Answers to reviewers for 'Ice Motion Across Incised Fjord Landscapes' by Barndon et al.

We would like to thank both reviewers for their time and valuable comments.
All minor remarks with the note 'Fixed!' are addressed as the reviewer suggest. Amendment of broader comments are explained in the notes below before listing the changes to the manuscript. After reading both reviewer comments, major revisions have been made to the discussion chapter.

Sjur Barndon on behalf of all co-authors
September 26, 2025.

Reviewer 1

Hello and nice paper! This one seemed like a good one to sign given that I reference my own work. Feel free to take the comments and suggestions with a grain of salt: I am excited that you all are working in this area and looking forward to chatting more about it. Feel free to reach out offline with any questions, <colinrmeyer@gmail.com>.
Cheers, Colin R. Meyer

> Thank you.

General

In this paper, the authors analyze ice flow over deeply incised fjords using a finite-element model that includes a nonlinear ice rheology, a model for temperate ice, and a sliding law. This extends earlier work by Gudmundsson (1997) and Meyer and Creyts (2017), both of which focused on idealized geometries, and applies it to a realistic landscape. In Meyer and Creyts (2017), I performed a seedling version of this type of work, when I analyzed data from the Gamburtsev mountains in Antarctica. I have thought about continuing along this thread and doing something like this piece of work, so I am glad that they have gone down this path. The paper is in good shape: it is relatively straightforward. I have a few remarks and some small comments, but otherwise, I am happy to support publication after minor revisions.

> Thank you.

Remarks

1.1. I emphasized the role of the critical angle to form Moffatt eddies in my paper, but this idea doesn't seem to show up in the current paper. I find this curious, since it was a crucial piece of information that I used to assess whether a certain area may support Moffatt eddies. Something like my figure 10(c) would add value to figure 1 in the current paper. Or at least connecting the slope angles to the onset of Moffatt eddies. Apologies if I missed this point.

It is a little challenging to directly compare the critical angle found of the idealized wedge-shaped valley investigated in Meyer and Creyts (2017) to the natural topography of Veafjorden. We also treat Moffatt eddies as a secondary result of this paper, rather than the primary focus. Nonetheless, we have introduced the concept of Moffatt eddies more prominently:

Starting at Line 36: [...] with Meyer and Creyts (2017) investigating the role of a critical angle in V-shaped valleys to predict the onset of Moffatt eddies (spiralling ice flow that form in topographic hollows).

Also, by estimating the opening angle for Veafjorden and its smoothed counterpart, it is clear that the critical angle found by Meyer and Creyts (2017) holds for our results. We recognise the importance of linking these concepts together in the paper, now mentioned in the discussion:

Starting at Line 217: The idealised topography in these simulations invite ascribing a critical angle for Moffatt eddy formation. However, this is complicated somewhat by our use of real topography which does not fit as neatly as the triangular geometry found in (Meyer and Creyts, 2017). Nonetheless, an opening angle perpendicular to the fjord of $\sim 100°$ falls within the critical opening angle $\alpha_c = 134°$ for the rheological exponent $n = 3$ in (Meyer and Creyts, 2017), while the opening angle $\sim$170° for the smoothed simulation is above this α_c value. The opening angle along the prescribed 45° flow in the oblique simulations are $\sim 118°$ predicting Moffatt eddies to form. No eddies are seen in simulations with these settings. However, this may be explained by the shift in flow direction exerted by the topography (See Fig. A3d-f) further widening the opening angle. This indicates that a critical value may also have efficacy in predicting Moffat eddy occurrence in real settings but that low fidelity bed topography products where depressions such as fjords are not resolved are unlikely to be effective indicators of possible Moffat eddy locations.

1.2. The oblique simulations follow directly from the oblique simulations that I performed – my figure 9. It is cool that we see similar things, which is a point that the authors could make. The novelty of the simulations is clear, so there is no need to avoid pointing out connections to prior work.

