Reviewer Comment Response

Dear Dr Karlsson, Dr Zwinger

We thank the reviewers for their constructive comments and the improvements that they will bring to the manuscript.

In what follows, the reviewer comments are in black, our responses to the reviewer comments are in blue, and suggested edits to the manuscript are italicised. The line references are to the revised manuscript unless otherwise specified.

In addition to the changes suggested by the reviewers, we have also condensed the appendices for improved readability, by merging all appendices that relate to sensitivity analyses into subsections of Appendix A. Appendix B gives more details on the inversion procedure, and Appendix C gives more details on the new, proposed empirical parameterisation for the effective pressure.

Best regards,
Koi McArthur and co-authors.

1 Author Comments

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.

Thank you for your comments on our paper, which we address below.

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 $N_G(x)$ 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 $\mathrm{m\,a^{-1}}$, 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).

As with all inversions and model runs, we start with our best estimate and use this to iterate towards a reasonable result. 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), using the enthalpy formulation implemented in ISSM (Seroussi et al.,

2013) and the Blatter-Pattyn (BP) approximation to the full Stokes equations. These fields are used to initiate the hydrology model, from which the basal water pressures are used as inputs to the inverse model using ISSM. This is a soft-coupled approach between the ice sheet and hydrology systems – a precurser to full two-waycoupling between GlaDS and ISSM that we are actively working towards. Even with two-way coupling we require input fields from the ice sheet system to initialise the hydrology model. Unfortunately without two-way coupling available at the moment we are unable to run the hydrology and ice dynamics forward in time together; we do hope, however, that this study demonstrates why that is an important next step. The 800 m/year cap is a standard approach to GlaDs modeling where it is necessary to apply a limit to subglacial cavity opening for stability. This is a limitation of the model but in tests for Greenland (Poinar et al, 2019) had little impact on the basal water pressure. The ISMIP model setup is well-described, widely used and cited, and the data sets are freely accessible through GHub. Hence, we refer readers to the references that detail the setup rather than repeating them here. We now include the added detail on the GlaDS inputs of basal water and sliding velocity (at lines 82 to 90):

Basal water and sliding velocity inputs are taken from the JPL_ISSM ISMIP6 Antarctic control run final time step (Seroussi et al., 2019) which was a thermal steady state simulation using the enthalpy formulation implemented in ISSM (Seroussi et al., 2013) and the Blatter-Pattyn (BP) approximation to the full Stokes equations. This model solved for ice viscosity of the floating ice shelf and the basal friction coefficient using data assimilation techniques (MacAyeal, 1993; Morlighem et al., 2010), which differs from our application of ISSM to assess basal boundary conditions. However, lacking an alternative starting point for basal sliding and velocity, this is the best available option prior to full coupling between hydrology and ice dynamics in ISSM. The sliding velocity acts to open up distributed system cavities and, at velocities greater than 800 $\mathrm{m\,a^{-1}}$, can cause model instabilities and so is capped at this value. Tests of similar caps for model runs at Helheim Glacier (Poinar et al., 2019) demonstrate it has little impact on the model results.

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.

This is a good point. Even with the Blatter-Pattyn approximation, the unenhanced Glen flow relation cannot, by nature, capture the bed-parallel vertical shear deformation profile expected in regions where the ice sheet is frozen to the bed (McCormack et al., 2022). We expect that in our simulations using SSA and the Glen flow relation, that our model overestimates flow by sliding, and underestimates deformation, most likely compensating by making the ice too stiff. However, in this work we're primarily interested in the differences between different basal friction laws, so it's likely that this is an issue inherent in each of our simulations, and it is unlikely to change our main conclusions. If we were to perform a prognostic simulation, we would certainly want to consider the relative contributions of deformation and sliding, and more appropriately treat the processes of deformation that occur in tertiary creep, e.g. anisotropy.

