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2 The authors sincerely thank the RC2 for his helpful and constructive comments. All comments have been
3 carefully considered in the revised manuscript. Our responses are written in blue, and excerpts from the
4 manuscript are italicized. Line numbers refer to the revised manuscript.

5

6 **Summary**

7 van Aalderen et al. investigate how the mechanism of the Marine Ice Sheet Instability (MISI)
8 affected the Barents-Kara Ice Sheet (BKIS), a marine based ice sheet which existed in the
9 past. Studying past behaviours of marine based ice sheets prone to MISI may inform us how
10 other marine ice sheets, such as West Antarctic, could respond.

11 Based on their findings they suggest that atmospheric forcing can be a trigger for MISI and
12 should be accounted for in future Antarctic projections. I think that it is valuable to
13 investigate the state of past ice sheets and informative for future studies, and the scope is well
14 within the scope of Climate of the Past. However, I am missing stronger proof that MISI is
15 really being observed, as for now it seems rather a qualitative assessment. Transects of
16 affected basins would convince me that MISI is indeed occurring.

17 **General comments**

18 **MISI**

19 The abstract states: "*Contrary to current interpretations found in the literature, which*
20 *suggest that a MISI is primarily initiated by ocean-induced basal melting.*" I disagree with
21 this framing.

22 MISI is a self-sustained retreat of marine ice sheets on retrograde bed slopes. If the grounding
23 line sits at a local bedrock maximum (below sea level), perturbations in grounding-line flux
24 can destabilize it and trigger retreat (Pattyn et al., 2012; Schoof 2007). For the Antarctic Ice
25 Sheet, it is widely accepted that ocean warming thins ice shelves, reducing their buttressing
26 effect and increasing grounding-line flux (also, as stated in the discussion, other mechanisms
27 can also limit ice-shelf stability, such as hydrofracturing). Hysteresis experiments on the
28 Antarctic Ice Sheet show that atmospheric warming alone can trigger MISI (Swierczek-
29 Jereczek et al., 2026).

30 While we do expect ocean-induced melting to be the dominant trigger for the West Antarctic
31 Ice Sheet, I would not state that MISI is *primarily* initiated by ocean-induced basal melting.
32 In milder climates, the atmosphere could plausibly act as the trigger instead. I'd suggest either
33 removing this sentence from the abstract, or rephrasing it to something like: "*We find an*
34 *atmospherically triggered MISI.*"

35 We thank the reviewer for this remark, and we agree with this more accurate framing. We
36 have revised the abstract accordingly, replacing the original statement with a phrasing that
37 presents our finding as an atmospherically triggered MISI, rather than a general claim about
38 the primary trigger of MISI.

39 (17) "*While ocean-induced basal melting is often considered the primary trigger of MISI,*
40 *largely due to a focus on the West Antarctic Ice Sheet in the existing literature, our results*
41 *show that atmospheric warming can also directly trigger such a dynamic instability, and*

suggest that atmospheric warming can trigger a MISI and therefore be responsible for the retreat of the BKIS.”

If your MISI is atmospherically driven, what is the actual trigger mechanism? Is it surface melt at or near the grounding line, or is it a warmer ice column reducing ice viscosity and thereby increasing ice velocities? Finally, it would be good to provide transects of the regions where you claim MISI is occurring. Plotting a transect along an ice-flow streamline would clearly demonstrate the deeper bedrock and show how the grounding line potentially stabilizes at bedrock minima. This is difficult to assess from the 2D bed elevation maps alone.

To directly address the question of the triggering mechanism, we have added transects along an ice-flow streamline through the region where the MISI occurs (new Fig. S15), as suggested by the reviewer. This allows a much clearer visualization of the underlying bed topography than the 2D maps alone.

Figures S15a, S15c, S15d correspond to the experiments where the atmospheric warming is halted at 14.5, 15, and 15.5 ka respectively. Figure S15b corresponds to the zero-lapse rate experiment and the warming also ceases at 14.5 ka (Section 3.4, Fig. 6).

These transects confirm that the bedrock deepens progressively toward the interior of the BKIS, but critically, the retrograde section is preceded by a bedrock high (see point “x” in figure S15) near the LGM grounding-line position. The triggering mechanism we identify is surface melt-driven thinning of the ice near the grounding line. As atmospheric temperatures rise, surface melting progressively thins the ice near the grounding line. Once this thinning is sufficient to bring the ice close to flotation, the grounding line is able to migrate across the bedrock high. Beyond this topographic high, the grounding line reaches the retrograde section of the bed, where it becomes mechanically unstable and MISI is initiated, driving a self-sustained retreat.

In the 15.5 ka experiment, atmospheric warming is stopped before ice thinning is sufficient to drive the grounding line across the bedrock high (see point “x” in figure S15). Consequently, the grounding line remains stable on the still-prograde section of the bed. By contrast, in the 15 ka case, warming continues long enough to drive the grounding line beyond the bump and onto the retrograde slope, thereby triggering the self-sustained retreat (see Fig S15 and Fig 7).

The comparison between Figures S15 and S15 provides complementary evidence. When the lapse rate is set to zero from 14.5 ka onward (removing the elevation-temperature feedback), the grounding line stabilizes on the bedrock high itself (see point “y” in figure S15), where the bed slope is non-retrograde, thereby halting the MISI. When the lapse rate is instead kept active, the elevation-temperature feedback continues to enhance surface melt as the ice thins, allowing further ice thinning until flotation is reached and the grounding line advances beyond the bump. Once the grounding line reaches the unstable retrograde section, a self-sustained retreat occurs, allowing the MISI to resume. This confirms that the atmospheric forcing is the mechanism by which our simulated MISI is triggered. The elevation-temperature feedback helps the grounding line crossing of the stabilizing bedrock high rather

than being the trigger itself. We note, however, that this experiment does not allow us to disentangle the respective contributions of ice viscosity changes and surface mass balance changes to this effect. Doing so would require additional experiments in which SMB is allowed to evolve as in our reference simulations, but without the corresponding change in ice temperature and viscosity. We consider the role of viscosity to be minor compared to that of SMB, but this cannot be formally demonstrated with the experiments presented here.

