

Reply to comments by Reviewer 1: "Estimating the impact of ice sheet surface height and ice divide variability on ice core based climate reconstructions"

Summary of Changes

We are grateful to the reviewer for evaluating our work, and for the valuable and constructive comments that help improve the manuscript. To address the major comments, we now

- add a new Sect. 2.1.4 with an overview table of the structural differences between the three model setups.
- add two appendix tables with the Δh lags per glacial cycle for all sites and simulations, and a table of the ice divide and dome displacements.
- add sensitivity tests for the bandpass filter of the phase analysis and for the velocity threshold of the divide detection.

Below, we respond to the reviewer's individual comments in detail and describe the actions we took to address them.

Detailed response

(Original report cited in italics; our replies in blue.)

This manuscript uses three transient ice-sheet-model simulations (PISM-CI, PISM-CESM, and PSUISM) spanning the last two million years to quantify how surface elevation and ice-divide/dome migration at major Antarctic ice-core sites could bias ice-core-based climate reconstructions. The study finds substantial glacial–interglacial elevation variability at East Antarctic sites, a lag between simulated surface-height changes and Antarctic climate signals, and ice-divide/dome displacements of up to ~120 km, with implications for both existing ice cores and prospective "oldest ice" drilling sites. The topic is timely, and the three-model comparison is a genuine strength, but the manuscript would benefit from a number of clarifications, additional justifications, and presentation improvements detailed below. I recommend that the authors address the following comments before the manuscript is considered ready for publication.

Reply: We thank the reviewer for this positive assessment and for the detailed and constructive list of comments, which we address individually below.

Comments on the Abstract

1. *"Ongoing prospective future drilling locations" is vague for an abstract headline; naming LDC, Dome Fuji, and Dome A explicitly (as the Introduction does) would make the abstract more self-contained.*

Reply: We agree and now name the sites explicitly in the abstract.

Proposed text: "[...] we use results from three transient ice sheet model simulations covering the last two million years to investigate surface height and ice divide variability for major Antarctic ice core sites and for the prospective oldest ice drilling locations at Little Dome C, Dome Fuji and Dome A."

Comments on the Introduction

2. *The intro lists prior ISM-based corrections (Ritz et al. 2001, Huybrechts et al. 2007, Parrenin et al. 2007b, Cuffey et al. 2016, Sutter et al. 2019) but the gap statement ("earlier ISM simulations... are often on coarse*

resolution grids and regional and 1D modeling approaches might not capture the full dynamics of the AIS..." could be improved. Consider being more specific: no prior study has compared multiple independent ISMs (and forcing approaches) against each other over the full 2 Myr window at ice-core sites. This narrower claim better motivates the three-simulation, two-model design used here.

Reply: We agree that the narrower claim motivates our design better and have rewritten the gap statement accordingly.

Proposed text: "Nevertheless, earlier ISM simulations and ice sheet reconstructions are often performed on coarse resolution grids, and regional or 1D modeling approaches might not capture the full dynamics of the AIS, in particular the propagated and delayed impact of downstream changes in ice flow. More importantly, no previous study has compared several independent ISMs, driven by different climate forcing approaches, against each other over the full 2 Myr window at Antarctic ice core sites. The structural spread of ISM-based surface height corrections has therefore not been quantified, although such corrections are routinely applied to ice core temperature reconstructions. In addition, surface height variations and ice divide variability have not yet been quantified at all prospective oldest ice core drilling sites (Fischer et al., 2013) using improved bedrock topography maps (Morlighem et al., 2020) [...]"

3. *The three research questions are good, and question (ii) is an interesting one to raise, but it might benefit from a sentence of physical motivation earlier in the text — e.g. why a lag between temperature and elevation would be expected (ice sheet response time). As is, readers unfamiliar with ice dynamics may not see why this matters until the phase analysis section.*

Reply: We agree and add a short physical motivation to the Introduction paragraph on the lapse-rate mechanism, directly after the description of the amplitude bias and before the research questions.

Proposed text: "In addition, an ice sheet does not adjust instantaneously to a change in accumulation or in its boundary conditions, but on its dynamic response time scale, which scales with the ratio of ice thickness to accumulation rate (Hindmarsh, 1996; Cuffey and Paterson, 2010) and is of the order of up to 10^5 years on the East Antarctic plateau (Cuffey and Paterson, 2010). Surface elevation at an ice core site is therefore not expected to follow the climatic signal recorded in the ice in phase, and a lag between the two may bias the relative timing of the reconstructed temperature and of other quantities retrieved from the same core."