> Thank you for bringing up this comparison. First of all, I believe we are using different terminology when describing the orientation of the fjord relative to the flow direction. Our oblique simulations, with flow direction striking 45° in relation to the fjord orientation, show no Moffatt eddy formation. We agree that there are clear similarities between the Figure 9 simulation in Meyer and Creyts (2017) and the simulations of Veafjorden with perpendicular flow. They are essentially showing the same general patterns of ice motion and formation of Moffatt eddies. This comparison has now been added to the discussion.
>
> **Starting at Line 215**: The complex flow patterns of Moffatt eddies are also dependant on topographic fidelity. The patterns of flow shown in our perpendicular simulations using real topography (Fig. 5a,b), including eddies and lateral transport along the fjord hollow, closely resemble the 3D simulations of Meyer and Creyts (2017) (their Fig. 9). The similarity to the idealised topography in these simulations invite ascribing a critical angle for Moffatt eddy formation. However, [...]

1.3. The question of the effect of Moffatt eddies on the effective sliding law is fascinating and something I have wanted to dig into more. Thus, I agree with your conclusions that "future work should focus on parameterising the net influence of this behaviour," but I will admit that I was a little sad not to see more of that included in the current paper – building off the discussion around figure 8.

> We very much agree that this is a fascinating problem (and we would be happy to talk further about this direction externally to this review process). The short answer is that this is a difficult question and the conclusions of this paper may not be the appropriate place to address it (though some ongoing work is). Nonetheless, we think it would be remiss not to mention the possible importance of these features for ice-sheet motion in certain settings. In response to this, comment 1.5, and a bit of reflection on our part after submitting but before reviews we have made some major changes to the discussion, particularly surrounding Eq. 14 (which was incorrect, now removed) and Fig. 8 (now updated). We hope this simplifies the discussion somewhat while retaining the most salient points.

1.4. At the end of the paper, I am left struggling for the main message. I agree that ice flow over deeply incised fjords will "induce significant complications into ice sheet motion," but this wouldn't be surprising after Gudmundsson (1997). The connection with the sliding law is novel, and could be the main message but it is underdeveloped. The different directions of flow are interesting but not clearly actionable. For this reason, the conclusions section falls a little flat. I think including some onset angles as suggested above and doing some statistics to suggest how widespread this phenomena could be along the path to developing a stronger conclusion.

> Thanks for the comment. We agree that there are a fair few directions coming out of the work and that it's a bit tricky to tie these together. This came from the 'what if' nature of the original question, rather than a specific hypothesis led investigation, but we hope that with the new changes to the conclusion we have neatened things up. We agree that one could infer from Gudmundsson (1997) and your own work that fjord/relict landscapes are likely to induce significant complications to ice flow. However, we do think it is well worth stating this firmly at the opening of our conclusions and situating it as a thermodynamic process that could be dynamically important at a regional scale. We hope the revised conclusions help emphasise these points. Given that the entire conclusion is re-written, we have not re-printed it here.

Specific Comments

1.5. Around line 100: I think it is worth referring to the Schoof and Hewitt (2016) enthalpy model, given that it is a different formulation.

> Fixed!

1.6. Simulation ensemble: my sense/memory is that the velocity and ice thickness shouldn't matter for the eddy formation.

> Thank you for bringing this up. We agree that the configuration of simulations do not attempt to explore the conditions for eddy formation. Also, yes the velocity should not influence eddy formation when the Reynolds number is low enough (which is always, for realistic ice flow settings). We have removed the reference to ice velocity as something influencing Moffatt eddy formation in the manuscript.
> We have now attempted to make it more clear in the introduction that the ensemble is performed primarily to investigate the effect of simulation fidelity on simulation results. The goal of the ensemble is to compare a heavily smoothed topography, a stand in for BedMachine, to the real topography of the same area. See reply to **Comment 2.1** for

a summary of changes.

1.7. I have wondered about how subglacial Moffatt eddies might be tested in the geologic record. Would there be special deposits in these areas?