We added these results in the manuscript as follows:

(l633) “A streamline transects through the region of retreat (Figure S15) confirms that the bedrock deepens progressively toward the interior of the BKIS, but that the retrograde section is preceded by a local bedrock high near the LGM grounding-line position (point “x”, Figure S15). As atmospheric temperatures rise, progressive surface melting brings the ice close to flotation, allowing the grounding line to migrate beyond this bedrock high and onto the retrograde slope. The ice sheet therefore becomes mechanically unstable allowing a self-sustained retreat characteristic of the MISI. This indicates that the triggering mechanism is surface melt-driven thinning of the ice near the grounding line,

This is illustrated by comparing the experiments where the climate is stabilized at 15.5 (Figure S15d) and 15 ka (Figure S15c).

In the 15.5 ka experiment, atmospheric warming is stopped before ice thinning is sufficient to drive the grounding line across the bedrock high (see point “x” in figure S15). Consequently, the grounding line remains stable on the still-prograde section of the bed. By contrast, in the 15 ka case, warming continues long enough to drive the grounding line beyond the bump and onto the retrograde slope, thereby triggering the self-sustained retreat (see Figure S15c and Figure 7).

The comparison between Figures S15a and S15b provides complementary evidence. When the lapse rate is set to zero from 14.5 ka onward (removing the elevation-temperature feedback), the grounding line stabilizes on the bedrock high itself (see point “y” in figure S15), where the bed slope is non-retrograde, thereby halting the MISI. When the lapse rate is instead kept active, the elevation-temperature feedback continues to enhance surface melt as the ice thins, allowing further ice thinning until flotation is reached and the grounding line advances beyond the bump. Once the grounding line reaches the unstable retrograde section, a self-sustained retreat occurs, allowing the MISI to resume. This confirms that the atmospheric forcing is the mechanism by which our simulated MISI is triggered. The elevation-temperature feedback helps the grounding line crossing of the stabilizing bedrock high rather than being the trigger itself. We note, however, that this experiment does not allow us to disentangle the respective contributions of ice viscosity changes and surface mass balance changes to this effect. Doing so would require additional experiments in which SMB is allowed to evolve as in our reference simulations, but without the corresponding change in ice temperature and viscosity. We consider the role of viscosity to be minor compared to that of SMB, but this cannot be formally demonstrated with the experiments presented here.”

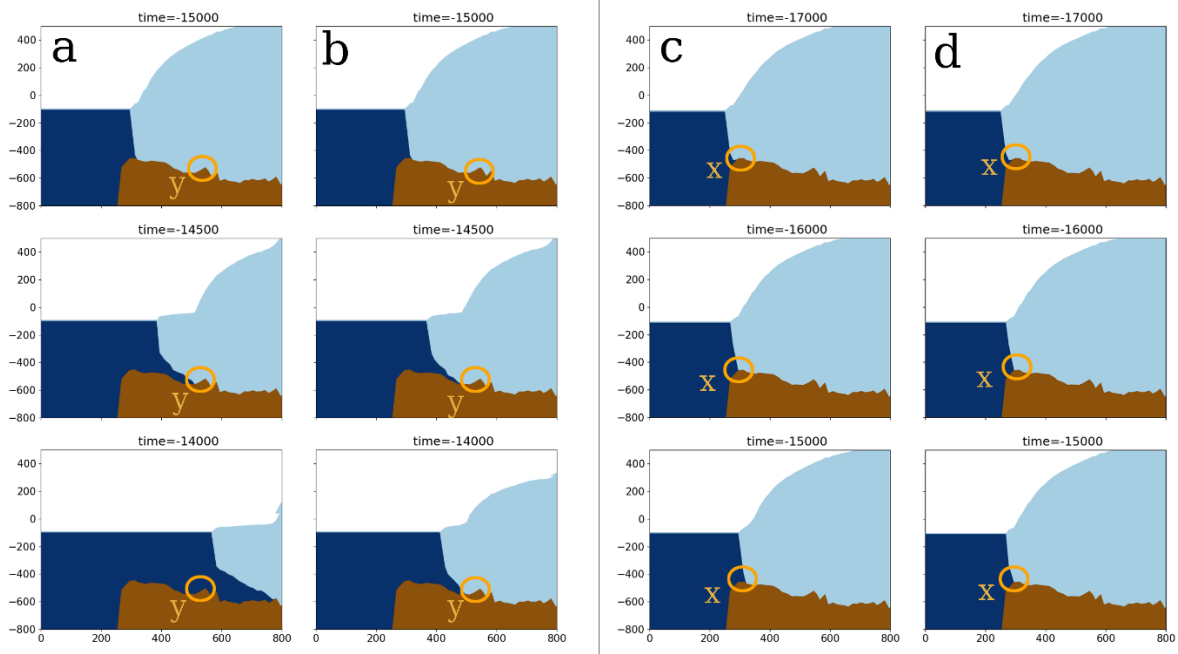


Figure S15: Transects of ice-flow streamlines through the region of BKIS retreat, showing bed topography (brown), grounded ice and floating ice (light blue) and sea (dark blue) at successive time steps for different fixed-forcing experiments. a: Stabilization experiment at 14.5 ka, shown at -15, -14.5 and -14 ka. b: Same as a, but for the stabilization experiment at 14.5 ka with the lapse rate set to 0 °C km⁻¹ from 14.5 ka onward. c: Same as a, but for the stabilization experiment at 15 ka, shown at -17, -16 and -15 ka. d: Same as c, but for the stabilization experiment at 15.5 ka. Points "x" and "y" mark the local bedrock highs preceding the retrograde slope.

LGM state

It appears you are simulating a too-thick ice sheet compared to reconstructions. You permute your $\beta_{\min}$ value, which caps your friction, but as already noted, this is more of a numerical artifact than a physically meaningful behavior. Given that $\beta = C_f \cdot C_{\text{sedim}} \cdot N$ I would expect your friction coefficient (C_f) to play a stronger role, since it will affect the whole ice sheet and not just grid points beyond a threshold. You use a spatially uniform friction coefficient of 1.5×10^{-6} m/yr, could you clarify why this value was chosen? Is it based on other experiments or other ice sheets? Does it provide a good match to your LGM constraints? I recognize this may not be the aim of the paper and could involve substantial extra work, but I believe permuting friction-related parameters, rather than $\beta_{\min}$, would produce a more realistic ice sheet and a more informative comparison against reconstructions.

We agree with the reviewer. Although $\beta_{\min}$ directly caps friction only at grid points where the computed β would otherwise fall below this threshold, its influence is not restricted to those specific points but propagates across the whole ice sheet, since local basal conditions influence upstream ice flux and thickness through the coupled ice-flow equations.

