Answers to 2nd review for 'Ice Motion Across Incised Fjord Landscapes' by Barndon et al.

We would like to thank both reviewers again for their time and valuable comments. In addition to the points raised by the reviewers some spelling mistakes and grammatical errors not listed here have also been addressed.

Sjur Barndon on behalf of all co-authors
March 25, 2026.

Reviewer 1

In the revised draft, the authors have made numerous improvements: thanks to the authors for incorporating my suggestions.

> Thank you.

1.1. Page 1, line 12-13: I think this statement about regularized Coulomb (rC) could be too specific, because the ice flow in these unresolved valleys could be the reason the rC works (or not!) I suggest removing that parenthetical. I understand the reference to the discussion section where you discuss this point, but in the abstract it is a little confusing and could be simplified by saying less.

> Addressed and amended as suggested.

1.2. Page 3, line 38: I did use topographic data derived from radar for the Gamburtsevs. Did the authors mean that the resolution wasn't high enough? This comment comes back in the discussion section, so I think it is worth mentioning.

> Thank you for pointing this accidental misrepresentation on our part! Now updated to:
>
> However, these studies did not include ~~real topographic data,~~ temperate ice rheology or rate-dependent resistance for slip at the ice-bed boundary, with simplified 2D topography in the case of Gudmundsson (1997) and simplified 3D topography and a 2D radar transect profile in the case of Meyer and Creyts (2017). Here, we incorporate these additional processes and consider fast ice motion for a section of the palaeo Scandinavian Ice Sheet over 3D high-resolution digital elevation data

Reviewer 2

The authors have made significant improvements to the manuscript, including major revisions to the Abstract, Introduction, Discussion and Conclusions. Many thanks for your time and effort. I believe that the manuscript could benefit with some additional minor revisions before publication to improve clarity, details of which are included in the attachment. This is my second review of this manuscript. I would like to thank the authors for the time taken to revise the manuscript. The clarity and readability of the text have improved substantially. I also welcome the inclusion of further details on the opening angle, as suggested by the other reviewer. Despite these improvements, I believe that the manuscript still has some shortcomings, which are outlined below. For this reason, I suggest further minor revisions to the text before publication. I have separated my suggestions into a section that covers general concerns followed by line-by-line comments.

> Thank you.

General Comments

2.1. My main criticism is in relation to the discussion about friction laws in the paragraphs beginning on lines 227 and 245. I find the text interesting, but somewhat long and too detailed given that the study does not compare friction laws. I would try to emphasise what the main message is to the reader and how it builds on your work. In doing so, I would suggest reducing the text to one paragraph. The text could be strengthened by (1) providing a stronger link to your results, and (2) emphasising which details are suggestions for future work. For example, the text "...present an obstacle to..." in the opening sentence is quite vague. I see that your results are referred to, but they are somewhat lost in a long sentence (line 239) that is difficult to parse.

> Thanks for pointing this out. We agree that this section could be a bit clearer and linked in to the results more effectively.
>
> This said, we also think that the two topics here are each worthy of their own paragraphs, with the first paragraph now slightly longer given the connection back to our results. The first relates directly to the slopes found in our study site and across western Norway (Fig. 1), and the second to cavitation. We think that reducing this to one paragraph would not allow sufficient background information for the reader.
>
> In response to this point we have included a strengthened connection back to our results:

As we hold basal-slip parameters (Eq. 11) constant in both settings, our simulations allow quantification of the influence of fjord geometry on driving stress, showing a significant increase required for the same surface velocity when a high-resolution fjord is use rather than a smoothed representation (Fig. 3).

and made the cavitation configuration problem more prominent:

Determining the configuration and influence of subglacial cavities in a setting such as Veafjorden should be a focus of future research. Nonetheless, for now we suggest that features such as Veafjorden provide major upslope resistance that is (i) not overcome by cavitation and (ii) not captured by the basal boundary position of ice-sheet models.

We have also made several other smaller edits for clarity, and combined the paragraphs discussing cavities and application to Greenland, removing one long sentence in the process.