This is very interesting indeed. We speculate that for flow perpendicular to fjords, striation markings at the down-flow side of the valley should show flow in the opposite direction. It is also possible to imagine distinct deposition patterns arising in areas with Moffatt eddies. However, the velocity of the ice in the Moffatt eddies is very low ($<5\ \mathrm{m\,a^{-1}}$), even with high surface velocities. This suggests that very little erosion occur in areas where eddies form. The area where Moffatt eddies are located in Veafjorden are now mostly under water. Moreover, as the ice sheet retreats, flow direction shifts to align with the valley, and could likely 'overwrite' any existing deposition or striation. We have now added mention of this in the discussion.

Starting at Line 285: Last, geological evidence for the occurrence of the described flow patterns and Moffatt eddies is presently limited to the plateau striations surrounding Veafjorden (Mangerud et al., 2019). These striation markings indicate near-perpendicular flow across the fjord, but do not provide information about the motion within. Reverse direction striations within the valley are possible, but given subsequent ice-flow reorganisations (Mangerud et al., 2019; Reilly, 2023) it is likely that these lineations will have been removed by erosion or overwritten when ice flow switches to follow the fjord orientation.

Reviewer 2

The article “Ice motion across incised fjord landscapes” by Barndon et al. presents an investigation of the influence of the orientation of fjord incisions on the flow of overlying ice. The manuscript highlights that unresolved features in coarse topography datasets can cause errors in the velocity field in ice-flow simulations. The authors investigate the development of Moffat eddies, spiralling flow features that form in topographic hollows. This work is of particular relevance in areas of Greenland where the ice-flow direction and the orientation of underlying fjords are not aligned. The results of this study are novel and relevant, and the manuscript is well structured. However, the emphasis on particular aspects of the study changes throughout the manuscript. I support publication once the following comments have been addressed.

> Thank you.

General Comments

2.1. Introduction: A number of important aspects of the study could be better emphasised toward the end of the Introduction. It should be clear to the reader what the specific research gap is and how it is addressed. For example, I am missing an explicit, yet short, description of the numerical experiments performed, which metrics are used and what the specific aim of these experiments are. In particular, I suggest emphasising (1) the effect of anisotropic topography orientation on ice flow and (2) the influence of the data resolution on ice flow. It would also be helpful to briefly mention aspects that become important later in the manuscript, including the role of temperature, friction and Moffat eddies. Although implications are briefly mentioned at the end of the Introduction, I suggest strengthening these arguments. For example, what are the implications of under-resolving anisotropic topography for inverse methods? How might coarse resolution datasets influence large-scale simulations and sea-level projections?

> Thank you for the detailed feedback. In the revised manuscript, these comments are addressed in the rewritten introduction. Some paragraphs have been moved around for improved flow and clarity. The concept of Moffatt eddies is now introduced (see Line 39). The new paragraph regarding the simulation ensemble is given below (Line 44).
>
> **Starting on Line 39**: In order to assess the difference in controls on ice motion between this *real* topography, and the smoother bed products used to model the GrIS and AIS (BedMachine and BedMap), we ~~additionally~~ create a smoothed representation of Veafjorden (henceforth referred to as the smoothed topography). This substitute provides ~~an opportunity~~ the basis to examine the impact of the

generally low fidelity elevation models presently in use.

Starting on Line 44: The simulation ensemble consists of 16 simulations, each with a unique ice flow scenario. The varying parameters are (1) target surface velocity, (2) flow direction – parallel, oblique and perpendicular to the fjord incision (See Fig. 2) – and (3) plateau ice thickness. We also perform 4 comparison simulations with the smoothed topography. The smoothed topography simulations all have perpendicular flow direction while the other two parameters are still varied. By simulating Veafjorden with both the smoothed topography and the *real* topography, we reveal the influence of realistic anisotropic basal conditions and limited bed topography fidelity in ice-sheet models.