4. *The individual mechanisms at plateau vs. grounding-line sites are both explained well, but the fact that they produce opposite signs of bias (attenuation vs. amplification) is currently left for the reader to infer. It might strengthen the motivation if this sign-flip were stated explicitly.*

Reply: We agree and state the sign of the bias explicitly at the end of that paragraph.

Proposed text: "The two mechanisms therefore bias the reconstructed glacial-interglacial amplitude with opposite sign, attenuating it on the plateau and amplifying it closer to the grounding line."

5. *The phrase "and beyond (Barbante, 2025)" would be clearer with a short clause spelling out what it refers to, since not all readers will immediately recognize the citation. Something like: "...up to 800 thousand years (EPICA community members, 2004) and now extending to roughly 1.2 million years with the Beyond EPICA ice core (Barbante, 2025)..." would make the claim self-contained without requiring the reader to look up the reference.*

Reply: We agree and adopt the reviewer's wording.

Proposed text: "Especially, the combination of long continuous records, reaching 800 thousand years (EPICA community members, 2004) and now extending to approximately 1.2 million years with the Beyond EPICA Oldest Ice core at Little Dome C (Barbante, 2025), and their high temporal resolution enable studies of centennial to orbital scale changes in the climate system (Brook and Buizert, 2018)."

Comments on the Methodology

6. *PISM-CI vs PISM-CESM justification. You explain PISM-CI's limitation (linear interpolation between snapshots, can't capture spatial pattern shifts) but it would help to state directly why this specific limitation is relevant to elevation/divide reconstructions.*

Reply: We agree and now make the relevance of this limitation for the analyzed quantities explicit in Sect. 2.1.1.

Proposed text: "Because the fields are linearly interpolated between snapshots, the method captures only a first-order, amplitude-scaled estimate of past temperature and accumulation changes and can only partly reproduce true shifts in the spatial climate patterns. Because the divide position responds to accumulation gradients (Weertman, 1973; Hindmarsh, 1996), which a rescaled fixed pattern can only partly change, the climate index approach can only reproduce a limited part of the elevation and divide response. To assess the sensitivity to this assumption, an auxiliary PISM simulation was forced with transient simulation data from the Community Earth System Model (CESM) version 1.2 (Yun et al., 2023)."

7. *Note that PISM (9.8 K/km, dry-adiabatic) and PSUISM (5°C/km, an empirical/environmental lapse rate) use physically different lapse-rate assumptions. The choice of lapse-rate physics differs, which could itself be a major driver of inter-model spread in surface elevation change. Worth flagging this explicitly as a structural difference between the model setups, since it is currently only apparent by comparing separate subsections.*

8. *Bedrock deformation approaches differ too (viscoelastic in PISM vs. ELRA with a single 3000-yr relaxation time in PSUISM) — again, worth an explicit sentence acknowledging this as a source of divergence.*

Reply: We address comments 7 and 8 together. We agree that these structural differences are currently only visible by comparing the individual subsections, and we now collect them in a new short subsection 2.1.4 together with an overview table, and refer back to it in Sect. 4.2.

Proposed new subsection 2.1.4: "Structural differences between the model setups": "Besides the climate forcing, the three simulations differ in several structural choices that directly affect the simulated surface elevation (Tab. A1). Most importantly, our PISM simulations scale the surface air temperature to the evolving ice sheet geometry with a dry-adiabatic lapse rate of 9.8 K km⁻¹, whereas in PSUISM an empirical lapse rate of 5 K km⁻¹ was applied. For a surface height change of 100 m this amounts to a difference of about 0.5 K in the temperature seen by the surface mass balance scheme, so that the strength of the height–SMB feedback, and with it the amplitude and equilibration time of the simulated Δh , differ between the models by construction. Bedrock deformation is treated with a viscoelastic Earth model (Bueler et al., 2007) in PISM and with an ELRA formulation with a single relaxation timescale of 3,000 yr (Pollard and DeConto, 2012b) in PSUISM, which affects the delayed isostatic contribution to Δh ."