To clarify this we add lines 290-299:

In this work we have used an SSA ice flow model, which fails to capture bed-parallel vertical shear deformations. This may affect the results of our inversions for the basal friction coefficient in areas of non-negligible vertical shear, such as at the onset of fast-flowing ice streams of the Denman and Scott troughs. However, the use of the Glen flow relation may also impact the capacity of even higher-order models to accurately capture bed-parallel vertical shear deformations. For example, McCormack et al. showed that even when the BP approximation to the full Stokes is used, the unenhanced Glen flow relation fails to capture the vertical shear profile expected in regions where ice is frozen to the bed of the glacier. In our simulations that use the SSA approximation and the Glen flow relation, it is possible that sliding is overestimated, and the basal friction coefficient underestimated, where vertical shear is an important deformation process. However, this is a common issue to all of our model runs and is therefore unlikely to alter the main conclusions of this work that compare how the form of the effective pressure impacts the basal friction coefficient.

We explain in more detail the GlaDS setup in our response to your first major comment, noting that the thermal model there employs the BP approximation, and hence the inputs to the GlaDS simulations may take into account some bed-parallel vertical shear deformation. However, as argued above, in the absence of a flow relation that incorporates effects of deformation that are present in tertiary creep, even the BP approximation may not accurately capture the contribution of bed-parallel vertical shear deformation to overall flow. Although it's outside the scope of what we focus on in this work, the relative contributions of sliding and deformation to overall flow is an important question generally, and should definitely be considered in future work.

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)

The rigidity field is intended to capture processes related to the rheology of ice that are not explicitly modelled or parameterised in the flow relation. These may include effects such as: damage, anisotropy, chemical impurities, and liquid water. These ice properties or processes evolve spatially and in time, for example due to changes in the stress, temperature, and climate forcings, and such temporal evolution of ice rheology is not typically accounted for in the ice rigidity field (although this is not an issue for our diagnostic model simulations). In our study, uncertainties in the rheological parameters do add to uncertainty in the model results. Some of these uncertainties are discussed/summarised in recent papers, including Graham et al., (2018) and McCormack et al. (2022). Although it is outside the scope of the current study to consider the effects of ice rheology, we have elaborated on these uncertainties in Section 2.2.1 line 136-138:

Though it is not the primary focus of this work, we invert for ice rigidity as well, while initializing our model for the inversion for the basal friction coefficients. By inverting for the ice rigidity we capture ice rheological processes which are not explicitly accounted for in the model such as damage, anisotropy, chemical impurities, and liquid water.

There will be differences in the rigidities generated here using ISSM and those used for the GlaDS initialisation; however, since in the GlaDS simulation we relied on standard ISSM ISMIP6 outputs we are unable to change this. These issues in terms of the subglacial hydrology vs. ice dynamics setup in ISSM are excellent questions to address once we have a two-way coupled model that we can apply initialisation experiments to.

Using surface temperatures from RACMOv2.3 as opposed to depth averaged temperatures could impact our basal friction coefficient inversions. Zhao et al. (2018) found that initializing the temperature field to be colder resulted in a lower basal friction coefficient. Using a depth averaged temperature which may be warmer than the surface temperature could increase the value of the basal friction coefficient calculated from inversion, though this is likely an issue present in all of our model runs and hence shouldn't affect our conclusions regarding the impact of the effective pressure on the basal friction coefficient inversion. We add lines 300-307 to discuss the impact of using RACMOv2.3:

Initializing our temperature field with surface temperatures from RACMOv2.3 (van Wessem et al., 2018) could have an impact on the rigidity and basal friction coefficient inversions we performed in this work. Zhao et al. (2018) showed that initializing a model with a colder temperature field resulted in a decrease of the basal friction coefficient computed from inversion, due to stiffer ice. It is possible that if we used a thermal model and computed depth averaged temperatures to use in our model initialization – effectively increasing the initialization temperatures – that we could similarly see an increase in the basal friction coefficients. Like with the SSA approximation, these are issues that would be common in all of our model runs and would be unlikely to alter the main conclusions we came to regarding the effect of the form of the effective pressure on the computation of the basal friction coefficient.