This is illustrated in Figure S3 and S4 which compare ice volume and extent to reconstructions (see below). These figures show that ice thickness at the center of BKIS itself differs depending on the $\beta_{\min}$ value used, confirming that this parameter affects the ice-sheet geometry well beyond the specific grid points where the $\beta_{\min}$ cap is locally active.

As shown in Fig. S3 and S4 (see below), lower $\beta_{\min}$ values consistently produce ice volumes in better agreement with the GLAC-1D and ICE-6G_C reconstructions. This is why we adopted $\beta_{\min} = 10 \text{ Pa} \cdot \text{s} \cdot \text{m}^{-1}$ as our reference value. We also note that a similar lower-bound approach on basal friction is implemented in other ice-sheet models.

The value used in the present study ($1.5 \times 10^{-6} \text{ m yr}^{-1}$) was adapted from the default values calibrated for Antarctic simulations in Quiquet et al. (2018), rescaled to obtain basal velocities of a realistic order of magnitude for the BKIS configuration. We acknowledge that systematically testing different C_f values could yield a more realistic ice-sheet geometry and a more informative comparison against reconstructions than permuting $\beta_{\min}$ alone. However, in our previous study (van Aalderen et al., 2024), we tested different basal sliding laws and consistently found a similar sensitivity of the ice sheet to atmospheric and oceanic forcings regardless of the specific friction formulation adopted. While we agree that a dedicated exploration of C_f would be valuable future work, we do not expect that variations in this parameter alter the main conclusions of the present study, which focus on the relative sensitivity of BKIS retreat to atmospheric versus oceanic forcing rather than the absolute LGM ice-sheet geometry.

We clarified in the manuscript:

Line 140 “where C_f is an internal constant parameter, adapted from the value calibrated for Antarctic simulations by Quiquet et al. (2018)”

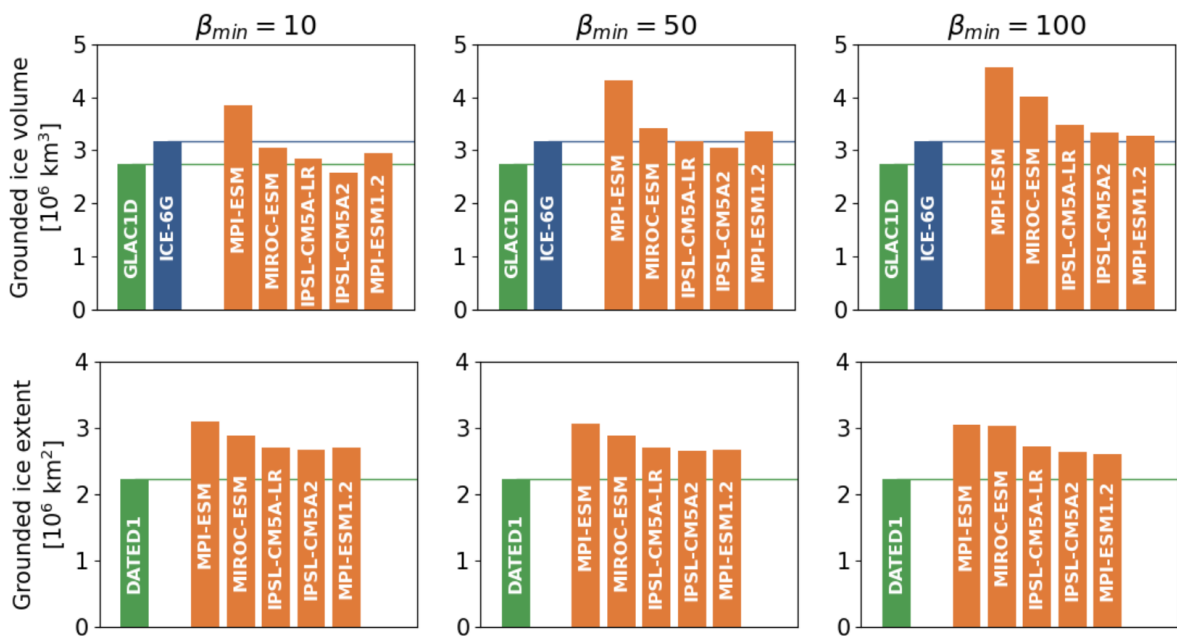


Figure S3: up: Ice volume simulated at the end of the 100.000 years of spin up simulations for different values of β_{min} compared to reconstructions. down: same as up but for ice extent.

