RC1 - Authors answer to comments

The authors thank the reviewer for their useful comments. They have all been considered and the manuscript will be updated accordingly. Below is our answers to each individual comment.

Line 53. Paxman not in references.

The reference will be added in the bibliography

Lines 74-79. I had to read this paragraph several times to follow it. Perhaps less dynamic words such as: 1) *absence* of glacial deposits offshore, ii) *infill* of fjords and glacial valleys onshore, and iii) *replacement* of a wedge……?

The authors will provide an updated description according to the reviewers suggestions

Fig 5. Caption. You are asking the reader to flip back to Fig 3 to follow the diagram. Why not label the columns and the two rows?

The figure will be updated

Fig 6. Boost caption a little to explain what it shows? I know it is in the text but the reader needs more help when looking at the diagram.

The caption will be expanded

Line 419. It might be clearer to start a new paragraph when moving on to retreat.

This will be updated

Fig 7. Caption. You ask us to go back to Fig 3 to understand the colour scale. Why not add a colour scale to this diagram?

The figure will be updated

References. Details of many of the references in my pdf are messed up. Check before publication?

The references will be fixed

RC2 - Authors answer to comments

The authors thank the reviewer for their useful comments. They have all been considered and the manuscript will be updated accordingly. Below is our answers to each individual comment.

Minor changes:

Please change the background used in the figures so that the colours used to display the data are not also used within the background.

The authors will change the opacity of the background layer in order to separate the background from the data to be displayed while still keeping a lifelike colormap for topography and bathymetry

A table summarising the different experiments may be useful, even if just in the Supplementary Information.

As the only thing separating the three experiments are changes to the topography and bathymetry (presented in figure 2) the authors think a table would be redundant

Figure 1: The graticule labels are hard to read, the white labels work better.

The figure will be updated with white graticules

Line 74: Quaternary, not Quaternar

This typo will be fixed

Line 117: A_flow, not A

This will be fixed

Table 1: This is a very useful table. I would recommend including all the notation used in the paper in this table. It would also be more usable if the rows were in alphabetical order (as much as possible) to find the parameters easier. I would also change the range of values to [min, max], as opposed to [max:min], and label the middle column something such as 'Description of parameter'.

The table will be changed according to the reviewers suggestions

Line 165/166: Define T_positive

The text will be updated

Line 192: Represents, not represent

Typo will be fixed

Line 198: Between geothermal heat flux, not between of geothermal heat flux

Typo will be fixed

Line 198/199: Include the parameter symbols inline, e.g. geothermal heat flux from the bed, q_b, and the heat flux from the temperature gradient in the basal ice, q_c.

The text will be updated

Figure 2: The c and d panels are unclear.

Details in this map are not necessarily key to the study and we have focused on using a colorblind friendly perceptually uniform divergent colormap to make the figure simple. This way we can show added material in blues and removed material in reds, sacrificing some minor details in the map while keeping important features (Norwegian channel, North Sea during PREQ) visible.

Figure 3: The panels g-i might benefit from a non-linear scale to make the differences clearer. The colour gradient for g-i and j-l should be different if they are different outputs, and this particular colour choice is hard to see.

The color scale for g-i will be updated according to the reviewers suggestions. The choice of these specific scientific colorscales have been made in order to comply with colorblindness accessibility (Crameri, F., G.E. Shephard, and P.J. Heron (2020), The misuse of colour in science communication, Nature Communications, 11, 5444.)

Section 3.1: This section could benefit from being split up into more than one paragraph. Suggest new paragraphs on lines 302 and 311.

The section will be split at these locations

Line 332: 10,000, not 10.000

Typo will be fixed

Line 431: Capitalise figure

Will be fixed

Figure 7: This figure would benefit from including the colour bar so it is easy to interpret without the previous figure.

A colorbar will be included

Lines 470/471/472/506: Swap the years round, e.g. (477-429 ka)

According to the suggestions of another reviewer these lines have been removed

Line 476: 0.5 Ma, not Ma ka

Typo will be fixed

Line 500: Build-up

Typo will be fixed

RC3

The authors thank the reviewer for their useful comments. They have all been considered and the manuscript will be updated accordingly. Below is our answers to each individual comment.

More specific comments:
32: The focus of this citation was on chronological data, rather than geomorphology. Stroeven et al 2016 would probably be more appropriate here.

The reference will be changed according to the reviewers suggestion

Sec 2.1.1.
- Is there a eustatic sea-level forcing? Are the relative sea level changes observed in Fig 7, for example, purely isostatically driven? Presumably, any SL forcing is also consistent between experiments. Mention of calving in the model too would be useful as it seems to be an important regulator of ice sheet extent in the North Sea. The model and parameters used are otherwise well described.

The eustatic sea level is changed with the glacial index defined by the normalized LR04 stack. A sentence describing this will be added to the manuscript. Calving is not implemented in the model. In section 2.1 we describe how floating ice is removed by enhancing melt.

3.1.
321: There are additional ice-divide reconstructions over the North Sea that could be used to contrast with here: see Clark et al. 2022 and the BriticeChrono reconstruction.