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

Changed

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

Changed

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?

The point we had intended to make here is that if temperature, substrate, or other variables play an important role in basal friction then, in the ideal case, we might either directly model them or find parameterizations that could be incorporated into friction laws so that their effects could be removed from the friction coefficient. We have reworded:

However, the dependency of the basal shear stress on other quantities, or unexpected dependency on sliding velocity and effective pressure, is implicitly captured in the friction coefficient. Therefore, a spatially variable friction coefficient suggests a friction law which either fails to capture the proper functional dependency on sliding velocity and effective pressure, or omits the dependency of the basal shear stress on other quantities. Therefore, a friction coefficient that is both smooth – has little local variability – and has limited domain wide trends is desirable.

to

However, if the basal shear stress does not actually have the functional dependence on the basal sliding velocity and effective pressure proposed in the basal friction law, or if it has a functional dependence on other properties of the bed such as roughness, substrate, or temperature, then this will be implicitly captured in the basal friction coefficient. Therefore, a spatially variable basal friction coefficient suggests a friction law which either fails to capture the proper functional dependency on basal sliding velocity and effective pressure, or omits *the dependency of the basal shear stress on other quantities. By consequence, a basal friction coefficient that is both smooth – has little local variability – and has limited domain-wide trends is desirable.*

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 icedynamics) 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?).

We have addressed these concerns in the response to the major comment.

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.

This point in time was chosen in order to reach near steady-state conditions. It's not fully steady state because the presence of lakes and large channels means that there are always some small adjustments happening in small regions of the domain. However, for our analysis, these small regions of change do not impact our results. We have now clarified this in the text as follows: *The model is run for 10,000 days in order to reach near steady state (there are changes in the water sheet thickness in deep pockets on the order of* $\mathrm{mm\,day^{-1}}$*), providing outputs including channel size and discharge, distributed system discharge, water depth, and effective pressure.*

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 $2.5 \times 10^{-25}\mathrm{Pa}^{-3}\mathrm{s}^{-1}$. Provided I interpret it correctly, such value refers (e.g. Greve and Blatter, 2009) to a relative ice-temperature of around -10○C. What motivates this setting? How does this choice influence the inversion for slip coefficients and rigidity?

We have discussed most of these points in response to the major comment. For example, we now discuss the choice of parameters and include equations to demonstrate how they are incorporated in GlaDS (lines 61-77).

The model parameters used in our simulations are summarized in Table 1. The bedrock bump height, cavity spacing, and sheet conductivity act together in the distributed system equations to either constrict or open the system to water flow as show in :

$$w - \tilde{A}h|N|^{n-1}N + \frac{\partial h_e}{\partial t} + \overrightarrow{\nabla} \cdot \overrightarrow{q} = m \quad (1)$$

$$\overrightarrow{q} = -kh^{\alpha}|\overrightarrow{\nabla}\phi|^{\beta-2}\overrightarrow{\nabla}\phi \quad (2)$$

Where $\tilde{A}$ is the rheological constant of the ice multiplied by an order one factor depending on the cavity geometry, h is the hydrology sheet thickness, N is the effective pressure, n is the exponent in Glen's flow law, h_e is the englacial storage, t is time, m is a prescribed source term, $\overrightarrow{q}$ is the hydrology sheet discharge, k is the sheet conductivity, ϕ is the hydraulic potential, and $\alpha = 5/4$ and $\beta = 3/2$ are exponents in the Darcy-Weisbach law describing fully turbulent flow. w is the hydrology sheet opening rate equal to $u_b(h_r - h)/l_r$ when $h < h_r$ and zero otherwise, here u_b is the basal ice speed, h_r is the typical bedrock bump height and l_r is the typical cavity spacing.