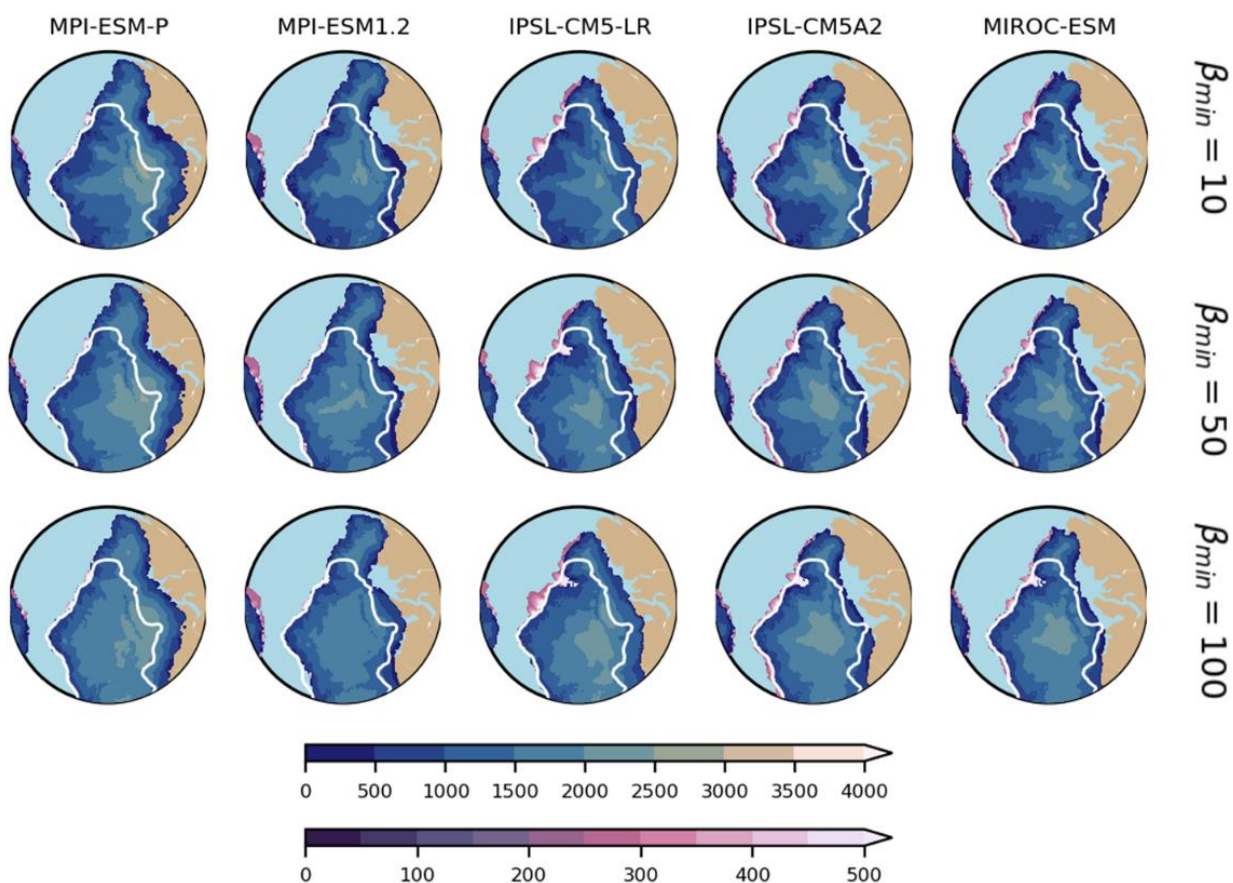
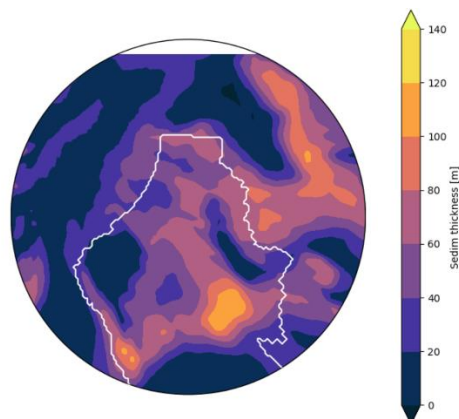


Figure S4: Initial ice thickness, in meter, of grounded ice (in blue) and ice shelves (in pink) for different values of β_{min} . The white line shows the DATED1 ice extent at the LGM.

187 You reduce friction over sediments, could you provide a spatial map of the sediment
 188 distribution used in the model? This would make it much clearer how and where friction is
 189 being reduced.

190 We have added a supplementary figure (Fig. S2) showing the spatial distribution of sediment
 191 thickness used to compute the C_{sedim} term of the basal friction coefficient (Section 2.1),
 192 overlaid with the LGM ice-sheet extent from the DATED-1 reconstruction



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194 **Figure S2:** Sediment thickness (C_{sedim}) in the BKIS region used to compute basal friction
 195 (see Section 2.1). The white line shows the LGM ice-sheet extent from the DATED-1
 196 reconstruction (Hughes et al., 2016).

197 **Climate index**

198 Experiments used to attribute the MISI trigger to atmospheric warming rely on the
 199 iLOVECLIM index because it "offers a better agreement with the reconstructions when used
 200 in the GRISLI reference simulations" (L480-482). However, the authors also note that the
 201 LGM ice sheet is likely too thick/too extensive compared to reconstructions and that
 202 iLOVECLIM "appear[s] to overestimate the warming at the onset of deglaciation (21-15 ka)"
 203 (L322-324). This is precisely the window in which the key early retreat (18-16 ka,
 204 Bjørnøyrenna) is simulated. Could the good agreement with reconstructed retreat timing
 205 partly reflect compensating errors? An oversized initial ice sheet requiring a warm-biased
 206 forcing to retreat on time, rather than iLOVECLIM correctly capturing the deglaciation
 207 trigger?

208 We thank the reviewer for raising this important question of compensating errors, which
 209 deserves careful consideration.

210 Regarding the apparent overestimation of warming by the iLOVECLIM index relative to
 211 Osman et al. (2021) at the onset of deglaciation (21–15 ka), we acknowledge this
 212 overestimation and agree it is worth being investigated further. However, we note that the
 213 Osman et al. (2021) reconstruction has very sparse proxy coverage at high northern latitudes
 214 and explicitly excludes records within present-day sea-ice extent. This means there is likely
 215 little to no direct proxy constraint over the Barents-Kara shelf itself. The reconstructed signal
 216 there is instead largely carried by long-distance covariance with more distant records. Given
 217 this, we consider the discrepancy between iLOVECLIM and Osman et al. (2021) over the
 218 BKIS region to be an open question rather than firm evidence that iLOVECLIM is biased
 219 warm, and we present it as such in the revised manuscript. We added in the manuscript:

(1363)“However, that the Osman et al. (2021) reconstruction has very sparse proxy coverage at high northern latitudes, and explicitly excludes records within present-day sea-ice extent. This means that there is likely little to no direct proxy constraint over the Barents-Kara shelf itself. The reconstructed signal in this region is instead largely carried by long-distance covariance with more distant records. We therefore consider the discrepancy between the iLOVECLIM index and the Osman et al. (2021) reconstruction over the BKIS region during this time period to be an open question warranting further investigation, rather than firm evidence that the iLOVECLIM index is biased warm at the onset of deglaciation. Osman et al. (2021) nonetheless still provides a useful indication of the overall amplitude of warming to be expected over this period, even if the precise regional signal over the BKIS itself should be interpreted with caution.”

Second, while we acknowledge that our LGM ice sheet overestimates ice extent in some sectors of the BKIS domain, the overall simulated LGM ice volume is in good agreement with the GLAC1D and ICE-6G_C reconstructions (Fig. S2). Importantly, in the Bjørnøyrenna region specifically (the sector where the early retreat (18–16 ka) discussed by the reviewer actually occurs) neither the simulated ice volume nor the ice extent at the LGM shows the systematic overestimation found elsewhere in the domain (see Figs. S3, S4). Since this is precisely the region driving the early deglaciation signal, we do not believe the good agreement in retreat timing in this specific sector can be attributed to a compensating error between an oversized initial ice sheet and a warm-biased forcing.