9. *Results throughout are reported as single numbers (m, km, °C, %) without uncertainty bounds — understandable given each simulation is a single deterministic run rather than an ensemble. A brief sentence*

early in the Methods or Results, clarifying that the inter-model spread reflects structural/forcing differences rather than a formal statistical uncertainty, would help readers calibrate confidence in the reported figures.

Reply: We agree that this should be stated explicitly and add a corresponding sentence at the end of the Methods.

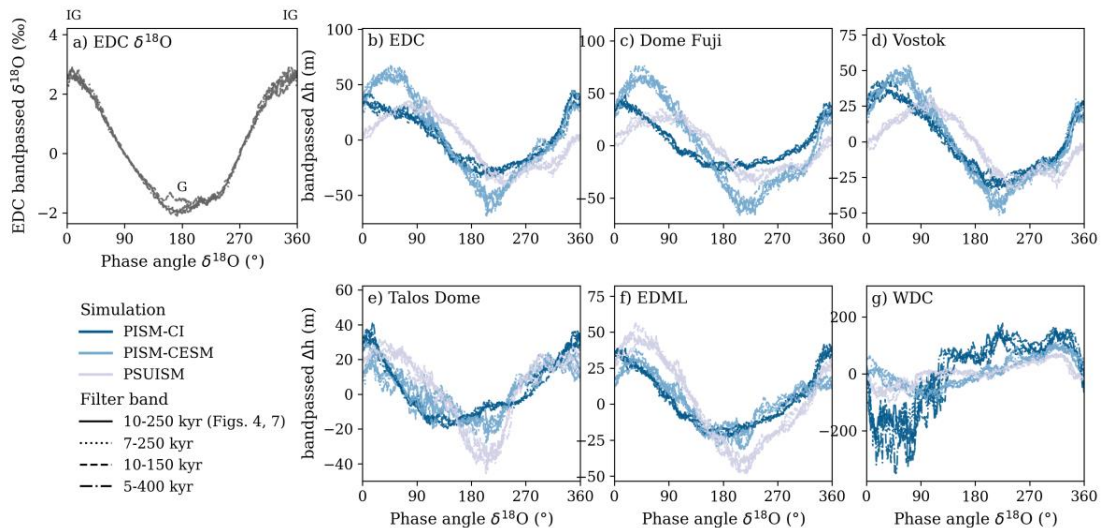
Proposed text: “Each of the three simulations is a single deterministic realization. All values reported in the following are therefore point estimates without formal confidence intervals, and the spread between the three simulations should be read as an indication of structural and forcing-related uncertainty rather than as a statistical uncertainty range.”

Comments on the Results

10. Section 3.1 — phase-shift estimates (Fig. 4/7) — The magnitude of the reported phase/timing shifts (e.g., “up to 45 kyr” for PSUISM at the Last Interglacial) is presented as a point estimate derived from a single, fixed bandpass filter (7–250 kyr). It would strengthen confidence in the quoted values if the authors briefly indicated the sensitivity of the phase-shift magnitudes to reasonable variations in the filter band (e.g., a narrower or wider cutoff) or justified why 7–250 kyr was chosen as the appropriate band.

Reply: The band was chosen to retain the precessional, obliquity and eccentricity periods that dominate both the isotope records and the simulated Δh , and to exclude millennial variability and the long-term trend, the exact cutoffs within these limits are a choice. To validate the robustness of our method to the choice of the cutoff frequencies we repeated the phase analysis for a narrower (10–150 kyr) and a wider (5–400 kyr) band, applying each band identically to the $\delta^{18}O$ record and to Δh , and show the composites for all bands in Fig. R1 below, which we propose to add to the appendix of the manuscript. As Figure R1 underlines our statements on the phase and time shifts are robust under varying cutoff windows.

While performing the additional analysis, we realized that the cutoff window used in Figure 4 and 7 was 10–250 kyr instead of 7–250 kyr. We will change this accordingly in the revised manuscript.



