

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

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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 pattern 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 ($\sim 1 \text{ m}^3/\text{s}$) 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.