Review of "GlaDS-2: modeling subglacial drainage including ice uplift and free-surface flow in two dimensions"

Submitted to The Cryosphere By Wells and others.

Overview

This manuscript presents a new subglacial hydrology model: GlaDS-2, which models both inefficient and efficient drainage in two-dimensions through various pressure regimes, including ice uplift, pressurized flow, and free surface flow. The model is similar to that presented by Schoof et al. (2012) and Hewitt et al. (2012), which models efficient and inefficient drainage across different pressure regimes in one-dimension, and similar to the Glacier Drainage System (Werder et al., 2013, GlaDS) model, which models efficient and inefficient drainage of pressurized flow in two-dimensions. The current model is run to steady-state on synthetic topography to showcase the different pressure regimes the model can deal with and compared against the one-dimensional models of Schoof et al. (2012) and Hewitt et al. (2012), and is run on both synthetic topography and North Lake in Greenland to model traveling subglacial blisters from supraglacial lake drainage events. A sensitivity analysis of the englacial void ratio is performed on the synthetic topography lake drainage model.

I really appreciate the contributions made in this manuscript. The inclusion of these different pressure regimes in a two-dimensional model allows for modeling of subglacial features such as subglacial lakes and blisters with a stronger physical basis than past subglacial hydrology models. However, I have questions regarding free surface flow in the channel system, and the applicability of the model to the proglacial landscape, which must be addressed before the manuscript is ready for publication.

General comments

1. Applicability of the model to the proglacial landscape: Throughout the manuscript, the authors claim that the model is applicable to the proglacial zone, and apply the model in regions where there is no ice. The basis for this is that, in the proglacial zone, water is at atmospheric pressure and exhibits free surface flow, which the model can deal with. However, there are many differences between water flow underneath a glacier, and

surface water routing past the glacier's terminus. Of particular concern is the channel system, which opens from melting of the channel walls due to turbulent dissipation of potential energy. If the channel walls are not made of ice, as is the case in the proglacial zone, then the channels cannot increase in size due to melt, and more importantly, water from this melt cannot be added to the hydrologic system. Application of the channel equations in the proglacial zone could create an unintentional water source in the model. Furthermore, it is unclear if the discharge parameterization in the sheet system is applicable in the proglacial zone. There is certainly a physical discrepancy between overland flow and free surface flow through subglacial cavities, where preferable water routing pathways may be blocked by ice. The authors provide no discussion on the applicability of the sheet discharge parameterization and channel equations to the proglacial zone, assuming that free surface flow is the only important physical mechanism describing this landscape.

2. Free surface flow in the channel system: The authors assume that channels have a semi-circular geometry, and are filled to a water level that is dependent on the water pressure during free surface flow. In such a situation, there should be more creep at the top of the channel than at the sides, resulting in broad and narrow channels, which may become pressurized once more, after sufficient creep at the top of the channel brings it back to the water level. When the sheet system is pressurized, this may result in presurrized broad low channels of maximum height R_w. This type of channel evolution would be difficult to simulate and is likely beyond the scope of this study. However, I found it odd that in Fig. (3c), in areas where there was free surface flow in the channel system, the channel size grew out of the y scale of the plot, while the channel water area remained quite small in comparison. This could result in an overestimate of channel size, and if there were to be a sudden influx of water into the system, an enhanced capacity to deal with that water. I think that the manuscript would benefit from a more in depth discussion/investigation of the ramifications of the channel equations when the cavities are pressurized but the channels experience free surface flow. Furthermore, S and S_w in Fig (3) are small to the point that it is difficult to interpret such features as actual channels, especially in the low melt scenario. A scenario with larger channels would be helpful to demonstrate the model is capable of simulating useful channel dynamics.

3. Overselling model capabilities: The manuscript tends to oversell the usefulness of the

model, this is most obvious in the case of promoting the ability to model the proglacial landscape (see general comment 1), for which more justification is required. There are other instances of this as well. When discussing modeled blister dimensions and timescales (lines 489-494), the authors claim that the 8 m modeled blister height is consistent with observed ice uplift (0.7 m-2 m) given that GlaDS-2 is uncoupled to an ice flow model. This claim is made without running a coupled ice flow/hydrology model as demonstration, and without citing any other works that investigate discrepancies between blister height and ice uplift. The authors later discuss that the ice sheet would undergo elastic deformation during supraglacial lake drainage, brought on by an overpressured subglacial system (which is not possible in GlaDS-2). However, on timescales of more than a couple hours, the ice sheet may actually exhibit visco-elastic behavior (Tsai and Rice, 2010), not elastic behavior. Regardless, this missing physics could in part be the explanation for the discrepancy between modeled blister height and observed ice uplift, but is not mentioned in this part of the paper. Lines 527-537 discuss future work regarding model coupling, this is mostly okay, however, the last sentence of the paragraph suggests that continental scale coupling is possible with an appropriate stress balance approximation and uplift parameterization. I find it difficult to believe that the ice flow model would be the computational bottleneck to continental scale coupled modeling with GlaDS-2. GlaDS-2 requires timesteps on the sub-second scale (line 503), which is not feasible for modeling an entire continent for an appreciable amount of time. The authors further suggest that GlaDS-2 provides "the foundation for improving future coupling of subglacial drainage to ice-flow models" because of the bounded effective pressure in the model. While this is not technically false, it is misleading because it is already possible to apply effective pressure bounds before putting the effective pressure into the friction law of the ice flow model, and there is already a lower cap that is applied for a nonzero finite value of N, instead of at $N = 0$ when running a coupled model. A stronger argument would probably take the form of seeing how the effective pressure field differs in pressurized models and in GlaDS-2 in regions near the pressure regime change boundary. Given its ability to handle different pressure regimes, GlaDS-2 opens up the door for improved subglacial hydrology modeling in many situations. The manuscript would benefit from focusing on a discussion of said situations.

