Response to reviewers

Hughes (2023) Fjord circulation induced by melting icebergs, doi:10.5194/egusphere-2023-2106

Thank you to both reviewers for the careful reading of the paper and encouraging feedback.

My responses to Reviewer 2 begin on page 7 of this document.

Reviewer 1

Review of “Fjord circulation induced by melting icebergs”, by Kenneth G. Hughes, (egusphere-2023-2106)

The manuscript describes the development and application of an analytical model of fjord circulation forced by meltwater input at the surface (the meltwater stems from icebergs in the fjord). The build process of the model is described in an easy to follow fashion by considering and assembling different configurations with increasing complexity. The solutions agree reasonably well (considering the approximations implicit to the analytical solution) with high-resolution numerical simulations. A parameter study shows expected and not so intuitive results that can explain some observed behaviour in glacial fjords. The manuscript is well written and easy to follow.

Thanks for your review

As my main critique, the manuscript introduction does not describe the purpose nor the context of the study, so that the manuscript appears to be “just” a model description (“The core contribution of this paper is an analytical model explaining the first-order dynamics of a fjord’s response to hundreds of melting icebergs.”) as a very well designed and carried out exercise of geophysical fluid dynamics. In the same way the conclusion is a useful and alternative summary of the model steps, but no geophysical/scientific conclusion are drawn. Adding context and purpose would make this manuscript an even better paper. That’s why I recommend minor revisions.

The impetus for this paper was a suite of numerical simulations that I began a couple of years ago that were much like those described in the paper, but that also included subglacial discharge. In trying to make sense of those simulations, I realized that there is a big discrepancy between our thorough knowledge on the role of subglacial discharge and our dearth of knowledge on the role of iceberg melt when it comes to explaining fjord dynamics. To me, there was a clear need in the literature for a paper that explains the mechanism by which iceberg melt is translated into currents. I have added a paragraph to the Introduction that expresses this discrepancy and therefore provides context for my study.

I also agree that the Conclusion was missing a statement on the consequences/significance of the work and that it ended in a weak way. I have taken what was the opening paragraph of Section 5.2 and moved it to the end of the Conclusion where it will be more visible. This paragraph about the potential for the analytical model to be extended and then applied to fjords across Greenland is, in my opinion, the most relevant high-level implication of the paper.

Not as important:

The model description uses some formulae, but it is not always clear (to me) where they come from, or how they were derived. I appreciate the briefness and clarity of the presentation, but the presentation requires that the reader “believes” the text. Maybe more information in an appendix would help?

Reviewer 2 had a similar concern. As I also state in response to their comments, there are several expressions in this paper that are simple yet that result from elaborate derivations that I want to avoid repeating since the details are tangential. I now point the reader to places where they can find more details if they please.

There are some technicalities, questions, and notes from reading the manuscript are listed below:

Abstract: no motivation, no concluding remarks about implications

At a guess, I’m inferring that the reviewer is suggesting a motivating statement or two that touches on, say,

- the role fjords have in being gateways for heat to the Greenland ice sheet;
- the role icebergs have in modifying glacier-adjacent water properties; or
- processes that are missing in climate models.

Such statements are appropriate if the paper has an expected audience than spans a range of scientific fields. However, I foresee my paper being read primarily by physical oceanographers and glaciologists: people who will already be familiar with its broad purpose. Therefore, I prefer to get straight to the point and keep the abstract concise.

At the moment there are few obvious geophysical implications of my work. As I and others build on the work and start invoking the analytical model in more applied studies, then there will be more tangible implications. But at this point, I am hesitant to conjecture in the abstract about what these implications will be.

l2: “Down-fjord” Is this a proper term? I would use downstream.

Down-fjord is the best term here and a google scholar search shows many uses of the term in other papers. *Downstream* could be an ambiguous term since the current direction changes with depth.

l24-25 “over the top 200 m is a few cm/s over the top 200 m”
repetition, also I would not associate “outflow” with velocity units. Maybe “outflow velocity”?

Wording fixed as suggested.

l27: this paragraph is very different in style from all other paragraphs, consider rewriting

This style of this paragraph is intentional. I want to emphasize that this is a process study that will address a parameter space. The four short questions in a row is a concise way to evoke this idea.

This idea of listing questions provides a nice transition to the following paragraph where I state what I plan to do to answer those questions.

page 4: The icebergs do not seem to move. Is that justified? Later this is described, but it may be useful to do it here already.

