Reply to Reviewer 1

We thank the reviewer for their thoughtful evaluation of our paper, and the comments offered to improve it. Our replies follow. We use bold face for comments, normal face for our reply, and put changes to the manuscript in quotation marks, with italics indicating new text. Strikethrough text denotes deletion.

This manuscript describes how to incorporate the effective pressure computed by a subglacial hydrology model into a basal drag inversion. Three different sliding laws are used: (1) a linear sliding law that does not depend on the effective pressure, (2) a "Budd"-type sliding law and a (3) a Schoof sliding law that both depend on the effective pressure. The authors propose here to do an inversion based on the first sliding law (1), to get a good initial guess of basal sliding, then run a subglacial hydrology model for the winter season, which computes the basal melt rate and basal water pressure that is then used to invert for the basal friction parameters used by the two other laws (2) and (3). They find that the basal drag and sliding ratio (ratio between the surface and basal velocities) of the linear and Budd sliding laws are in good agreement, but that their results using the Schoof sliding law show higher spatial variability.

Overall, this manuscript is easy to read and the methods are explained in detail, so that this manuscript is accessible to readers that are not necessarily familiar with model inversions. While I enjoyed reading it, I would have liked to see more discussion on their results instead of focusing mainly on the technical aspect. In its current state, the manuscript is limited to the introduction of a new model and a new approach, which would be more suitable to GMD for example. I do think that there is potential here for more scientific discussion. I am also a bit puzzled by their results with the third sliding law (see below), and why the slip ratios are so different. Since the same viscosity parameters are used for all three models, if the basal velocities are the same, the surface velocities should also be the same. Since we are trying here to reduce the misfit between InSAR velocities and modeled surface velocities, I would expect to see the same basal sliding velocities in all three cases, and this is not what is found here.

1 Major comments

I have one main concern, I don't understand the results of the Schoof sliding law. I read several times the explanations (page 22), but I still don't understand why the results are so different.

First, and maybe I am wrong, I don't think we need to do a second inversion once we have a good estimate of basal drag (τb) and basal velocities (ub) because there are always ways to change the friction parameters of different sliding laws to end up with the same basal velocities. If this is achieved, then the surface velocities are the same since the internal deformation is the same irrespective of the sliding law. In other words, one can invert for basal friction using a linear sliding law, and then use the results of a subglacial hydrology model to constrain the parameters of a different law using the results of the first inversion without performing another inversion.

For example, here, the three sliding laws are:

$$\boldsymbol{\tau_b} = \beta^2 \boldsymbol{u_b}$$
$$\boldsymbol{\tau_b} = \mu_a N_+^p {U_b}^q \frac{\boldsymbol{u_b}}{U_b}$$
$$\boldsymbol{\tau_b} = \mu_b N_+ \left(\frac{U_b}{U_b + \lambda_b A_b N_+^n} \right)^{\frac{1}{n}} \frac{\boldsymbol{u_b}}{U_b}$$

and the authors invert for β in (1), μa in (2) and μb in (3). If we invert for β and compute Ub and N+, one can determine μa and μb by simply doing:

$$\mu_a = \frac{\beta^2 U_b}{N_+^p U_b^q}$$

and

$$\mu_b = \frac{\beta^2 U_b}{N_+ \left(\frac{U_b}{U_b + \lambda_b A_b N_+^{\frac{1}{n}}} \right)^n}$$

Using these values for µa and µb, the forward model should produce the same surface velocities and therefore the same misfit to observations. The only problem might be the smoothness of these fields. So, I guess I have 2 questions:

Why do we need such a complicated procedure, when a simple single inversion would potentially enough?

This was our initial approach. However, the issue of smoothness led us to consider whether the values determined using the algebraic approach reflected conditions at the ice sheet bed.

The aim of an inversion is not to simply find a field of basal drag that can reproduce surface velocities. Inversions problems are underdetermined, and there are an infinite number of possible solutions. Rather, the aim is to set-up the inversion problem such that the values of µa/µb are representative of physical properties of the bed. The constraint we have to solve this aim is that those properties need to be able to reproduce surface velocities. This is important as we run the model into the future, and use the inverted values in our coupled model to predict surface velocities through a summer melt season.

