We are pleased that reviewer thought this was a worthwhile study, and appreciate the constructive comments. 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.

General comments:

This study is an investigation of hydrologically-forced ice-flow model initialization using multiple inversions for basal drag. It explores three commonly-used sliding-law formulations in attempting to initialize seasonal runs with an end-of-winter hydro-mechanical state. The general scientific question addressed is worthwhile for all the usual reasons of improving model fidelity to observations and the need for practical and sensible means of incorporating the effects of basal hydrology on ice-sheet dynamics. The paper is clearly written.

The paper appears to report on part of a PhD thesis that seems a fulsome combination of model development, numerical implementation and glaciological application. Presumably for this reason, the paper has excessive detail in some places (particularly where the model development appears to mimic previous work) and omission of detail elsewhere where it would be warranted. The paper could also make better use of space with many of the figures. A related consequence of the paper's origin is that it skates over the scientific justification for the development of a new ice-flow model that seems to implement what is already in the literature. One can imagine the reason for this: the author(s) coded this part of the model from scratch, but used existing code for the coupled hydrology. This is an excellent experience for a PhD student, but now the task of the authors is to justify to the scientific community why the world needs another ice-flow model, and this one in particular.

One of the main results of the paper is that using a Coulomb-friction-type sliding law, with a modelled distribution of effective pressure, yields a markedly different distribution of basal drag (and therefore sliding rate) than using a linear sliding law. This result is closely tied to the behavior of the hydrology model, and presumably to the parameters used in the sliding law. The differences are explained in terms of the non-linearity of the sliding law and its sensitivity to effective pressure, as well as the continuum nature of the hydrology model. The dependence of this result on model details warrants more emphasis on the parameters chosen for the hydrology model and Coulomb-friction sliding law, as well as the behavior of the latter.

This is a worthwhile study and I hope the comments below serve to improve the final paper.

Introduction of a new ice-flow model:

It appears that this depth-integrated model closely follows the work summarized in two sources (Goldberg, 2011; Arthern et al., 2013), with the novelty that the new model allows periodic boundary conditions (related to the ISMIP-HOM experiments). The authors even acknowledge that their model is more limited in some ways due to soft- ware (bottom of pg 6). Are there other departures from the two sources that could be highlighted as new innovations? How does this formulation differ or improve upon the coupled (also depth-integrated, if I recall) model of Hewitt (2013), whose hydrology model is employed in this study?

For the problem presented in this paper (a single season and a single catchment), one might legitimately ask why it wouldn't be better to simply use an existing code like Elmer/Ice, which includes a built-in inversion for basal friction and may well also include the hydrology model of Werder et al (2013):

http://elmerice.elmerfem.org/wiki/lib/exe/fetch.php?media=courses:elmerice_2015_friction.pdf

The numerics of the ice sheet model are not particularly novel. The model differs from that of Hewitt (2013) in that the momentum equations are written in terms of depth integrated velocities rather than basal velocities.

There are a variety of reasons to write a new ice sheet model. The reviewer has identified a primary motivation, which is to understand the numerics/equations. For this study, the ice sheet model/inversion code needed to include sliding laws not implemented in existing models and be coupled with the subglacial hydrology model of Hewitt (2013). At the beginning of

the project, we were uncertain what this would entail in terms of links to any existing ice sheet model code, and pragmatically, writing the code from scratch ensured familiarity and flexibility of the code. Additionally, we have limited experience coding in low-level languages. Writing our own code allowed us to rapidly test model ideas/configurations, which could perhaps later be integrated in to more stable and full featured project such as ISSM / CISM / Elmer/ice. We haven't seen any publications from Elmer/Ice in regards to coupled modelling. Even so, subglacial hydrology model is in its early stage, and a diversity of models/approaches is beneficial to the community.

Imbalanced detail:

The basic governing equations, simplifications, boundary conditions and sliding-law formulations given on pp 3-5 are needed, but section 2.1.2 (Implementation) could be condensed, as it seems to closely follow Arthern et al (2015). Section 2.2 (Inversion) is long and detailed, particularly considering that it seems to closely follow Goldberg and Heimbach (2013). For example, the information in the text on pg 7, lines 1-18, is pretty standard fare for inversions, so could be shortened. Section 2.2.2 is detailed and didactic; is the discussion of the TLM necessary? It is nice to have a brief description of the adjoint model, but I expected most readers would be somewhat familiar with these methods already.

On the other hand, the hydrology model is fundamental to this study but is only briefly described (p 10, lines 2-10). The hydrology model seems as important as the numer- ical details of the ice-sheet model. Consider presenting the key governing equations here. Although the equations are currently absent, the hydrology model includes parameters whose values must play an important role in the results (p. 13, lines 7-9). It would be worthwhile reporting values for the cavity step height, the effective hydraulic conductivity/permeability and the incipient channel-width length scale, along with any other parameter settings that differ from Hewitt (2013) and Banwell et al (2016).