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 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. Future work with full coupling of hydrology and ice dynamics can explore sensitivity to different distributed system inputs. Channel conductivity is, similarly, a median value applied in GlaDS in Antarctica (Dow et al., 2020). The ice flow constant is set for an average ice column temperature of

-10°C.

We also now include the parameter symbols in the table. In terms of the ice flow constant, this parameter is used differently in GlaDS compared to ISSM. For the former, it is the mechanism by which channels and cavities close (Eq. 1 of the revised manuscript), whereas with ISSM the rigidity field is initialised with a temperature field and then updated as the model is run (Eq. 5 of the updated manuscript). Unfortunately, we do not know the impact of changing the rigidity/rheology on the GlaDS outputs because, to date, coupled complex hydrology and ice dynamics modeling have not been applied in the Antarctic and these questions have not been examined. We hope our future work with full coupling will allow us to examine these questions.

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".

Changed to *non-uniform*

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?

This is described in Appendix A2. We have changed:

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).
to
The ISSM mesh is comprised of 66,518 nodes, with non-uniform mesh refinement for faster-flowing ice using the MEaSUREs v2 ice surface speed (Rignot et al., 2011, 2017; Mouginot et al., 2012, 2017), described in Appendix A2.

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).

The order of inversions is now discussed in this section of the methods. That is, we invert for rigidity over the ice shelf, then for the Budd friction law basal friction coefficient, we invert for rigidity over the entire domain, we invert for the Schoof and Budd friction coefficients. We now include in Section 2.2.1: *We perform the following inversion procedure. First, we invert for the ice rigidity over the floating portion of the domain. Next, we invert for the Budd basal friction coefficient over the grounded portion of the domain, using an ice rigidity on grounded ice specified by the Paterson function from Cuffey and surface temperatures from RACMOv2.3 (van Wessem et al., 2018). After the Budd inversion we invert for the ice rigidity over the entire domain. We next use the basal friction coefficient estimated using the Budd friction law to compute an initial estimate of the basal friction coefficient for the Schoof friction law. We perform inversions for the basal friction coefficients of the Budd and Schoof friction laws with the ice rigidity from the inversion prior, these are the main simulations discussed in the text that follows (it is worth noting that the Budd friction coefficient converges to the result of the initital Budd friction coefficient inversion). We perform a final rigidity inversion over the entire domain. The cost functions to be minimized for each inversion are described in detail in Appendix B.*

We have now included in Appendix C the cost functions which we minimize for each inversion. Namely:

$$\mathcal{J}_a(\overrightarrow{u}) = \iint_s \frac{1}{2}\left((u_x - u_x^{\text{obs}})^2 + (u_y - u_y^{\text{obs}})^2\right) dS, \tag{B1}$$

$$\mathcal{J}_l(\overrightarrow{u}) = \iint_s \left(\log\left(\frac{||\overrightarrow{u}|| + \varepsilon}{||\overrightarrow{u}^{\text{obs}}|| + \varepsilon}\right)\right)^2 dS, \tag{B2}$$

$$\mathcal{J}_t(\overrightarrow{u}) = \iint_s \frac{1}{2}||\overrightarrow{\nabla} k||^2 dS, \tag{B3}$$

$$\mathcal{J}(\overrightarrow{u}) = c_a\mathcal{J}_a + c_l\mathcal{J}_l + c_t\mathcal{J}_t. \tag{B4}$$

Here, $\mathcal{J}_a$ is the linear velocity misfit cost function, $\mathcal{J}_l$ is the logarithmic velocity misfit cost function, $\mathcal{J}_t$ is the regularization cost function, $\overrightarrow{u}$ is the modeled ice surface velocity, $\overrightarrow{u}^{obs}$ is the observed ice surface velocity, ε is a small number (around machine precision) acting as the minimum observed velocity in Eq. (B2), S is the two dimensional spatial domain of the model, and k is taken to be α in a Budd inversion, C in a Schoof inversion, and $\bar{B}$ (the rigidity) in a rigidity inversion. The full cost function to be minimized is given in Eq. (B4) where c_a, c_l, and c_t are the cost function coefficients for $\mathcal{J}_a$, $\mathcal{J}_l$, and $\mathcal{J}_t$ respectively. c_t is referred to as the Tikhonov regularization coefficient.