We agree this compensating error concern remains a legitimate consideration for other sectors of the BKIS where the initial ice-sheet extent is overestimated. This is precisely why we chose to focus our analysis on the early atmospherically-triggered retreat on the Bjørnøyrenna ice stream.

Sensitivity experiments

Instead of only plotting snapshots, it would be informative to also plot sea-level and ice-extent curves for your sensitivity experiments. I would also suggest using a quantitative metric to assess whether an abrupt change is genuinely occurring in your simulations, for example, the sea-level rise rate, or AR(1) coefficients

-We have added a new supplementary figure (Fig S14) showing the temporal evolution of ice extent and the rate of ice-extent loss in the Bjørnøyrenna region for the DEGLA-ATM-LGM, DEGLA-NOBMELT, DEGLA-NOBMELT-NOGL and DEGLA-NOSL experiments. This figure confirms the same ordering already discussed qualitatively in Section 3.3. The DEGLA-NOSL experiment (sea level at –1000 m) exhibits the slowest and least extensive ice loss than the other experiments. The DEGLA-NOBMELT-NOGL and DEGLA-NOBMELT experiments also lose ice slower and less extensive ice loss than the Reference experiment.

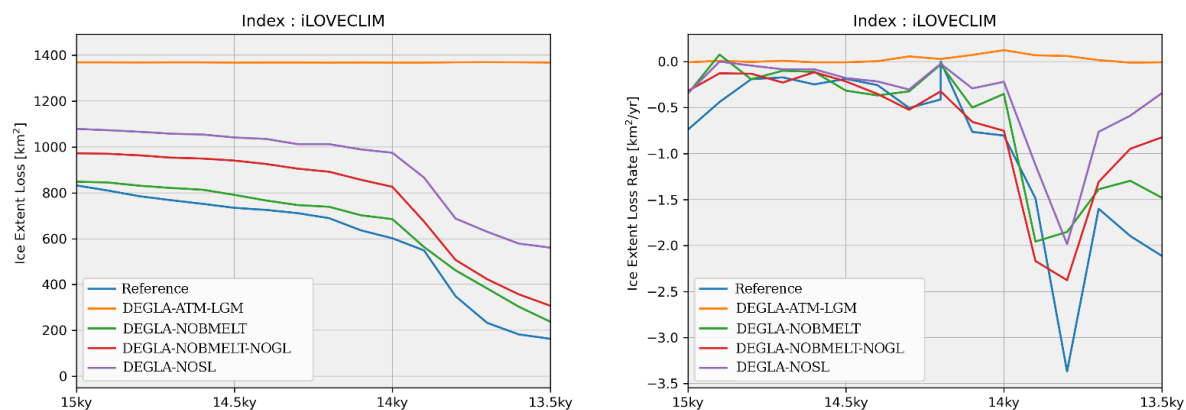


Figure 14: Left: Evolution of BKIS ice extent through time for the reference, DEGLA-ATM-LGM, DEGLA-NOBMELT, DEGLA-NOBMELT-NOGL and DEGLA-NOSL experiments (Section 3.3). Right: Same as left, but showing the rate of ice-extent loss through time for the same experiments.

You show snapshots starting at 15 ka BP, but I would be curious to see how the ice-sheet configuration looks at the LGM as well

-The LGM ice-sheet configuration for these simulations are already in the supp (Fig S4).

In your permutations, I am missing an experiment with oceanic forcing but without the flux condition. I think this would be more informative for isolating the role of flux conditions than permutation 4, since lowering sea level by 1000 m effectively creates a different environment altogether rather than isolating the flux effect. Also, in experiment 4 I am surprised that the simulated ice sheet is similar in extent, or even smaller, than some of the other permutations. Are you imposing a mask that prevents further extension?

-Regarding the extent of DEGLA-NOSL appearing similar to, or smaller than, other experiments despite the absence of basal melt and calving: ice extent in this experiment is entirely controlled by the surface mass balance, since basal melt, grounding-line flux, and calving are all eliminated. The ice sheet does not advance further because the SMB derived from the PMIP forcing is already negative over the continental shelf areas beyond the LGM ice margin (see figure below), preventing further southward or seaward advance. There is no additional imposed mask limiting extent.