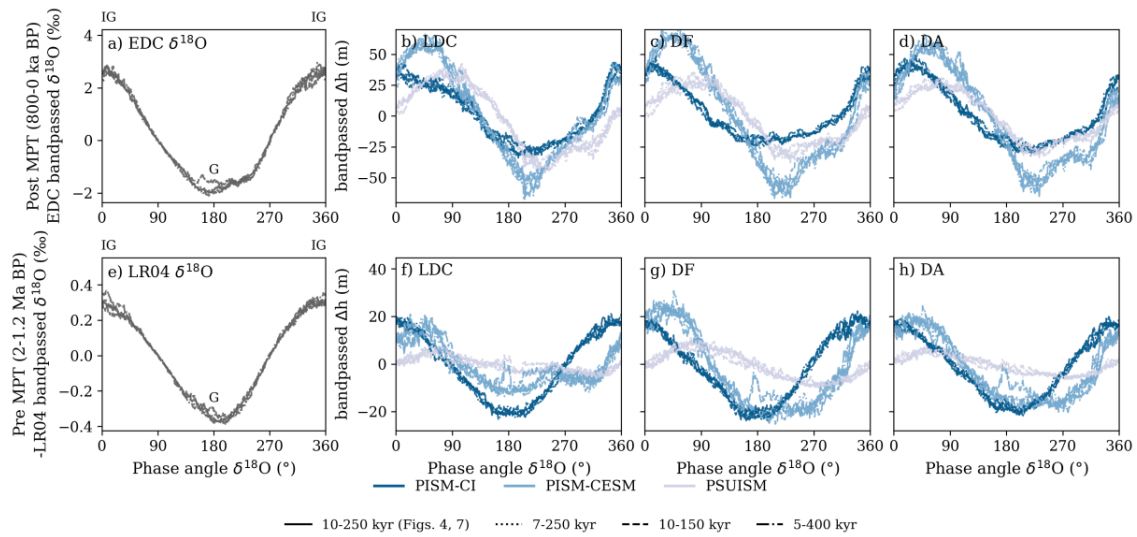


Figure R1. Composites of the bandpass filtered $\delta^{18}\text{O}$ record and the simulated Δh as a function of the $\delta^{18}\text{O}$ phase angle, as in Fig. 4 and Fig. 7 for four filter bands: 10-250 kyr (solid, the band of the published figures), 7-250 kyr (dotted), 10-150 kyr (dashed) and 5-400 kyr (dash-dotted).

11. Section 3.1 — The text states "PISM-CI seems to be most in sync with the $\delta^{18}\text{O}$ record, while PSUISM exhibits the largest phase shift" — but PISM-CI is forced using a climate index that interpolates between climate snapshots "along a paleoclimatic index" (Sutter et al., 2019; Wirths et al., 2025), described as "representative for the climate evolution of the last 2 million years." The Methods do not state what this index is derived from. If it is itself based on an isotope stack ($\delta\text{D}/\delta^{18}\text{O}$ -type record), then PISM-CI's apparent synchrony with $\delta^{18}\text{O}$ may partly reflect the forcing design rather than an independent physical result. A brief clarification of the index's basis, and an explicit caveat if it is isotope-derived, would help readers weigh this specific comparison appropriately.

Reply: This is an important point and we thank the reviewer for raising it. The climate index is derived from isotope-based records, so that the apparent synchrony of PISM-CI is to some extent built into the forcing, in contrast to the CESM forced simulations. We now state the basis of the index in Sect. 2.1.1 and add an explicit caveat where the comparison is made in Sect. 3.1 and in Sect. 4.2.

Proposed text (Sect. 2.1.1): "[...] in combination with a climate index time series representative for the climate evolution of the last 2 million years as described in Wirths et al. (2025) and Sutter et al. (2019). The index is derived from the EDC deuterium record (Jouzel et al., 2007) for the last 800 kyr and the LR04 benthic $\delta^{18}\text{O}$ stack (Lisiecki and Raymo, 2005) for older periods."

Proposed text (Sect. 3.1): "Across all ice core sites PISM-CI appears to be most in sync with the $\delta^{18}\text{O}$ record, while PSUISM exhibits the largest phase shift. However, we stress that the climate index used to force PISM-CI is itself derived from isotope records (Sec. 2.1.1). Thus, any lead or lag simulated by PISM-CI can largely be attributed to the ice sheet response itself."

12. Section 3.1 — Only one lag value is quantified in the text ("up to 45 kyrs for the Last Interglacial peak in PSUISM"); the rest are qualitative ("shifted by several millennia," "delayed"). Figures 4, 7, and A1 are graphical phase-angle composites, not numeric summaries, so lag magnitudes must be read off plots by eye.

A short summary table (lag in degrees and/or kyr, per site, per model, covering both the EDC $\delta 18O$ and LR04-based analyses) would make Section 3.1's desynchronization claim much easier to evaluate.

