

The authors sincerely thank the RC1 for his helpful and constructive comments. All comments have been carefully considered in the revised manuscript. Our responses are written in blue, and excerpts from the manuscript are italicized. Line numbers refer to the revised manuscript.

## Summary

The Barents-Kara Ice Sheet (BKIS) was a mostly marine-based remnant of the larger Eurasian ice sheet, that existed during the last deglaciation. Geomorphological evidence shows that this ice sheet disintegrated rapidly around 14,000 years ago, with worrisome implications for the future of the West Antarctic ice sheet.

The authors present results of a number of simulations of the deglacial evolution of the BKIS, performed with the GRISLI2.0 numerical ice-sheet model. By including/excluding atmospheric forcing, oceanic forcing, and ice shelves, they investigate the relative importance of these different processes in driving the retreat of the BKIS. Based on their findings, they argue that sudden atmospheric warming provided the initial trigger for the retreat, which was then amplified by a combination of ocean-induced basal melt, and self-reinforcing retreat of the grounding line along the retrograde marine bed of the (isostatically depressed) Barents-Kara Sea, which the authors attribute to the Marine Ice-Sheet Instability (MISI).

## General comments

I find the topic of this study interesting. Paleoglaciological evidence for rapid ice-sheet retreat can be very valuable for studying the future evolution of the Greenland and Antarctic ice sheet. Process-based studies like this one can be particularly useful.

Generally, the paper is well-written and easy enough to follow. There are some instances where the technical details of the methodology are not clear, but nothing that I think cannot be easily remedied.

However, I do have some concerns about the numerical model that the authors used, and its applicability to this particular study.

GRISLI2.0 was presented in 2019 by Quiquet et al. That study does not include any of the idealized-geometry experiments that have, in the last decade or so, become de facto benchmarks for numerical ice-sheet models (e.g. EISMINT, ISMIP-HOM, MISMIP, MISMIP+, MISOMIP, ABUMIP). The current study revolves around the concept of MISI, which is now known to be particularly difficult to represent in numerical models. While the model described by Quiquet et al., having a 20-km square grid and a grounding-line flux condition to circumvent the inaccurate velocity solution, should in principle be able to produce reasonably accurate grounding-line dynamics, they do not provide any evidence of this. Having spent the last few years working on ice-sheet model development, I am only too painfully aware of how easy it is to make mistakes when building something so complicated, and how important it therefore is to perform these benchmarks regularly. Without evidence of how GRISLI performs relative to other ice-sheet models in these standardized experiments, I find it difficult to assess the results presented in this manuscript.

We thank the reviewer for this important point.

GRISLI has been shown to be able to reproduce past variations in grounding-line position at paleo timescales. In particular, Quiquet et al. (2018) and a more recent study (van Aalderen et al., 2026, in preparation, preprint available at <https://doi.org/10.22541/essoar.176833267.78739367/v1>) show that the grounding-line flux parameterization used in GRISLI allows the model to reproduce large-scale glacial-interglacial ice-volume cycles, including grounding-line retreat during deglaciation, notably for Antarctica.

Moreover, GRISLI has produced results comparable to those of other ice-sheet models in the model intercomparison projects it has participated in, including ISMIP6, ABUMIP and initMIP. In particular, a 16 km-resolution version of GRISLI, using the same grounding-line flux parameterization used in this study, participated directly in ABUMIP (Sun et al., 2020), which tests the sensitivity of ice-sheet models to a complete and sustained loss of ice-shelf buttressing. In this exercise, GRISLI simulated substantial grounding-line retreat, comparable in nature to the other participating models.

We are, however, fully aware that the Schoof parameterization used here can become numerically unstable, particularly at finer resolutions where the computed flux is more sensitive to local gradients. For this reason, we have, over the past several months, begun benchmarking GRISLI against the MISIMIP+ (<https://doi.org/10.5194/tc-14-2283-2020>) idealized-geometry experiments. We present preliminary results from this ongoing study (Fig. RP 1), showing that GRISLI reproduces a reasonable grounding-line retreat under the standard MISIMIP+ protocol, providing direct model-intercomparison evidence to support the grounding-line dynamics discussed in this study.

However, we chose not to include these results and this discussion in the manuscript, as we consider them somewhat beyond the scope of this study

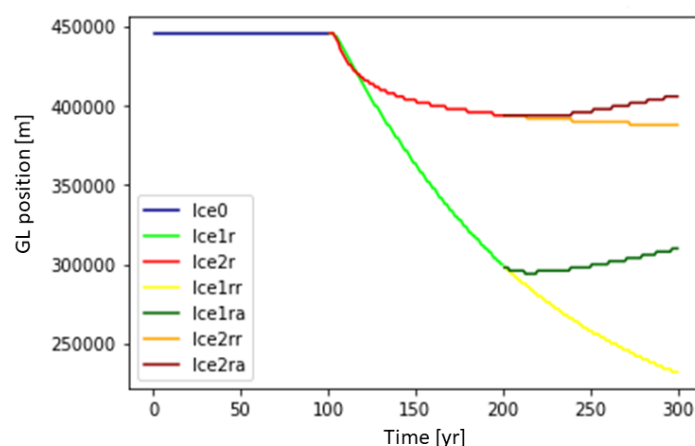


Figure RP 1: Evolution of grounding-line position (y-axis, in meters) as a function of time (x-axis, in years) for the different MISIMIP+ experiments (Cornford et al., 2020). Ice0 is a steady-state control run with no basal melting; Ice1r applies an idealized basal melt rate under the ice shelf for 100 years, forcing grounding-line retreat; Ice1rr continues Ice1r for a further 100 years with the same melt rate; Ice1ra continues from the end of Ice1r with melting set to zero,

allowing the grounding line to readvance; Ice2r applies a stronger, spatially uniform melt rate mimicking a calving event, over 100 years; Ice2rr continues Ice2r with the same melt rate; and Ice2ra continues from the end of Ice2r with melting set to zero, allowing readvance.

The same holds for the basal melt parameterization. In lines 677-680, the authors state that “ocean temperatures were artificially increased by 10 °C, [...] producing basal melt rates at the grounding line [...] on the order of 30 m yr<sup>-1</sup>.” In Antarctica, basal melt rates of over 50 m/yr are observed in the Amundsen Sea region, with ocean temperature that are nowhere near as warm as what you’re describing. This suggests that the basal melt parameterisation in GRISLI is not tuned correctly, and produces values that are too low. One of the main conclusions of this manuscript is that ocean-induced basal melt plays only a secondary role in the collapse of the BKIS, which would be undermined if there is indeed an error in this parameterization.

First, we would like to point out that, to our knowledge, there is no data allowing the reconstruction of basal melt rates along the BKIS during the last deglaciation. This makes it hard to properly calibrate this parameterization for our specific region and period. Without such data, we used the default transfer coefficient proposed by Pollard and DeConto (2012), the same one used in our reference simulations.

Comparing our results to other existing simulations of this region is also difficult, because very few studies provide basal melt or basal mass balance output that we can directly compare to ours. One exception is Scherrenberg et al. (2024), who made their model outputs publicly available, including for the last deglaciation. Their baseline simulation gives basal mass balance rates between 2 and 10 m yr<sup>-1</sup> in the BJR region, during the last deglaciation, close to the values from our own reference simulation ( $K_t = 7 \text{ m yr}^{-1} \text{ K}^{-1}$ ). Another point of comparison is Álvarez-Solas et al. (2019), who simulated the Eurasian ice sheet (including the Barents-Kara sector) using a linear basal melt parameterization, with a transfer coefficient ( $K_t = 5 \text{ m yr}^{-1} \text{ K}^{-1}$ ) comparable to the one used in our reference simulation ( $K_t = 7 \text{ m yr}^{-1} \text{ K}^{-1}$ ). However, because their parameterization is linear rather than quadratic in the ocean-ice temperature difference, for a comparable warming above 1°C (which is the case throughout the last deglaciation) their scheme gives lower basal melt than our reference simulation. Taken together, these two comparisons show that our simulated basal melt rates fall broadly within, or even above, the range of basal melt rates simulated under BKIS ice shelves in other studies.