Specific comments

line 12: See general comment 1.

lines 14-15: See general comment 3.

lines 34-35: I suggest rephrasing this sentence so that it is clear what the proposed implementations are. At the very minimum I think it should be "the thus far..."

line 38: I understand free-surface flow was explained in the abstract, but it would be helpful to reiterate in the introduction.

lines 41-43: See general comment 3, it is not obvious how much more benefit this would provide over a pressurized model, if pressure regimes can be determined once there is an effective pressure output.

line 42: add some earlier references for effective pressure impacting ice sliding speed, ie. (Iken, 1981; Budd et al., 1979).

lines 45-47: These two papers came before GlaDS, so GlaDS shouldn't be mentioned in this sentence.

line 68: what specific variables require initial conditions and boundary conditions? It might just be better to discuss these once we know what the variables are.

line 75: I think "which was previously solved" should be rephrased. It currently sounds like this was an earlier iteration of the model.

lines 82-84: See general comment 1.

lines 92-93: I think the sentence would flow better if it was reordered to say "Note that our notation is slightly different from that used in the original GlaDS model where only the pressurized case was considered, and h was used to mean h_w here.

Figure 1: Resolution seems a bit low in panel a. The diagram is depicted with a crack leading to the ice surface, in an ice uplift scenario, the water could just flood onto the glacier, the diagrams could be altered to remove the crack at the ice surface.

line 105: Looks like there is a double space between "relation" and "that".

line 157: See general comment 2 regarding free surface flow in the channel system.

lines 177-178: The original GlaDS included this term, and I don't quite see how there would be much more added complexity given that the water produced from melt in the channel system is already added in the equation. Could you elaborate a bit more on the choice to drop this term?

lines 179-180: I'm not quite sure I understand the Dirichlet BC for the marine terminating glacier. Why would the top of the water column be at sea level? Wouldn't that only make sense if there was no ice at the grounding line?

line 189: the double bracket equation is strange formatting, maybe (Eq. 17) is better.

lines 197-198: the PDE in Eq. (17) or just Eq. (17) would be better I think. Same comment for step 4 regarding ODEs.

Figure 3: channel size grows without much water being in the channel, see general comment 2. I understand that the setup was taken from the past papers, but the topography seems strange. The ice appears to be very thin, and the channel water level is small enough that if this were not a model, it would be difficult to think of it as a channel, especially in the low melt scenario.

line 230: It would maybe be helpful to understand "negligible" by giving a Δh value for a representative ice thickness, say 100 m or 1000 m.

Figure 4: some legends are small, c) is difficult to read, and mainly only the solid lines are

visible. The scale is awkward because the blister is so much larger than the typical bedrock bump height which acts as a natural height scale otherwise. The color scheme in b), c) and d), is also not friendly for color blindness.

Figure 5: In a1 and a2 the blister makes a strange c shape, are there any thoughts on this?

line 311: I think (Eq. 4) is better formatting.

lines 340-341: I'm not exactly sure what overflowing the domain boundary means.

Section 4.2: What is used for ice speed input?

lines 396-397: What is meant by a constant sea level water height?

lines 413-414: Borehole drilling sometimes suggests an overpressured system, which is at odds with the cap at overburden pressure assumed in this study, this should be discussed somewhere.

lines 421-422: This is somewhat repetitive with the previous sentence.

lines 448-449: See general comment 1.

lines 453-454: Were steady-state configurations of a depression tested in this manuscript? Or is it just hypothesized based on past results?

lines 492-493: See general comment 3.

lines 495-500: See general comment 1.

lines 535-537: See general comment 3.

lines 555-558: See general comment 3.

References

Budd, W. F., Keage, P. L., and Blundy, N. A. (1979). Empirical studies of ice sliding. *J. Glaciol.*, 23(89):157–170.

Hewitt, I. J., Schoof, C., and Werder, M. A. (2012). Flotation and free surface flow in a model for subglacial drainage. part 2. channel flow. *Journal of fluid mechanics*, 702:157–187.

Iken, A. (1981). The effect of the subglacial water pressure on the sliding velocity of a glacier in an idealized numerical model. *J. Glaciol.*, 27(97):407–421.

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