Many of these issues are explored in much greater depth in a paper in review that RL and AB are involved in (https://arxiv.org/abs/2407.13577). We didn't want to repeat efforts here, but we also don't feel it is appropriate to cite the pre-print as the manuscript has not yet passed review and there is large author-overlap between the two studies. We hope we have arrived at a solution to the problems described, but it is not possible to derive in the space available in this paper.

2.2. Some other sentences in the manuscript remain quite long, unclear or overcomplicated. I have outlined several in the line-by-line comments below. Generally, the sentences that are most difficult to parse and require multiple readings are those divided by brackets with further information or em dashes (—).

These points are addressed in the line-by-line comments, see **2.14**.

2.3. The paragraph beginning on line 215 in the Discussion describes results related to the opening angle that have not been mentioned in the Results section. I would suggest moving these details to the appropriate section.

> This information is now added to the last section of the results, where the smoothed control and high-resolution topography are compared:
>
> Furthermore, the opening angle (the angle between the two valley sides measured along the flow direction) is much wider in the smoothed control topography (~170°) than the high-resolution model (~100°), and there is no indication of flow reversal or Moffatt eddies.

2.4. Some confusion remains regarding terminology, e.g. regarding the use of the terms "real topography" and "smoothed topography" (please see my line-by-line comments). In some places, the word "thickness" is used, and I am left wondering whether you mean "plateau thickness" or some other thickness. I would suggest clarifying this throughout the manuscript.

> The phrase 'smoothed topography' is now changed to 'smoothed control topography' throughout. The term *real* topography is replaced with high-resolution topography throughout. The introduction of the high-resolution topography is now updated in the methods so that it does not mention 'smoothing', see comment **2.8**. The word 'plateau' is now added to all remaining mentions of 'plateau ice thickness'.

Minor Comments

2.5. Line 13: This sentence is a bit unclear. I suggest explicitly stating what you mean by "These results" and "surface velocity variations". I would also explicitly state whether you are referring to observed or modelled velocities. Also consider changing "under-appreciated" to "under-represented in models".

> The following changes have been made:
>
> **Line 13:** ~~These results ...~~ Similar topographic features may explain ...
> **Line 15:** ...ice-sheet motion may be ~~under-appreciated~~ under-represented in models.

2.6. Line 34: What kind of "glaciological instabilities"? The word "a" is repeated.

> Addressed like this:
>
> ...it may be that geological factors, rather than ~~inherent glaciological instabilities~~ dynamic self-organisation of ice streams (Kessler et al., 2008), exert a first-order control on fjord orientation.

2.7. Line 44: To help tie this paragraph together, I would add one sentence that explains briefly why you perform simulations with different flow directions.

> Added sentence **Line 46:**
>
> The flow direction is varied both to simulate the historical shift in flow angle during deglaciation, and to assess how anisotropic bedrock influence ice dynamics.

2.8. Line 57: There is currently no mention of "smoothed topography" or "real (high-resolution) topography" here. This leads me to wonder which topography dataset you are describing. If it is the "real (high-resolution)" dataset that you are describing here, then the mention of "smooth out artifacts" is quite confusing. In this section I would describe both the "real (high-resolution)" and "smoothed" datasets so that it is very clear what the difference is.

> To clear up these overlapping terms, the phrase "to ~~smooth out~~ artifacts" has been replaced with "to remove artifacts". Additionally, the dataset we refer to as the smoothed topography is now described directly afterwards, as per comment **2.11**. The term 'smoothed control topography' is now used throughout.

2.9. Line 65: Was the mesh generation done also with gmsh? If so, please mention this. In addition, this sentence is not so clear and could do with rephrasing or splitting into two sentences.

> The definition of 'plateau ice thickness' is moved the introduction where it is first mentioned. The following changes are made at **Line 70**:
>
> The domain was discretised with gmsh for Elmer/Ice Version 9.0 (Gagliardini et al., 2013) using ~~with~~ a triangular mesh with a representative element side length of 25 m and 20–30 vertical layers with resolution increasing towards the base. The smoothed control topography mesh has a representative side length of 100 m and 15 vertical layers ~~for the smoothed topography simulation~~ with 500 m

plateau ice thickness ~~ice thickness measured from the highest point~~.