We do acknowledge that present-day Antarctica shows area-averaged basal melt rates of up to ~50 m yr<sup>-1</sup> in the Amundsen Sea sector (Rignot et al., 2013). To test whether the BKIS could be sensitive to oceanic forcing at melt rates of this size, we increased the transfer coefficient  $K_t$  step by step ( $K_t = 30, 50, 70, 90$  and  $110 \text{ m yr}^{-1} \text{ K}^{-1}$ , compared to  $7 \text{ m yr}^{-1} \text{ K}^{-1}$  in the reference case), using only the MPI-ESM1.2 forcing. This lets us find a threshold above which the ocean starts to have a real effect on BKIS retreat. Below this threshold, increasing  $K_t$  raises basal melt under the ice shelves (BJR region) but does not cause significant ice loss. For example, the  $K_t = 70 \text{ m yr}^{-1} \text{ K}^{-1}$  simulation already gives a time-averaged basal melt rate above 60 m yr<sup>-1</sup>, without triggering a major retreat (apart from a small early loss of part of the ice shelves at the start of the deglaciation, linked to the sudden change in initial conditions, and the start of a

retreat in the Siberian sector around 12 ka). A clear retreat in the BJR sector only appears once grounding-line melt rates go above  $\sim 80 \text{ m yr}^{-1}$ .

This threshold is clearly higher than the  $\sim 50 \text{ m yr}^{-1}$  area-averaged melt rates typical of the Amundsen Sea. It therefore represents a very high basal melt value compared to what we observe in Antarctica today. Still, given the large uncertainty in the reconstructed BKIS ocean warming during this period ( $+1$  to  $+7^\circ\text{C}$ ; Annan et al., 2022; Osman et al., 2021) and the lack of any direct constraint on past basal melt rates, we cannot rule out that such conditions occurred at the BKIS during the last deglaciation. Importantly, this result does not go against the main message of the manuscript which it stays fully consistent with the idea that atmospheric warming can play the main role in the retreat of the BKIS.

For all these reasons, and because it is difficult to properly calibrate basal melt for the BKIS, we have revised the manuscript (including abstract) so as not to rule out a possible role for the ocean in the BKIS retreat, while keeping our main claim focused on the role of the atmosphere in probably triggering MISI, and specifically in driving the retreat of the BKIS.

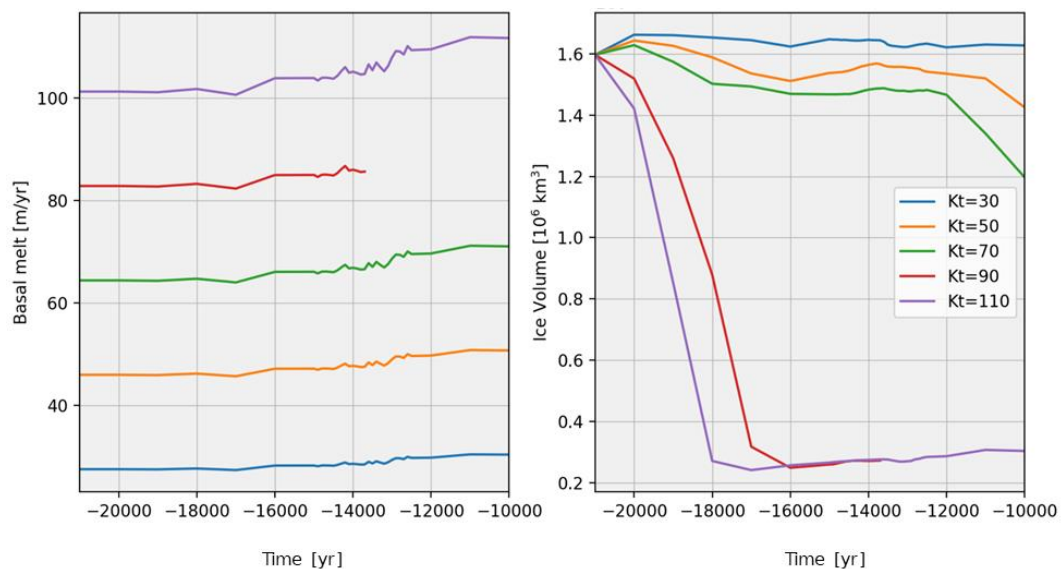


Figure S16: Left: Evolution of basal melt (BJR region) through time for the sensitivity experiments varying  $K_t$  between 30, 50, 70, 90 and  $110 \text{ m yr}^{-1} \text{ K}^{-1}$ , forced by MPI-ESM1.2. Right: Same as left, but for the BKIS ice volume.

we added this new result in the abstract and in the discussion as follow and we also changed the title:

Title: «From Surface Processes to Marine Ice-Sheet Instability: A Plausible Scenario for the Collapse of the Barents–Kara Ice Sheet during the Last Deglaciation»

Abstract:” During the last deglaciation, the Barents-Kara Ice Sheet (BKIS), a marine-based sector of the Eurasian Ice Sheet, was subject to a drastic retreat over only a few centuries. While the timing of the BKIS deglaciation is well documented, the mechanisms driving the ice-sheet retreat remain debated. Using the GRISLI2.0 ice sheet model, we investigate the behavior

of BKIS during this period and identify the marine ice sheet instability (MISI) as the primary driver of the BKIS collapse. While ocean-induced basal melting is often considered the primary trigger of MISI, largely due to a focus on the West Antarctic Ice Sheet in the existing literature, our results show that atmospheric warming can also directly trigger such a dynamic instability, and therefore be responsible for the retreat of the BKIS. Our results highlight the combined roles of atmospheric and oceanic forcings, with atmospheric warming triggering the initial retreat at the onset of the deglaciation and oceanic processes subsequently controlling its dynamics. We therefore encourage future studies on marine ice sheet instability to consider variations in atmospheric conditions and their impact on ice-sheet destabilization..”

*Discussion (1749): “Our relatively weak sensitivity of the BKIS to oceanic warming contrasts with previous modeling studies (Petrini et al., 2020; Alvarez-Solas et al., 2019). Differences in basal melting parameterization between ice-sheet initialization and deglaciation experiments in those studies may partly explain the stronger ocean-driven retreat reported in these studies as explained in van Aalderen et al. (2024). In the present work, besides applying a consistent basal melting parameterization between the equilibrium initialization and the deglaciation simulations, we performed experiments in which atmospheric and oceanic perturbations were imposed separately (see Figure 6). This experimental design allows a clearer identification of the processes controlling BKIS retreat, in contrast to Petrini et al. (2020) where both forcings were varied simultaneously.*

*However, it is well established that increasing ocean temperatures can have a significant impact on marine-based ice sheets (Pritchard et al., 2012), and the choice of basal melt parameterization may affect the ice sheet sensitivity to oceanic variations (Burgard et al., 2022). In particular, sub-shelf melt rates depend not only on ocean temperature and salinity but also on the ocean circulation within the ice-shelf cavities. Simple parameterizations based only on the thermal forcing, such as the one used in this study, do not account for these processes, whereas more physically based parameterizations explicitly represent the overturning circulation in the sub-shelf cavities (Reese et al., 2018; Ockenden et al., 2026). But, this would not affect our main conclusion, namely that atmospheric warming may have played an important role in the BKIS retreat by triggering a MISI through surface processes, since this result is obtained in experiments in which the ocean forcing is kept at LGM conditions.*

*Our low sensitivity of BKIS to a change in oceanic condition could be due to a poor calibration of the basal melt parameterization. Unfortunately, there is no data allowing the reconstruction on the basal melt rates that likely occurred at the BKIS during the last deglaciation, which makes this parameterization difficult to properly calibrate for our specific region and period. In the absence of such data, we followed the default transfer coefficient ( $Kt = 7 \text{ m yr}^{-1} \text{ K}^{-1}$ ) proposed by Pollard and DeConto (2012) and may underestimate the basal melt rate simulated in the BJR region. As a comparison with other modelling studies, Scherrenberg et al. (2024) report basal mass balance rates of  $-2$  to  $-10 \text{ m yr}^{-1}$  in the Bjørnøyrenna region for their baseline simulation, close to those of our reference simulation, while Álvarez-Solas et al. (2019) used a comparable transfer coefficient ( $Kt = 5 \text{ m yr}^{-1} \text{ K}^{-1}$ ), which gives lower melt rates than our scheme for the warming levels reached during deglaciation. Our simulated basal melt rates therefore fall broadly within, or above, the range reported by other studies of the BKIS.*

To further test whether the weak sensitivity of the BKIS to oceanic forcing could instead reflect an underestimation of the simulated basal melt rates, we performed an ensemble of sensitivity tests with the MPI-ESM1.2 forcing, in which basal melt rates were progressively increased by raising the transfer coefficient  $K_t$  (Eq. 3) from  $7 \text{ m yr}^{-1} \text{ K}^{-1}$  in the reference simulations to 30, 50, 70, 90 and  $110 \text{ m yr}^{-1} \text{ K}^{-1}$ . These tests were motivated by the high melt rates ( $> 50 \text{ m yr}^{-1}$ ) documented in the Amundsen Sea, Antarctica (Rignot et al., 2013). No significant retreat is simulated for melt rates of up to  $\sim 70 \text{ m yr}^{-1}$  in the BJR sector, comparable to the highest local melt rates observed near the grounding lines of Amundsen Sea glaciers. A retreat emerges only above  $\sim 80 \text{ m yr}^{-1}$ , and occurs then between 20 and 17 ka, too early compared with reconstructions. Given the large uncertainty in the reconstructed ocean warming over the BKIS region during the last deglaciation ( $+1$  to  $+7^\circ\text{C}$ ; Annan et al., 2022; Osman et al., 2021) and the absence of direct constraints on past basal melt rates, we cannot formally exclude that such conditions were met. Nonetheless, this does not change our main conclusion: atmospheric warming alone can trigger MISI and reproduce the observed BKIS retreat."

