Response 1 on "Influence of water extraction on subglacial hydrology and glacier velocity"

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April 1, 2026

We are grateful to the referee for highlighting potentially confusing aspects of the paper and suggesting areas for improvement. The reviewer's comments are in black text and our responses are colored in blue text.

1 General comments

The title of this paper is too general to represent the contents of the article accurately. The scenarios examined are very specific (e.g. wintertime only) and this should be acknowledged in the title. The influence of water extraction goes far beyond this work.
We have changed the title to "A model of water extraction from subglacial hydrology under idealized conditions"

The level of mathematical detail here was appreciated, but I found it frequently to be at the cost of the ease of understanding of the paper overall. For a journal such as The Cryosphere it should be possible for a reader without a detailed mathematical background to still understand the overall study, but the details of the simulations run and their purpose are often lost in the weeds of the mathematical solutions.
We have streamlined the mathematical detail, reordering the sections according to the reviewer suggestions for clearer flow, and moved section 3.2.1 to the appendix

The authors frequently do a good job of stating the assumptions made and the limitations of this work in the main body of the text. They do, however, then jump to much more general conclusions that go beyond what their results show in the discussion section. The results do not suggest that water extraction can slow glaciers, except under very specific circumstances. More work is needed to determine if glaciers can be slowed under realistic scenarios (i.e. with summer melt). The paper is a little inconsistent in sometimes doing a good job of being realistic about the results (i.e. that more modelling work is needed) and sometimes making broader statements that could be taken out of context.
We have added qualifiers to soften our statements about the effects of pumping. In addition, we have responded to the specific comments below to address the reviewer's concern.

While the authors state that the practical, moral, ethical implications of field testing are not considered, the fact that possible future field deployment is mentioned and the location studied has a local population should not be ignored. Can the authors please acknowledge the Indigenous population of Greenland and their rights to decide about possible future field testing that happens in their home.

We have removed any statements about field deployment. We have now included an acknowledgment of the indigenous population of Greenland.

The authors have acknowledged that two of them have connections to the Arête Glacier Initiative, yet describe this as 'focussed on sea level rise' whereas Arete's home page states 'Arête is leading sea-level rise forecasting and glacier stabilization efforts'. While it is appreciated that the authors are declaring this interest, the fact that this is an initiative with a specific goal to research glacier stabilization should be made clear. For a topic that has attracted a lot of controversy to the cryospheric community 100% openness is essential.
Agreed, we have updated our conflict of interest to reflect these comments.

2 Specific line by line comments:

Line 71 - Could the authors please expand on why the Theis method would be expected to be applicable to a subglacial environment e.g. as presumably this evolves much more quickly than a groundwater system
We agree that the paragraph required some clarification. In the updated text, we have described the subglacial analogy in more detail and reduced the technical details about the Theis method. We have reworked this section to describe the similarities (and differences) with the Theis solution.

Line 105 (eqn 1) - How much does the choice of sliding law matter here? Given it's directly proportional to N it seems quite important.
The sliding law is indeed important. We did not test other sliding laws in these initial simulations. We are currently running simulations that utilize a generalized Coulomb sliding law.

Table 1 - This would be much easier to follow if the parameters were also named as for anyone not familiar with SHAKTI there is a lot of back of forth to try and understand the equations.
Good idea. We have added words next to the variable definitions in the table.

Line 134 - Does the continuous pumping assume no movement of the glacier (i.e. connection to the bed at the same place)? How might that impact future work that looks at feasibility of keeping drill holes open?
Yes, here we assume that the pump site is fixed relative to the motion of the glacier. We agree, in reality, it would be a Lagrangian tracer following the flow and quantifying its speed would be important.

Line 146 - I got a little confused between q, $\boldsymbol{q}$ and if these equations are looking at total water flux from the whole circle in figure 2 or just water movement across the boundary at the circumference. Possibly the above suggestion of making parameter names clearer might help with this.
The axisymmetric flux in SHAKTI only points radially inward here, so we were not careful enough with the distinction between q and $\boldsymbol{q}$. We also streamlined the section describing the equations, combining parts of 3.2.2 back into the original description. One result of this reshuffling is that we hope that the definition of the flux should now be clearer.

