Reviewer Comment Response

1 Editor Comments

Public justification (visible to the public if the article is accepted and published):

Dear Koi McArthur and co-author,

In view of the reports from the referees, I am happy to recommend your manuscript for publication subject to minor revisions. Please go through the recommendations from the referees and make adjustments in a revised version of your manuscript as appropriate.

In response to the request from referee 1 re. the need for more information on the distribution of the basal meltwater production, I suggest this could be added as Fig. 1c. I have two technical comments that I outline below. I look forward to receiving a new version of your submissions. Best,

Nanna B Karlsson

Thank you Dr. Karlsson. We have added the meltwater added to GlaDS as Fig 1c. and your technical comments are addressed below. We keep the reviewer comments in black and our responses in blue. New text or changes to to manuscript are italicized.

Additional private note (visible to authors and reviewers only):

Referee 2 notes that Åkesson et al. 2022 should be at the beginning of the manuscript. This is not correct. The letter Å goes at the end and thus your placement is correct.

Thank you, we have left it at the end.

L 306: Word missing "... to the full Stokes is used..."

This has been changed to *...to the full Stokes equations is used...*

Let me know if you cannot access the second reports from the referees, and I will send them to you.

2 Reviewer 1 Comments

Thank you to the authors for their answers and for having taken into consideration my remarks and suggestions.

I really appreciated that you took the time to run additionnal simulations using the Budd friction law with $m = 1/3$ to answer to my first general comment that related to the choice of the $m = 1$.

I also noticed that you modified the figures as suggested. Here, I found the addition of the sensitivity tests with various $C_{\max}$ values really interesting and I am convinced this will be an added value to your manuscript.

From my side, this paper is ready for publication, although I still have a few specific comments (mainly typo's and suggestions for symbols and references) for the revised manuscript, which I listed below in order of their appearance.

Note : I noticed a few differences between your italic responses and the revised manuscript. Here, while commenting I have always considered the revised manuscript. Finally, I also noticed some layout issues in the revised manuscript, which are probably due to the << track changes format >>, but which I nevertheless report here for clarity.

Thank you for your comments, we believe that they have improved the quality of the manuscript. We are glad to hear that you believe that the manuscript is almost ready for publication, we address your following comments below.

Figure 1 (b) : typo: Ice surface (s is missing)

Changed.

Eq 1 : maybe use another letter than m, which is already used for another variable in Eq. (6)-(7)

the variable m has now been changed to η throughout the manuscript.

Eq 2 : maybe h_w is better than h for the hydrology sheet thickness

h is what is used in Werder et al., (2013), so we keep it here.

L74 : ice overburden **pressure**

Changed.

L87 : << Blatter-Pattyn approximation to the full Stokes equations **(Blatter, 1995, Pattyn, 2003)** >>

- Blatter, H.: Velocity and stress fields in grounded glaciers: a simple algorithm for including deviatoric stress gradients, J. Glaciol., 41, 333– 344, `https://doi.org/10.3189/S002214300001621X`, 1995.

- Pattyn, F.: A new three-dimensional higher-order thermomechanical ice-sheet model: basic sensitivity, ice-stream development and ice flow across subglacial lakes, J. Geophys. Res., 108, 2382, `https://doi.org/10.1029/2002JB002329`, 2003.

Changed.

L101 : layout issue

This is a track changes layout issue.

Eq 5 : maybe use another letter than B for the ice rigidity, since this is already used for the bedrock elevation

We have now changed to $\tilde{B}$ instead of B.

L125 : layout issue

This is a track changes layout issue.

L154 : specify the value of the water density (because Akesson et al, 2021 used sea water density value and Yu et al, 2018 used fresh water density value in the No calculation)

We have now specified that we used sea water (this ensures complete hydrological connectivity to the ocean at the grounding line). Line 153 now says ρ_w *is the density of sea water*.

L197 : typo : (Fig. 1a**)**.

Changed.

L229 : typo : GlaD**S**

Changed.

L247 : my apologize, as opposed to what I suggested in my original review, Budd and Jensen (1987) is a good reference when N is expressed as an hydrological potential. Huybrechts (1990) used an N corresponding to an << height above buoyancy >> (Budd et al., 1987 ; Van der Veen, 1987 ; referred to as HAB in Pattyn, 1996), which fits more with the Brondex et al., (2017), N_B, defined in L257

We are unsure what the reviewer is looking for here. We have removed the Huybrechts reference and we made sure that all the references use N_O as an effective pressure, not a hydraulic potential.

L253 : (Fig. 3**b**) → and place it : [. . .] below sea level **(Fig. 3b)**, yielding [. . .]

Changed.

L273 : typo : start with the '.' of the L272's end

This is a track changes layout issue.

L345-L352 : repetiton of [. . .] H is defined in Eq. (9) and x is defined in Eq. (10) [. . .]

The second occurrence has been removed. Line 342 now reads *Here,* ρ_i *is the density of ice,* g *is the gravitational acceleration,* H *is ice thickness, and the effective pressure is given by Eq. (7).*

Figure 6 (e) : typo : [. . .] GlaDS output effective ~~**e**~~pressure [. . .]