Starting at line 51 we mention anisotropy but leave the main exposition of its relevance to the discussion. At **line 277**:
Our results also illustrate the anisotropic resistance to ice-motion provided by real subglacial landscapes (Fig. 3), and situations where the basal velocity vector may be non-parallel to its corresponding basal traction vector over a large area (Fig. A3) – previously explored in an idealised case by Hindmarsh (2000). This implies that basal traction patterns should be expected to vary over the lifecycle of an ice sheet, as drainage basins evolve and ice motion patterns shift during growth and decay. Bulk basal motion may furthermore be misaligned with the applied basal shear stress, which could result in complications for dynamic modelling where fine details are important. We leave direct quantification of the influence of landscape anisotropy on basal sliding relationship anisotropy for future work, but recognise that the impact of basal sliding anisotropy is likely small for most predictive timescales (100-1,000 yr) in the event that flow orientations do not shift substantially.

Given there are no anisotropic sliding relationships with the exception of Hindmarsh (2000), and none incorporated into large-scale models to our knowledge, we lack a little bit the vocabulary to describe these features and do not wish to be too prescriptive given that many other anisotropic landforms (aside from fjords) exist. We therefore leave more in depth discussion on applications to inversions to future work.

2.2. Vorticity of Moffat eddies: The results are nicely presented and I am particularly intrigued that such simulations can resolve Moffat eddies. A natural method to quantify vortices or eddies is by calculating the curl of the velocity field, i.e.

$$\boldsymbol{\omega} = \nabla \times \mathbf{u} = \left(\frac{\partial u_z}{\partial y} - \frac{\partial u_y}{\partial z}, \frac{\partial u_x}{\partial z} - \frac{\partial u_z}{\partial x}, \frac{\partial u_y}{\partial x} - \frac{\partial u_x}{\partial y} \right). \tag{1}$$

The magnitude of this vector quantifies the spin, and the direction indicates the axis of spin. It would be valuable, though not essential, to provide some quantitative analysis of the vorticity in the different flow configurations.

> Thank you for these positive comments. The Moffatt eddies found in the ensemble are limited to four of the sixteen simulations included. These four simulation all have perpendicular orientation with varying target velocity and plateau ice thickness. The two varying parameters do not seem to have a large impact on the formation of the Moffatt eddies. It would be interesting to quantify the eddies by curl to investigate the effects of surface velocity and ice thickness on the shape and spin of Moffatt eddies. On the other hand, we regard the occurrence of Moffatt eddies as the main finding in this section, rather than a quantification of their features. We compare these perpendicular simulation eddies to the lack of Moffatt eddies in the other simulation settings. In the revised manuscript we have tried to make these points more clear. See reply to **Comment 2.1** for a summary of changes.

2.3. Discussion and Conclusions: I suggest better aligning the Discussion and Conclusions with the Results section as a few key areas of investigation are missing, such as the Moffat eddies. Some parts could benefit from an improvement in structure and clarity, such as the paragraphs beginning on lines 205 and 242. I would also suggest placing greater emphasis on the potential impacts of the anisotropic topography and the resolution on inverse methods and ice-sheet projections. I appreciate the approach of suggesting a parameterisation of unresolved hollows in the bed topography. However, Eq. (14) does not follow directly from Glen's flow law and I suggest describing the idea in the text or providing a formal derivation. Given that there is very little mention of friction laws in the Results section, I find that there is an over-emphasis and I suggest condensing this text.

> Thanks for the comment. Also following points 1.4, 1.5, and some internal discussions we have made substantial changes to the discussion and conclusion. We have given some further emphasis to anisotropy and issues relating to inversions (also tied to your comment, 2.1). At line 250 for example we cover the issue that ice sheet models may be mis-parameterising the behaviour at Veafjorden and we further highlight possible issues with misinterpretation of inversion-derived traction fields

at line 271. However, considering reviewer 1's comments on expanding the section related to sliding laws, we have aimed for a middle road in keeping the discussion of it at a similar length but tightening up its coverage somewhat. Eq. 14 has been removed to focus on slope angles.

Specific Comments

2.4. A number of sentences throughout are long and/or unclear. Specifically, the sentences beginning on lines 8, 16, 23, 27, 33, 75, 127, 131, 143, 197, 201.

