regions, respectively.

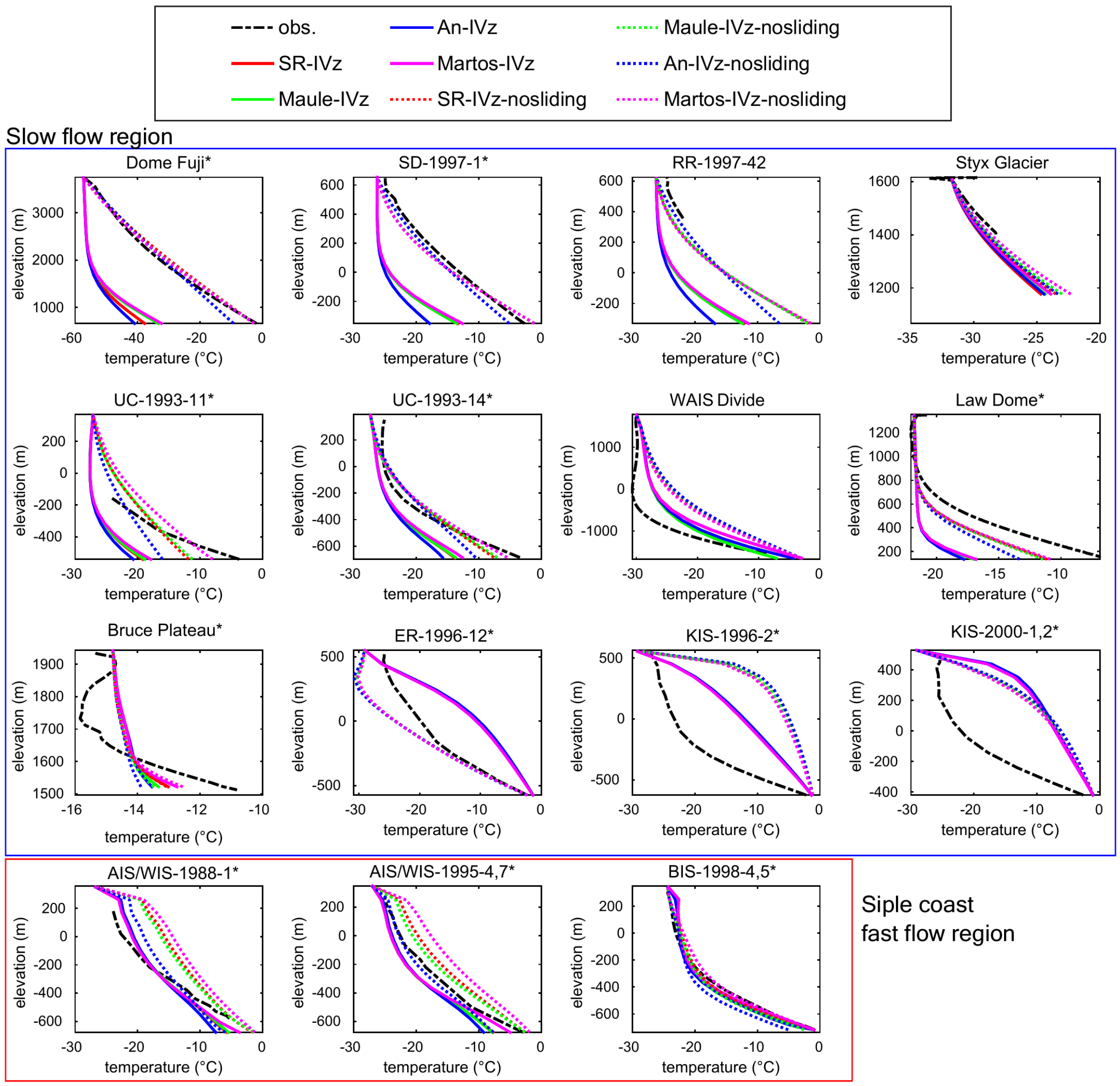

Figure 12: Observed and modeled vertical temperature profiles from eight different experiments at 15 borehole locations. Blue and red boxes indicate slow flow and Siple coast fast flow regions, respectively. The bottom elevation at each borehole is set with considering the ice thickness, as listed in Table 1. An asterisk on borehole name indicates that the drilling reaches the bed rock.

