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

May 11, 2023

1 General impression

The article presents the incorporation of a previously determined effective pressure in the inversion of basal friction coefficients for a shallow-shelf ice flow model of the Denman-Scott catchment for two sliding laws, namely a Budd-sliding law with linear coefficient and a regularised Coulomb law, as presented by Schoof (2005). This is a timely topic and in general I see it suited to be published in The Cryosphere.

I have three major points I would see necessary to be addressed before the publication can proceed - I put them in a separate section below.

The article in general is concisely written. The majority of figures is good to read and conveys the information well. On top of the major points, there are a few questions and suggestions I placed in my review. I hope that these may contribute to improve the quality of the manuscript.

2 Major points to be addressed

The first item I would see to be addressed is a **more detailed description of the inputs to the GlaDS simulation**, in terms of parameters but mainly the imposed slip velocity and melt-water production. You seem to run GlaDS as a pre-processing step to produce $N_G(\vec{x})$ for the inversions of the specific friction coefficients. Yet, the hydrology computations needs input in form of a slip velocity and a water-production that themselves will be a result of the ice-flow dynamics and hence the friction coefficients applied in the ice-flow model providing those. To me this appears to be a little bit of a cat-catches-its-tail problem. From the text (line 59): *Basal water and sliding velocity inputs are computed from ISSM (Seroussi et al., 2019)*, I would conclude that you pick initial sliding and velocities from a completely different inversion, subject to certain constraints: *The sliding velocity acts to open up distributed system cavities and, at velocities greater than 800 ma^{-1} , can cause model instabilities and so is capped at this value.* Can you please clearly state what ice-flow setup you base the GlaDS computation on? What are the approximations to the Stokes equation of this initial model? What has been used to represent the sliding-law and the applied effective viscosity therein - the latter also in terms of thermodynamics (if any) or damage? How do you deduce the water production from that result? If all this is addressed, I would also hope to see some conclusion if and if so, how this initial settings could have influence on the distribution of the effective pressure arising from GlaDS and if there further might be a possibility that they could pre-condition the result of the following inversion. For the pasteurisation in GlaDS itself, I have difficulties in lack of any equations and symbols to interpret values displayed in Table 1 (see detailed comments).

The second topic I would ask to have elaborated is a **discussion on how the approximations to the Stokes equations could influence your inverted slip coefficients in regions of significant vertical shear**. From what I read in the text, I understand that you are applying the Shallow Shelf Approximation (SSA). To my understanding, the dynamics in fast flowing outlet parts will be well represented by SSA. Of my concern are rather those regions, where the onset of the outlets takes place, where I would expect internal vertical ice deformation to still play a significant role. Ignoring this component, in my view, would have a bias to over-predict the slip. This highly also links to the missing detailed information on which input the GlaDS simulations are based on. If this is also based on SSA, altered slip can bias the hydrological system (as it alters melt-water production and slip-induced opening rates) over a wide range of the catchment area.

Finally, I would also like to **better understand the whole inversion procedure and the impact of the rigidity inversion on your results**. In my view this is best achieved by presenting the inversion procedure in terms of equations. You shortly mention that you invert for rigidity of the ice (line 77), which could be somehow interpreted as inverting for a (depth-averaged) damage or temperature field. You are presenting a result of this rigidity distribution in Appendix C (which is hard to interpret) but – in my view equally important – do not provide some kind of physical interpretation of it. Also, starting the averaged viscosity based on the (atmospheric?) temperature distribution given by RACMO (line 353), in my view would need some explanation. How much, for instance, does the inverted rigidity differ from the one used to compute the bedrock velocity used in GlaDS simulations? In my opinion the reader already would benefit if you could introduce the equations of the SSA system where rigidity is plugged in (to my understanding how you define your averaged viscosity in the SSA model)

3 Detailed comments

Listed in order of their appearance. If some of the comments link to the main points, I indicate it - else, they are mainly meant as suggestions on how to improve readability of the manuscript or corrections to typos. I did not sync anything with the already published other review - so, sorry for cross-postings.

