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# *Interactive comment on* "Incorporating modelled subglacial hydrology into inversions for basal drag" *by* Conrad P. Koziol and Neil Arnold

## Anonymous Referee #2

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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

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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 software (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

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 numerical 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.

Space:

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:

- 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.

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.

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.

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

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

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

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.

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?).

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.

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).

Technical corrections/queries (page.line):

1.13-14 "evolution of THE subglacial system" 1.16-17: "result IN faster flow" 2.10: AND missing 2.13: "one" should be "some" 5.1 "integrating" => "integrated" 5.12: Looks like u_b should be \bar{u} in Eqn (17) 8.4: "with respect to in the" too many words 14.3: "show" => "shown" 17.2: "the the" 19.Fig8c: Us/Ub? Sliding ratio sounds like it should be Ub/Us or Ub/Udef. 25.5 "model output of the model"

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