Reply: We agree that time lags for specific periods would make the desynchronization easier to assess. The phase angle used in Figs. 4 and 7 is constructed to compare the timing across glacial cycles of different length, which is why it does not map linearly onto time: the same phase lag corresponds to different time lags in different cycles, and a uniform conversion is not possible. To make the time lags accessible, we propose to add the tables below to the appendix of the manuscript. For all ice core sites and simulations they give the time lag between the Δh extremum and the corresponding $\delta 18O$ extremum for the interglacial peaks MIS 1, 5e, 7, 9 and 11 and the glacial maxima LGM, PGM, MIS 8, 10 and 12, i.e. the periods of Table 1, together with the median and range over all nine interglacial peaks and eight glacial maxima of the last 800 kyr, and the median and range over all cycles between 2.0 and 1.2 Ma BP for the prospective oldest ice sites. The rounded value of "45 kyr" quoted in Sect. 3.1 for the Last Interglacial in PSUISM is replaced by the values of this table. In addition, the Δh time series of all three simulations will be deposited in a public repository with the revised manuscript, so that lags for any other period can be derived directly.

Table R1. Lag of the simulated Δh extrema behind the EDC $\delta 18O$ record for individual glacial cycles of the last 800 kyr. Values in parentheses fall outside the period covered by the respective ice core record. All values are point estimates from single deterministic simulations; the range gives the spread between cycles, not an uncertainty.

		Interglacial peaks, lag of Δh maximum [kyr]						Glacial maxima, lag of Δh minimum [kyr]					
		MIS 1	MIS 5e	MIS 7	MIS 9	MIS 11	median (range)	MI S 2	MIS 6	MIS 8	MIS 10	MI S 12	median (range)
EDC	PISM-CI	+6	+4	+6	+2	-5	+4 (-5, +10)	+5	+3	+6	+3	-2	+4 (-2, +10)
	PISM-CESM	+9	+1	+5	+9	-4	+5 (-4, +20)	+1	--	+7	+4	-1	+2 (-1, +11)
	PSUISM	+9	+48	+45	+15	+11	+14 (+1, +48)	+5	-3	+19	+15	+1	+12 (-3, +22)
Dome Fuji	PISM-CI	+5	+6	+5	+3	+1	+4 (-3, +9)	+6	+4	+4	+6	+0	+4 (-2, +6)
	PISM-CESM	+9	+12	+9	+9	+5	+9 (+0, +19)	+4	--	+8	+8	-2	+6 (-2, +17)
	PSUISM	+9	+23	+43	+21	+14	+14 (+3, +43)	+5	-4	+10	+16	+4	+8 (-4, +19)
Vostok	PISM-CI	+9	+9	+8	+5	+0	+7 (-1, +12)	+5	-5	+9	+6	(-2)	+6 (-5, +11)
	PISM-CESM	+9	+13	+8	+12	-5	+9 (-5, +20)	+4	--	+11	+12	(+0)	+11 (+0, +23)

	PSUISM	+9	+23	+47	+22	+14	+22 (+3, +47)	+6	-1	+22	+16	(+3)	+13 (-1, +26)
Talos Dome	PISM-CI	+2	+4	+2	+2	(-15)	+2 (-15, +12)	-4	+1	+3	(-4)	(-2)	-1 (-4, +3)
	PISM-CESM	+2	-4	+0	+3	(+36)	+2 (-4, +36)	-5	+10	+7	(+6)	(-5)	+2 (-5, +10)
	PSUISM	-1	-6	-5	-2	(-7)	-1 (-7, +17)	-5	-5	+4	(+3)	(-4)	-2 (-5, +4)
EDML	PISM-CI	+3	+3	(+4)	(+1)	(-11)	+1 (-11, +8)	+4	-5	(+1)	(-3)	(+5)	-2 (-5, +5)
	PISM-CESM	+2	-3	(+4)	(-1)	(+5)	+2 (-4, +15)	-2	+10	(+6)	(+2)	(+0)	+4 (-3, +43)
	PSUISM	+5	+12	(+7)	(+9)	(+8)	+8 (-2, +17)	+1	-5	(+5)	(+4)	(-2)	+2 (-5, +15)

Table R2. Same as Table R1 for the prospective oldest ice sites between 2.0 and 1.2 Ma BP, with the LR04 benthic stack as reference).