We have addressed the physical processes that are captured by inverting for rigidity in our response to the main comment. These rigidity inversions have no relation to the GlaDS modeling and are used to obtain simulated ice surface velocities close to observed ice surface velocities prior to and after inversions for the basal friction coefficients of the Budd and Schoof friction laws.

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.

We have changed the formula for N_O as suggested and have now included the following variable definitions: *(1) An effective pressure given by assuming water pressure equals the ice overburden pressure plus the gravitational potential energy of the water $N_O = \rho_i g H + \rho_w g B$. Here, ρ_i is the density of ice ρ_w, is the density of water, g is the absolute value of the gravitational acceleration, H is the ice thickness, and B is the bed elevation which takes negative values below sea level.*

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.

The 100 % isoline has been added to Fig. 2c.

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.

Changed

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.

To address this, we now report these as normalized values (i.e. adjusted by the mean value of each friction coefficient). We have changed:

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).*
to
This leads to a greater standard deviation in the Budd friction coefficient compared with that of Schoof when both are normalized to their respective means (0.228 for Schoof and 0.385 for Budd).

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?

We use the same strategy for picking the optimal regularization parameter as in Schoof. To clarify this we have added in Appendix B: *Ideally* c_t *is chosen such that any smaller value of* c_t *will not have a significant affect on* $c_a \mathcal{J} a + c_l \mathcal{J}_l$. Where these cost functions are described as per our response to your comment on line 77.

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

Changed *degree of regularization* to *magnitude of the Tikhonov regularization coefficient.*

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?

This was poorly worded, we mean that we invert with both τ_b and N_G given. This has been changed to: *Our study uses inverse methods to calculate the basal friction coefficient for a given* τ_b *and* N_G.

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.

Yes, we meant lower basal friction coefficient which had a negative correlation with the effective pressure for the Budd simulations, not basal friction which is roughly the same to get a similar ice surface velocity match. The line has been changed to: *That is, using the Schoof friction law, regions of lower effective pressures (both* N_G *and* N_O*) tend to also have lower basal friction coefficients – evidence for the controlling role of the hydrological system.*

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?

The GlaDS domain differs from the ISSM domain since GlaDS cannot run on ice shelves and because the limited number of nodes available for GlaDS runs (20,000) requires restricting the domain to the primary hydrological outlets of Denman and Scott glaciers. This node limitation will not be an issue for future iterations of GlaDS model outputs as we are aiming to switch over to the parallelised ISSM GlaDS environment.

Furthermore, the GlaDS domain was set up based on the hydrological catchment calculated from overburden potential, which is different than the ice flow catchment. To clarify, we have changed:

The domains of the ISSM and GlaDS models differ, with the GlaDS domain being a subset of the ISSM domain.

to

The domains of the ISSM and GlaDS models differ, with the GlaDS domain being a subset of the ISSM domain due to the differing subglacial hydrological and ice catchments and limits to the GlaDS domain size requiring restriction to the primary hydrological outlets of Denman and Scott Glaciers.

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).

The figure has been changed so that it is composed of line graphs, not overlaid translucent bar graphs. We now also include in Appendix B the following explanation of the accumulated probabilities: *The accumulated probability at the lower and upper bounds in Fig. B1 correspond to the regions of low/negative effective pressure near subglacial lakes and troughs and the area of negative effective pressure upstream of the subglacial lake feeding the Denman and Scott troughs (Fig 2v) respectively.*

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.

The cost functions have been added to Appendix B where the order of the inversions is described in detail. See our response to your comment on line 77.

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.

The marker for the chosen parameter has been enlargened, turned red, and turned into a diamond.