Thank you, this is helping to improve the readability of the manuscript. We've listed the changes in the table below.243

Line		Changes
8	-	When compared to smoothed topography, perpendicular flow over *real* topography requires ∼41–89% greater area-averaged driving stress, dependent on the overlying ice thickness, while a switch from fjord-parallel to fjord-perpendicular flow for real topography requires a ∼28–45% area-averaged driving-stress increase.
	+	Area-averaged driving stress in simulations with real topography is ∼41–89% greater than in simulations with smoothed topography for an equivalent surface velocity. In comparison, simulations with fjord-perpendicular flow show ∼28–45% greater area-averaged driving stress than simulations with fjord-parallel flow.
16	-	Basal topography exerts a critical control on ice-sheet motion at all scales considered (...), but its influence at the intermediate scale – above the ∼0.5-25 m scale typically used to determine sliding parametrisations yet below the ∼400-4,000 m scale of resolution or basal topography fidelity of most ice sheet models – is particularly poorly constrained
	+	Basal topography exerts a critical control on ice-sheet motion at all scales considered (...), but it's influence at the intermediate scale – between the 0.5-25 m scale typically used to determine sliding parametrisations and the 400-4,000 m scale typical of ice-sheet model fidelity – is particularly poorly understood.
23	-	In the fjords of western Norway, striations (Kleman et al., 1997; Mangerud et al., 2019) and consideration of palaeo flow directions (Jungdal- Olesen et al., 2024) evidence widespread perpendicular ice flow over deep subglacial fjords, as well as in obtuse and parallel orientations (Fig. 1) with unclear implications for ice motion.
	+	In western Norway, glacial striations (Kleman et al., 1997; Mangerud et al., 2019) and paleoglacial flow directions (Jungdal- Olesen et al., 2024) show perpendicular ice flow over deep subglacial fjords, as well as in oblique and parallel orientations (Fig. 1). The implications for ice motion remain unclear.

Line		Changes
27	-	Ice motion perpendicular to an idealised subglacial valley has been modelled in 2D and 3D in a relatively straightforward manner (Gudmundsson, 1997; Meyer and Creyts, 2017), but not previously using actual topographic data and with the inclusion of temperate ice rheology, implementation of a rate-dependent resistance for slip at the ice-bed boundary, or 'fast' (≥100 m a^{-1}) ice-surface velocity.
	+	Ice motion perpendicular to an idealised subglacial valley has previously been modelled in 2D and 3D (Gudmundsson, 1997; Meyer and Creyts, 2017), with [...] However, these studies did not include real topographic data, temperate ice rheology, or rate-dependent resistance for slip at the ice-bed boundary.
33	-	The fjords were likely incised into pre-existing river valleys and geological weaknesses through erosion as the Scandinavian Ice Sheet grew and shrank, but not at its fullest extent (...), however the general pattern of fjord formation in western Norway has not been comprehensively investigated to our knowledge.
	+	The Norwegian fjords were likely incised into pre-existing river valleys and geological weaknesses through erosion as the Scandinavian Ice Sheet grew and shrank, but not at its fullest extent (...).
75	-	[...] and A ($MPa^{-3}a^{-1}$) is the creep parameter. A is set using the homologous temperature, T_h (K), below the pressure melting point, T_m, or water fraction, ω, when above the pressure melting point:
	+	The creep parameter, A ($MPa^{-3}a^{-1}$), varies depending on whether ice is above or below the pressure melting point T_m. For ice below T_m, A is set using the homologous temperature, T_h (K), while ice above T_m, is determined by the water fraction ω:
127	-	Note that the H used in τ_d is the average ice thickness of the domain giving 1398.6 m and 898.6 m for 1,000 m and 500 m above the plateau respectively.
	+	The domain-averaged ice thickness, including the fjord hollow, used in τ_d is 1398.6 m and 989.6 m for plateau ice thickness values of 1,000 m and 500 m, respectively.