### Specific comments

Line 17: "MISI is primarily initiated by ocean-induced basal melting" The way I've always understood it is that the instability is an inherent property of an unbuttressed ice sheet grounded below sea level on a retrograde slope. This produces an unstable equilibrium, where any perturbation, be it from changes in the surface mass balance, basal melt, or the thermal regime of the ice (as in the MISIP experiments that studied MISI), can result in self-sustaining retreat. A lot of recent literature about MISI focuses on West Antarctica, where the causal chain of oceanic warming, increased sub-shelf melt, thinning shelves, and loss of buttressing is the prime suspect for causing grounding-line retreat in the future – but that does not mean that MISI is, by itself, an ocean-induced process.

We agree with the reviewer and thank him for this important clarification. We have revised the abstract following the remark:

(117-18) "While ocean-induced basal melting is often considered the primary trigger of MISI, largely due to a focus on the West Antarctic Ice Sheet in the existing literature,"

Line 47: Please include here a reference to Seroussi et al. (2024), the most recent and comprehensive model ensemble for the future evolution of the Antarctic ice sheet.

Done

Line 48: The article by Mercer (1970) doesn't contain anything related to MISI. The first publication to delve into this is Weertman (1974).

We agree that Mercer (1970) did not himself develop the MISI mechanism, and we have corrected the manuscript accordingly, replacing this reference with Weertman (1974).

Line 50: "positively correlated" suggests a relation deduced from statistics, please rephrase.

We agree that "positively correlated" is not the appropriate terminology here. We have changed the manuscript as follow:

(148) "This instability is related to the fact that the ice flux at the grounding line increases with the ice thickness."

Line 53: “can only be tempered by ice-shelf buttressing” Well, or until the grounding line reaches a prograde slope, or the friction underneath the grounded ice can change.

We have revised the manuscript accordingly:

*(l51)“The MISI is a self-sustained instability that can be tempered by ice-shelf buttressing (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)”*

Line 66: There have been other studies that simulated the evolution of the entire Eurasian ice sheet complex during the last glacial cycle, using models that *did* include proper grounding-line dynamics (e.g. Scherrenberg et al., 2024), although they did not focus specifically on the Barents-Kara sector.

We thank the reviewer for pointing out this omission. We have added a reference to Scherrenberg et al. (2024):

*(l81-84)“Other studies have simulated the evolution of the entire Eurasian ice sheet complex over the last glacial cycle using models that do include proper grounding-line dynamics (Scherrenberg et al., 2024), although these did not focus specifically on the Barents-Kara sector.”*

Line 69: Please also cite Schoof (2007), who derived the first semi-analytical solution to the grounding line flux, and Pollard and DeConto (2009), who were the first to implement it into a numerical model.

We thank the reviewer for this suggestion. We have added these references in the introduction:

*(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. Such dynamics rely on the semi-analytical solution to the grounding-line flux derived by Schoof (2007), first implemented in a numerical ice-sheet model by Pollard and DeConto (2009), and later refined by Tsai et al. (2015).”*

Line 121: “we impose a linear basal friction law” Please discuss why you chose this rather unusual sliding law; most ISMIP models currently use either an  $n=3$  (or equal to whatever exponent they use for their constitutive equation) power-law, a constant-friction Coulomb law, or a ‘pseudo-plastic’ hybrid between the two.

A non-linear ( $n = 3$ ) basal friction law is available in GRISLI. However, since GRISLI2.0 (Quiquet et al., 2018), all model testing and calibration has been carried out using a linear ( $n = 1$ ) friction law, and the model is therefore well tuned and documented for this configuration. In our previous study (van Aalderen et al., 2024), we also tested an  $n = 2$  friction power-law and found no difference in the sensitivity of the ice sheet to atmospheric and oceanic forcings compared to the  $n = 1$  configuration used here.

Line 132: “to prevent excessively fast ice flow” Since this parameter is crucial to some of your experiments, I’d like to see some more discussion here. Please define what constitutes “excessively fast” ice flow, also considering observed velocities of extant ice streams in

Greenland and Antarctica. The current phrasing suggests that this is purely a ‘numerical’ parameter, intended to reduce artefacts in the velocity solution. However, later on, you interpret it as a ‘physical’ parameter, which you use to study the sensitivity of your modelled ice sheet to basal sliding.

We thank the reviewer for this remark. We tested a value of  $\beta_{\min} = 0.1$ , which produced basal velocities exceeding  $7000 \text{ m yr}^{-1}$  at some locations along the Bjørnøyrenna ice stream, compared to velocities on the order of  $2000\text{--}3000 \text{ m yr}^{-1}$  for  $\beta_{\min} = 10$ . "Excessively fast" ice flow here refers to this kind of behavior: without a sufficiently high lower bound, the model fails to produce a stable ice sheet, as basal velocities near the margins become unrealistically high, and a limit is therefore required for the model to remain numerically stable. A similar approach is used in other ice-sheet models. For instance, other ice sheet models apply a comparable control by imposing a minimum value on the effective pressure at the ice base, set to a small fraction (4%) of the ice overburden pressure, which in turn places a corresponding lower bound on basal friction. In our model, this is achieved directly through  $\beta_{\min}$ . Later on in the manuscript, we test the sensitivity of our results to different values of  $\beta_{\min}$  (which makes it possible to reproduce velocities comparable to present-day analogues). This parameter does not appear to be critical to our main conclusions. While different  $\beta_{\min}$  values yield LGM ice sheets in varying degrees of agreement with the reconstructions, the retreat of the ice sheet driven by atmospheric warming is consistently reproduced across all tested values.

We corrected the manuscript as follow:

*Line 144 “We fixed a lower bound of the basal drag coefficient,  $\beta_{\min}=10 \text{ Pa.s.m}^{-1}$ , to prevent excessively fast ice flow (i.e. for lower  $\beta_{\min}$  velocities exceeding  $\sim 7000 \text{ m yr}^{-1}$ ) and ensures the preservation of grounded ice in warm-based areas. For ice shelves, basal friction is neglected.”*

Line 138: I am confused. Tsai (2015) derived their grounding-line flux solution using a Coulomb sliding law; Quiquet et al. (2018) state that, when GRISLI uses a power-law sliding law, it uses Schoof’s (2007) grounding-line flux solution. The combination you describe in the text here (power law + Tsai solution) is not consistent; is this what you did, or is your text not correct?

We thank the reviewer for this remark and agree that our description of the friction law was confusing. In this study, we use a linear Budd friction law (i.e.  $q = 1$  and  $m = 1$  in the formulation of Brondex et al., 2017). We have corrected this throughout the manuscript (line 134). Regarding the grounding-line flux, Tsai et al. (2015) derived their solution for a Coulomb friction law. However, to our knowledge, there is no evidence in the literature against combining a Budd friction law with the Tsai et al. (2015) grounding-line flux parameterization.

Line 151-156: Does this mean that your model set-up does not include any large-scale feedback of ice geometry changes onto the climate, but only a local lapse rate-based adjustment? And no adjustment of the precipitation?

Our model set-up does not include any large-scale feedback of ice-sheet geometry changes onto the regional or global climate (e.g., via changes in atmospheric circulation). The only adjustment applied is a local, lapse-rate-based correction of surface air temperature to account

for differences between the PMIP model orography and the GRISLI ice-sheet topography (see Section 2.2.1). Precipitation is similarly adjusted locally, following a Clausius-Clapeyron scaling with the corrected temperature, but no large-scale atmospheric feedback is represented.