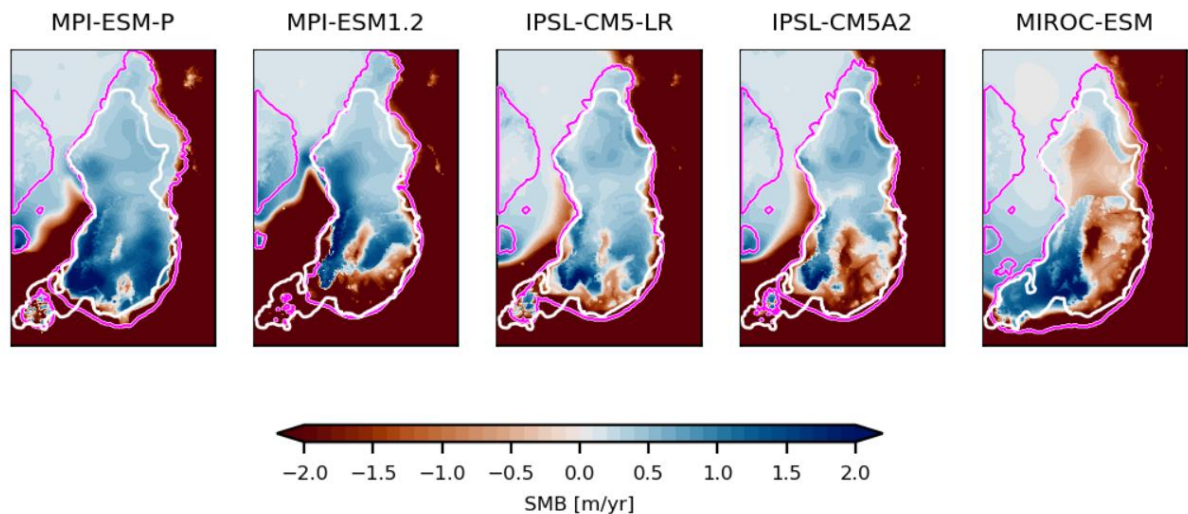
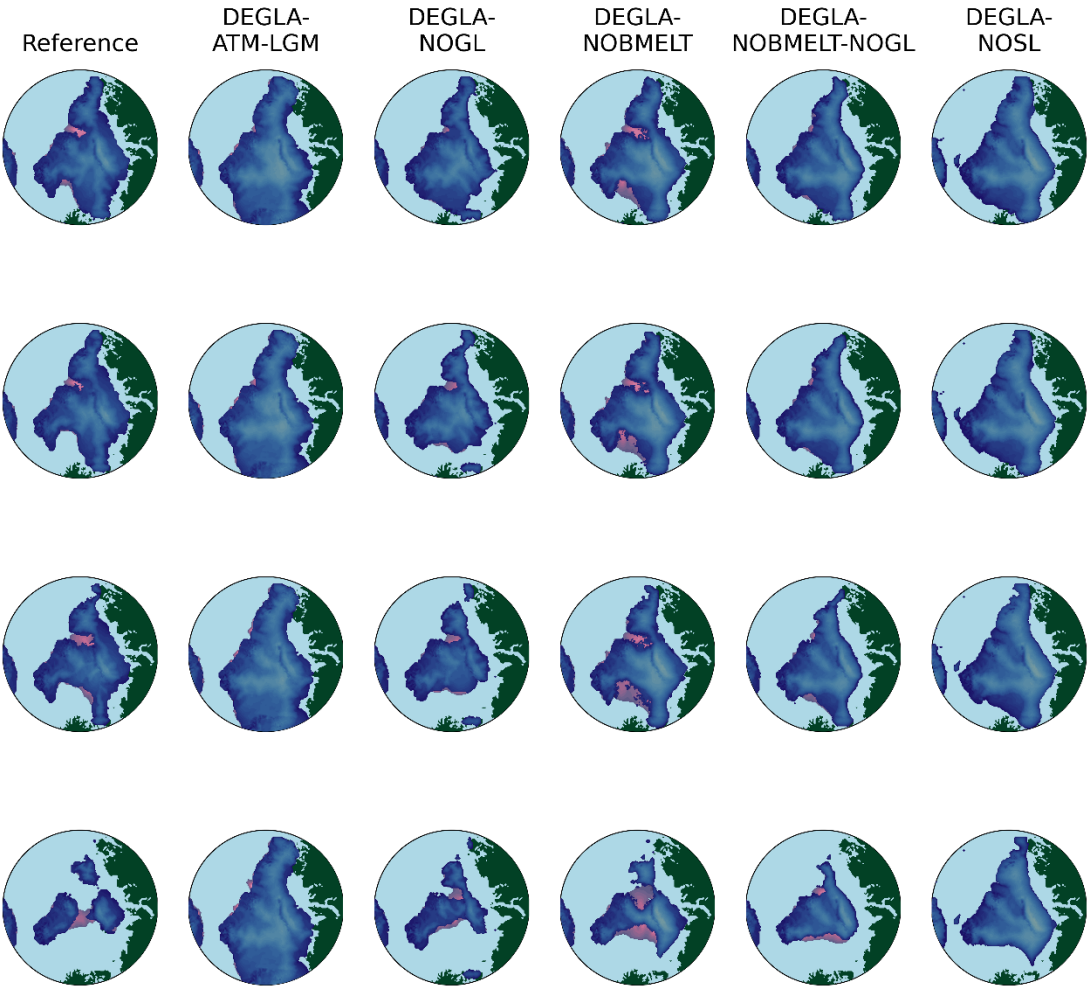


Figure: Surface mass balance calculated for an ice-free state using each GCM forcing. The white line indicates the ice extent from DATED1, while the pink line shows the ice extent at the end of the equilibrium state simulations.

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-Following the reviewer's suggestion, we conducted a new experiment (DEGLA-NOGL, Fig below), in which ocean basal melting is retained while the grounding-line flux parameterization is removed. As expected, comparison with the reference simulation indicates that the grounding line retreats slower and over a shorter distance in the Bjørnøyrenna sector. This suggests that, in our reference simulations, the grounding-line flux parameterization, allowing the model to simulate grounding-line instabilities, could be responsible for the rapid retreat observed in this region. However, as in DEGLA-NOBMELT-NOGL, we also observe a comparatively faster retreat in the Siberian sector of the BKIS. We attribute this behavior to the abrupt removal of a parameterization of the flux at the grounding line that was included in the construction of the initial-state, which introduces an additional perturbation beyond the intended isolation of the flux effect.

We then added this experiment in the supplementary (fig S17).



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Fig: Ice thickness (in meters) of grounded ice (blue shaded areas) and ice shelves (pink shaded areas) at different time steps using the iLOVECLIM index, the MPI-ESM1.2 forcing and $B_{min} = 10 \text{ Pa.s.m}^{-1}$ for the reference, the DEGLA-ATM-LGM, DEGLA-NOGL, DEGLA-NOBMELT, DEGLA-NOBMELT-NOGL and DEGLA-NOSL.

305 **Specific comments**

306 **Summary figure**

307 Although this is included in the supplementary material, I think it would be valuable to have
308 a figure in the main manuscript that summarizes the BKIS location alongside the
309 reconstructed ice extent. This would give readers a clearer visual sense of the ice sheet's
310 configuration. Also, you mention regions like Bjørnøyrenna but it is not really informative
311 without a figure summarizing these zones.

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313 Done, see figure 1 in the manuscript.

315 **Flux scheme**

316 Does your flux scheme include any buttressing coefficient?

317 Yes. As described in Quiquet et al. (2018), the Schoof (2007) grounding-line flux
318 parameterization used in GRISLI explicitly depends on the buttressing effect. This is
319 computed by calculating ice velocities twice (once with, and once without, the ice shelves)
320 and using the ratio of these two velocity fields to estimate the buttressing factor applied to
321 the grounding-line flux.

322 We clarified it in the manuscript as follow:

323 (l152)“The buttressing effect is accounted for in this parameterization by computing ice
324 velocities twice, with and without the ice shelves, and using the ratio of the two resulting
325 velocity fields to estimate a buttressing factor applied to the grounding-line flux (Quiquet et
326 al., 2018).”

328 **Oceanic climate index**

329 Does your index include depth?

330 Yes. The oceanic index (Eq. 4) is computed independently at each depth level of the ocean
331 column, so that the resulting transient ocean temperature and salinity forcing (Eq. 5) is also
332 depth-resolved, rather than applying a single depth-averaged or surface-only index
333 throughout the water column. We have clarified this point in the manuscript:

334 (l303)“The oceanic index is computed independently at each depth level within the ocean
335 column, resulting in a is depth-resolved transient ocean forcing (Eq. 5) rather than applying
336 a single depth-averaged index uniformly across the water column.”