Further, the results and discussion would be more accessible if the reader knew a bit more about what went on with the hydrology model behind the scenes. For example, see p. 22, lines 4-6.

As the reviewer noted, there is a strong focus on the ice sheet model/inversion code details as this introduces a new code. However, the hydrology model is an 'off-the shelf' component.

We avoid detailed treatment of the hydrology model for a few reasons. The main innovation in this study from our perspective is the workflow, which is independent of the subglacial hydrology model. In this study, we use the model from Hewitt (2013) to generate the winter field of effective pressure. The parameters used are those we need to initiate summer runs based on calibration/validation of the integrated model using summer velocities. The winter results are an outcome of the summer calibration. Because the continuum model would not capture the fragmentation (as noted by the reviewer), the sheet thickness doesn't correspond to radar data, and due to a general lack of data on subglacial conditions, we don't scrutinize the winter hydrology output closely. Rather, the field of effective pressure is a-priori guess – plausible -- but not definitive for the above reasons.

This depends on your background, but our impression is that there is less familiarity with inversion procedures than subglacial hydrology in the community, as the latter lends itself to a conceptual understanding. From personal experience, understanding the methods behind inversions is involved. While the text was necessary for a thesis, our hope is that the in-depth writing could be useful for those without a background in adjoints. The crux in significantly cutting out sections would be that the paper remains verbose for experts, but not detailed enough for everyone else.

Overall, for reasons of length we think that only one of the models can be described in detail. There are good reasons for focusing on the ice sheet model/inversion code, although we acknowledge that some readers would benefit from the alternative choice suggested by this reviewer. Since reviewer one noted that our approach was accessible for readers without a background in inversions, we've decided to only make the minor changes suggested by reviewer one rather than rewrite the section.

Consider moving the two blocks of pseudocode into an Appendix. Likewise, the flow chart in Figure 4 could be omitted. There is a fair bit of blank space and redundancy in some of the figures. Here are some suggestions for a more efficient and impactful presentation:

Moved pseudocode to appendix, jettisoned flow chart.

- Combine Figs 2 and 3 (unlabeled E, N coordinate values can be removed from axis tick labels, as long as there is a scale bar) - Omit Figs 7a, 13a, 16a, or make them small insets in the corresponding b panels. - Omit or move to an appendix Fig 6. Nice to know how convergence occurs, but not necessary to show as a figure. - Combine Figs 8, 14, 17 into a single figure with 9 panels. This facilitates comparison. - Combine Figs 5, 12, 15. Could be done in a single panel figure. - Omit Fig 10. So much white space that could be replaced by a sentence. If it must be retained, consider a log plot.

We have:

A) Combined Figs 2 and 3.

B) Omitted figs 7a, 13a, 16a. Combined 7b, 13b, 16b into one figure with three panels.

Removed the text:

Page 17, Lines 1-6

"~~The histogram of the the difference between observed and modelled velocities (Figure 7) has a maximum in the lowest bin, with a rapid decrease into a long tail. The maximum difference is approximately 165 myr−1. The difference between modelled and observed velocities is less than 10 myr−1 for 88% of the cells in the study area, and less than 20 myr−1 for 96% of the cells in the study area.~~"

Page 18, Lines 6-9

"~~The histogram of the absolute difference between observed and modelled surface velocities for both non-linear sliding laws shows a similar distribution to the linear sliding law inversion (Figure 13 and 16). The Weertman sliding law results in a spatial distribution of misfit similar to the linear sliding law, while spatial distribution of error from the Schoof sliding law shows higher frequency variations. Model mismatch again is highest in the vicinity of the nunatak.~~ "

C) Omitted the plot of convergence.

Removed the text:

Page 17, Line 1

"~~The inversion converges in 46 iterations (Figure 6).~~"

D) Combined plots 8, 14, and 17

E) We have put figs 5,12, and 15 into a single plot with 3 panels. Overlaying them onto one panel obscured the scaling factors.

F) Omitted Figure 10

Removed the text:

Page 17, Line 13:

"~~Relative to discharge at the base, changes in effective pressures have a much lower magnitude.~~"

In addition, we have updated the colour palette in in some figures for consistency, removed the axis tick labels except in

Figure 1, and added a scale bar to figures which were lacking them.

Specific comments (page.line):

1.8: "a recent subglacial hydrology model" This sounds like it must be a different model than is used in the paper, but by the end it is clear that the model is that of Hewitt (2013). Please reword to clarify.

Apologies for the confusion, we hope this rewording of the last sentence eliminates the issue:

Additionally, we compare the modelled winter hydrological state to radar observations, and find that it is in line with summer rather than winter observations.

3.16 "Ab is the creep parameter set to an appropriate value for basal ice". One should explain why the flow-law rate factor should be different by an order of magnitude (see Table 1) for basal ice, particularly in light of the differences in the results between the Coulomb-friction sliding law (which uses Ab) and the other two sliding laws.