We then changed the manuscript as follows:

*(l168) “To address the resolution mismatch between the PMIP and GRISLI models, the output from the climate models are resampled onto the ice sheet model grid using a bi-linear interpolation. Additionally, to correct for differences in orography due to the interpolation, a constant vertical temperature gradient is applied on the surface air temperatures. van Aalderen et al. (2024) indicate that the lapse rate plays only a very minor role in the retreat of the BKIS. Based on this result, we decided to set the lapse rate to the fixed value of  $7^{\circ}\text{C km}^{-1}$ . Large-scale feedbacks of ice-sheet geometry changes onto the atmospheric circulation are not represented in our model set-up.*

*[...]*

*(l180) Precipitation is scaled with the local temperature anomaly following a Clausius-Clapeyron-type relationship.”*

Line 174-177: “small-scale processes not explicitly resolved by the ice-sheet model” What processes are these? Other models that use this approach (usually called a ‘partial melt parameterisation’ after Leguy et al., 2021) usually justify it from numerical considerations.

We agree that our original justification could have been stated more precisely. This approach is not primarily motivated by unresolved small-scale physical processes, but rather by the numerical difficulty of precisely defining the grounding-line position on a discretized grid. At the grid-point scale, the last grounded point is by definition simultaneously in contact with both the bedrock and the ocean, and therefore should physically experience both oceanic melting and basal melting from geothermal flux and basal friction. Applying a fraction of the adjacent floating-ice melt rate to this point is a numerical treatment intended to approximate this transitional behavior; This is consistent with the “partial melt parameterization” approach described by Leguy et al. (2021). We have revised the text accordingly.

*(l201) “In the GRISLI model, each grid point is classified as either floating or grounded ice. Because the exact position of the grounding line cannot be precisely defined at the grid scale, the last grounded point is, by construction, in contact with both the ocean and the bedrock. It should therefore physically experience both ocean-driven basal melt and melt induced by geothermal flux and basal friction. To account for this transitional behavior, we apply a fraction of the melt rate from adjacent floating ice to the last grounded grid point.”*

Line 233-234: “the ice-sheet extent remains fixed at LGM conditions throughout the simulation” This seems quite unrealistic, producing a strong overestimation of the albedo in the region and consequently biasing the GCM to colder temperatures. I wonder if this can (partially) explain the large difference in temperature between the two GCMS, which you discuss in lines 290-305.

We mixed up two different boundary conditions. As stated in the supplementary material of Liu et al. (2009), the continental ice sheets, including their extent, were updated approximately every 1000 years according to the ICE-5G reconstruction, whereas the coastlines were kept fixed at their LGM configuration throughout the simulation: "The continental ice sheets were modified approximately once per thousand year according to the ICE-5G reconstruction. The coastlines at LGM were also taken from the ICE-5G reconstruction and were not modified during the simulations in this study." We have corrected the text accordingly. For the BKIS, this means that the Barents and Kara Sea shelves remain continental surfaces in TRACE21k as the ice retreats, instead of being replaced by open ocean. This likely leads to an overestimation of the surface albedo and to the absence of ocean–atmosphere heat exchange over this region, which may contribute to the cold bias discussed in the manuscript.

*Line 253 “To perform the transient simulations, Bouttes et al. (2023) and Liu et al. (2009) forced their climate model (iLOVECLIM and CCSM3 respectively) in the same way, accounting for variations in greenhouse gases and astronomical parameters. However, in both simulations the ice-sheet extent is updated approximately every 1000 years following the respective reconstructions. However, in TRACE21k the coastlines were kept fixed at their LGM configuration during the deglaciation (Liu et al., 2009). As a consequence, the Barents and Kara Sea shelves remain continental surfaces in the model as the ice retreats, instead of being replaced by open ocean. This likely results in an overestimation of the surface albedo and an absence of ocean–atmosphere heat exchange over the BKIS region, which may contribute to a cold bias in the simulated atmospheric and oceanic temperatures, and may partly explain the pronounced difference between the two climate indices discussed below. Moreover, one of the main differences lies in the reconstructed ice sheet that is used as a boundary condition of the deglaciation experiment. The TRACE21k simulation was based on ICE-5G (Peltier, 2004), whereas the iLOVECLIM simulation used the ICE-6G\_C reconstruction (Peltier et al., 2015). The latter refines regional ice thickness and shifts the timing of deglaciation, notably by incorporating a more rapid ice loss during the MWP1A to better align with sea-level observations. The two simulations also differ in their treatment of freshwater fluxes from ice-sheet melting into the ocean. TRACE21k applies a scenario specifically designed to reproduce abrupt deglaciation events, whereas in iLOVECLIM, we chose to use simulations in which the release of freshwater fluxes is not considered, because when freshwater fluxes are included, the Atlantic Meridional Overturning Circulation is no longer active. Differences in both experimental protocols (TRACE21k and iLOVECLIM) lead to discrepancies in the simulated evolution of atmospheric and oceanic temperatures by the iLOVECLIM and CCSM3 models. Bouttes et al. (2023) also support this analysis, highlighting that the use of different ice-sheet reconstructions results in differences in both the amplitude and timing of the deglaciation. Moreover, the two simulations differ in their temporal resolution. Indeed, iLOVECLIM provides annual outputs while TRACE21k provides data every 10 years.*

Lines 264-265: “the index was constrained between  $-2$  and  $2$ ” This suggests that the two GCMs predict temperatures that are, at some point during the deglaciation, either colder than  $T_{LGM} - \Delta T_{LGM}$ , or warmer than  $T_{PI} + \Delta T_{LGM}$ . Is this so? Do you have an explanation for this?

Yes, this is the case. At certain points during the deglaciation, both TRACE21k and iLOVECLIM can locally predict temperatures colder than T\_LGM or warmer than T\_PI. This mainly reflects the substantial interannual-to-decadal climate variability present in the transient simulations, which is not represented in the LGM and PI snapshot states used to define the bounds of the index (Eq. 4). The LGM and PI reference states represent long-term averages, whereas the transient simulations retain their internal variability. Brief excursions beyond these bounds can occur, particularly around the rapid warming phases of the deglaciation.

Moreover, these bounds were set specifically to avoid overshoots that can arise from the way the index is calculated (Eq. 4), where the LGM and PI temperature difference in the denominator is small, even a modest anomaly in the numerator can produce an extremely large index value. To prevent this, we bounded the index between  $-2$  and  $2$ . This situation occurs at very few locations, most notably in the center of the Greenland Sea, where the LGM-PI temperature contrast is small.

We modified the manuscript consequently:

*Line 298 “To prevent excessively large warming or cooling (which can occur because of the internal variability of the transient simulations, not represented in the LGM and PI reference states), the index was constrained between  $-2$  and  $2$ ”*

Lines 330-331: “consistently with the subsurface temperatures calculated using the TRACE21k index” I do not see any similarity between the sediment core data, and the two indices. If you want to argue there is one, please substantiate this with some sort of statistics.

We agree that our original claim of consistency between the sediment core record and the TRACE21k and iLOVECLIM indexes was not sufficiently substantiated. We have revised the text as follows:

*(l375) “We also compared the evolution of subsurface ocean temperature anomalies with those reconstructed from the MD01-2461 (Peck, 2016) sediment core, located off the present-day British coast (Fig. 2d). Although the sediment core is not located exactly within the BKIS region, it provides valuable information on North Atlantic oceanic variability during the deglaciation. The centennial-scale variability of the reconstructed subsurface temperatures does not clearly correspond to either the iLOVECLIM or TRACE21k index. However, the overall amplitude of warming between the LGM and  $\sim 9$  ka appears broadly consistent with both indices, with all three curves converging toward a similar temperature anomaly by 9 ka.”*

Figure 2: What’s the difference between panels a and c? The only difference I can find in the caption is that c shows ‘anomalies’, but with respect to what? And shouldn’t that just result in the same curve, but with an offset? Also, what’s going on with the shallow ocean temperature from TRACE21k between 17ky and 14.5ky in panel d? Did their model freeze? :

Panel (a) shows the atmospheric temperature at sea level, i.e., the climate forcing as directly applied to GRISLI following the iLOVECLIM/TRACE21k index (Eq. 5). As explained in Section 2.2.1, the PMIP surface air temperatures are adjusted to sea level before being used to force GRISLI, in order to avoid counting two times the vertical temperature correction that GRISLI subsequently applies internally based on the simulated ice-sheet elevation. Panel (c), by contrast, shows temperature anomalies relative to the LGM, for which this sea-level

adjustment was deliberately not applied, since the reconstruction of Osman et al. (2021), used for comparison, already accounts for the effect of the evolving ice-sheet topography on surface air temperature. Leaving our own values unadjusted to sea level in panel (c) therefore allows for a consistent comparison with this reconstruction.