I have reworded part of the section in question as follows:

Each iceberg is assumed to be a stationary cuboid. Although there are limitations with this approach, it is their cumulative meltwater flux, rather than their movement, that matters most here. Further detail and justification of our approach is given by Hughes (2022).

l64: (practical) salinity has no units, as it is a ratio of g salt / kg sea water, see UNESCO reports etc. “psu” should not be used (I know that is done commonly, but it’s still not correct). Absolute salinity (according to TEOS10) has units of g/kg.

I have removed the units. Personally, I have no strong opinion either way on whether they should be there or not. I am not using g/kg units because the equation of state that I’m using in the MITgcm predates TEOS10.

l73: At this resolution, non-hydrostatic dynamics may start to become important. Why use hydrostatic dynamics?

Non-hydrostatic dynamics are most needed when one is interested in accurately simulating the dynamics of overturning structures. This is not the case for my setup, which features a very gradual forcing.

The one place where non-hydrostatic dynamics will matter is beside the ice—where buoyant meltwater rises vertically—but I’m not going to come close to resolving that at 10-m resolution. This unresolved physics beside the ice–ocean interface is a whole different scientific question that is way beyond the scope of my paper.

I did run a few simulations on a smaller domain with non-hydrostatic dynamics included. The difference in the solutions was negligible for the purposes of this paper. Any improvement in accuracy is not worth the 2–3× slowdown in wall time needed for the simulations.

l77: “(designated as scheme 33 in the MITgcm)”, I think the reference for this DST (direct space time) scheme is

Hundsdorfer, W. and Trompert, R. A.: Method of lines and direct discretization: a comparison for linear advection, Appl. Numer. Math., 13, 469–490, doi:10.1016/0168-9274(94)90009-4, 1994.

Hundsdorfer, W., Koren, B., van Loon, M., and Verwer, J.: A positive finite-difference advection scheme, J. Comp. Phys., 117, 35–46, doi: 10.1006/jcph.1995.1042, 1995.

I have added the Hundsdorfer et al. (1995) reference

l91, are the initial conditions (especially T=2deg) reasonable? The high melt rate will probably reduce with lower ocean temperatures (and it does according to the parameter study later on).

The choice of temperature is reasonable. As noted in Section 2.1, the temperature being 4°C above the freezing point is "an average thermal forcing for Greenland fjords (Wood et al. 2021)".

The temperature that I use for the simulations is somewhat arbitrary because melt rate also depends on the (poorly constrained) turbulent transfer coefficients at the ice–ocean interface. Hence, inaccuracy in melt rate is dominated by the uncertainty in γ_T and γ_S (and their velocity dependence), not by temperature.

Ultimately, I focused on choosing a combination of parameters (θ_a, γ_T, and γ_S) that give iceberg melt rates that are comparable to remotely sensed observations as noted in Section 2.2.

l97: only small -> only over a small? Fixed

l99: the exponential function implies Kelvin waves, why should they be the only ones? What about Poincare waves (mentioned in the text) and Rossby waves?

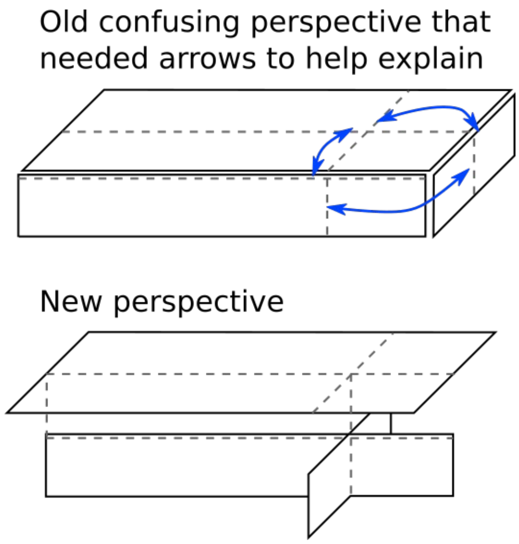
The effects of Poincare waves can safely be ignored because they are much smaller than Kelvin waves. This can be seen in Figure 4a: the Poincare waves extending across the channel are only just visible in the second panel and are non-existent in the panels below. It can also be seen in Figure 8c, where I needed to annotate the Poincare waves because otherwise they would be easily missed.