line 26 *One such parameter is the basal friction coefficient, which is a key component of friction laws including the Weertman (1957), Budd et al. (1979) and Coulomb laws (Schoof, 2005; Gagliardini et al., 2007).* I would refrain from calling these laws to be *Coulomb laws* but use *regularised Coulomb laws* instead. There are several occurrences of this term in the text

line 27 Typo: *... case of the ~~of the~~ Budd ...*

line 32 *Therefore, a friction coefficient that is both smooth – has little local variability – and has limited domain wide trends is desirable.* Is that really the case in all flow situations? Could there not be situations of either a drastic change in the properties of the substrate underneath the glacier and/or the thermodynamic conditions that would also imply a significant change of those coefficients?

line 59 *Basal water and sliding velocity inputs are computed from ISSM (Seroussi et al., 2019).* This directly links to one of my main points above: As you state yourself that the flow conditions (I presume you mean ice-dynamics) are of essence, I think you should declare in the main part of the manuscript what ice-dynamic input you used to drive the GlaDS simulations. Are you directly using the results from the cited paper (Seroussi et al., 2019)? Even then, in my opinion, the manuscript would benefit from spelling this out (methods, input data, at which time of this simulation you pick the ice-dynamics input?).

line 63 *The model is run for 10,000 days, providing outputs including channel size and discharge, distributed system discharge, water depth, and effective pressure.* What was the motivation for 10k days? Was this necessary to reach some steady state? If not, which point in time was chosen for extracting the effective pressure distribution? This links to the main point of critics, namely, the in my opinion missing details for the GlaDS-step.

Table 1 I appreciate that you report on values of used model constants, which adds to the reproducibility of your experiments. You, though, report them without context to equations (hence also not providing symbols), which to me (and perhaps any reader not using GlaDS) makes them difficult to interpret. This links to the first main point of critics. Also, some background (sentence of motivation or a reference) on the choice of the numerical value could enhance the understanding of the reader. To pick one example: You report the "Ice flow constant", which I understand to be linked to the rate factor in Glen's flow law (though confused by the sign in the exponent of Pa in units) as $2.5 \times 10^{-25} Pa^{-3} s^{-1}$. Provided I interpret it correctly, such value refers (e.g. Greve and Blatter, 2009) to a relative ice-temperature of around $-10^{\circ}C$. What motivates this setting? How does this choice influence the inversion for slip coefficients and rigidity?

line 66 *ISSM is a finite-element model that uses an anisotropic mesh to simulate ice dynamics.* Just a suggestion: I understand *anisotropy* as a local variation depending on direction. I, personally, would rather use the terms "non-uniform" or "adaptive in size".

line 72 *The ISSM mesh is comprised of 66,518 nodes, with anisotropic mesh refinement for faster flowing ice using the MEaSUREs v2 ice surface speed (Rignot et al., 2011; Rignot, 2017).* How are you using the MEaSUREs ice velocity product to refine the mesh?

line 77 *The ice rigidity is calculated using inverse methods.* Can you please elaborate? Are you performing a dual-parameter inversion for sliding and rigidity? Or is this another step on top of the previous one? And what effects do you think you cover in the SSA application by inverting for rigidity (vertically averaged temperature, damage)? And what effects you neglect by the simplified physics of your ice-flow model? Are you using this rigidity only in the final inversions or already in the GlaDS runs and how does this connect to the value given in Table 1?

This links to my second point of main critics. To elaborate from my side: Temperature, in particular, is reported to be an important factor in what comes to the quality of inversions using full-stress (a.k.a. full-Stokes) models (see, e.g., Zhao et al., 2018), in particular in the regions of onset of the fast-flow outlets, where internal vertical deformation has a significant role (an effect that is not included in the here applied SSA approximation).

line 98 Variables H and B are not explained right after their first occurrence. Similar, you lack definition for ice and water density and the absolute value of the acceleration by gravity, ρ_i, ρ_w and g, respectively. These definitions appear somewhere later in the text. H and B seem to be the thickness and the elevation of the ice-sheet bottom. Please, add definitions of symbols at their first occurrence. For me it would be also of benefit to directly annotate the N with a subscribed 0 (N_0) in the formula.