Panel (d): the apparent plateau in the TRACE21k subsurface ocean temperature anomaly between 17 and 14.5 ka does not reflect a numerical artefact or a "frozen" state of the underlying CCSM3 simulation, but results from the lower bound imposed on ocean temperature at the local freezing point, which is reached via our index-based reconstruction method during this interval.

(l385)“**a:** Evolution of the PMIP3/PMIP4 multi-model mean atmospheric temperature at sea level over the BKIS region, in the domain defined using the ice mask from DATED1 at 21ka, and computed using the iLOVECLIM (blue) or TRACE21k (brown) climate indices. **b:** Evolution of the PMIP3/PMIP4 multi-model mean ocean temperature in the Barents Sea, averaged over depths between 200 and 750 m, computed using the iLOVECLIM (blue) or TRACE21k (brown) climate indices. **c:** Evolution of the PMIP3/PMIP4 multi-model mean atmospheric temperature anomalies (compare to LGM) over the BKIS region, computed using the iLOVECLIM (blue) or TRACE21k (brown) climate indices; unlike panel a, temperatures here are not adjusted to sea level, for consistency with the Osman et al. (2021) reconstruction, which accounts for the effect of the evolving ice-sheet topography. The white curve shows atmospheric temperature anomalies over the BKIS region derived from data assimilation (Osman et al., 2021). Evolution of the PMIP3/PMIP4 multi-model mean ocean temperature anomalies in the Greenland Sea, averaged over depths between 100 and 200 m, computed using the iLOVECLIM (blue) or TRACE21k (brown) climate indices. The white curve shows subsurface ocean temperature anomalies (LGM against PI) at 150 m depth derived from a sediment core located off the present-day British coast (Peck, 2016). In the 4 panels, the shaded areas indicate the standard deviation across the five PMIP3/PMIP4 models.”

Lines 352-355: You describe a steady-state initialization, which you achieve by prescribing a fixed LGM climate for 100 kyr. However, the LGM climate only existed for a few thousand years at the most, shorter than the response time of the ice sheet, which implies that the ice sheet would not yet have been in equilibrium with this cold climate before it started warming again. I know there have been several studies that demonstrated this (only the ones by Stap et al., 2022/2024 come to mind right now). I’m not sure what (if anything) you did to tune your model to get an ice sheet at the end of these spin-ups that looks reasonably LGM-like (you do not mention any tuning in the manuscript), but assuming equilibrium between the ice sheet and the climate at LGM bias this tuning towards an ice sheets that is too small.

We agree that the assumption of equilibrium between the ice sheet and a constant LGM climate is a simplification, given that LGM conditions likely persisted for a duration shorter than the response time of the ice sheet, which could bias our initial states toward an ice sheet that is too small in volume relative to a transient approach, for a given ice extent.

This concern was directly addressed in our previous study (van Aalderen et al., 2024), in which we initially tested 10 different PMIP climates to force the ice-sheet spin-up and retained only the 5 ones that produced an LGM ice-sheet geometry in the best agreement with the available

reconstructions (the same 5 GCMs used as forcing in the present study). In this study (van Aalderen et al., 2024), we also tested an alternative, transient spin-up procedure starting from an interglacial state rather than assuming LGM equilibrium, along with sensitivity tests on model parameters governing the ice-sheet response. As reported there and discussed in the present manuscript (see Discussion section), this transient initialization did not substantially affect the response of the BKIS to variations in atmospheric and oceanic forcings, which is the primary focus of the present study, and produce only 4 initial states in good agreement with reconstructions.

We changed the manuscript as follows:

Line 404: *‘We chose this equilibrium approach rather than a transient spin-up, as the latter produced LGM ice sheets consistent with reconstructions for only four of the five GCMs (van Aalderen et al., 2024).’*

Lines 358-359: “sea level according to the reconstructions of Waelbroeck et al.” I assume this means your ELRA model does not include the gravitational effects on sea level? There’s a few nice papers from Natalia Gomez that show how this stabilizes the ice sheet, preventing MISI’s self-sustaining retreat on mild retrograde slopes. Please discuss the relevance of these findings for your work.

Our sea-level forcing is based on the globally uniform eustatic reconstruction of Waelbroeck et al. (2002), applied uniformly across the domain, and our ELRA bedrock model does not include the gravitational effects on relative sea level associated with ice-mass redistribution. As demonstrated by Gomez et al. (2010, 2015), self-consistent sea-level modeling introduces a negative feedback near a retreating marine ice sheet which can partially counteract grounding-line retreat and halt a MISI on mild retrograde slopes. Because our model does not include this feedback, our simulated BKIS retreat may be faster and more self-sustained than it would be with a gravitationally self-consistent sea-level treatment. We have added a discussion of this limitation and its implications for our results

*(1842) “A further limitation concerns our treatment of sea level, which is prescribed uniformly across the domain following the eustatic reconstruction of Waelbroeck et al. (2002), rather than computed self-consistently from the gravitational effects of ice-mass redistribution. Gomez et al. (2010, 2015) showed that gravitational effects can generate a stabilizing feedback near a retreating marine ice sheet potentially halting grounding-line retreat on mild retrograde slopes. Because our simulations do not include this feedback, the self-sustained retreat simulated here may be overestimated in rate and extent compared to a fully self-consistent sea-level treatment. Incorporating gravitational effects in future BKIS simulations would help assess the robustness of the MISI mechanism identified here under a more complete representation of solid-Earth feedbacks.”*

Lines 372-373: “we set the basal melting to zero and removed the analytical parameterization of grounding line flux”. The flux condition is, essentially, a trick to reduce the huge numerical errors that arise from the discontinuity in basal friction at the grounding line. Without it, your spatial resolution of 20km would produce numerical errors in grounding-line position and

505 migration rate that far exceed the signal you would get from oceanic or atmospheric forcing.  
506 As such, I don't think this experiment can be meaningfully interpreted.

507 We thank the reviewer for this important point. In our experience with this model, the effect  
508 runs in the opposite direction from what the reviewer describes. Without the grounding-line  
509 flux parameterization, the model becomes far too stable, and retreat becomes much harder to  
510 simulate, rather than producing spurious excess retreat. As shown in previous applications of  
511 this model (Quiquet et al., 2018; van Aalderen et al., in preparation), without this  
512 parameterization, the grounding line remains essentially fixed at its glacial position throughout  
513 the deglaciation, effectively behaving like an older-generation ice-sheet model unable to  
514 reproduce MISI. This is precisely why we designed the DEGLA-NOBMELT-NOGL  
515 experiment, to explicitly test how strongly this parameterization (and the grounding-line  
516 instabilities it allows the model to capture) contributes to the BKIS retreat simulated in our  
517 reference experiments. We have revised the description of this experiment to make this rationale  
518 clearer.

519 (l423)“We conducted a third series of experiments, DEGLA-NOBMELT-NOGL, in which we  
520 set the basal melting to zero and removed the analytical parameterization of grounding line  
521 flux (Tsai et al., 2015). This formulation can induce strong grounding line retreat, particularly  
522 over retrograde bed slopes such as those commonly found beneath BKIS (Amante et al., 2009,  
523 see Fig. S1), by amplifying the flux with increasing ice thickness. Without it, the grounding line  
524 becomes considerably more stable and largely fails to retreat, consistent with previous GRISLI  
525 applications (Quiquet et al., 2018). By eliminating this parameterization, we therefore isolate  
526 the specific contribution of grounding-line flux-driven instabilities to the simulated BKIS  
527 retreat, and by extension, the role of oceanic forcing acting through this mechanism.”

528 Lines 383-385: “in which grounding line migration was entirely cancelled by imposing an  
529 artificially low sea level of -1000 m relative to present-day” Well, I mean, it's true that you  
530 cannot have grounding-line migration when there is no grounding line... But I really struggle  
531 to see the point of this experiment. I am also surprised that the ice extent in this experiment  
532 (Fig. 5) is not larger than in the other ones. In the absence of basal melt and calving, the ice  
533 extent in this experiment should be entirely dictated by the surface mass balance, implying that  
534 you already have enough surface melt to prevent any further advance south into mainland  
535 Siberia; is this indeed the case?

536 1. Regarding the purpose of the DEGLA-NOSL experiment: in the previous experiments, we  
537 progressively removed the influence of the ocean on the ice sheet (first sub-shelf melt (DEGLA-  
538 NOBMELT), then the grounding-line flux (DEGLA-NOBMELT-NOGL). The only remaining  
539 oceanic influence at this stage was the transition from a grounded to a floating point, and the  
540 associated change in basal friction. The DEGLA-NOSL experiment, in which an artificially  
541 low sea level prevents any point from becoming afloat, was designed specifically to test the  
542 impact of this grounded-to-floating transition, by removing it entirely.