Rossby waves will not occur in this system. Conventional Rossby waves can't occur here because Coriolis frequency is constant, and topography Rossby waves can't occur here because there isn't sloping topography.

I reworded the opening paragraph of Section 3.1 to make clear that Kelvin waves are the dominant process. Poincare waves are mentioned for completeness.

Figure 2, it took me a while to understand this complicated figure. I can appreciate it's value, but maybe extend the description in the caption to say that the vertical slices are at the dashed lines in the top view (or put them, where they belong?). The "pseudo"-realistic perspective implies something different and confused me (e.g. I thought that (c) and (d) form the sides of the displayed domain of (b) and it took me a little to figure out that (b) is only part of (a), etc.)

I agree that this depiction was confusing. I have revised the figure so that it is now intuitive how the three slices relate to each other in space. Below is a figure summarizing the change I made.



l115: typo in “$x— —z$ domain”? Fixed

l142: “overkill” maybe too colloquial?

I have left this as is. Personally, I don’t see issues with using colloquial language in scientific papers provided that it is clear what is meant.

l145: “For an infinitely narrow channel, the tanh term goes to zero”, not clear why. Eq5 and 7 do not have any x in them that can got infinity.

Channel width is the y direction. It is W that goes to zero. I have clarified this in the text: “For an infinitely narrow channel ($W \rightarrow 0$), ...”

Section 3.2 is physically plausible but no formulae support it.

I fail to see how adding math/formulae to this section would help. Geometrically the argument is simple.

My guess is that the reviewer is questioning one of two processes. Either (1) that the Kelvin wave maintains its shape while turning the corners or (2) that destructive interference leads to complete cancellation of the Kelvin waves. I was also skeptical at one point that this would be the case. But the comparison of the analytical and numerical models in Section 4 shows that these two processes must be happening.

Further, although it is not noted in the paper, Figure 4 was created using MITgcm simulations with sea surface height varying in time just as described in Section 3.1–3.3. The fact that the results in Section 3.2 arise in a primitive equation model is good evidence that the process described can be trusted.

page 12

The figure needs more explanation, e.g. there’s density in a+e, but pressure (anomaly) in b-d, but the caption doesn’t describe this, nor the labels in the figure, …

I have added panel descriptions to the caption.

l200: Figure 4 -> Figure 5? Fixed

l209: “where z* is a dummy variable used to avoid ambiguity with the integral’s lower limit”. I guess that’s common practise and need not be described. I have seen usually z’ used for this.

I agree that it is common practice, especially if I were using the *z’* notation. However, I’m already using primes a lot in this paper to denote anomalies, so I want to avoid prime notation here. I decided that z* was the next best option, and then added the clarifying phrase in question to ensure there was no confusion as to what I was doing.

Eq18: integral over z? dz is missing. Not clear. Good catch. Fixed

Fig6 caption: relatively low-mode components. -> relatively larger low-mode components?

I now simply say “lower-mode” instead of the awkward “relatively low-mode”

page 17

l254: “The integer n_E is the maximum n” and the higher modes all destructively interfere?

No. The modes with $n > n_E$ are those that are too slow to have reached a given location x. The destructive interference is for modes $n \leq n_W$. Per a suggestion from reviewer 2, I have made this clearer in Equation 25.

page 23

l366: “by looking at” -> by evaluating (colloquial?) Fixed as suggested

ll370 maybe it would be a good idea to add the stationary role of the iceberg and neglected drag already in the model setup description? (See earlier comment on page 4)

Added as suggested

page 24

l380 Please discuss why the overestimation is reduced for the stronger stratification and weaker melt cases.

I have added the following sentence:
In fact, the agreement is better in these two scenarios compared to the default settings because the relative role of linear wave dynamics (compared to nonlinear advection) is larger when the outflow is weaker or the stratification is stronger.

page 29

l643: helps brings -> helps bring? Fixed

Reviewer 2

The author compares an analytic linearized fjord circulation model in the presence of an iceberg melange with numerical model simulation. I like this work and urge the author to keep refining their analytical model. I am not sure whether it will find applications in climate models; however, at the very least, it is a valuable tool to understand the outputs of numerical simulations.

Thank you for the review and encouragement to continue with this analytical approach. I agree that there's not an obvious way to add it to a climate model, and instead that its value is to help interpret numerical simulations.