Changed.

L390 : typo : GlaD**S**

Changed.

L428 : layout issue

This is a track changes layout issue.

L448 : typo : (Fig. 2d (v))

Changed.

L462 : remove Kazmierczak et al., 2022

Changed.

L462-463 : layout issues

This is a track changes layout issue.

L466 : typo : var~~**ia**~~iance

Changed.

L481-482 : layout issues

This is a track changes layout issue.

L526 : same comment than in Eq 5 and B is written with a – on top not like in the Eq. (5)

This has been asnwered in response to your Eq. (5) comment.

L546 : the number of the figure showing the final rigidities is missing (I imagine it is Fig. B5)

This is a track changes layout issue.

Table B1 : typo : **V**alue I4- Budd

Changed.

Table B1 caption : typo : '-' is missing after I5

The fifth inversion has the same cost function coefficients for both the Budd and Schoof runs so we do not need to distinguish between them.

Fig B5 : layout issue, I cannot read the caption

This is a track changes layout issue.

L773 : Akesson et al. 2022 is not at the right place in the bibliography (it is at the end of the final bibliography)

This reference is in the correct location confirned by Dr. Karlsson in editing.

3 Reviewer 2 Comments

2nd Review of: Basal conditions of Denman Glacier from glacier hydrology and ice dynamics modeling

August 20, 2023

General impression

The authors have addressed all of my comments from the previous review. Necessary corrections have been applied and most unclear parts explained. In my opinion, the manuscript has improved during this iteration and in general is good to be published.

The applied changes and additions, in particular the newly introduced notation of the melt rate (including the equation of GlaDS where it contributes) and the argumentation on justification of limiters in sliding velocities for me revealed a few new questions, I still would like to see to be addressed. I do not think they are of major concerns, yet, clear statements and more information on the melt-rates leading to the resulting water pressure are in my view needed to get a clearer picture. If this is come after, I recommend publication.

We appreciate your comments and agree that they have improved the quality of the manuscript. We address your comments on our new iteration of the manuscript below.

2 Still open point(s) recommended be addressed

The remaining issues I see with the current version for me boil down to not provided complete information on the distribution of the basal meltwater production, i.e., the water source passed to GlaDS input obtained from the initial runs done by Seroussi et al. (2019), that in the revised text now has been introduced with the symbol m (which is somewhat unfortunately coinciding with the already used exponent in the sliding law). Although I asked about clarification on the water source input in my previous review, it might not have come clear that in my view it would be beneficial to show the melt-water distribution in a figure, such that the reader would get a better understanding on the distribution of the water sources and the hydrological balance in general. With this information, one could get a better estimation on how water- and consequently also effective pressure correlate with the supply of water. This, in my opinion, also would help to evaluate the newly introduced statement for justification of the unaffected water pressure distribution by the sliding velocity cap (see in 3 *Detailed comments*).

To avoid confusion we agree that it would be a good idea to not use the symbol m for two different variables. We now denote the melt water input into GlaDS as η. We have included a map of the water input to GlaDS in Fig. 1c. The regions of the greatest melt water production do not occur where ice is flowing faster than the cap of $800\,\mathrm{m\,a^{-1}}$ except within a small region of the Denman trough. The small overlap between the areas of melt water production and high ice velocity justify the use of the cap.

3 Detailed comments

I am referring to line numbers in the file (tc-2023-28-manuscript-version4.pdf) rebuttal letter In the rebuttal you write: *In terms of the GlaDS setup, we use standard basal velocity and water input from the JPL ISSM ISMIP model outputs of a thermal steady-state simulation (Seroussi et al.,2020), ...* I did not find such a reference (the year), neither in the revised paper, nor in the reference list of the rebuttal letter. Did you mean (Seroussi et al., 2019)?

Yes, we mean (Seroussi et al., 2019). Apologies for the confusion.

line 72 *As discussed in Dow (2023), when the system is overconstricted the pressures are unrealistically high and the model ceases to converge. When the system is underconstricted the pressures are below ice overburden for much of the domain. While there is some variation within the range of acceptable pressures, the output we present is the median and therefore is the most appropriate for representing the hydrology pressure in ice sheet dynamics equations.* Browsing through the reference (Dow, 2023), I cannot really learn what exactly the terms *over-* and *underconstricted* mean. Can you please explain? Is it in terms of imposed water supply or conditions of the hydro-potential at the boundaries or constraints on the channels? What I mainly conclude from Dow (2023) is that it is difficult to get a working set of parameters for Antarctic subglacial water sheets (which I can confirm from our own attempts with GlaDS) and - instead of testing out the whole parameter space - those cutoffs are introduced to get converged results. For me, that deserves more justification or explanation. I would like to have spelled out how you define an *acceptable waterpressure* in lack of available measurements and observations of Antarctic subglacial hydrological systems?