A sentence will be added with this reference

4.1.
- It's not critical but it could be interesting (and within scope) in this section to discuss differences in the inception of the ice sheet, rather than just the maximum extents. For example, the similarity of your PREQ-onshore and reference volume trajectories seems to indicate that the uplift of plateau areas/changing hypsometry during the Quaternary has not introduced any major ice-elevation feedbacks that may have enhanced ice-sheet growth through time. Or maybe that they are countered by other feedbacks such as enhanced ice drawdown through fjord outlets?

We would argue that to answer such questions we would need to be able to run these models at a higher resolution and possibly with the inclusion of transient glacial erosion, which would be interesting, but require a different study.

- The volumes of Hughes et al. do not provide a particularly robust validation in my opinion, based on their over-simplified derivation. For example, their total EISC volume is >30% larger than the SLE value stated earlier on line 37. A more effective comparison might be to compare patterns and magnitude of loading with previous studies e.g., Vachon et al. 2022 Fig 7A, or similar.

The comparison with Hughes et al. in this regard was mainly to state that our peak volume approximately resembles the lgm peak. We will add additional references and change the wording as to not rely solely on their (not so robust) estimate, however comparing patterns and magnitude of loading would require an additional figure, which we feel would be excessive for a relatively minor point in the discussion.

- Though I understand you aim to isolate the impact of landscape geometry, there are knock-on effects on the grounded ice by not accounting for ice shelves. For example, I imagine buttressing effects resulting from a relatively larger ice shelf across the PREQ North Sea would reduce the volume differences in this sector (e.g., Fig 5C). Gasson et al. 2018 is a palaeo example to compare with, and think it would be a useful caveat to mention E.g., '10% is a potential maximum relative volume reduction during PREQ, though second-order ice dynamics resulting from processes such as x and x could reduce this value.'

This consideration will be added to the section

- While I agree that landscape evolution is one of many factors complicating the use of a consistent proxy for ice volume, I don't think it's a fair assumption that the proportional differences between the global LR04 record relate directly to proportional volume differences of this one ice sheet. Take the volume evolution of the Barents Sea ice sheet through the Weichselian, which does not track d18O. You do argue this later in the paragraph so feel this LR04 comparison is not particularly useful. I would suggest instead removing this sentence starting on 491, and emphasise this added uncertainty for Quaternary ice-sheet reconstructions e.g., Batchelor 2019; Knies et al 2009, particularly in marine sectors. For example, the insights here are particularly relevant for the Barents Sea domain too which experienced an opposite transition from terrestrial to marine-based dominated.

Following the reviewers suggestion this paragraph will be modified and the suggested sentence will be removed.

4.2.

514: Boulton and Hagdorn were in fact explicitly unable to reproduce an 'ice stream funnelling ice along the entire length of the Norwegian Channel', and were highly sceptical of the idea, demonstrating similar time-transgressive zones of streaming as in your experiments.

This will be corrected in the manuscript text

526: I think there are many other major reasons why you cannot rule out continuous ice streaming in the channel (besides the sliding limitations given), which mainly revolve around how the ice saddle and regional ice divides developed. This is not a critique though - it is a well-known modelling challenge, so a wider context here would be relevant. That the saddle persists through to late-stage deglaciation may be down to more fundamental aspects e.g., the presence of the 'ice wall', or that the lake drainage event was crucial here. Gandy et al. 2020 and their approach using a negative SMB anomaly in the southern North Sea would be useful to contrast with here in this respect.

The authors will include this point about the saddle in the retreat of the ice sheet as well as the Gandy et al study

529: In terms of the main discussion point here, being the formation of the Norwegian Channel, I think it would be useful to touch a bit deeper and be more explicit on the ice dynamics between the experiments. My takeaway is that the streaming patterns needed to erode the trough (shown in MLQ onwards) only appear when a saddle forms. The limiting factor for this based on PREQ seems to be the central North Sea depression, which effectively restricted the margin through calving? What are the reconstructed water depths here for example? Maybe there are other mass balance feedbacks onshore though? Just think it would be useful to be a bit more explicit on what's driving the differences between experiments. It could be illustrated simply in a figure showing calving/melt anomalies from the ref figure, like in Fig 6B.

We will expand our discussion to add more on the driving mechanisms behind the different ice flow patterns seen in our experiments. In addition, we include supplementary videos showing every time step of our model simulations. Among others, these videos will illustrate much better that the sliding patterns in the outer part of the Norwegian Channel (even before the depression is formed) are controlled by the ice-surface gradient toward the shelf break and that this pattern of high sliding starts before the formation of the saddle. The saddle will however enhance the sliding velocities in the channel toward the north as the ice cannot flow westward.

I appreciate the choice of the 'scientific' colour ramps on the figures.
The details in Fig2 are not particularly obvious at 100% scale, but the resolution in the pdf was sufficient when zooming in.
Fig 5/7: Suggest to include all the necessary labels/legends in the figure.
There are a fair number of typos/grammatical mistakes that should be checked throughout e.g., the very last line 555: ...*the North Sea. The manuscript otherwise reads very well.

Some citations have unusual formatting of the page numbers.

All these points will be addressed in a revised manuscript