Author's response to Anonymous Referee 2

March 10, 2026

1 Overall comments

An overall well-written and interesting manuscript on an important aspect of ice-shelf hydrofracturing and its impact on ice decay, but several aspects listed below would benefit from clarification and expansion to strengthen the interpretation of the results presented in the current version. Presentation of results (i.e., figures) could also be enhanced to support the key points.

Dear Reviewer 2,

Thank you for your review. A point-by-point response is provided below, with the reviewer comments shown in blue and our responses in black.

2 Ice dynamics, deglaciation drivers, and isostacy

The statement "We then disentangle the drivers and controls of its subsequent deglaciation" would benefit from a broader discussion of glacial isostatic adjustment. In particular, consideration of its sensitivity to heterogeneous mantle viscosity and the resultant impact on regional sea level would strengthen the interpretation of deglaciation processes, as well as their rates.

We have now carried out a sensitivity suite of subensembles to the approximate inferred range of upper mantle Earth viscosities for the region for a deglacial timescale (5×10^{18} Pa s to 5×10^{19} Pa s, [Le Breton et al., 2010, Auriac et al., 2013]), along with a subensemble with stiffer rheology appropriate for parts of Greenland (5×10^{20} Pa s). The difference in half-space timescale for the inferred rheology range (@ 1050 yr) is not much larger than the nominal ice sheet dynamical timescale during early to mid deglaciation (average grounded ice thickness/average accumulation = about 850 years). We will include a brief summary of results in the revised submission, but initial analysis indicates that the impact is secondary compared to that of climate forcing and hydrofracturing.

Then , the discussion of ice stream behaviour could be expanded. In the statement "Most of these ice streams activate and deactivate independently...", basal velocities periodically drop to zero, implying complete shutdowns. How is subglacial meltwater involved in these shutdowns? A more detailed discussion of basal melting, geothermal heat flux, and meltwater production would be useful here.

The only way to get complete ice stream shutdown with 0 basal velocity is for the bed to freeze. Even if there were no basal water, there would still be (albeit strongly reduced) basal sliding. So we do not understand what specifically about the role of basal water the reviewer wants more details on, especially since basal meltwater production depends on basal thermal energy balance. The latter is quite non-trivial, e.g., increased basal water pressure reduces basal drag, which in turn increases basal velocities. These two effects have opposing impacts on basal heating. Basal energy balance also depends on ice velocities, basal geothermal heat flux (from the bedthermal module), ice thickness, upstream ice temperature, as well as surface temperature and precipitation histories. As such, we are skeptical that any simple conclusions can be drawn about the exact controls on ice stream activation/shutdown beyond that inferred from the physics of the system.

We have rerun the reference ensemble to save basal water thickness for the ice stream gate analysis and will examine if there is anything of value this can add. We are also rerunning the ensemble with the maximum basal water thickness limit increased from 2 to 10 m to isolate the impact of this limit. Though the GSM has a full distributed subglacial drainage module, previous work for the context of Hudson Strait ice stream cycle [Drew and Tarasov, 2023] has shown that the differences between simplified local drainage and full complexity subglacial drainage modelling are largely within that due to parametric uncertainties. As such, we restrict our analysis to the GSM configuration with the much cheaper local drainage module.

3   Representation of hydrofracturing and ice shelves

The paper would benefit from a clearer discussion of the limitations associated with representing hydrofracturing at a 5° ($\sim 7 \times 6$ km) grid resolution. How well can hydrofracturing processes be captured at this scale? Furthermore, ice shelves appear to occupy relatively limited areas in the northern sector (Fig. 8), whereas southern part lacks floating ice. Quantification of ice-shelf area relative to the total ice-sheet area would be helpful as this is central part of the study and heterogeneous variability would add nuance to the results. In addition, further discussion is needed on how much hydrofracturing occurs in the southern ice-sheet sector and how this compares with the north.

It is generally the case that representing a process at a resolution significantly coarser than the process scale is less challenging than representing that process at a grid scale near the process scale. The coarse resolution effectively allows the process to be smoothed out as a

2

semi-random process (invoking the law of large numbers). On a 5 km grid, it is irrelevant if an actual subgrid drainage channel and set of crevasses are located 500 or 800 meters from a grid cell edge. So we don't see the resolution as a major issue compared to all the other sources of uncertainty in paleo ice sheet modelling (with climate forcing generally standing out as the largest source). The appropriate representation of ice calving (both tidewater and shelf) is not well constrained, and we at least partly address this by including 3 relevant ensemble parameters. In terms of addressing structural uncertainties of the parametrized calving and hydrofracturing, we will carry out an additional sensitivity ensemble with linear (as opposed to the current quadratic) capture of surface melt flux into the crevasses.