By overconstricted and underconstricted we refer to the ease at which water can flow through the hydrology system. Overconstricted means that it is difficult for the water to flow through the system due to a combination of low bedrock bump height and sheet conductivity and high water input. This leads to unrealistically pressurized water (water pressure is above ice overburden pressure for much of the domain), and a model which fails to converge. Underconstricted means that water flows through the hydrology system with greater ease due to relatively high sheet conductivity, bedrock bump height, and little water input. This leads to water pressures which are unrealistically low (much of the domain is below 50 % of overburden) which is unexpected for steady-state Antarctic systems that are not driven by surface water input. We lack the ability to know exactly what the correct water pressure is given lack of measurements as you state, so extensive tuning exercises of basal sheet conductivity would not be particularly useful. Instead we use our best estimate which is where most of the domain is near overburden pressure (with water pressure generally >0.8 of overburden). It would be an interesting question to pursue whether small changes in the basal water pressure would have larger impacts on basal friction parameter applications but that is beyond the scope of this study. However, to clarify these issues we include in lines 72-80.

As discussed in Dow (2023), when the system is overconstricted (i.e. it is difficult for water to flow through the hydrology system) the pressures are unrealistically high – much of the domain is above ice overburden pressure – and the model fails to converge. When the system is underconstricted (i.e. water flows through the hydrology system with ease) the pressures are well below ice overburden pressure for much of the domain (much of the domain is below 50 % of ice overburden pressure), which is unrealistically low for steady-state Antarctic systems that are not driven by surface water input. The variables controlling the constriction of the hydrology system are k*,* h_r*, and* η*, with a more constricted system arising from larger* η *and smaller* k *and* h_r*. We test order of magnitude changes in* k *to determine a suitable level of constriction of the system. While there is some variation within the range of acceptable pressures, the output we present is the median and therefore is the most appropriate for representing the hydrology pressure in ice sheet dynamics equations without further information from in situ measurements for example. Future work with full coupling of hydrology and ice dynamics can explore sensitivity to different distributed system inputs.*

line 90 *Tests of similar caps for model runs at Helheim Glacier (Poinar et al., 2019) demonstrate it has little impact on the model results.* Can you please provide more evidence/ argumentation why the situation at a Greenlandic outlet glacier should transfer to a system at Antarctica? I see differences, for instance, in terms of water sources. In Greenland runoff has certainly an impact that even can introduce a strong seasonal variation (hence question the assumption of a steady state), whereas I would expect friction heating to be the dominating source of water production for an outlet glacier in Antarctica (see e.g., Dow, 2023). As mentioned before, a picture showing the spatial distribution of water supply could help with getting more insight and justification of the applied analogy between these two ice sheets.

The caps applied to Helheim Glacier in Poinar et al. (2019) assumed winter conditions which would be steady state and comparable. However, the main point we wanted to make here was that large velocities lead to cavity opening which is faster than the model is able to converge. This has to do with the model configuration, not the area being tested (Greenland vs Antarctica). Determining a workaround for applying this cap when ice velocities are large is a potential improvement for future modeling studies. We now have on lines 94-95: *Tests of similar caps for model runs of winter conditions at Helheim Glacier Poinar et al. (2019) demonstrate it has little impact on the effective pressure.*

line 104, eq. (5) In the new version you introduce the effective viscosity μ, but in the component-wise SSA equations (3) and (4) – which I would combine to one equation number – before you use $\bar{\mu}$. Can you please either explain, how you come from one to the other or correct the annotation? I then presume that the rigidity B is the field you invert for. Please, add the symbol rather than the wording *Ice Rigidity* to the annotation of Figure B5.

$\bar{\mu}$ is the depth averaged effective viscosity μ which we mention in the manuscript but before the explanation of what μ is. We have now combined the two SSA equations into a single equation and explain what μ and $\bar{\mu}$ are before the equations rather than after. On lines 104-106 we have:

The inverse model uses the shallow-shelf approximation (SSA; MacAyeal, 1989; Morland, 1987) to the full Stokes equations, described in Eq. (3) with $\bar{\mu}$ the depth-averaged effective viscosity μ which is given in Eq. (4). The SSA is described in full in Larour et al. (2012).

You are correct, $\tilde{B}$ is what we invert for. The caption of Figure B5 has now been changed to:

Ice Rigidities from inversion ($\tilde{B}$, $Pa\,s^{1/3}$). **(a)** *Ice rigidity from Schoof with the GlaDS effective pressure (N_G) run;* **(b)** *Ice rigidity from Schoof with the typically prescribed effective pressure (N_O) run;* **(c)** *Ice rigidity from Budd with the GlaDS effective pressure (N_G) run; and* **(d)** *Ice rigidity from Budd with the typically prescribed effective pressure (N_O) run.*

line 390 GlaDs → GlaDS

Changed.

references Quite a few of the references, like Dow (2023), are missing the DOI - please provide those.

We have now added DOIs for the papers that were missing them.

References Dow, Christine F. (2023). “The role of subglacial hydrology in Antarctic ice sheet dynamics and stability: a modelling perspective”. In: Annals of Glaciology, 1–6. doi: 10.1017/aog.2023.9. Seroussi, H et al. (2019). “initMIP-Antarctica: An Ice Sheet

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