I have several comments about the mathematical exposition of the author's work. I do not doubt the correctness, but the model derivation (being analytical) lacks certain rigor (or perhaps I lack understanding of the author's process, either of which merits further explanation).

Thanks for the suggestions/clarifications below. To develop the analytical model, I relied a lot on existing results. My contribution was effectively to repackage and combine various results, and apply some simple geometrical arguments so as to solve a new problem. In some cases, like Equations 11 and 19 in your comments 2 and 3, the simple expressions result from elaborate derivations that I want to avoid repeating since the details are tangential. I now point the reader to places where they can find more details if they please.

1. In sub-sections 3.x I am unsure whether the author follows Hermann 1989 or derives original equations. Please specify which part summarizes previous work and which is new. Most of this section reads as a recap of other work, but perhaps the exposition lacks detail. The following points provide specific questions stemming from this concern:

Hermann et al. (1989) technically only dealt with the open channel, abrupt release case in Section 3.1. I developed the generalizations in Section 3.2 and 3.3. However, these generalizations are simple geometrical arguments.

I now clarify in Section 3.2 what Hermann et al. (1989) did and didn't do.

2. How does the author derive relation (11)? Is this taken from another paper? If so, which one? Is N (which the author refers to as stratification) the same as buoyancy frequency in equation (3)?

I now point the reader to either the Kelly et al. (2010) or Gill (1982) for details on where this particular expression comes from. And I have changed the word "stratification" to "buoyancy frequency" to avoid confusion.

3. I do not understand how the author came up with equation (19). It is presented as a statement, not an assumption, and without much justification. Yet, the derivation of Fourier coefficients for velocity depends on it, so please explain more.

Again, Kelly et al. (2010) proves to be a good reference for this expression. Specifically, compare their Equations A12 and A14 with $\omega = 0$. I now note this in the text.

4. The author comments that they derive equations (23) and (24) from (21) and (22) by x-integration - please show how this happens, as I do not see the relation clearly. If the factor 2L comes from the integration, then are the terms in (21) and (22) space-independent? Also, integrated energy will not have the same unit as the original, so perhaps a different notation is required.

In hindsight, I agree my description of this step was too terse. It was also presented in an awkward order with both equations presented first and then described afterward in a single paragraph. I have revised the wording and dealt with each expression individually. It is now easier to follow the derivation.

And the reviewer is correct about units. I now write Equations 23 and 24 as $\int E_p$ and $\int E_k$ instead of just E_p and E_k.

5. In Equation 25 the author subtracts two sums from each other. As stated, the result of the operation is in my mind is the sum of terms between n_E and n_W as the summation terms $u_n(z)$ are identical in both sums. Perhaps differentiate the notation here to indicate that terms in the n_E sum are different than in the n_W sum.

The reviewer is correct in interpreting the difference of the summations. I have added a second, equivalent expression showing that it is a sum of modes from n_W+1 to n_E.

6. Please include the numerical values of velocity in the figures comparing analytical and numerical results (i.e. Fig. 7, 8, etc). This is important for reproducibility.

I appreciate the suggestion to make the work reproducible, but I don't think that adding the numerical values to Figures 7 and 8 is the best way to do that because the actual current speeds are not the focus of those figures. Instead, I have uploaded scripts to recreate the results from Figures 7 and 8 to the Github repo and the Zenodo archive. These scripts produce velocity fields that *do* include numerical values.

7. Are the equations (32-35) justifiable in the light of the assumption of small perturbations made in Section 3.3?

In Section 3.3, I assume a *constant, gradual input of buoyant water*. In my opinion, this is an accurate description of the effects of ice melting in the ocean, which is what is being calculated in Equations 32–34. Of course, this doesn't necessarily imply that my assumptions are justifiable *a priori*. But the good agreement between the analytical and numerical models in Section 4 is evidence that the assumptions are valid.

8. It would be interesting to see the results for x<15 to get an idea of how far off the results are from the simulation. Perhaps including results at several locations (including further downstream than 20km) would increase the reader's sense of confidence in where the model can be applied, and where it fails.

I have added a second panel to Figure 11 that shows the total outflow for $0 < x < 100$ km at $t = 7$ days. This panel shows that the analytical and numerical models agree reasonably well provided $x > 20$ km, but diverge for smaller distances.