The distinction between ice shelf and tidewater margins, and their respective roles during deglaciation, was also raised by Reviewer 1. We address this in detail in our response (AC1; `https://doi.org/10.5194/egusphere-2025-5319-AC1`). In summary, we explicitly showed via a heat map (see Fig. 1 below) that the northern margin is mostly bounded by ice shelves, while the southern margin is predominantly tidewater at 15 ka. Hydrofracturing is also a key component of tidewater calving in the GSM, and sensitivity experiments isolating each mechanism show that both contribute comparably to deglaciation. Thus, regardless of whether margins terminate in ice shelves or grounded tidewater glaciers, hydrofracturing remains critical, with ice shelf hydrofracturing dominating over the northern margin and tidewater hydrofracturing dominating over the south.
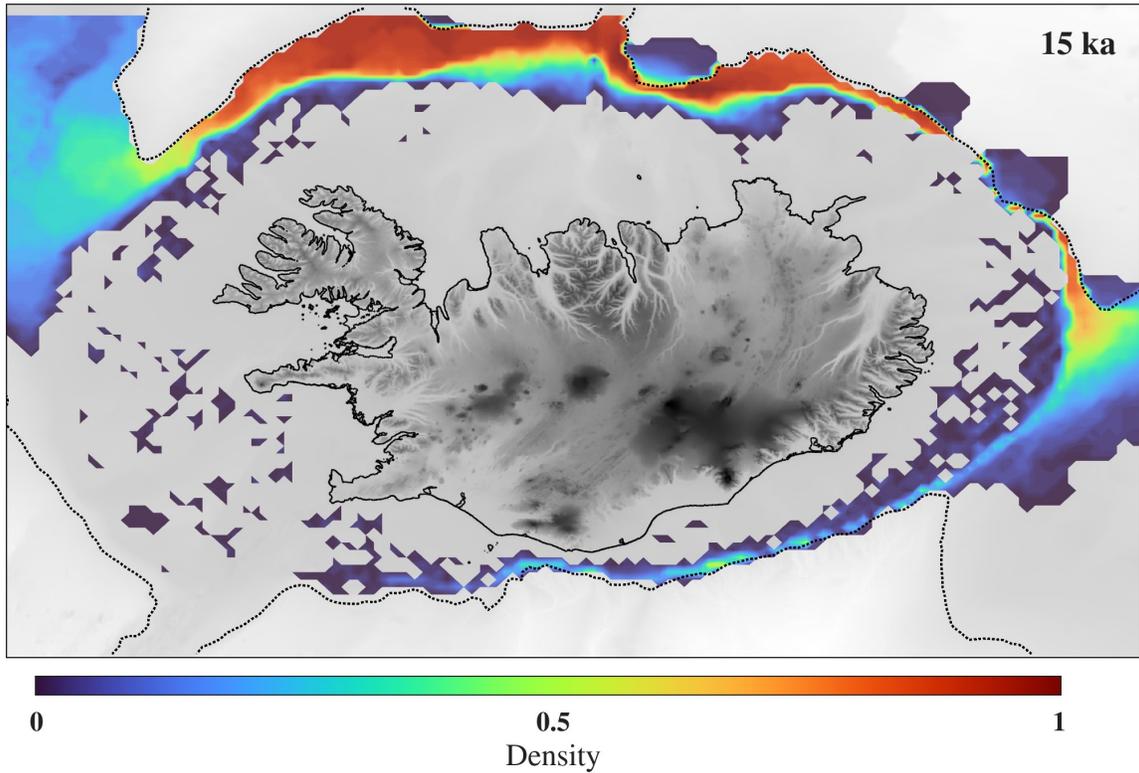
Figure 1: Density distribution of floating ice within the NROY$_{\text{tier1}}$ sub-ensemble at 15 ka. The -860 m contour is represented by the black dotted line.