In Fig. 2, I would also find it helpful to see the boreholes that go all the way to the bed somehow indicated in Fig. 2.

RC: The elevation of each borehole profile is limited with considering ice thickness, as listed in Table 1. See also above Figure 12.

Line 69: HO should be defined as higher order, with appropriate citation given.

RC: Revised as reviewer's comment. Add Pattyn (2003) for High-Order model (HO) in the revised manuscript, L56-58.

Modified part:

> L57-58: Due to scarcities of internal ice velocity measurements, three-dimensional mechanical models, such as Higher-Order (HO; Pattyn, 2003) and Full Stokes (FS), are used to estimate internal ice velocities.

Line 79: Be clearer about what the temperature rigidity relation is (e.g. give page # in Cuffey).

RC: Revised as reviewer's comment.

Modified part:

> Line 83-84: The ice rigidity under grounded ice is estimated using the temperature-rigidity relation (Cuffey and Paterson, 2010, pp. 72–77).

Line 99: Give version of BedMachine

RC: Revised. We used BedMachine version 1, and it is clearly stated in the revised manuscript.

Modified part:

> Line 103-104: The bed geometry is from BedMachine version 1 (Morlighem et al., 2020),

For Fig. 4, I could see on the colorbar writing GHF instead of G to be more consistent with the text.

RC: Revised. We have changed colorbar title for GHFs with "GHF".