2.10. Line 68: What is meant by “First” and “Second”? Do you mean after running the simulations for X number of years? Please clarify in the text.

The following changes have been made:

In a first stage, the free surface is allowed to vary and the inflow and outflow and two lateral boundaries are matched periodically for velocity, stress, and free-surface position. This simulation stage runs until a steady state is reached and surface elevation no longer varies. In the second stage, the free surface...

2.11. Line 129: This sentence would fit in well in the first paragraph of Section 2.

Moved and altered:

Starting on Line 64: Additionally, a ~~The~~ smoothed control topography was created for comparison runs by applying a Gaussian filter with a standard deviation of 50 grid cells to the original DEM.

2.12. Line 235: “maximum up-slope angle in the mean flow direction” Could you please clarify what is meant by this?

Thank you for the comment. The sentence refers to the maximum angle of the bedrock slope measured along the flow direction, and we feel the original phrasing is sufficiently clear in context. We have therefore left this sentence unchanged.

2.13. Fig. 8: I would suggest increasing the size of z_r and z_d

Done!

2.14. Remaining line-by-line comments.

Line		Changes
2	-	thermodynamic
	+	dynamic
9	-	real
	+	high-resolution
18	-	it's
	+	its
31	-	but not at its fullest extent
	+	but not while the ice sheet was at its fullest extent
44	-	The simulation ensemble
	+	In this study, the simulation ensemble
53	-	ice thermodynamics
	+	ice dynamics
80	-	axes
	+	axis
109	-	Change
	+	The change
209	-	In contrast to smoothed topography, real topography can result in much higher driving stresses in the same surface velocity setting, with clear evidence for a strong anisotropic response of ice motion to the alignment of the underlying landscape.
	+	Comparing smoothed control and high-resolution topography simulations, when held at similar surface velocities by adjusting the domain slope, reveals substantially higher driving stresses in the case of high-resolution topography. This is clear evidence for a strong anisotropic response of ice motion to the alignment of the underlying landscape.

Line		Changes
217	-	the 3D simulations of Meyer and Creyts (2017) (their Fig.9).
	+	the 3D simulations in Fig. 9 of Meyer and Creyts (2017).
218	-	fit as neatly
	+	not well represented by
220	-	falls within
	+	is smaller than
221	-	The opening angle along the prescribed 45° flow in the oblique simulations are $\sim 118°$ predicting Moffatt eddies to form. No eddies ...
	+	Oblique flow simulations have an opening angle of $\sim 118°$, which would predict the formation of Moffat eddies. However, no eddies ...
228	-	In bounded basal traction relationships (if effective pressure, N, is constant) the traction provided by the bed approaches a maximum value that it does not exceed (such as the regularised-Coulomb relationship used for individual elements in this model, Eq. 11.
	+	In bounded basal traction relationships (i.e. Eq. 11) the traction provided by the bed approaches a maximum value for a given effective pressure that is not exceeded.
245	-	Classically, cavities may be anticipated to drown out high bed slopes in many instances thereby facilitating bounded basal traction (Schoof, 2005) – including in the inverted ellipsoid geometry of Gagliardini et al. (2007) which bears similarities to the across-fjord profile in our simulations.
	+	Classically, cavities may be anticipated to drown out high bed slopes thereby facilitating bounded basal traction (Schoof, 2005; Gagliardini et al., 2007).

Line		Changes
257	-	As slopes, cliffs, and fjords exceeding 30° in the palaeo flow direction are common across western Norway (Fig. 1) and at least partially representative of the basal topography of the GrIS, we suggest that interactions with these features may go some distance towards providing a physically based background to the persistent utility of unbounded power-law sliding relationships across the GrIS ...
	+	As the bed slope of cliffs and fjords with flow-aligned slopes exceeding 30° are common across western Norway (Fig. 1) and at least partially representative of the basal topography of the GrIS, we suggest that interactions with these features provides a physically based background to the persistent utility of unbounded power-law sliding relationships across the GrIS ...
291	-	We show that incised fjord landscapes induce significant complications into ice sheet motion.
	+	We show that the orientation of fjords relative to the flow direction has a substantial influence on the ice flow magnitude.