Review of "SUHMO: an AMR SUbglacial Hydrology MOdel v1.0"
Submitted by Felden et al. to *Geoscientific Model Development*

General comments:

This manuscript documents the development of a new model that simulates the evolution of water flow and pressure beneath glaciers and ice sheets. SUHMO adopts the general equations and continuum approach of the SHAKTI model (Sommers et al., 2018) with modifications to facilitate incorporation of adaptive mesh refinement (AMR), in order to resolve individual subglacial channels.

Overall, the paper is clearly written and well supported with references and figures. I do not find the simulations based on SHMIP tests to be especially helpful as there is no AMR and channel resolution involved in the SUHMO results presented. However, I understand the motivation of the authors to include those results as a sort of benchmark or verification of the approach, and I leave the decision up to them whether or not to retain those.

In general, some aspects of the numerical experiments and results presented in the paper could be more clearly explained. For example, the melt rate in SUHMO assumes no geothermal flux and no frictional heat. These can be large sources of basal melt, particularly frictional heat for fast-moving glaciers, and the decision to neglect these should be explained.

As already commented by the Editor in the online discussion: the model code, input, and output need to be made available in a repository that complies with GMD's guidelines.

Please see specific comments and technical corrections below.

With some revisions, I feel this manuscript should be published. As conveyed by the authors, challenges remain in incorporating the influence of subglacial drainage into ice dynamics models, and this AMR approach is a promising step to evaluate the role of individual drainage features and better understand the spatial resolution necessary to capture relevant subglacial pressure for large-scale ice sheet simulations.

Specific comments:

Line 11: Consider strengthening this last sentence by including the main point or points of what you find out about effective pressure as resolution increases (instead of simply saying you discuss it, which is vague).

Lines 72-73: Studies that have used subglacial hydrology modeling on real Greenland glacier domains find widespread areas of channelization (for example, Cook et al., 2020). In that light, I'm not sure that channels are only expected to occur in very localized areas – each channel is

distinct, yes, but with AMR you would need to have many areas of refinement that could cover a large area of a glacier bed. You may want to consider re-wording this sentence.

Line 73: You could define AMR here, in the first instance of using it.

Lines 74-75: Suggested re-phrasing: "We follow the approach of Sommers et al. (2018), with adaptations to implement a subglacial hydrology model using the Chombo AMR framework (Adams et al., 2001-2021)."

Line 75: Consider briefly describing what Chombo is here.

Line 102: What value of omega is used?

Line 118: The term in the melt rate that accounts for changes in the pressure melting point with changes in water pressure as written here is not negligible in places with steep topography, for example (see Creyts and Clarke, 2010 about supercooling). There are good reasons to drop this term, however, that have to do underlying assumptions about the ice and water pressure being equal at the interface.

Equation (6): Why introduce the general b' here and not in Eqn. (1)? I would recommend to either present a more thorough treatment of partially filled drainage, or just go with the assumption from the start that the gap being ice and bed is always filled with water.

Line 125: This method of using beta for the opening by sliding is based on GlaDS (Werder et al., 2013).

Line 130: Please describe the "creep length scale" and the physical justification for Equation (7). What qualitative behavior is captured with this equation? Why does creep shut off below a threshold? (Hint: it may have something to do with ice being supported by asperities on the bed). GlaDS and SHAKTI use the gap height (b) as this length scale. Why is that, and why should it be improved upon?

Line 131: Again, see comment above about b'.

Table 1: tau_b and G are both listed as 0. You should make clear in your results that the melt rate does not include contributions due to geothermal flux or frictional heat from sliding. Why?

Table 1: The description for omega is not very informative. You may want to include a brief description near Equation (2) about what this parameter is and how it functions to make the laminar-turbulent transition.

Table 1: Why did you choose this value of A that corresponds to very cold ice? The equations described assume that the ice and water are both at the pressure-melting-point temperature, so a larger flow law parameter would be more appropriate.

Line 151: The diffusion-like term is basically a diffusion of gap height. It may be worth explaining in more detail what this represents physically.