Site	ISM	Lag of Δh maximum [kyr], median (range)	Lag of Δh minimum [kyr], median (range)
LDC	PISM-CI	+0 (-3, +15)	+0 (-5, +9)
	PISM-CESM	+8 (-5, +19)	+6 (-5, +15)
	PSUISM	+10 (-5, +20)	+8 (-5, +18)
Dome Fuji	PISM-CI	+1 (-5, +15)	+0 (-5, +9)
	PISM-CESM	+8 (-4, +18)	+4 (-2, +9)
	PSUISM	+11 (+0, +19)	+8 (+1, +16)
Dome A	PISM-CI	+2 (-3, +15)	+1 (-3, +9)
	PISM-CESM	+7 (-5, +20)	+5 (-2, +10)
	PSUISM	+10 (-5, +19)	+7 (+0, +15)

13. Section 3.2 — PSUISM's absence from the ice-divide/dome analysis is noted twice as a scope limitation (in 3.2 section and again in 4.3 section), but neither mention addresses that PSUISM diverges most strongly from the PISM runs in elevation history elsewhere (3.1, 3.3). A brief added sentence — in 3.2 or 4.3 — noting that the reported divide/dome variability estimates (up to ~120 km) are therefore based on two of the three ISMs only, and may not bound the true spread that a PSUISM-inclusive analysis would show.

Reply: We agree and add this caveat.

Proposed text (Sect. 4.3): “As these estimates rest on the two PISM simulations only, and PSUISM deviates most strongly from them in the simulated surface height evolution (Sect. 3.1 and 3.3), the reported displacement of up to about 120 km is likely a lower bound on the spread that a PSUISM-inclusive analysis would show.”

14. Section 3.2 — *It might be worth considering a summary table here. This subsection reports many individual numbers in prose (~120 km, ~45 km, ~70 km, ~50 km, ~65 km, ~70 km, ~40 km, etc., across six sites and two models), and Figures 5 and 8 are spatial maps rather than numeric summaries. Since Section 3.1 already includes Table 1 for Δh , an analogous table for ice divide/dome displacement (per site, per model) could improve readability, make it easier for readers to compare sites at a glance, and incidentally make PSUISM's absence from this analysis more visually apparent.*

Reply: We will add such a table to Section 3.2 of the revised manuscript.

15. Section 3.2 — *The divide-detection method uses a fixed velocity threshold ($\bar{u}_{\max} = 0.5$ m/yr), but no justification is given for this value. It would be helpful if the authors could briefly explain why 0.5 m/yr was chosen or ideally include a short sensitivity check showing how much the reported displacement estimates would change under a different reasonable threshold.*

Reply: The threshold is used only for the ice divide detection. The dome position is detected via the local surface height maxima and does not depend on the threshold. Since the divide lies somewhere within a grid cell, the model does not produce a cell with zero velocity, and a threshold is needed to identify it. 0.5 m yr⁻¹ keeps the detected divide as narrow as possible while still captures it mostly continuous at all sites. A sensitivity analysis is done in Fig. R2, which shows the divide composites for $\bar{u}_{\max} = 0.25, 0.5, 1.0$ and 2.0 m yr⁻¹. We propose to add both figures to the appendix of a revised manuscript.

Proposed text: “... as long as it is lower than a threshold velocity of $\bar{u}_{\max} = 0.5$ m yr⁻¹, and vice versa. As the divide does not coincide with a grid cell of zero velocity, this threshold is chosen to keep the detected divide as narrow as possible while capturing it almost continuously at all sites. The sensitivity of the ice divide location to $\bar{u}_{\max}$ is shown in Fig. A3 and A4.”