543 2. In the absence of basal melt and calving, ice extent in DEGLA-NOSL is entirely controlled  
544 by the surface mass balance, and our simulations confirm that SMB is already negative over  
545 the regions bordering mainland Siberia and the continental margins under the reference

atmospheric forcing, which prevents any further southward advance of the ice sheet in this experiment (see figure below).

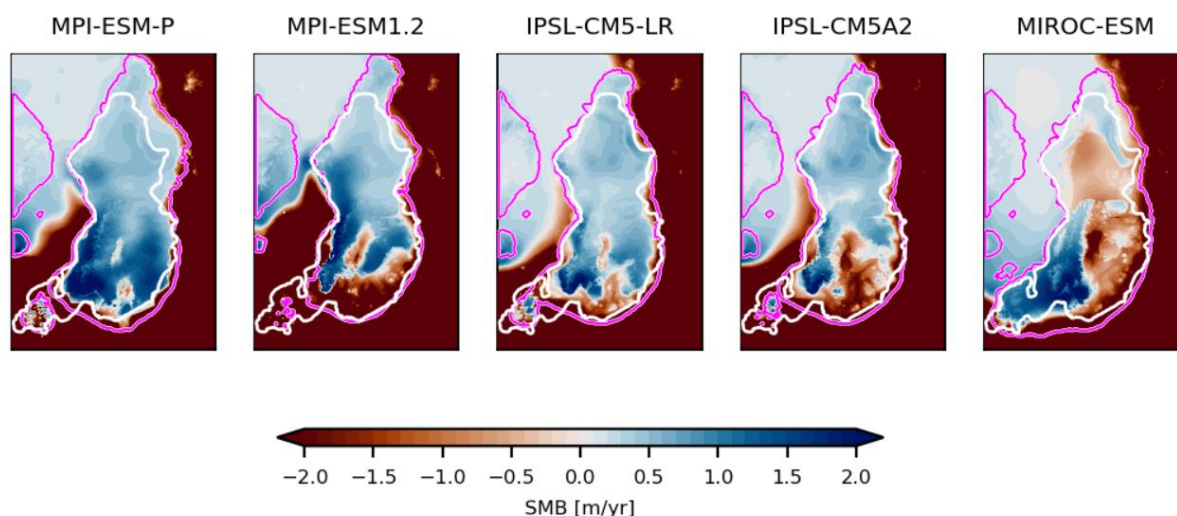


Figure : Surface mass balance calculated for an ice-free state using each GCM forcing. The white line indicates the ice extent from DATED1, while the pink line shows the ice extent at the end of the equilibrium state simulations.

Lines 394-396: “following the DEGLA-ATM-LGM design, but with amplified basal melting at the grounding line” What do you mean by this? Earlier on, you mention that you use the PMP scheme for sub-grid basal melt at the grounding line. Do you still use but with a higher melt rate before scaling? Also, this again seems more like a study into numerical errors to me, rather than looking at physical processes.

In this experiment, atmospheric temperatures are fixed at their LGM values, as in DEGLA-ATM-LGM, while ocean temperatures are uniformly increased by +10°C everywhere under the ice-shelf domain. We confirm that the same sub-grid partial melt parameterization described in Section 2.2.1 is retained in this experiment. The last grounded grid point still receives a fraction of the melt rate computed from its adjacent floating grid points, but this floating-point melt rate is itself computed from the +10°C ocean temperature before this fraction is applied. This results in a higher basal melt rate at the grounding line compared to the reference or DEGLA-ATM-LGM experiments. We agree with the reviewer that this experiment is best understood as a targeted numerical sensitivity test on the basal melt parameterization and the PMIP ocean temperature forcing, rather than an attempt to represent a physically plausible climate scenario. As already stated in the Discussion, this experiment was specifically designed to test the sensitivity of our results to the basal melt parameterization and the PMIP ocean temperature forcing, rather than to reproduce a plausible deglaciation scenario.

Because of the new sensitivity tests on the transfer coefficient  $K_t$ , we decided to remove this experiment (DEGLA-ATM-LGM1+10) from the manuscript.

Line 405: “3.1 Ice sheet geometry at the LGM” Please make it clear from the start that you describe here some reconstructions that you use to compare your model results against.

We have changed the section title to "3.1 Reconstructed ice sheet geometries at the LGM" to make this distinction clear from the start.

Figure 3: The vertical axes of the panels all show integrated ice volume (erroneously called ‘ice thickness’ in panels a,b) and ice area as a percentage of a value at 21 ky. The fact that they all start at exactly 100% suggests to me that you scaled all the curves by their own respective value at 21 ky, is that correct? If so, I find this misleading, as this approach obscures any differences in absolute ice-sheet volume or area between different model simulations and data-based reconstructions. Please show the absolute values as well. Also, in panels c-d, please improve the legend – right now, the black outlines around the boxes make it very difficult for me to distinguish the grey and black markers.

Yes, we confirm that each curve was normalized by its own value at 21 ka. We chose this normalization in order to compare the dynamics and timing of deglaciation across simulations and reconstructions, independently of differences in absolute ice volume or extent between them. We agree, however, that this approach can obscure other relevant information, such as differences in absolute ice volume between simulations and reconstructions. We have therefore added a supplementary figure (Fig S6) showing the evolution of ice volume in absolute terms for each simulation and each reconstruction, to complement Figure 3. We have also revised the legend of panels c and d, improving the marker outlines to make the grey and black symbols more distinguishable.

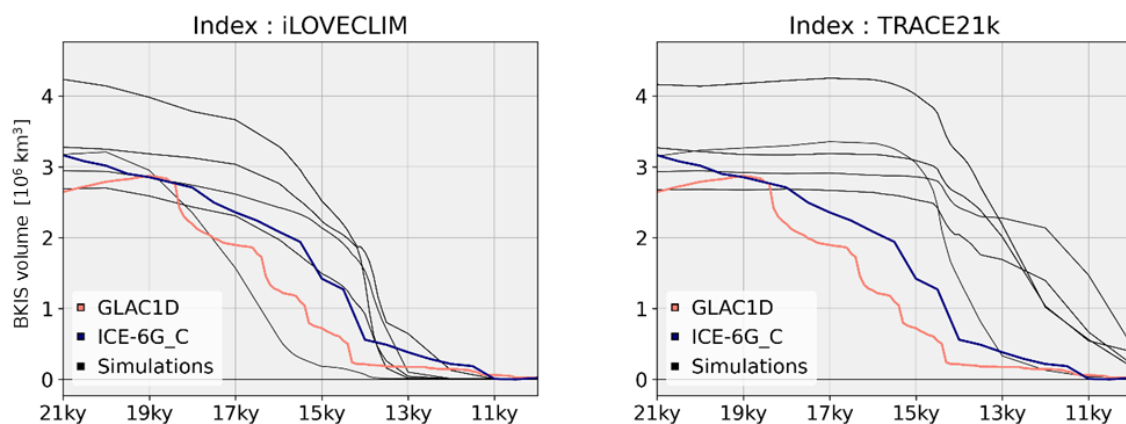


Figure S6: **left:** Ice volume loss in the BKIS region, for each of the five GCM-forced simulations using the iLOVECLIM index (black lines) compared to the GLAC1D (red) and ICE-6G\_C (blue) reconstructions. **right:** Same as left, but with the TRACE21k index.

Line 449: “the initial states we built (see Fig. S3) overestimate the ice extent in this region” Which is why you need to show the absolute ice volume/area in Fig. 3.

Done

Line 505: “when the ice flux at the grounding line is ignored” it’s not ignored, it’s just modelled very inaccurately.

We agree that “ignored” was not accurate, since a flux is still computed at the grounding line even without this parameterization. We have replaced “ignored” with “computed without the grounding-line flux parameterization” throughout the manuscript, to be more precise.

Figure 6: is the “atmospheric index” in panel A the spatial average of I\_T\_iL? Averaged over the entire region, or only over the ice area? And in panel B, is the dashed line the “zero lapse rate” experiment? If so, please correct the legend.

Yes, the "atmospheric index" shown in panel a corresponds to the spatial average of I\_T\_iL over the ice-covered area of the BKIS domain, not the entire region. Yes, the dashed lines in panel b correspond to the "zero lapse rate" experiment, as already indicated in the caption. We have clarified both points in the revised figure caption and legend.