Line 187 (Fig 4) - More details about the simulations carried out here would be helpful, is this revisiting simulations from another paper? There aren't any other simulations plotted previously.
We are referring to the simulations in figure 1 that we use as a guide for our analysis. We have updated the language in this sentence to clarify. We have also added addition details about the simulations when they are introduced and when we discuss the simulation results.

Line 369 - This is a very minor impact, but is described as a 'modest' impact in the abstract which is overstating 0.5-1%.
We have rephrased the language describing our results in the abstract and throughout the paper.

Figure 10 - Can the authors please explain what is happening around day 90, and what the small fluctuations in the effective pressure are showing?
There is an initial overshoot then equilibration.

Line 377 - Can you see the reconfiguration of the drainage network with SHAKTI?
Our results suggest that some reorganization occurs: in figure 12d we see a channel forming in the main trunk of Helheim.

Line 379 - typo
Thanks!

Line 417 - The results don't show this, they show it can slow them under winter conditions only.
Fair point, we have adjusted the language accordingly.

Response 2 on "Influence of water extraction on subglacial hydrology and glacier velocity"

C. R. Meyer, K. L. P. Warburton, A. N. Sommers, B. M. Minchew

April 1, 2026

Thanks for highlighting potentially confusing aspects of the paper and suggesting areas for improvement. Your comments are in black text and our responses are colored in blue text.

1 General comments

This is a theoretical study that uses a model of the subglacial drainage system and ice mechanics to assess the effect of extracting water from the glacier bed. The study is motivated by an interesting (though frankly ludicrous) proposal that such extraction could be used as a mechanism to slow down the motion of parts of a glacier and thereby (perhaps) delay ice loss and consequent sea level rise.

I found the study interesting, but I feel that aspects of it are quite confused and it seems a bit rushed. In particular, I found the approximate analytical solutions - with many terms neglected initially and then slowly re-introduced - quite hard to follow, and then when it came to the fuller numerical solutions coupled to ice velocity I felt that the restriction to winter conditions when the basal water fluxes are small rather limits what can be concluded about the influence on glacier velocity.
Thanks for voicing your reservations. We have taken these comments into account as we have developed a revised version of our manuscript. We address specific points below.

The general approach of looking at simplified solutions (where certain terms are neglected) is potentially useful, but I felt it was quite confusing here, with so many different approximations introduced that it was hard to keep track of what the point was, or which of the results/figures were relevant in the end. I think the section 3 with approximate solutions could usefully be made more concise, and perhaps be guided more by what has been learnt through these (which seems to be that it is important to account for the melt from dissipation and the impact this has on the flux, and that the prescribed effective pressure at the 'outer radius' is important). E.g. you could just analyse the system in section 3.2.2 from the outset, perhaps noting simplifications that occur if/when some of the parameters are small.
Good points. We have reorganized this section in response to your comments.

A second issue I have with the simplified solutions is that they seem quite dependent on what is taken as the outer radius (r_d), and what value of effective pressure is prescribed there. Ideally you'd like the solutions not to depend too much on this but just to tend to some far-field undisturbed behaviour, but it doesn't look like that's what happens (the equations don't allow it, I suppose). Instead, you have to prescribe a value of effective

pressure N_0 at a particular radius, and the solutions seem to depend quite a lot on that. I felt that there should be more discussion about what is an appropriate value for both r_d and N_0.
We agree that the connection between the finite-domain axisymmetric simulations and the larger-scale simulations was not as clear as it could have been. In addition, we share the reviewer's wish that the solutions not depend on these quantities, but as noted, the equations don't allow for it. However, the larger-scale simulations are close to axisymmetric near the pumps, so our finite-domain results provide some insight even if the choice of matching is not yet understood. We have clarified this section by reorganizing our results, moving some of the mathematical details to the appendix, and focusing on describing the results, as suggested.