Figure 2 These are suggestions: Perhaps some elaborated color-map to distinctively highlight the excess to flotation in (c) and/or the 100% as iso-line would in my opinion enhance the information of this figure.

Figure 4 Please, add an explanation what the black line in the right column represents. Same situation also in Figure D1 – Figure A1 contains the correct description in its caption.

line 147 *This leads to a comparatively greater standard deviation in the Budd friction coefficient compared with that of Schoof (1240* $kg^{1/2}m^{-2/3}s^{-5/6}$ *for Schoof and 250* $s^{1/2}m^{-1/2}$ *for Budd).* I have difficulties to interpret the relative magnitude of standard deviations between two friction coefficients based on different physics and hence of different units. Thus, I would either report normalised values (as you seem to do in your graphs) or drop this sentence.

line 148 *Despite this, the Budd friction coefficient is generally smoother than the Schoof friction coefficient, which may be a consequence of the choice of the Tikhonov regularisation coefficient used in the inversion procedure.* For the reader, I think it would be beneficial if you could explain how exactly you determined the optimal regularisation parameter in a – at least for me – difficult to interpret L-curve given in Figure C1? Was, for instance, the relation between misfit and regularisation smaller as in Schoof?

line 166 *. . . irrespective of the degree of regularization (Appendix C).* What exactly do you mean by “degree of regularization”?

line *Our study uses inverse methods to calculate the friction coefficient for a given* τ_b/N_G*.* Is this a hint that you invert for traction and then interpret in terms of the specific friction law?

line 285 *That is, using the Schoof law, regions of lower effective pressures tend to also have lower simulated basal friction and faster flow – evidence for the controlling role of the hydrological system.* Is that not also the case for Budd-sliding? To my understanding, the main difference is the more complex relation in the regularised Coulomb law to the effective pressure, yet, Budd-law has an inverse proportional relation of the sliding speed to the effective pressure and should result in faster flow over lower pressures.

line 303 *The domains of the ISSM and GlaDS models differ, with the GlaDS domain being a subset of the ISSM domain.* What was the main motivation to not make these domains the same? Is it so expensive to run GlaDS on a wider domain? Or are there issues with boundaries? Would that not be worth it to get rid of all these extrapolation issues?

Figure B1	To me the coloured lines have a different colour to the ones in the legend, such that I am not able to retrieve significant information out of this graph. Also, please explain the accumulated probabilities at the lower and upper end of the spectrum (I guess it is because of the cap).
line 345	*We use inverse methods to calculate the basal friction coefficient* α *from the Budd friction law and the ice rigidity in the Glen flow law. The inverse method works to reduce the mismatch between the simulated and observed velocities, here taken from MEaSUREs v2 (Rignot et al., 2011; Rignot, 2017), by minimising a cost function that includes both linear and logarithmic velocity misfit components.* I (and perhaps some of the readers) would benefit from having the whole cost-function including regularisation terms being written out as equation in order to easier interpret what you are doing in your inversion. It would add to make your experiment more reproducible. I am also confused on how you introduce the inversion of rigidity (e.g., do you use a penalty term for deviation to prior?). This links to the third main point of critics.
Figure C1 and C2	A suggestion to improve the information for the reader: Perhaps you can distinctively mark the parameter configuration of the run that finally was chosen to provide the optimal inversion according to your L-curve analysis.
line 361	*. . . , and* C_{max} *is Iken's bound, here* $C_{max} = 0.8m^{-1/3}s^{1/3}$. As Iken's bound is related to the roughness of the bed (Schoof, 2005), can you please explain how the exact value reported here came to be? Maybe by backing it up with information on properties of the glacier substrate?
line 368	*In these regions, the inverse method compensates by increasing the friction coefficient upstream of the anomalously low effective pressure, leading to an underestimate of surface speeds there compared with the observations. The surface speeds are also generally overestimated in the region of vanishing shear stresses.* This links to my suggestion to better discuss the implication of the SSA approximation. Could it be that this effect is pronounced by the fact that stress bridging between low and higher friction region is not represented in the model/approximation?
line Figure C4	To me, this figure is not clear. I see 11 different colours in the graph and only 4 in the legend - I do not get a clear idea on which curve represents what probability distribution.