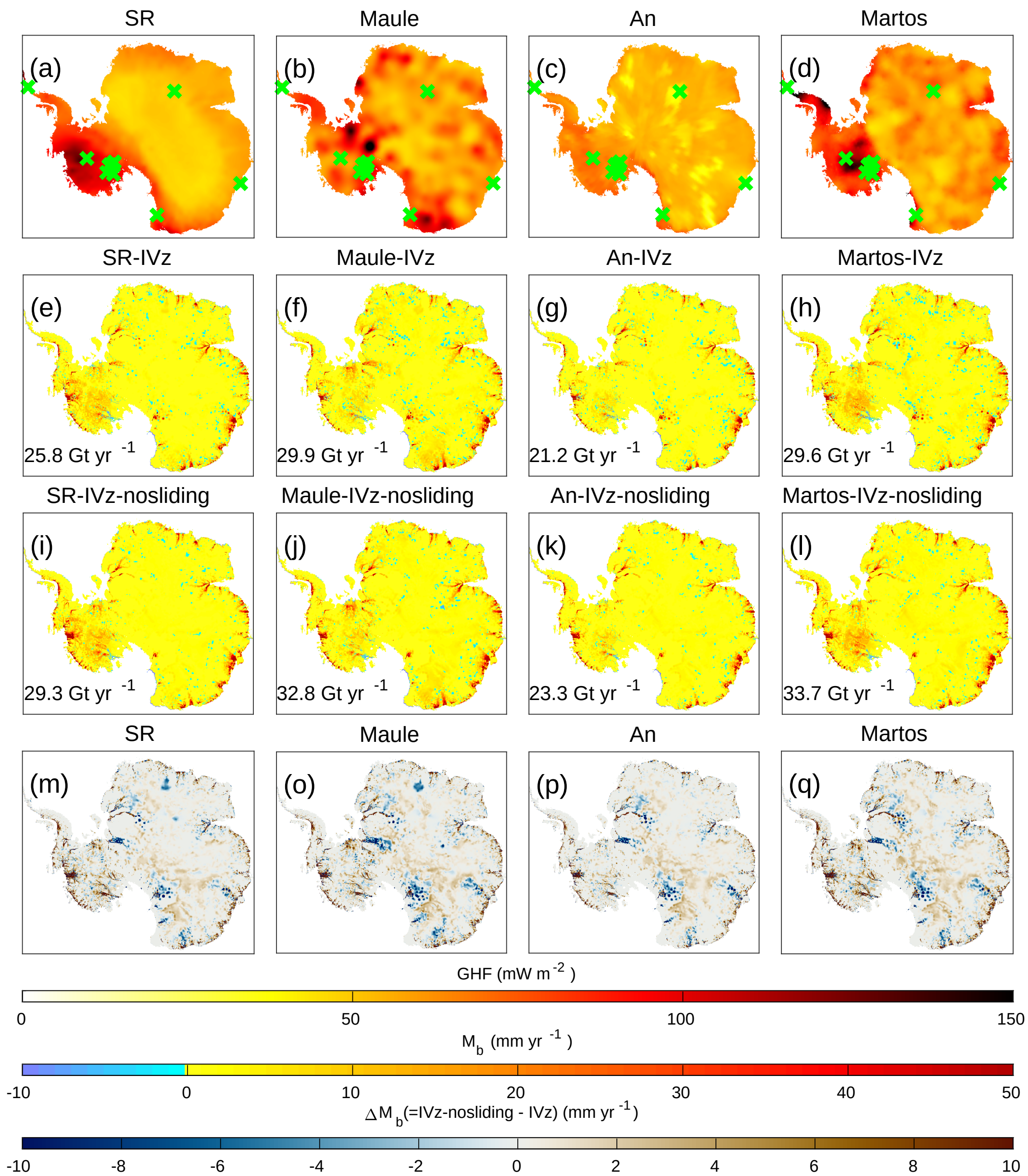

Figure 13: Figure 5 in revised manuscript.

Paragraph starting on line 292: I am confused what is being reported here. I think you mean total melt water volume rather than melting rates. Melting rates should be reported in mm/yr or m/yr (such as the author's Fig. 4 and Pattyn, Jouquin, Llubes) while total melt water volume is Gt/yr. This paragraph should be clarified what measure is being discussed. It would also be helpful if the authors state what their values are rather than just saying they are lower than Pattyn and higher than Llubes.

RC: Thank you for the insightful and constructive suggestion. To avoid confusion between "total grounded ice volume" and "melting rate", we change the notation of "total grounded ice melting rate" to "total grounded ice melting volume" in Table 4 and throughout the manuscript.

On line 308-309, elaborate more on the mass conservation $->$ melting rates $->$ understanding subglacial hydrology comment. I'm not sure I understand what this sentence is trying to say. I think the paragraph could use some rewriting to clarify the point.

RC: Revised as reviewer's comment.

Modified discussion:

> Line 311-324: Thermal models have been used to reconstruct the thermal regime of ice and estimate the melting volume beneath grounded ice. Regarding the advection term in the thermal model, horizontal ice velocity is estimated with Higher Order or Full Stokes (FS) models, while the vertical velocity is recovered with the ice incompressibility. Under kriging-based geometry, the vertical velocity in fast flow region does not coincide with physical property. In contrast, state-of-the art bed geometry, such as BedMachine (Morlighem et al., 2017, 2020), is generated with the mass conservation, which of equation is based on ice incompressibility. We confirm that using the equation of ice incompressibility to reconstruct the ice vertical velocity provides a viable way of computing temperature profiles that exhibit good agreement with observations in Siple coast fast flow regions, such as the BIS. Given that the geometry of other fast flow regions, such as Thwaites Glacier, is generated using the mass conservation method (Morlighem et al., 2011, 2020), therefore, we expect that this study provides a method to generate reliable temperature profiles. Note that the good agreement in modeled temperature at fast flow region, not only Siple coast fast flow region, does not guarantee the magnitude of basal melting volume because the basal melting volume at fast flow region is associated with the frictional heat. However, at slow flow region, the basal temperature is mainly affected by the GHF and the vertical advection, rather than the low frictional heat. Therefore, it is noteworthy that the basal melting rate produced using IVz-nosliding in slow flow regions would be reliable.

Regarding the data availability statement, I believe that this paper would have broader impact and community interest, myself included, if the ISSM thermal model results from this analysis were made available as part of this study. I recommend providing a link to download the gridded temperature fields or simply providing the ISSM outputs for each run. This would enable further comparisons and validation of thermal modeling efforts.

RC: Thank you for the valuable suggestion. In accordance with the reviewer's recommendation, we have shared the gridded basal temperature fields via KDPC (Korea Polar Data Center) with DOI in revised manuscript

Inconsistent with the use of Gt vs. Gton throughout the manuscript.

