Review of **From surface processes to Marine Ice-Sheet Instability: The Collapse of the Barents–Kara Ice Sheet during the last deglaciation**
by Victor van Aalderen et al.

Summary
van Aalderen et al. investigate how the mechanism of the Marine Ice Sheet Instability (MISI) affected the Barents-Kara Ice Sheet (BKIS), a marine based ice sheet which existed in the past. Studying past behaviours of marine based ice sheets prone to MISI may inform us how other marine ice sheets, such as West Antarctic, could respond.
Based on their findings they suggest that atmospheric forcing can be a trigger for MISI and should be accounted for in future Antarctic projections. I think that it is valuable to investigate the state of past ice sheets and informative for future studies, and the scope is well within the scope of Climate of the Past. However, I am missing stronger proof that MISI is really being observed, as for now it seems rather a qualitative assessment. Transects of affected basins would convince me that MISI is indeed occurring.

General comments
MISI
The abstract states: *"Contrary to current interpretations found in the literature, which suggest that a MISI is primarily initiated by ocean-induced basal melting."* I disagree with this framing.
MISI is a self-sustained retreat of marine ice sheets on retrograde bed slopes. If the grounding line sits at a local bedrock maximum (below sea level), perturbations in grounding-line flux can destabilize it and trigger retreat (Pattyn et al., 2012; Schoof 2007). For the Antarctic Ice Sheet, it is widely accepted that ocean warming thins ice shelves, reducing their buttressing effect and increasing grounding-line flux (also, as stated in the discussion, other mechanisms can also limit ice-shelf stability, such as hydrofracturing). Hysteresis experiments on the Antarctic Ice Sheet show that atmospheric warming alone can trigger MISI (Swierczek-Jereczek et al., 2026).
While we do expect ocean-induced melting to be the dominant trigger for the West Antarctic Ice Sheet, I would not state that MISI is *primarily* initiated by ocean-induced basal melting. In milder climates, the atmosphere could plausibly act as the trigger instead. I'd suggest either removing this sentence from the abstract, or rephrasing it to something like: *"We find an atmospherically triggered MISI."*
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.

LGM state

It appears you are simulating a too-thick ice sheet compared to reconstructions. You permute your β_min value, which caps your friction, but as already noted, this is more of a numerical artifact than a physically meaningful behavior. Given that $\beta = C\_f \cdot C\_sedim \cdot N$ 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 β_min, would produce a more realistic ice sheet and a more informative comparison against reconstructions.

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

Climate index

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

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.

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.

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?

Specific comments

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

Flux scheme
Does your flux scheme include any buttressing coefficient?

Oceanic climate index
Does your index include depth?

L41
"sensitive to oceanic condition variations", suggest simplifying to "sensitive to oceanic variations."

L53
"can only be tempered by ice-shelf buttressing", I am not sure about this statement. I think that high accumulation could also avoid MISI (Feldmann et al., 2019). Maybe remove "only."

L70
Which type of biases? A more stable grounding line, you mean?

L136
"inlcuding" (typo)

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.

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?

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?

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.

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?

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.

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 β_{min}. β_{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 β_{min} values over your whole experiment.

L744
"Each of this steps", typo, should read "Each of these steps."

References

- Feldmann, Johannes, Anders Levermann, and Matthias Mengel. "Stabilizing the West Antarctic Ice Sheet by surface mass deposition." *Science advances* 5.7 (2019): eaaw4132.

- Pattyn et al., 2012.: Results of the Marine Ice Sheet Model Intercomparison Project, MISMIP, The Cryosphere 6, 573-588.

- Schoof, 2007: Ice sheet grounding line dynamics: steady states, stability and hysteresis, J. Geophys. Res., 112, F03S28, doi:10.1029/2006JF000664

- Swierczek-Jereczek, J., Alvarez-Solas, J., Robinson, A., Gutiérrez-González, L., and Montoya, M.: Highlighting processes underlying the stability and hysteresis of the Antarctic Ice Sheet, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2025-6566, 2026.