**(1665)“a:** Temporal evolution of the atmospheric index, averaged over the BKIS ice mask, for the different stabilized experiments. **b:** Evolution of the BKIS ice volume for the different stabilized experiments averaged over the 5 GCMs, using the iLOVECLIM index. Solid lines represent simulations with a lapse rate of  $7\text{ }^{\circ}\text{C km}^{-1}$ ; dashed lines show the zero-lapse-rate experiment, where the lapse rate is set to  $0\text{ }^{\circ}\text{C km}^{-1}$  from 14.5 ka onward ( $7\text{ }^{\circ}\text{C km}^{-1}$  before).”

Figure 7: This is an interesting way to analyse the relative importance of mass balance changes vs. ice-dynamical processes. The ‘SMBB’ and ice flux divergence panels are integrated over the 500-yr period, right? In that case, use an integral sign in the title, not a summation sign. Also, do the panels show the ice mask at 14 ky or 14.5 ky? I think the latter option would be the most informative. Lastly, how can it be that the integrated surface melt in the NOSL experiment is so much larger? You mention that the ice sheet in this experiment is larger and thicker than in the baseline experiment, which implies that the surface slope at the margins would also be steeper, right? That would imply that an increase in surface temperature would result in a smaller increase in ablation zone area, than in the baseline experiment (due to the steeper slope), so I would expect this ice sheet to be more ‘resilient’ to atmospheric warming. Or am I missing something here?

Regarding the summation sign: yes, the SMBB and ice-flux-divergence panels are integrated over the 500-yr period shown. We intentionally used a summation sign rather than an integral sign because this quantity is computed as a discrete sum over the model's individual time steps, rather than a continuous integral.

Regarding the ice mask used: panel a shows the ice mask at 14.5 ka, and panel b shows the ice mask at 14 ka, i.e., in both cases the mask corresponding to the earlier date of the respective 500-yr window, as suggested by the reviewer.

Regarding the larger integrated surface melt in the NOSL experiment: we agree that a thicker ice sheet is expected to be more resilient to atmospheric warming, because of the expected steeper slopes, as suggested by the reviewer. However, this comparison must be considered in the context of the different time windows analyzed for each experiment. Because the ice margin retreat is substantially delayed in DEGLA-NOSL, the acceleration phase analyzed for this experiment occurs later (14–13.5 ka) than in the reference experiment (14.5–14 ka). Over this later time window, atmospheric temperatures computed with the iLOVECLIM index are already considerably higher (Fig. 6a), so the larger surface melt in DEGLA-NOSL primarily reflects the substantially warmer climate forcing prevailing during this later period, rather than an intrinsically lower resilience of the ice sheet itself.

Lines 615-616: “he stabilized experiments... ...demonstrated that the BKIS retreat is both abrupt and irreversible” They do not; you didn’t do any ‘reverse’ experiment. ‘Irreversible’ means the retreat will go on even when the forcing is reversed, i.e. the climate is set back to LGM conditions. The original MISMIP experiments (Pattyn et al., 2012) demonstrate irreversibility; even when the ice viscosity is reversed to its original value, the grounding line does not advance back to its original position (i.e. hysteresis). The fact that your ice sheet continues to shrink when you ‘freeze’ the forcing only demonstrates that your system has a long response time.

“irreversible” is not the appropriate term here, since we did not perform any experiment reversing the forcing back to LGM conditions to test for hysteresis. We have replaced “irreversible” with “self-sustained” throughout the manuscript, to describe a retreat that continues once triggered, even under constant forcing.

Lines 631-639: For readers who, like me, are not familiar with your earlier paper, please provide a very brief (2-3 lines) summary of what you did there.

We have added a short summary at the start of the Discussion.

(1714) “In van Aalderen et al. (2024), we conducted a series of idealized experiments applying constant perturbations in basal melting and atmospheric temperatures to LGM equilibrium states of the BKIS. Using the same ice-sheet model as in the present study, these experiments were designed to assess the relative sensitivity of the BKIS to oceanic and atmospheric forcings.”

Lines 647-648: “ice sheet reconstructions (DATED-1, ICE-6G\_C, and GLAC-1D) indicate that confined ice shelves were indeed very limited at the LGM” DATED-1 and ICE-6G cannot represent shelves; they were not created with ice-dynamical models, but rather derived by inversion from observed uplift rates, combined with geomorphological evidence. Only GLAC-1D can be used for a comparison like this.

We thank the reviewer for this important clarification. We have corrected the text accordingly: “ice sheet reconstruction (GLAC-1D) indicate that confined ice shelves were indeed very limited at the LGM”

Lines 684-692: your “minimum friction coefficient” looks to me like a numerical parameter, not something physical. Changing it does not tell you anything about the uncertainty in the bed roughness beneath the BKIS; it only tells you something about the importance of numerical artefacts in your model.

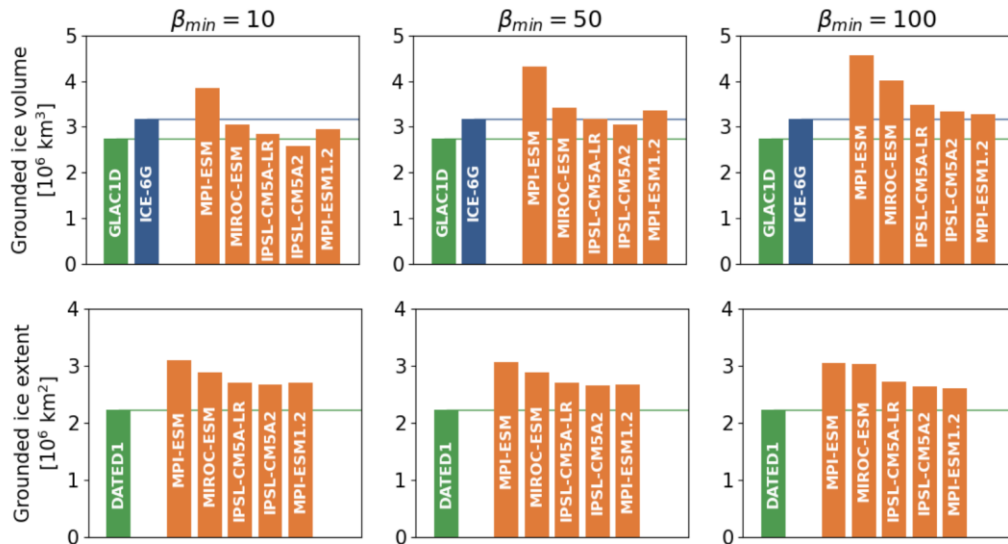
The lower bound  $\beta_{\min}$  is primarily a numerical control. It limits unphysically fast basal sliding velocities that can otherwise arise when the basal drag coefficient  $\beta$  becomes very small (i.e., under low effective pressure), preventing numerical instabilities. We have revised the corresponding paragraph to clarify that these sensitivity tests assess the influence of this numerical control on our results, rather than an exploration of physical basal friction uncertainty.

(1799) “Moreover, basal friction between the ice sheet and the bedrock is a poorly constrained parameter. In our configuration, the lower bound  $\beta_{\min}$  imposed on the basal drag coefficient

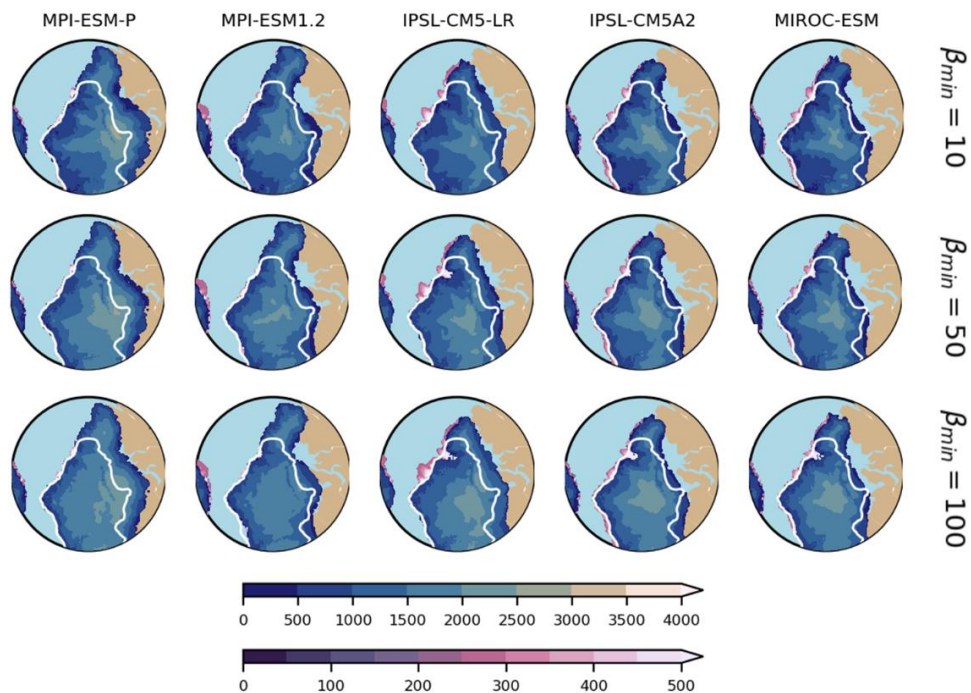
(Section 2.1) acts primarily as a numerical control, preventing unrealistic fast basal sliding velocities that can otherwise arise under low effective pressure. To assess the sensitivity of our results to this numerical parameter,....”