RC: Revised. We have used "Gt" as default unit in revised manuscript.

Writing style in general is mixing past and present tense, which should be resolved.

RC: Revised. We have modified to the present tense for all statements.

Technical corrections

- Typo on line 43: incompressbility

RC: Revised.

> Line 47-48: The vertical velocities used in one-dimensional thermal model are generally recovered through the equation of ~~incompressbility~~incompressibility, assuming a stationary bed ...

- Mistake on line 107: "three" $->$ "two"

RC: Revised.

> Line 106: the thermal state of the ice sheet using ~~three~~ two different vertical velocity profiles:

- Typo on line 114: exptrapolate

RC: Revised.

Line 119: model to ~~exptrapolate~~ extrapolate

- Typo on line 142: extra space after Y?

RC: The weighted correlation factor is removed in revised manuscript. As Dr. Tyler Pelle (other reviewer) commented this part and we have discussed and replaced the use of the R^2 value with the correlation coefficient between the observed and modeled temperatures, as shown in Figure S1 of the revised manuscript (Figure 14)

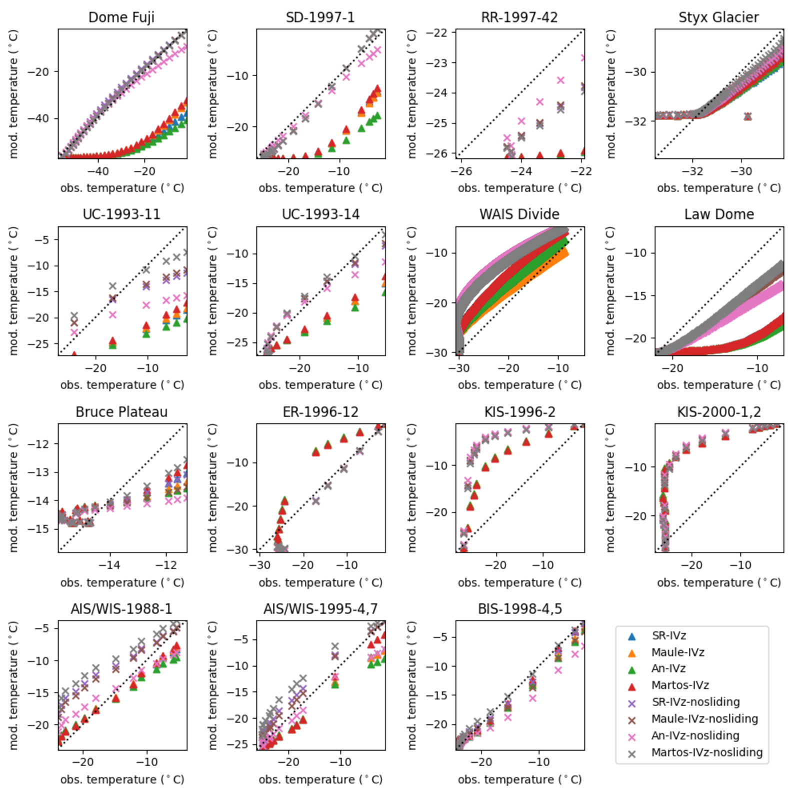

Figure 14: Scatter plot showing the relationship between modeled and observed temperatures at each borehole depending on each experiment. Cross and triangle dot indicate IVz and IVz-nosliding group, respectively. A dashed solid line indicates modeled temperature equal to observed temperature.

- Typo on line 156: Missing a space ("datasets.Table")

RC: Revised.

- Typo on line 163: two commas

RC: Revised.

Line 159: toward the bed dominates, while the other group has more linear shap

- Typo on line 226: indicates ← indicate

RC: Revised.

> Line 222: IVz-nosliding group; these values indicate high vertical advection toward the bottom.

Typo on line 306: delete “a” and “goods” → “good”

RC: Revised.

> Line 317: computing ~~a~~ temperature profiles that exhibit ~~goods~~ good agreement with observations in Siple coast fast flow regions, such as the BIS

Typo on line 340: velocitiy

RC: Revised.

> Line 368: we confirm that the vertical ice velocity based on the equation of incompressibility (IVz) is suitable for fast flow regions, such

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