We compare total and relative (to total ice area) ice-shelf area below in Fig. 2. The total ice-shelf area timeseries was already included in the appendix of our initial submission, and we see no additional insights from inclusion of the relative area timeseries, especially with the much more informative heat map elucidating the spatial density of ice shelves. To better address your concerns, we will instead include 2 additional ice-shelf area heat maps for the 14 ka timeslice (just after the marine deglaciation interval in the reference ensemble) comparing the reference and no hydrofracturing ensembles.
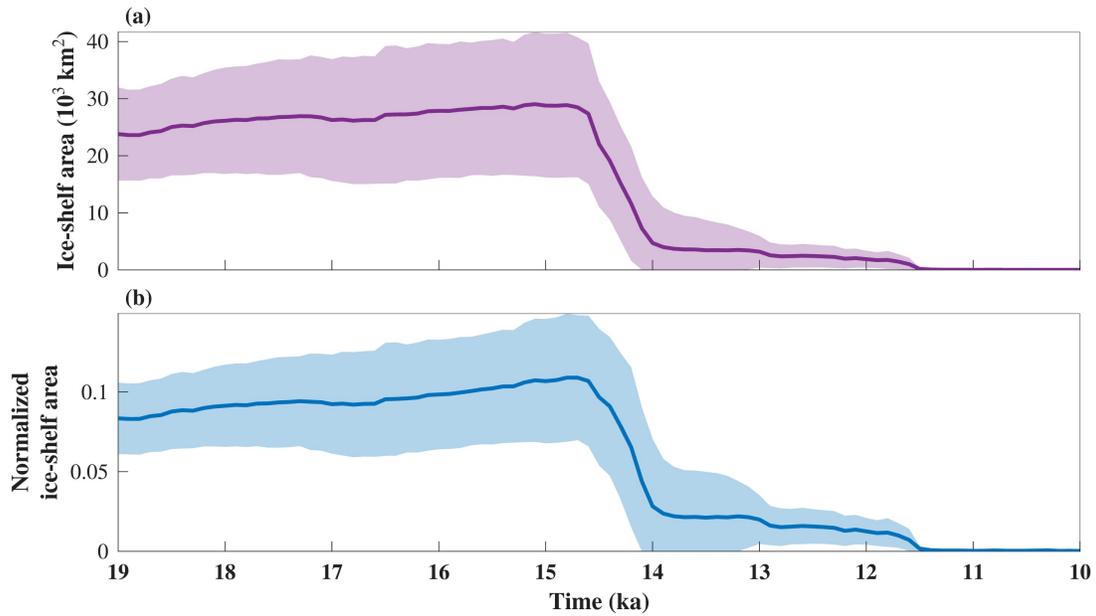
Figure 2: Time series of the ice shelf area (a) and ice shelf area normalized by the total ice sheet area (b) during the last deglaciation for the NROY$_{tier1}$ subset. Lines and shaded areas represent the means and the 2σ ranges.

.

4 Geological constraints and model–data comparison

I think that perhaps a more explicit figure showing empirically reconstructed ice margins, including dating uncertainties, is needed to allow a clearer visual comparison between geological reconstructions and the model runs. Finally, heat-flux reconstructions and borehole locations from Flóvenz and Sæmundsson (1993) and Hjartarson (2015) should be shown on one of the maps to better contextualise the geothermal forcing used in the model.

We will include a new figure or modify the current Fig. 6 in the manuscript to explicitly compare our NROY minimum and maximum bounds to the empirical reconstructions for key time periods. We will also include in the supplement two maps displaying the geothermal heat-flux field distributions, and the locations of the boreholes used to interpolate these fields.

5 Minor comments

1. Line 20: Robel et al. (2019) is cited twice.
   Thanks for noticing : one has been removed.

2. Line 270: Section heading "3.2.1 pre-LGM" should be capitalised.
   This has been addressed.

3. Figure 7: Ensure that minimum and maximum extent labels are clearly identified and, if possible, differentiated by colour.
   This will be addressed by changing either the colour or the linestyle of the minimum and maximum ice extent contours.

References

A Auriac, KH Spaans, F Sigmundsson, A Hooper, Peter Schmidt, and Björn Lund. Iceland rising: Solid earth response to ice retreat inferred from satellite radar interferometry and visocelastic modeling. *Journal of Geophysical Research: Solid Earth*, 118(4):1331–1344, 2013.

Matthew Drew and Lev Tarasov. Surging of a Hudson Strait-scale ice stream: subglacial hydrology matters but the process details mostly do not. 17(12):5391–5415, DEC 19 2023. ISSN 1994-0416. doi: 10.5194/tc-17-5391-2023.

Eline Le Breton, Olivier Dauteuil, and Guillaume Biessy. Post-glacial rebound of iceland during the holocene. *Journal of the Geological Society*, 167(2):417–432, 2010.