line 376 *That is, 98% of the effective pressure in the ISSM simulation is derived directly from the GlaDS simulated effective pressure.* I would drop that to the previous sentence somewhat redundant statement.

line 415

$$\nabla\phi = \rho_i g f_B(H)\nabla H + \rho_w g\nabla B, \text{ (E6)}$$

$$f_B(H) = r_l \frac{1 + (1-m)(H/\tilde{H})^m}{(1 + (H/\tilde{H})^m)^2}. \text{ (E7)}$$

Here, $f_B(H)$ *is a dimensionless factor which describes the extent to which ice thickness gradients play a role in the hydraulic potential gradient.* To me it seems that the factor $f_B(H)$ is equivalent to what in basic literature is called "flotation fraction" (e.g., see Chapter 6 in Cuffey and Patterson, 2010), which you depict for GlaDS result in Figure 2c. If you agree, please, try to make that connection for the reader.

line 417 *It is seen that in the regime of larger ice thickness* $f_B(H)$ *goes to 0, and the gradient in the bed elevation becomes the sole control on the direction of water flow; in the regime of small ice thickness* $f_B(H)$ *goes to* r_l *, and gradients in the ice thickness become of similar importance to gradients in the bed elevation. This is a more intuitive picture of the subglacial hydrological system, where water can flow throughout the entire domain, and where flow is dependent on both the basal topography and the ice thickness, as is expected.* As you claim that this is expected, in my view that would need to be backed up by reference(s). Provided, we agree that f_b is equivalent to the flotation factor, for me it is even somewhat counter intuitive. Following standard literature (e.g., see Chapter 6 in Cuffey and Patterson, 2010) the flotation fraction has to go down to $f_B = 0.56$ for bedrock gradients (and not, actually, bedrock elevation itself) to reach same influence as surface (not thickness, though) gradients. In Figure 2c, though, the whole region west of 2200 km – which I connect to large thickness (although you do not provide a graph with ice thickness) – seems to be very close to a fully pressurised hydrological system (i.e. $f_B(H) \approx 1$), which would make me expect that surface gradients would almost by an order of magnitude dominate the flow direction of water. I, though, have difficulties to relate thickness to bedrock gradients, as the earlier are partly defined by the latter, i.e., $\nabla H = \nabla(S + B)$, if S is the ice-surface elevation. To sum up: I do not see your statement that $f_B(H) \to 0$ for thick parts of the sheet is reflected in Figure 2c and neither in Figure 3a. A graph depicting f_B and ice thickness might help me and perhaps some readers to understand.

References

Cuffey, K.M and V.S.B Patterson (2010). 4th ed. Butterworth-Heinemann. ISBN: 978-0-12-369461-4.

Greve, R. and H. Blatter (2009). *Dynamics of Ice Sheets and Glaciers*. Springer. DOI: `10.1007/978-3-642-03415-2`.

Schoof, C. (2005). “The Effect of Cavitation on Glacier Sliding”. In: *Proceedings of the Royal Society A* 461, 609–627. DOI: `10.1098/rspa.2004.1350`.

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`.

Zhao, C. et al. (2018). “Basal friction of Fleming Glacier, Antarctica – Part 1: Sensitivity of inversion to temperature and bedrock uncertainty”. In: *The Cryosphere* 12.8, pp. 2637–2652. DOI: `10.5194/tc-12-2637-2018`. URL: `https://tc.copernicus.org/articles/12/2637/2018/`.