Equation (10): I would not consider the second term in the parentheses (the pressure-melting term) to be dissipation. I suggest naming it separately.

Eqn. (16): What is rho_c?

Line 211: If only one Picard iteration is used, what's the point?

Equation (17) and Lines 214-216: I recommend using a different letter to indicate time in the superscript to avoid confusion with the flow law exponent n.

Line 231 and Eqn. (19): Here too, the use of little n as n_{tot} could be confusing.

Line 237: The domain (64x16 m) used for the channelizing test case is small. What about boundary effects?

Line 239 – 30 m3/s is pretty high flow for a moulin!

Line 240: Neumann (flux) boundary condition at the outlet and Dirichlet (value) boundary condition upstream? I think this is probably a typo and should be switched. Typically, the Dirichlet condition would be at the outlet to set head equal to bed elevation (for example, equivalent to atmospheric pressure). And the no-flux Neumann condition makes sense for the upstream condition based on your results.

Line 242: I was confused on first reading this, thinking that the Gaussian was referring to a time-varying input rate, whereas it was already stated that the input rate is 30 m3/s. It would be helpful to include wording that makes clear that the Gaussian distribution refers to the spatial footprint of the point source at the bed.

Line 248: It may be worth commenting here about why this type of super-narrow, tall opening should not be interpreted as a physically realistic basal crevasse.

Line 302: What do you mean by the effective system? Is this a typo for "efficient", or some other intended meaning?

Lines 306-307: I thought the moulin input is specified as constant. It is not clear what is referred to as Gaussian here.

Figure 5b: How high does the discharge calculated by SHAKTI go in case A1? (The vertical axis does not extend sufficiently). This seems like an error somewhere (quite possibly in the SHAKTI results submitted to SHMIP).

Lines 337-343: 5180 m3/s into 63 moulins = 82 m3/s into each moulin. This seems very high and could benefit from better justification or explanation of why you choose to use extreme input. What happens with more realistic moulin inputs (not as high)? Do you produce channelization?

Figure 9 – suggest moving the x,y,z coordinate in panel a to the right lower side so that it is oriented where x=y=0 (the red dot locator helps orient, but I found it confusing nonetheless). "Total height" could be called "surface elevation" instead.
Figure 9 – panel b: is moulin input in m/s or m3/s?

Line 351: What specific criteria for gap height and melt rate were used to determine regridding?

Line 370: Great! This is a big deal and will help make the case for AMR. It would be helpful to include a number here of what the minimum resolution is that you find to resolve channelization.

Line 383: I am not sure that it is correct to say that melting of ice walls was discarded in the original derivation of the SHAKTI equations. The equations treat gap height as a one-dimensional quantity, so two-dimensional cross-sectional area of a channel was never part of that derivation.

Conclusions section: Make a point of quantifying the minimum mesh resolution that you find necessary for resolving channelized features somewhere in here as a main take-away point.

Lines 391-395: This paragraph is arguably true, but I suggest rewording to strengthen the case for resolving channels in SUHMO. Describe why this may be necessary and helpful in ice-sheet modeling. (The way it currently reads make it sound like coupling with BISICLES is motivated by making sure individual channels don't matter – but we don't know yet if that's the case until you do it).

Technical corrections:

Line 3: comma before "however"

Line 71: typo in "subglacial"

Line 82: convergence analyses... *are* presented (or convergence *analysis*... is presented)

Line 124: should be kg m^{-3} (missing a negative sign in the exponent)

Line 162: Insert word "accuracy" after algorithm

Line 173: missing space after "of"

Figure 1. Should the schematics on the bottom left and right have delta x_0 (instead of delta x) in the grid level labels for consistency?

Line 276: Period after Figs.

Line 293: Extra) after 2a

Line 308: Extra)

Figure 9: The orientation of the two panels being different is spatially confusing. Is it possible to orient them so the red dot location is more consistent between perspectives? For example, rotate panel b 90 degrees counter-clockwise. Similarly for Figure 10b.

Line 366: Missing period after Fig.

Line 366: Either includ gap height b, or remove N

Line 373: Using lower-case n is again slightly confusing here.