Line		Changes
131	-	Our results demonstrate the strong control that subglacial fjords, and their orientation with respect to ice motion direction, exert on ice-sheet motion (Fig. 3).
	+	Our results demonstrate the strong control that subglacial fjords and their orientation exert on ice-sheet motion (Fig. 3).
143	-	For simulations with target surface velocity of 850 $\mathrm{m\,a^{-1}}$ (8Pe10), the surface directly above the fjord velocity is reduced by $\sim$120 $\mathrm{m\,a^{-1}}$ (Fig. 7d), while absolute velocity within the fjord the plateau elevation drops below 500 $\mathrm{m\,a^{-1}}$ (Fig. 4c).
	+	In simulations with target surface velocity of 850 $\mathrm{m\,a^{-1}}$ (8Pe10), the surface velocity directly above the fjord is $\sim$120 $\mathrm{m\,a^{-1}}$ lower than the maximum (Fig. 7d). Absolute velocity within the fjord (below the elevation of the plateau) drops to less than 500 $\mathrm{m\,a^{-1}}$ (Fig. 4c).
197	-	Our results show that realistic fjord geometries — which are (i) common across the western margin of the palaeo-Scandinavian ice sheet, (ii) likely present beneath the margins of the present day GrIS, and (iii) largely excluded from large-scale ice-sheet models — significantly complicate patterns of ice motion and temperate ice.
	+	Our results show that realistic fjord geometries — which are common across the western margin of the palaeo-Scandinavian ice sheet and also likely present beneath the margins of the present day GrIS, yet are largely excluded from large-scale ice-sheet models — significantly complicate patterns of ice motion and temperate ice.
201	-	Features as dramatic as Moffat eddies (Gudmundsson, 1997; Meyer and Creyts, 2017) may at first appear to be isolated features with limited influence on overall ice-sheet motion: our results and analysis of flow-aligned slope angles in western Norway (Fig. 1) suggest that they are likely a ubiquitous feature of ice motion over incised fjord landscapes.
	+	Moffat eddies (Gudmundsson, 1996; Meyer and Creyts, 2017) might seem like dramatic and isolated features with limited influence on overall ice-sheet motion. However, our results and analysis of flow-aligned slope angles in western Norway (Fig. 1) indicate that these features are likely widespread across ice motion over incised fjord landscapes.

2.5. The use of the future tense in a number of places is somewhat confusing, e.g. in lines 2, 41, 209, 229, 273, 278.

> Thank you for pointing this out. The changes are listed in the table below.

Line		**Changes**
2	-	[...] which will not fully capture topographic features.
	+	[...] which do not fully capture topographic features.
41	-	Over a single glacial cycle, the orientation of ice motion will vary (Jungdal-Olesen et al., 2024), while the landscape will remain effectively immutable
	+	Over a single glacial cycle, the orientation of ice motion varies (Jungdal-Olesen et al., 2024), while the landscape remains effectively immutable
209	-	[...] while in unbounded basal traction relationships basal traction will continue increasing as basal velocity increases
	+	[...] while unbounded basal traction relationships show continuous increase with basal velocity
229	-	[...] for perpendicular flow will be implicitly subsumed into the basal sliding relationship [...]
	+	[...] for perpendicular flow is implicitly subsumed into the basal sliding relationship [...]
273	-	[...] the impact of basal sliding anisotropy will be small for most predictive timescales [...]
	+	[...] the impact of basal sliding anisotropy is small for most predictive timescales [...]
278	-	Realistic fjord topographies will also introduce anistropy [...]
	+	Realistic fjord topographies also introduce anistropy [...]

2.6. Line 3: I suggest rephrasing ”extreme case, but are ubiquitous” as it sounds contradictory.

> Agree, and we have changed this to '...are an end member of this misrepresentation, but are ubiquitous...'

2.7. Line 4: Remove ”likely”.

> Fixed!

2.8. Line 9: The use of the word ”requires” or ”required” when describing the increase in driving stress between two simulations is confusing when it is not stated what this increase is ”required” for, i.e. for a target velocity magnitude. Please clarify here and throughout the manuscript.