337 **L41**

338 "sensitive to oceanic condition variations", suggest simplifying to "sensitive to oceanic
339 variations."

340 Done

342 **L53**

343 "can only be tempered by ice-shelf buttressing", I am not sure about this statement. I think
344 that high accumulation could also avoid MISI (Feldmann et al., 2019).

346 We adjusted the manuscript as follows:

347 (l52)“The MISI is a self-sustained instability that can be tempered by ice-shelf buttressing
348 (Schoof, 2012; Goldberg et al., 2009), or halted once the grounding line reaches a prograde

bed slope, encounters changes in basal friction beneath the grounded ice or by high accumulation rates (Feldmann et al., 2019)”

L70

Which type of biases? A more stable grounding line, you mean?

Yes, that is correct. We meant that the absence of a proper representation of grounding-line flux dynamics likely leads to a poorly represented retreat of the grounding line, which may have affected the relative contribution attributed to oceanic forcing in these studies.

We added in the manuscript:

(l69)“Both studies, however, do not include a representation of grounding-line dynamics. Without it, the retreat of the grounding line is likely to be poorly represented, which may have affected the relative contribution attributed to oceanic forcing in these studies.”

L136

"inlcuding" (typo)

Done

L154

You cite van Aalderen et al. (2024) as showing the lapse rate "plays only a very minor role," which is why it is fixed at 7 °C/km. However, in Fig. 6b, removing the lapse-rate/elevation feedback appears to roughly double the residual ice volume by the end of the -14.5 ka stabilization experiment compared to the standard 7 °C/km run. This does not look like a minor effect

We thank the reviewer for pointing out this apparent inconsistency. We agree that our original wording was misleading. Our previous study showed that the lapse rate has only a minor influence on the nature and occurrence of the processes driving BKIS retreat (i.e., whether atmospheric warming can trigger the deglaciation of BKIS), but not necessarily on the resulting dynamics or magnitude of ice loss once retreat is underway. As the reviewer correctly notes, our own results in Fig. 6b show that the lapse rate can have a substantial impact on the ice volume evolution through the elevation-temperature feedback, which amplifies the retreat once it is triggered. We have revised the corresponding sentence in the Introduction to reflect this distinction:

(l171)“While van Aalderen et al. (2024) show that the lapse rate can influence the dynamics of BKIS retreat, it does not affect whether atmospheric warming is able to trigger it in the first place. In order to limit the number of varying parameters in this study, we therefore fixed the lapse rate to a representative average value of 7 °C km⁻¹ in our reference configuration.”

L281

I do not think that your precipitation is calculated the same way, as this would imply potentially negative precipitation rates. Probably you are scaling with the index, right?

The reviewer is correct. The same temperature-based (for precipitation) or oceanic-based (for salinity) index is used to scale precipitation and salinity between their LGM and PI values, following the same interpolation approach as in Eq. 5. We modified the manuscript as follows:

(1320) “Salinity and precipitation are calculated using the same index as computed from atmospheric and oceanic temperatures, respectively”

Figure 2

In Panel d you have a plateau behaviour between 17-14 ka BP. It looks saturated around -3, probably because you are not allowing it to freeze more?

Yes, that is correct. As noted in Section 2.2.3.1, the ocean temperature is bounded at the local freezing point, which prevents it from cooling further once this threshold is reached. In addition, the climate index itself is constrained between -2 and 2 (Section 2.2.3.1) to prevent unrealistic numerical overshoots, which could also contribute to the apparent plateau visible in this panel between ~ 17 and 14 ka

Figure 3

Your Figure 3 shows the relative ice thickness and ice area loss, however this comparison is tricky since you do not have the same extent nor ice volume value. I would put absolute values.

Done, and added in the supplement (Fig. S6).

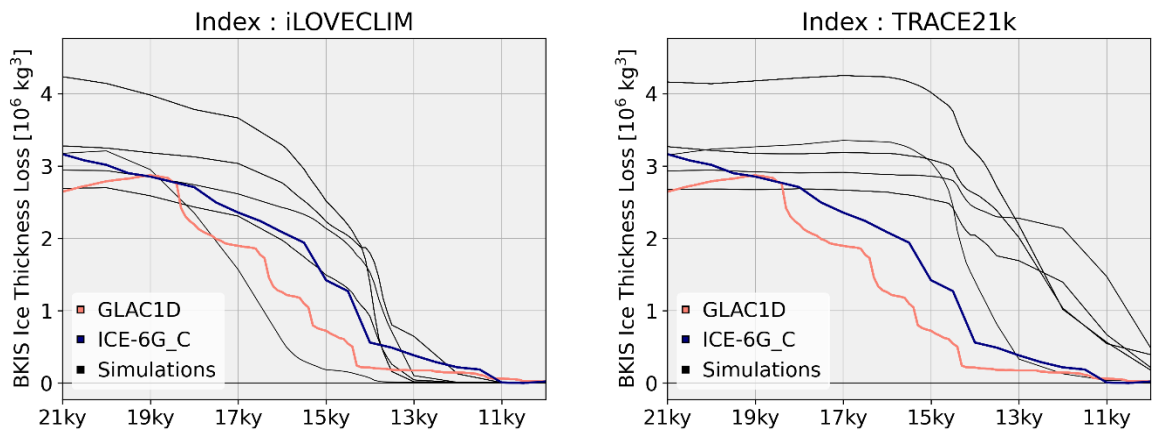


Figure S6: left: Ice volume loss in the BKIS region, for each of the five GCM-forced simulations using the iLOVECLIM index (black lines) compared to the GLAC1D (red) and ICE-6G_C (blue) reconstructions. right: Same as left, but with the TRACE21k index.

L546

"Although ice loss continues, mainly due to the temperature-elevation feedback", it could be just due to the drift and that you are not yet in an equilibrated state, right?