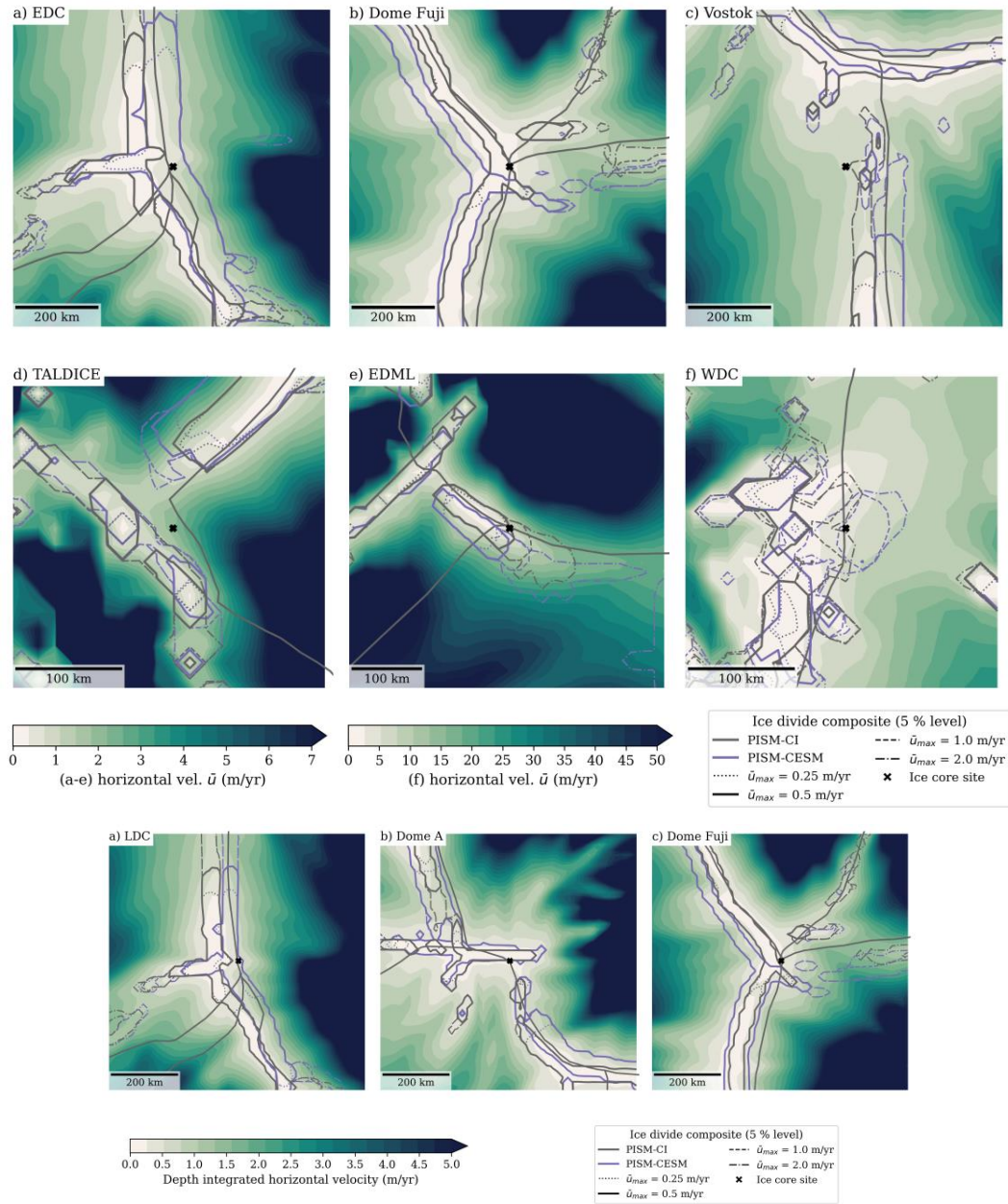


Figure R2. Ice divide composites as in Figs. 5 (top) and 8 (bottom) for velocity thresholds of 0.25, 0.5, 1.0 and 2.0 m yr⁻¹ (line styles) in PISM-CI (grey) and PISM-CESM (purple) over the simulated present-day PISM-CI velocity field. Crosses mark the drill sites.

16. Section 3.3 — Comment (line 240): Grammar/typo: "we observe the same pattern in Δh variability throughout the the last 800 ka at LDC than at EDC" — double "the," and "than" should be "as" ("...throughout the last 800 ka at LDC as at EDC").

Reply: Corrected as suggested.

Comments on the Discussion

17. Section 4.1 — *The Discussion states Δh "has not differed more than 152 m between all three simulations". It's unclear from the text whether this 152 m figure is (a) the maximum inter-model spread at a single site/time point, (b) the maximum such spread across all sites and the full 800 kyr record, or (c) something derived from single-model amplitude ranges in Table 1 (several of which — e.g. TALDICE/PISM-CESM at ~151 m — are close to this number by coincidence or not). Since this figure is later used to justify the ~10% glacial-interglacial temperature uncertainty at EDC, it would help if the authors stated explicitly, in or near Table 1, how "differed between simulations" is computed and at which site(s) the 152 m maximum occurs.*

Reply: The value refers to EDC only. It is the maximum, over the last 800 kyr, of the difference between the largest and the smallest Δh simulated by the three ISMs at the same time step, and is not derived from Table 1. We now state this in the text.