> Fixed! See, for example Comment **2.4** (Change of Line 8).

2.9. Line 11: I suggest ending the Abstract with this sentence, as the subsequent sentence draws attention away from the main findings of this study.

> We have switched and adjusted these two sentences.

2.10. Line 27: ”across” → ”perpendicular to”.

> Fixed!

2.11. Line 36: ”...the general pattern of fjord formation ...”: I suggest rephrasing this sentence as this falsely gives the impression that the study concentrates on fjord formation.

> This part of the sentence is now removed.

2.12. Line 37: The Introduction distinguishes between ”real” topography and ”smoothed” topography, yet in the Methods section it is stated that the ”real” topography is also smoothed. This caused some confusion and I suggest clarifying or rephrasing this terminology.

> Added clarification to Line 41: [...] we create a smoothed representation of Veafjorden (henceforth referred to as the smoothed topography).

2.13. Line 40: The Introduction should be independent of the Abstract, so I suggest rephrasing sentences that imply prior knowledge of the experiment design, e.g. "Our simulations can therefore reveal...".

> Good point, added at Line 47: By simulating Veafjorden with both the smoothed topography and the *real* topography, we reveal the influence of realistic anisotropic basal conditions.

2.14. Line 51: "models" → "model's".

> Fixed!

2.15. Line 53: Typo in "topography".

> Fixed!

2.16. Line 58: Please define the term "plateau ice thickness" when you first mention it.

> Fixed!

2.17. Line 63: Please provide a sentence or two describing which details are to be found in Law et al. (2023).

> We have simply removed 'with further details to be found in' such that the sentence now reads 'The central equations and boundary conditions follow Law et al. (2023) with minor adjustments to geometric set up.'

2.18. Line 70: I suggest giving a brief description of the x-, y- and z-axes when the gravity vector is introduced. Which axes are parallel and perpendicular to the fjord incision?

> Additions: In the simulations, the x-axis is oriented perpendicular to the fjord incision, and the y-axis along the fjord. The z-axes is normal to the horizontal plane, and does not match the $\boldsymbol{g}$ vector. Adjusting the orientation of $\boldsymbol{g}$ removes [...]

2.19. Line 74: I don't think it is absolutely necessary to mention all of the units within the sentence, but if you do use them, then please also provide the units for the strain rate.

> Fixed!

2.20. Line 86: The details on enthalpy are currently split by information about the surface evolution equation.

> The free surface equation is now moved after the enthalpy equations.

2.21. Eq. (10): Why is this defined at z = 0? I would remove "where z = 0".

> Fixed!

2.22. Line 104: u is already defined above. Please also make the velocity vector in the equation bold.

> Fixed!

2.23. Eq. (12): Please use appropriate Latex syntax for "sin"

> Fixed!

2.24. Line 132: Please define what a Moffat eddy is at first mention.

> Fixed!

2.25. Table 1: "base simulation" - might "reference simulation" be more suitable? I suggest moving the IDs to the first or second column. The sentence beginning "For example..." is currently missing a verb.

> Fixed!

2.26. Fig. 5 Could you please clarify the orientation of the eddies here?

> Added information in figure text: A secondary Moffatt eddy is formed in a small depression in the valley side (**b–d**). Panel (**c**) show the larger eddy streamlines flowing from right to left, while the smaller eddy spins counter clockwise in the topographic depression. Panel (**d**) shows the same secondary Moffatt eddy as in (**c**) from the opposite side.

2.27. Line 221: Please clarify what is meant by "in isolation".

> This section has been revised and this sentence removed.

2.28. Line 243: Please clarify or remove the sentence beginning ”These two studies non-dimensionalise...”.

> We have replaced this text to read: However, while these studies represent non-dimensionalised problems, their focus is ....
>
> We hope this is clearer. We are referring to the fact that many idealised cavitation do not address the scale of the cavities (rather focussing on a dimensionless roughness parameter).

References

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