The linear inversion problem (as we formulate it, although different regularization schemes are possible) is to find the field β^2 that reproduces surface velocities, yet is sufficiently smooth. If we simplify the Budd sliding law by assuming p, q= 1, and then match the results to the linear sliding law, we are solving:

$$\beta^2 = \mu_a N \qquad (1)$$

We don't think this is a physical relationship. It is illustrative to look at the limit of N→0 (e.g. a subglacial lake). In that case, this formula tells us that µa→Inf. We don't interpret this value of µa as reflecting physical properties of the bed. A nearby cell where N ~ 1MPa would have a much different value of µa. However, geologically, we would anticipate the properties of the bed beneath a lake and at a nearby location to be similar. Similarly the results of the inversion should be resilient to small changes. If we invert before a subglacial lake drains, and after a subglacial lake drains, we would want to recover similar values of µa/µb in that location. This is not possible with the algebraic approach. By using Equation 1, the results also take on the assumptions made by the regularization, that the product of effective pressure and bed properties should vary smoothly. We feel that this isn't the best assumption we can make.

The approach we propose states a different problem. We assume we know N. We invert for µa/µb. We still have to regularize the problem, but in this case, the regularization is stating that the bed properties vary smoothly. This is a more physical and justified assumption in our perspective. In areas where N→0, the regularization enforces that µa/µb vary smoothly. It also possible that we may at some point have a-priori information about µa/µb or N via measurements. This approach would allow us to incorporate this information via different/additional regularization. For instance, rather than assume µa/µb vary smoothly, we may want to minimize the difference from a background value.

Each of the inversions for the different sliding laws is a different problem. Here, we are using the linear inversion to be able to predict N. Then we incorporate this information into the problem relating to the values of µa/µb. The algebraic approach may be a practical alternative for predicting the values of µa/µb under conditions when the magnitude of N has limited variability and varies smoothly. Although the effective pressures shown in the manuscript have those properties, the output of the subglacial hydrology model is sensitive to the parameters used. With plausible parameters, we observed N→0 and vary by orders of magnitude. This required us to generate a general approach. A continuum model of subglacial hydrology also leads to a smooth solution. Different formulations of subglacial hydrology may result in higher and more rapid variations of N.

On a practical note using Equation 1, with N=0, leads to µa=NaN. You have a problem of infilling these locations. This does not happen in our procedure, as in this case, µa/µb have no impact on the first term of the cost function (least squares misfit), and are determined by the second term in the cost function (smoothness).

Why are the results of the Schoof sliding law so different? Is it because the inversion converged in a local minimum?

Comments from both reviews led us to scrutinize our explanation in greater detail. Previously we had done a simple calculation which showed that N was in the order of magnitude where the sliding law was in the Coulomb limit. However, when responding to the comments, we plotted the transition point for a range of N and U, and found that the Schoof sliding law was not in the Coulomb limit for the range of velocities and effective pressures in our domain.

Unfortunately, there was a discrepancy between the text and figure. While wrote in the text that the results were from \gamma_2=1e11, we had plotted the results of \gamma_2=1e09. The higher frequencies observed are not due to the sliding law, but because of a discrepancy in regularization. We have updated the figure with the results we intended to show, which are now similar to the other sliding laws.

Text has been updated as follows:

[Non-linear sliding laws subsection of Results] Page 18, 10-11 Lines + Page 21 Lines 1-3

~~“Figure (14 and 17) show the inversion results from the Weertman and Schoof sliding law respectively. Inverted basal drag using the Weertman sliding law is very similar to the results from the linear sliding law. In contrast, the inverted basal drag from the Schoof sliding law shows much higher frequency and magnitude spatial variations. This is reflected in the spatial distribution of the sliding ratio, with the Weertman sliding law resulting in a distribution similar to the linear sliding law, while the distribution from the Schoof sliding law shows much greater variation.”~~

“Figure 5 shows the inversion results from the Budd and Schoof sliding laws. Inverted basal drag and the consequent sliding ratio using the two non-linear sliding laws are very similar to the results from the linear sliding law. Model mismatch is also similar for all three sliding laws (Figure 4).”