Proposed text: "However, at EDC the Δh values of the three simulations have not differed by more than 152 m at any time during the last 800 kyr, where the difference is taken between the largest and the smallest simulated Δh at the same time step."

18. Section 4.1 — *Figure A2 compares the study's simulated EDC Δh against the widely-used Parrenin et al. (2007b) 1D reconstruction. This strikes me as a particularly valuable comparison, since it's the only place in the paper where the results are checked against an independent, previously published, community-adopted reconstruction. Given its importance, the authors might consider moving this figure (or a version of it) into the main text.*

Reply: We agree that this comparison is valuable, but we prefer to keep it in the appendix. The 1D reconstruction of Parrenin et al. (2007b) is itself a model result, constrained by the EDC chronology and accumulation history, and not an independent observation against which the 3D simulations could be validated. Its purpose in our study is to show where the correction that is currently applied to the EDC temperature reconstruction sits relative to the three simulations, which is a point of the Discussion rather than a result. We have therefore kept Fig. A2 in the appendix but made the comparison in Sect. 4.1 more explicit.

19. Section 4.2 — *the structural-difference explanation is convincing but could be tied more directly back to the Dome Fuji divergence noted in 3.3 (opposite-signed long-term trends between PISM-CI and PSUISM). Currently the paragraph explains the general causes (SMB scheme, lapse rate, bedrock deformation, marine ice-sheet physics) and illustrates them with LDC's 150–200 m spread but doesn't explicitly revisit the DF sign-flip as a second worked example.*

Reply: We agree and now refer to the Dome Fuji divergence explicitly in this paragraph.

20. Section 4.2 — *The growth-rate thresholds (13.1 m/kyr, 2.5 m/kyr) are converted into temperature-rate thresholds using a single fixed lapse rate (9.8 K/km), whereas a comparable calculation in 4.1 (WDC deglaciation bias) explicitly uses a lapse-rate range (7–9.8 K/km) to account for uncertainty. Applying the same range here — even though the "secondary importance" conclusion would likely survive it, since the resulting thresholds would still be small relative to observed ice-core temperature trends — would make the argument more methodologically consistent with 4.1 and more robust to the reader.*

Reply: In Sect. 4.1 the lapse-rate range is used to estimate the magnitude of a bias, whereas here we derive an upper bound on the temperature trend that a surface height change can produce. The dry-adiabatic lapse rate of 9.8 K km⁻¹ is the largest applicable value, so any lower lapse rate would only reduce the thresholds. We now state explicitly that the values are upper bounds.

Proposed text: "... indicating that a cooling trend larger than 0.13 K per millennium (an upper bound, assuming the dry-adiabatic lapse rate of 9.8 K per km) could not be created by a surface height change alone."

Comments on the Conclusion

21. *The Conclusion effectively summarizes the paper's general findings but stops short of a practical takeaway for readers. Since the paper's stated goal is to "assist ice core scientists in the interpretation of their results," it would strengthen the closing to add one sentence ranking which sites/time periods are most vs. least affected by elevation/divide bias — e.g., Talos Dome (3.1: the only site where inter-model Δh differences are not even consistent in sign), EDML (3.2: largest magnitude of dome-position-driven temperature and SMB bias, up to 4°C and >20%), and LDC pre-MPT (4.3: substantially reduced dome variability and bias). This would give readers a quick practical summary without requiring them to reconstruct it from Table 1 and Section 3.*

Reply: We agree and add a short practical summary to the Conclusion.

Proposed text: "The potential bias is largest at EDML, where dome migration changes temperature and SMB by up to 4 °C and 20 %, and least constrained at Talos Dome, where the Δh differences between the ISMs are not consistent in sign. At the East Antarctic plateau sites, Δh variations on the order of 100 m affect the glacial-interglacial amplitude while the dome position is comparatively stable. The prospective oldest ice sites, in particular LDC before the MPT, are least affected."

We thank the reviewer again for the careful reading and the constructive comments, which have helped us to improve the manuscript.