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?

Thank you for the comment. Maritati et al., (2016) suggests that the bed is rough in the high elevation areas of the bed and smooth in the low elevation areas of the bed. We decided that the best way to choose a value of $C_{\max}$ would be to perform a sensitivity analysis. We have now performed a sensitivity analysis with values of $C_{\max} = 0.5, 0.6, 0.7, 0.8$. We found that each value of $C_{\max}$ produced the same qualitative conclusions we came to using a value of $C_{\max} = 0.8$. That is, the Schoof friction law produced more desirable results than the Budd friction law and the GlaDS effective pressure produced more desirable results than N_O. We decided that a value of $C_{\max} = 0.7$ was the best choice to go with and we have changed this throughout the manuscript. The results of the sensitivity analysis are put into Appendix A5.

Iken's bound, C_{max}*, mathematically describes the idea that the bed can support a maximum stress (Iken, 1981; Schoof, 2005; Gagliardini et al., 2007). In the Schoof friction law (Eq. 7),* τ_b *cannot exceed* $C_{max}N$*, where* C_{max} *represents rheological properties of the till (Brondex et al., 2019) and ranges between 0.17 and 0.84 (Cuffey and Paterson, 2010). To determine an appropriate value of* C_{max} *we test the effect of using* $C_{max} = 0.5, 0.6, 0.7,$ *and 0.8 on the Schoof basal friction coefficient using* N_G *and* N_O*.*

We arrive at the same qualitative conclusions as in the main text for all four values of C_{max}*.*

In all cases, the variance of the normalized basal friction coefficient is smaller for N_G than for N_O, and it is smaller than the variance of the normalized Budd basal friction coefficient. Although the results for $C_{max} = 0.5, 0.6,$ and 0.7 are similar, there is a comparatively large decrease in the mean of the basal friction coefficient and increase in the variance of the normalized basal friction coefficient between $C_{max} = 0.7$ and $C_{max} = 0.8$ for both N_G and N_O. For lower values of C_{max}, a region of higher basal friction coefficient centered around 2200 km easting and -400 km northing (Fig. A5a) develops in the N_G simulations. Solving Eq. (7) for C yields

$$C = \frac{\left(\frac{\tau_b}{|u_b|^{m-1}u_b}\right)^{m/2}}{\left(1-\left(\frac{\tau_b}{C_{max}N}\right)^{1/m}\right)^{m/2}}, \tag{A1}$$

where we see that in the limit where $\tau_b \to C_{max}N$ then $C \to \infty$. The region of high basal friction coefficient for lower C_{max} also has low effective pressure (Fig. 2a), resulting from τ_b approaching $C_{max}N$ for larger values of N and the basal friction coefficient compensating this. To prevent potentially infinitely increasing values of the basal friction coefficient it is possible to increase the cap on the effective pressure, but again, this would reduce the area over which the GlaDS data is used and the effect of using modeled hydrology will be less impactful.

The Schoof friction law is a regularized Coulomb friction law which tends towards a Weertman sliding regime when $\tau_b << C_{max}N$ and a Coulomb sliding regime when $\tau_b >> C_{max}N$. Hence, the value of C_{max} will have an effect on what physical processes are being represented in the Schoof friction law. Figure F2 shows $\tau_d/(C_{max}N)$ for various values of C_{max}, where values close to zero correspond to a Weertman sliding regime and values close to one correspond to a Coulomb sliding regime. The choice of C_{max} appears to have little effect on where each of the Weertman and Coulomb sliding regimes occur, with distinct locations between the two for all values of C_{max}. This suggests that the choice of C_{max} will not have a significant impact on which physical processes are being represented throughout the domain and does not justify the use of one value of C_{max} over another.

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?