Amended as:

"Ab is the ice creep parameter *used for basal ice. It set an order of magnitude lower than A to account for warmer ice at the base (following Hewitt (2013)). The value of Ab is used in both the Schoof sliding law, and in the subglacial hydrology model for determining creep closure of channels and cavities. The value of A is used in the momentum equations.*"

12.14: "the magnitude of the change is relatively limited" Pretty vague. Can this be quantified?

The scale in the figure of Colgan et al. (2012) is quite coarse, while the data in Van de Wal et al. (2015) are averaged over several seasons. However, from the latter paper we make a conservative estimate of <25%. The text is changed as follows:

*"*the magnitude of the change is *limited (<25%)."*

16.15: Why choose \gamma_2 = 10^{-12} rather than \gamma_2 = 10^{-11} in this type of trade-off curve?

The L-curve is a method for selecting the trade-off between model fit and regularization. Although a common selection is the point of maximum curvature, this choice is subjective, and often results in over smoothing: (*https://www.sintef.no/globalassets/project/evitameeting/2005/lcurve.pdf*)

Our selection of \gamma_2 = 10^{-12} is subjective, although Morlighem (2013) selected a similar point on the L-curve.

Morlighem, M., et al. "Inversion of basal friction in Antarctica using exact and incomplete adjoints of a higher‐order model." *Journal of Geophysical Research: Earth Surface* 118.3 (2013): 1746-1753.

17.15: "bed roughness scale of 0.5 m" refers to lambda_b?

We were not clear with our terminology here. The 0.5 m refers to the cavity height scale used in the subglacial hydrology model, which controls the maximum sheet thickness. We chose omit this detail and to simplify this sentence so it reads:

"The maximum distributed system sheet thickness is 0.36 \unit{m}."

21.Fig 11b: Consider plotting pw/(rho_ice g h_ice) rather than (or in addition to) N, as N does not immediately reveal how close the bed is to flotation.

We think N is the appropriate field to plot as this is what we use in our sliding law. Since we don't discuss the hydrology model output in detail, plotting pw/(rho_ice g h_ice) isn't within the intended scope of the paper.

21.8-9: It seems intuitive that there would be a contribution from deformation, so what is going wrong in the simulations/inversions to produce a better match of observed and modelled surface velocities when the sliding ratio (assuming that means Ub/Us) approaches one (i.e. plug flow)? Is it entirely explained by the assumption of uniform A? It seems that A_b would be playing a key role here, as mentioned in lines 6-7. A_b influences the sliding speed in the Coulomb friction law, but the value of the flow-law coefficient that regulates creep closure is probably A in the model formulation (or is it A_b?).

Sliding ratio is Ub/Us. We fix figure 8C which had Us/Ub, and clarify this on page 14 with:
"and the sliding ratio ($\frac{U_b}{U_s}$) for the linear sliding law"

We use the value of A for the momentum equations in the ice flow model. We use A_b in the subglacial hydrology model, and the Schoof sliding law. (This has been clarified in the text in response to a previous question)

This issue is independent of the subglacial hydrology simulation, since the linear sliding law inversion does not depend on the hydrology. However, we're uncertain why stiffer ice leads to better fits in our inversions. We suspect that the topology of the minimization problem is more complex with a higher A (due to increasing possibility of tradeoff between basal sliding and internal deformation), leading to the inversion finding a local minimums.

22.16-17: It would be compelling here if the authors could help the reader identify the effective pressures at which the behavioral transition in the sliding law occurs and relate them to the effective pressures shown in Fig 11b.

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 τb = C · 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 ∝ 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."

23. The conclusion that the modelled hydrology more resembles summer than winter conditions is not incorrect, but not especially meaningful. If the fragmentation of the drainage system is, in reality, what permits water storage in areas of high topography, then of course a continuum model fails to capture this effect. The authors acknowledge as much, but it diminishes the value of presenting this as a finding or conclusion of the study (as reported in the Abstract).

We feel it doesn't detract from the abstract, and showing this empirically makes it worth reporting. It may be a less relevant result for those with a strong theoretical background.

Technical corrections/queries (page.line):

1.13-14 "evolution of THE subglacial system"

Fixed

1.16-17: "result IN faster flow"

Fixed

2.10: AND missing

Fixed

2.13: "one" should be "some"

Fixed

5.1 "integrating" => "integrated"

Fixed

5.12: Looks like u_b should be \bar{u} in Eqn (17)

Fixed

8.4: "with respect to in the" too many words

Removed 'in'

14.3: "show" => "shown"

Deleted.

17.2: "the the"

Deleted.

19.Fig8c: Us/Ub? Sliding ratio sounds like it should be Ub/Us or Ub/Udef.

Fixed

25.5 "model output of the model"

Fixed