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

August 20, 2023

1 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.

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 melt-water 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*).

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)?

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?

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.

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.

line 390 GlaDs → GlaDS

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

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 Model Initialization Experiment of ISMIP6". In: *The Cryosphere* 13, 1441–1471. DOI: 10.5194/tc-13-1441-2019.