Morlighem et al. (2010) compared basal friction coefficients calculated using inversion and three different ice flow models: full Stokes (FS), the Blatter-Pattyn approximation (BP; Blatter, 1995; Pattyn, 2003), and the Shallow Shelf Approximation (SSA; MacAyeal, 1989). They found that all three ice flow models produce similar results for most of the Pine Island Glacier catchment. The greatest differences occur in the ∼100 km region upstream of the grounding line, where bridging stresses are not negligible, with the result that both the BP and SSA models tend to overestimate basal drag there compared with the FS model. We have yet to runs FS over our domain of study. However, we note that the effect of ignoring bridging stresses in increasing the basal friction coefficient is common across the friction laws estimated in our study. The implications of this choice would be more significant in a prognostic simulation – with grounding line migration – than in the diagnostic experiments conducted here.

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.

The graph has been changed to a line graph where it should be easier to identify the 4 distributions.

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.

This has been merged into the sentence prior.

line 415

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

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

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.

In Chapter 6 of Cuffey and Paterson (2010) a spatially uniform fraction of flotation is assumed. Here the fraction of flotation varies spatially and so the gradient of the water pressure includes a contribution from the gradient of the fraction of flotation. This is therefore a more physically realistic application of hydraulic potential estimates. Because the gradient of the fraction of flotation is not zero, f_B is not the fraction of flotation. A change to the manuscript described in our response to your below comment has been made to clarify this.

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 $f_B(H) \rightarrow 0$ 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.

Thank you for your comment, after looking into this, it appears that we have made a mistake in the calculation of f_B which we have corrected.

$$f_B(H) = 1 - (1 - r_l)\frac{1 + (1 - x)(H/\tilde{H})^x}{(1 + (H/\tilde{H})^x)^2} \tag{E7}$$

f_B is not the fraction of flotation as we have explained in our response to the previous comment, but is a quadratic function of the fraction of flotation (f_w), with values larger than f_w for $f_w \in [r_l, 1]$ so surface gradients play a more important role in the hydraulic potential gradient for the empirical parameterization than when a spatially uniform fraction of flotation is considered. We present new analysis relating f_B to the fraction of flotation, updating the manuscript as follows:

$$\vec{\nabla}\phi = \rho_i g f_B(H)\vec{\nabla}H + \rho_w g\vec{\nabla}B = \rho_i g\left(f_B(H)\vec{\nabla}S + \left(\frac{\rho_w}{\rho_i} - f_B(H)\right)\vec{\nabla}B\right) \tag{G6}$$

$$f_B(H) = 1 - (1 - r_l)\frac{1 + (1 - x)(H/\tilde{H})^x}{(1 + (H/\tilde{H})^x)^2} \tag{G7}$$

Here, $f_B(H)$ is a dimensionless factor that describes the extent to which ice thickness gradients play a role in the hydraulic potential gradient. Cuffey and Paterson (2010) considered the gradient of the hydraulic potential with a spatially constant fraction of flotation and arrive at the form of Eq.(C6) with $f_B(H)$ replaced by the fraction of flotation, f_w. Here, f_B is not the fraction of flotation but is related to it via Eq. (C8) for $f_w \geq r_l$.

$$f_B = -\frac{x r_l}{1 - r_l} + \frac{2 + (x - 1)(1 + r_l)}{1 - r_l} f_w - \frac{x}{1 - r_l} f_w^2. \tag{G8}$$

f_B is quadratic in f_w and $f_B \geq f_w$ for $f_w \in [r_l, 1]$. This means that for N_E, gradients in surface elevation play a more important role in water routing than when a constant fraction of flotation is considered. The importance of surface elevation gradients reach a maximum when $f_w = 0.924$, corresponding to $f_B = 1.04$, values of f_w above this threshold will actually result in smaller values of f_B and a decreased role in the surface elevation gradient which is at odds with the analysis of Cuffey and Paterson (2010). Modeling (Dow

et al, 2014, 2020) outputs, including those from our Denman analysis, suggest that the fraction of flotation is not uniform (Fig. 6). Compared to using N_O, this parameterization provides 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/surface topography, as is expected.

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