Lines 696-697: “an ice sheet equilibrated under an LGM climate tends to develop a geometry fully adjusted to cold climatic conditions” See my earlier comment. You do not show how your modelled LGM ice volume compares to reconstructions (see my earlier comments on Fig. 3); this potential bias is another reason why this comparison needs to be made.

We would like to point out that this comparison is in fact already provided, with Figures S3 and S4 showing the simulated LGM ice volume and extent for each of the five GCM forcings against the GLAC1D and ICE-6G\_C reconstructions. Together, these comparisons allow the reader to assess the extent to which our LGM spin-up geometry differ or agree with the reconstructed ice volumes.



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



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

Lines 697-698: “a more pronounced glacio-isostatic depression of the bedrock” Good point, I hadn’t even thought of the GIA bias yet. Indeed, this would also affect the grounding-line stability during the deglaciation.

As explained above, in the context of the LGM equilibrium, we also assessed the potential bias in glacio-isostatic depression in our previous study (van Aalderen et al., 2024), where we compared the equilibrium spin-up approach adopted here with a transient initialization starting from an interglacial state. This transient approach did not lead to major differences in the response of the BKIS to atmospheric and oceanic forcing. In addition, as discussed elsewhere in the present manuscript (Discussion), we tested the sensitivity of our results to the relaxation time of the ELRA bedrock model, and found that it had only a very limited impact on the simulated deglaciation dynamics.

Lines 711-720: True, a more viscous mantle would delay the uplift during the deglaciation. On the other hand, since the really cold part of the glacial cycle didn’t last very long, the Eurasian ice sheet also didn’t keep its maximum extent for a very long time, so that the depression likely hadn’t reached its equilibrium depth yet when the deglaciation started. A more viscous mantle would then also lead to a less deep depression, and consequently a more stable grounding line. I’m not sure which effect would be stronger.

We thank the reviewer for this point, which we had not considered. We agree that these two competing effects (a delayed post-glacial uplift favoring retreat via retrograde bed slopes,

versus a shallower LGM depression favoring a more stable grounding line) could partly offset one another under a more viscous mantle, and we have no basis to determine which effect would dominate. This has not been examined in the present study, but it would indeed be worth exploring the relative importance of these two effects in a future work. We have revised the corresponding sentence accordingly.

*(1828)"The representation of glacio-isostatic adjustment (GIA) in our simulations could also bring some uncertainties. In GRISLI, bedrock deformation is computed using an Elastic Lithosphere–Relaxed Asthenosphere (ELRA) model with a relaxation time of 3 kyr. However, regional studies suggest that longer relaxation times may be more appropriate in some areas. For example, analyses of the Ångermanland relative sea-level record indicate decay times with a 2 $\sigma$  lower bound above 4 kyr (Nordmann et al., 2015). A longer relaxation time would delay post-glacial bedrock uplift during deglaciation, favoring a longer-lasting retrograde slope and thus potentially a more sustained retreat of the BKIS. However, a longer relaxation time would also imply that the bedrock depression had less time to reach its equilibrium depth during the relatively short cold phase of the glacial cycle, resulting in a shallower LGM depression to begin with, which could instead favor a more stable grounding-line configuration at the onset of deglaciation. A longer relaxation time could therefore either favor or dampen the retreat of the BKIS, depending on the relative importance of these two competing effects, which remains to be explored in future work."*

Lines 722-736: "Brendryen et al. (2020) conclude..." Brendryen et al. looked at (newly-calibrated) geomorphological evidence of ice margin/grounding-line evolution. The statements they make about the causes of the ice sheet's retreat are not based on their own work.

We modified the manuscript as follow to considering the reviewer remark:

*(1857)"Finally, based on evidence from marine sediment cores, Brendryen et al. (2020), reconstructed the timing of the BKIS retreat and suggested that the rapid collapse of its marine-based sectors occurred shortly before the MWP-1A, concomitantly with a rapid increase in subsurface ocean temperatures, while atmospheric temperatures appear to have remained comparatively stable. In discussing possible drivers of this retreat, they refer to several mechanisms including surface melting, temperature-elevation feedback, marine ice-sheet instability and marine ice-cliff instability, without independently establishing which of these mechanisms was dominant (Gregoire et al., 2016, Pollard and DeConto, 2015, Wise et al., 2017)."*

Ref:

Alvarez-Solas, 2019, <https://doi.org/10.5194/cp-15-957-2019>

Amante, 2009, <https://doi.org/10.7289/V5C8276M>

Annan, 2022, <https://doi.org/10.5194/cp-18-1883-2022>

Brendryen, 2020, <https://doi.org/10.1038/s41561-020-0567-4>

Brondex, 2017, <https://doi.org/10.1017/jog.2017.51>

765 Burgard, 2022, <https://doi.org/10.5194/tc-16-4931-2022>  
766 Cornford, 2020, <https://doi.org/10.5194/tc-14-2283-2020>  
767 Feldmann, 2019, <https://doi.org/10.1126/sciadv.aaw4132>  
768 Goldberg, 2009, <https://doi.org/10.1029/2008JF001227>  
769 Gomez, 2010, <https://doi.org/10.1038/ngeo1012>  
770 Gregoire, 2016, <https://doi.org/10.1002/2016GL070356>  
771 Leguy, 2021, <https://doi.org/10.5194/tc-15-3229-2021>  
772 Mercer, 1970, <https://doi.org/10.1126/science.168.3939.1605.b>  
773 Nordman, 2015, <https://doi.org/10.1093/gji/ggv051>  
774 Osman, 2021, <https://doi.org/10.1038/s41586-021-03984-4>  
775 Pattyn, 2012, <https://doi.org/10.5194/tc-6-573-2012>  
776 Peck, 2016, <https://doi.org/10.1594/PANGAEA.861240>  
777 Peltier, 2004, <https://doi.org/10.1146/annurev.earth.32.082503.144359>  
778 Peltier, 2015, <https://doi.org/10.1002/2014JB011176>  
779 Petrini, 2020, <https://doi.org/10.1016/j.quascirev.2020.106314>  
780 Pollard and DeConto, 2009, <https://doi.org/10.1038/nature07809>  
781 Pollard and DeConto, 2012, <https://doi.org/10.5194/gmd-5-1273-2012>  
782 Pollard, 2015, <https://doi.org/10.1016/j.epsl.2014.12.035>  
783 Pritchard, 2012, <https://doi.org/10.1038/nature10968>  
784 Quiquet, 2018, <https://doi.org/10.5194/gmd-11-5003-2018>  
785 Rignot, 2013, <https://doi.org/10.1126/science.1235798>  
786 Scherrenberg, 2024, <https://doi.org/10.5194/cp-20-1761-2024>  
787 Schoof, 2007, <https://doi.org/10.1029/2006JF000664>  
788 Schoof, 2012, <https://doi.org/10.1017/jfm.2012.43>  
789 Seroussi, 2024, <https://doi.org/10.1029/2024EF004561>  
790 Stap, 2022, <https://doi.org/10.5194/tc-16-1315-2022>  
791 Stap, 2024, <https://doi.org/10.5194/cp-20-257-2024>  
792 Sun, 2020, <https://doi.org/10.1017/jog.2020.67>  
793 Tsai, 2015, <https://doi.org/10.3189/2015JoG14J221>  
794 van Aalderen, 2024, <https://doi.org/10.5194/cp-20-187-2024>  
795 van Aalderen, 2026, <https://doi.org/10.22541/essoar.176833267.78739367/v1>  
796 Waelbroeck, 2002, [https://doi.org/10.1016/S0277-3791\(01\)00101-9](https://doi.org/10.1016/S0277-3791(01)00101-9)

797 Weertman, 1974, <https://doi.org/10.3189/S0022143000023327>

798 Wise, 2017, <https://doi.org/10.1038/nature24458>