2 Line-by-line comments

Line 28 - This sentence doesn't seem quite right to me. At least, I think it is trying to get at the difference between so-called 'efficient' and 'inefficient' drainage systems, which are characterised by the steady-state response of the pressure to an increase in discharge, but this is not necessarily the same as 'higher effective pressure' and 'lower effective pressure'.
We have added the words 'In steady state,' to the front of this sentence and added two additional citations.

Line 46 - this description of 'type-I' and 'type-II' glaciers is a bit mysterious to me as I don't think that is quite what is described in the referenced papers, which had more than two 'types' and where the distinction is based more on the patten of velocity change - with apparent influence of the glacier terminus as well as basal lubrication - rather than on the hypothesised response of the subglacial water pressure to adding meltwater. Whilst there are clearly differences in the ways that different glaciers respond to seasonal meltwater forcing, I'm not sure that the observations can be summarised this simplistically.
We decided not to include the other types of forcing (e.g., terminus forcing) because they are not relevant to our results. We have added a note to say that not all glaciers fit neatly into these two types.

Line 53 - I found this discussion of how such glaciers would respond to meltwater extraction a slightly bizarre thought experiment. If the glacier is responding seasonally to surface melt reaching the bed, it would surely be easier to find a mechanism to prevent the water from reaching the bed in the first place, rather than extracting it back from the bed (back to the surface presumably, from which it would just find its way to the bed again!).
Thanks for your comments. We agree that stopping meltwater reaching the bed could be a method to slow glacier flow.

Line 155 - What motivates the choice of the value of N_0? This seems like it may be quite important here, along with the size of the domain. r_d is not defined, and nor is $f(r)$.

We use values for N_0 that arise in the simulations. We have updated the text to include definitions of r_d and $f(r)$.

Line 167 - what is r_t? This doesn't seem to be used.
We have now included a definition of the turbulent transition radius in table 1. It shows up in the definition of R, i.e., line 169.

Line 190 - this sentence saying the SHAKTI simulations do not always evolve the gap height in time was a bit confusing - given that you've just described the model with an equation for the evolution of the gap height - and isn't really discussed later. It would be good to describe the model fully upfront.
Yes, we agree that it could have been clearer. We have added a description of the minimum gap height in SHAKTI. We now state that a minimum gap height is imposed in SHAKTI for practical purposes. If the gap height is equal to this minimum, it won't appear to evolve even if the creep closure dominates melt opening.

Line 208 - this condition on where the effective pressure blows up seems like it may depend on the size of R. For small R, I'd have thought the logarithmic term in (20) is more important, with N blowing up when $r \sim e^{-8M}$.
Yes indeed it does depend on R, which we now state. There is indeed a full nest of asymptotics: we now state the dependence on R explicitly and show the order of terms, as suggested.

Figure 4 - the blank region of the lower plot looks a bit odd. Why are there no points with larger gap heights close to the extraction site?
The point of this figure and associated text is to show that in these SHAKTI simulations, the value of the gap height does not evolve and is pinned to the minimum gap height.

Figure 6 - it's not clear what parameters have been used for the left hand panel. In the right panel it is very hard to tell how much of the difference between the points for different values of N_0 is due to the different value of N_0 or due to different values of N. It might make more sense to plot N rather than $N - N_0$. Secondly in this plot why are the points all over the place? This looks to me like the numerical solution is not working, or producing numerical errors, in which case how can you have faith in these results?
This is a good point. As we describe below, we have changed this plot so that it now shows N rather than $N - N_0$. The reason that the points are all over the place for large values of $N - N_0$, as described in the text, i.e. line 359, is because there is no solution to the problem and the numerics fail. We have clarified this point and described the problem in more detail in the text.

Figure 7 - I am quite confused by the plot, which seems to show very different behaviour to what's discussed around figure 6, in which it is suggested that N tends towards a constant value as r tends to 0 (i.e., as you approach the borehole), whereas here you seem to show N ever-increasing at small r. Maybe I am not understanding what is being shown here.
The lines in figure 7 show the maximum steady state effective pressure, i.e., the boundary where the problem no longer has a solution. We now describe the highlights of the 3.2.1 analysis in the main text and have moved section 3.2.1 into the appendix to streamline the story.