The continued ice loss after climate stabilization at 15.5 ka indeed reflects the ice sheet the progressive re-equilibration to the newly stabilized climate. This is done through the elevation-temperature feedback as well as other physical parameters (e.g., ice viscosity, basal friction) that continue to adjust as the ice-sheet geometry evolves toward a new equilibrium state. We corrected the manuscript as follows:

(l601)“When atmospheric warming is halted at 15.5 ka, the increase in surface melt is insufficient to trigger an abrupt retreat of the BKIS. Ice loss continues as the ice sheet progressively re-equilibrates to the newly stabilized climate. This is achieved, through the elevation-temperature feedback and other physical parameters (e.g., ice viscosity, basal friction) that continue to adjust as the ice-sheet geometry evolves. The response nonetheless remains moderate and no destabilization occurs”

Figure 6

You state at L562-569 that when removing the lapse rate factor, your ice sheet still retreats due to MISI, but the simulated curve is different. To me it looks like MISI is not present. If so, then the melt-elevation feedback could be the trigger for MISI.

We show in Fig. S15 that rising atmospheric temperatures place the grounding line in an unstable position, triggering a MISI. However, we also show that the bed is not strictly retrograde throughout. A local bedrock high (point "y", Fig. S15) interrupts the retrograde slope, and the grounding line can stabilize again, temporarily halting the instability. In the zero-lapse-rate experiment, once atmospheric warming stops, the grounding line stabilizes on this bump rather than continuing to retreat. As a result, the grounding line only crosses

this bump and re-enters an unstable configuration, allowing MISI to resume, once atmospheric forcing drives sufficient additional thinning. We note, however, that the zero-lapse-rate configuration is an idealized case designed to isolate this feedback specifically. In our reference experiments, it is the combined contribution of both the elevation-temperature feedback and continued warming from the forcing itself that places the grounding line in an unstable configuration (i.e., on the retrograde slope).

L689

"Increasing the lower bound of the basal friction coefficient reduces ice sliding", I agree that increasing friction reduces ice sliding, but for me your friction coefficient is c_f and not $\beta_{\min}$. $\beta_{\min}$ is a lower limit to avoid high velocities due to numerics. The way I understand this is that you are not reducing the friction, that would mean reducing c_f ; you are limiting your friction. Also, instead of 2D maps I would appreciate seeing the temporal evolution of ice volume for different $\beta_{\min}$ values over your whole experiment.

We agree with this comment and modified the manuscript has follow:

Line 806 "Compared to a low $\beta_{\min}$ value, increasing the lower bound of the basal drag coefficient leads to higher minimum basal friction, and therefore to lower ice velocities. Overall, the results indicate that the value of $\beta_{\min}$ has only a limited impact on the BKIS dynamics, as these additional simulations exhibit the same behavior than the references simulations."

The figure below (added in the supplementary, fig S18) shows the temporal evolution of BKIS ice volume for each of the tested $\beta_{\min}$ values (10, 50, 100 Pa·s·m⁻¹) over the full deglaciation experiment. This complements the 2D ice-thickness maps already shown in Figs. S3-S4, S8-S9.

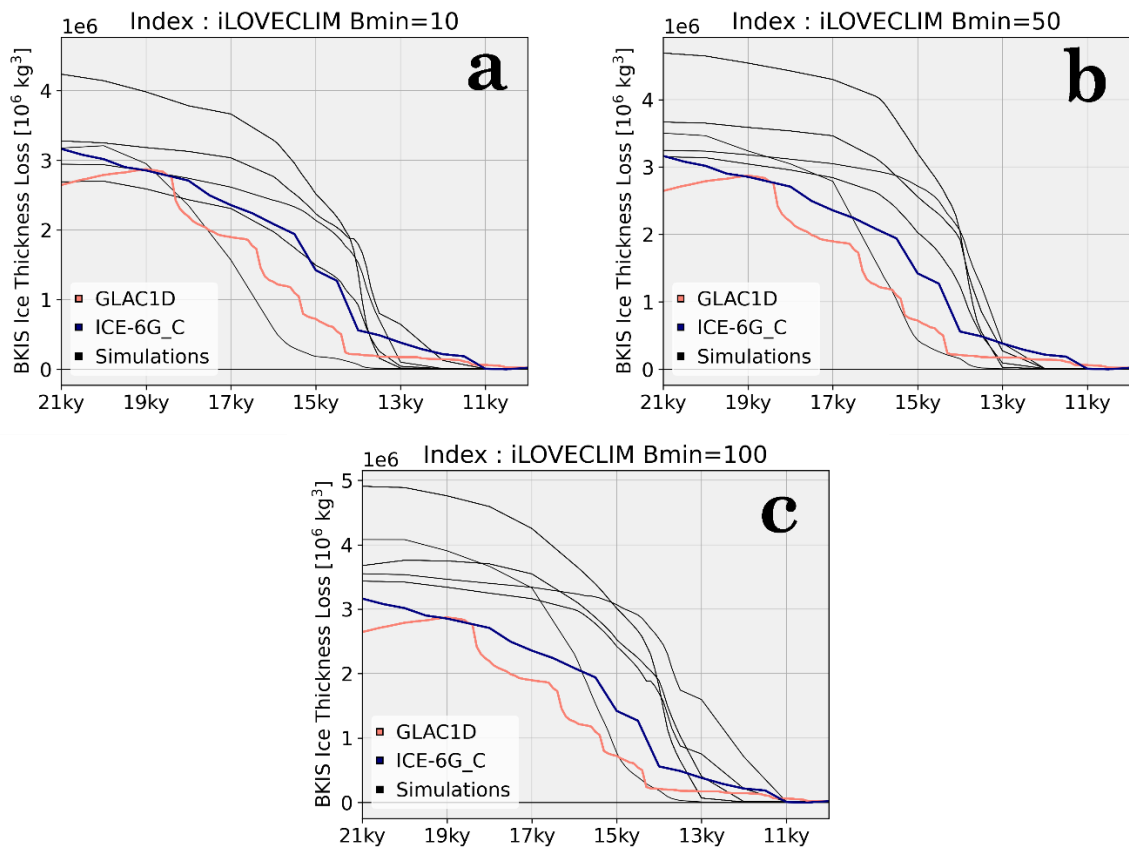


Figure: a: Ice volume loss in the BKIS region, for each of the five GCM-forced simulations using the iLOVECLIM index (black lines) compared to the GLAC1D (red) and ICE-6G_C (blue) reconstructions with $\beta_{\min}=10$. b: Same as a, but with $\beta_{\min}=50$. c: Same as a, but with $\beta_{\min}=100$.

L744

"Each of this steps", typo, should read "Each of these steps."

Done

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