[Results Section] Page 22, Lines 7-19

~~“The pattern of basal drag inverted using the linear and Weertman sliding law show limited differences. This is due to the 10 fact that basal shear traction must satisfy the global stress balance (Joughin et al., 2004; Minchew et al., 2016). Both the linear and Weertman sliding law have the form $\tau b = C \cdot u1/m$ in the inversion, since effective pressure can be incorporated into the constant C for the Weertman sliding law. Previous work shows that in this case $C \propto u{-}1/m$, and the recovered fields of basal drag are within a few percent of each other (Minchew et al., 2016). The basal drag and basal velocities from the the linear sliding law to initiate the subglacial hydrology model are therefore self consistent with the subsequent inversion results of 15 the Weertman sliding law. The pattern of basal drag inverted using the Schoof sliding law however, shows both higher spatial variability and a higher magnitude of variability. This is a result of the Schoof sliding law shifting to Coulumb-like behaviour at low effective pressures.”~~

“The pattern of basal drag inverted using the three different sliding laws show limited differences. This is due to the fact that basal shear traction must satisfy the global stress balance \citep{Joughin2004, Minchew2016}. The basal drag and basal velocities from the linear sliding law used to initiate the subglacial hydrology model are therefore self consistent with the subsequent inversion results of the two non-linear sliding laws. For the winter effective pressures predicted by the subglacial hydrology model, we find that the Schoof sliding law is in the viscous drag regime. For a representative basal velocity of 75 \unit{m yr^{-1}}, the transition to Coulomb friction occurs at effective pressures of approximately 0.7 MPa. This is below modelled effective pressures, which are above 1.3MPa for most of the study domain.”

It would be nice to try and start with the µa and µb from equations 4 and 5 and see if you indeed get the same sliding ratio for all 3 sliding laws.

We hope that our discussion above illustrates that while this can be done on a technical level, the approach we propose is more general.

2 Specific comments

- **p3 equations 3, 4 and 5: I think you are missing a minus sign for all these equa- tions (basal drag opposes motion)**

  This form appears commonly in the literature [e.g Hewitt (2013)]. We subtract basal drag in the momentum equations.

- **p3 eq 5: use \left(\right) rather than simple parentheses.**

  Fixed

- **p3 l20: I would rather call this equation a Budd sliding law since he is the one**

  who introduced effective pressure in basal stress.

  Updated throughout.

- **p3 l24: you should take the norm of τb here (not the vector) since you are com- paring to a scalar**

  Fixed

- **p4 eq 6: minus sign missing here two?**

  See note above

- **p4 l8: maybe mention "outward pointing"**

  Included

- **p6 l27: "is the control parameter." (period missing)**

  Fixed

- **p7 l11: exp (ζ (x, y)). (parenthesis missing)**

  Fixed

- **p8 l4: with respect to the initial input**

  Updated

- **p8 eq 26 and 27: I think you should use capital Bi at the numerator since your are deriving the function, not its output. Equation 26 should therefore be**

  $$\delta J = \left(\prod_{i=N}^{1} \frac{\partial B_i}{\partial b_{i-1}} \right) \frac{\partial B_0}{\partial \phi_i} \delta \phi_i$$

  updated equations

- **p8 l27: to generate a *derivative*?**

  Fixed

- **p8 l29: derivative?**

  Fixed

- **p9 l2: gradient of *a* function**

  Fixed

- **p9 l6: forward accumulation AD tool: I think this method is generally referred to**

  as “Object Overloading”

  Forward accumulation refers to the general idea/concept, while “Object Overloading” refers to a specific method of implementing forward accumulation.

- **p12 l9: 500 m (space missing)**

  Fixed

- **p13 l2: An L-curve analysis**

  Fixed

- **p15 figure 14: I think what matters is not so much that the sliding laws 2 and 3 are non linear, what is important here is that they depend on the effective pressure, so I would replace the third box to “Inversion: effective pressure-dependent sliding Law”.**

This is a good point. We have removed the flow chart in response to comments from Reviewer 2, but have incorporated this comment in a manuscript in preparation.

- **p16 l2: 500 m (space missing)**

  Fixed

- **p17 l1: the the**

  Fixed

- **pp17 l15: maybe mention water sheet thickness?**

  Updated to “The maximum distributed system sheet thickness”

- **p17 l17: is it really mPa or MPa?**

  Mpa, fixed

- **p18 l10: Figure 14 and 17 (no parentheses needed)**

  Fixed

- **p22 l2: will account for some of the effects, which would (comma missing)**

  Fixed

- **p23 l1: hydrology runs are reflective**

  Fixed

- **p28 l6: we would like to thank M. ...**

  Fixed