Line 279 - continuing this confusion, you earlier suggested that the solutions had a constant effective pressure as r tends to zero, given approximately by (29). What is different here?
This is the boundary between where there is a steady state solution and where there is not.

Figure 8 - the axes labelling seems confused about whether it is the dimensional or non-dimensional radius.
Thanks for catching this oversight, we have corrected it.

Figure 9 - why does the effective pressure seem to be insensitive to Q here (at least for smaller values of N_0), but not in figure 6. Which is 'correct'?
The difference is that figure 6 is plotted in loglog for the difference between N and N_0. We have adjusted figure 6 so that it is plotted on the same axes as figure 9 to avoid this confusion.

Line 351 - the expected timescale of 1 hour doesn't fit within the expected range of time scales just mentioned on line 345 (90 years to 4 days), or the value on line 343. It is a lot shorter. This is rather confusing.
We have increased the range of effective pressures discussed so that the range of timescales given in lines 344-345 now goes down to 1 hour.

Figure 10 - is this figure well-resolved in time? If it's supposed to be showing the transient dynamics it doesn't show them very well - it suggests an essentially instantaneous adjustment, but the marker at the maximum suggests the plot may not be resolving the transient.
Yes, the transient adjustment is very fast for this parameter configuration. SHAKTI does not save every timestep so the simulations are better resolved numerically than it looks in the plot. We show a sequence of transient simulations in figure 13.

Figure 11 - it would be helpful to give an indication on this figure or in the caption how appreciable these velocity changes are, and to indicate that this is for a winter situation. It is hard to imagine how extracting $1\text{m}^3/\text{s}$ in the summer can have much effect at all (this is much smaller than the uncertainty in the surface runoff), and even in the winter it looks like the velocity change is quite small. Perhaps plot the percentage slow-down? The colour scale in panel (b) looks to be saturated so we can't see how much the effective pressure changes near the borehole.
We have updated this figure in response to the comments. Notably, we include a panel for the total velocity and we expanded the colormap for the effective pressure.

Figure 13 - there are issues with labels on many of these panels. What are the units of the values of [t] quoted? It might also be emphasised that the magnitude of the effective pressure changes is very different between different panels, e.g. in the top panel the effective pressure is hardly changing at all.
Yes, there was a compiling issue when uploaded onto the submission server. We have corrected this problem. The units of $[t]$ vary from days to years.

Line 417 - I suppose this conclusion is somewhat in the eye of the beholder. I read this study and thought the results confirm that as a glacier intervention strategy this is not a viable idea! The reduction in ice speed is minimal, and the amounts of water that

it seems to be possible to extract per borehole (~1 m^3/s) are also small.
We have softened the language in this section. Our aim is neither to promote nor dissuade this idea, rather use it as a tool for further research. This point did not come through clearly enough in the first draft, so we have added language to clarify our goals.

Line 422 - simulations of 'the Greenland ice sheet', rather than of 'Greenland' I think.
Thanks, we have corrected this statement.

Line 423 - indeed this does beg the question why this study is not already looking at a west Antarctic setting if the proposal is not really being considered for the Greenland ice sheet. The neglect of the seasonal melt cycle seems an enormous caveat for this work - and makes its relevance quite questionable - whereas if you actually have the Antarctic setting in mind, why confuse the issue by looking at a Greenlandic setting?
We agree that this study would be improved by including an Antarctic example. We chose to start in Greenland because we could test the model in a setting of the coupled model that was already published and documented for reference. We are currently developing simulations in Antarctica which we may be able to include in a revised manuscript.

Line 444 - I'm confused by this statement, as I think the issue at small extraction fluxes is that the fluid is exiting the domain at its outer edge, not entering it. (i.e. the radial fluid flux is negative, since the way you define your radial flux is that positive q is inwards).
Yes thanks for catching that, we have corrected the statement.