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Geoscientific Model Development Discussions

Interactive comment on "SHaKTI: Subglacial Hydrology and Kinetic Transient Interactions v1.0" *by* Aleah Sommers et al.

Anonymous Referee #1

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The manuscript by Sommers et al. describes a subglacial hydrologic model which permits the representation of a range of turbulence regimes. The implementation of a spatially and temporally varying hydraulic transmissivity removes the need for individual representation of 'channelized' and 'distributed' model elements and allows for the inclusion of viscous dissipation across the model domain. This advance, modified from studies on water flow in rock fractures, is new to subglacial hydrologic models and could potentially represent the continuum of subglacial characteristics within a single model.

Overall, the relatively brief manuscript clearly lays out the model components and provides several idealized model experiments to characterize the system. The modeling results demonstrate that the implementation of this variable transmissivity results in

spatially and temporally varying subglacial pressures that qualitatively reproduce the expected seasonal evolution of the subglacial system. However, because the channelized component is represented on the model grid (not on grid edges as in previous modeling efforts), there is an unfortunate dependence of model output on the grid scale.

While this manuscript has strong points and is a novel extension of previous work, the key points of the model development effort are not strongly represented in the text. In particular, the text of the manuscript should better reflect both previous development of subglacial models and an improved description of the theoretical underpinnings. Details about this issue are indicated in the general comments and line comments below.

General comments

1. The introduction should be more clearly focused (and quite possibly expanded) on the topic of subglacial hydrology. There is a fairly extensive body of literature about subglacial model development, including extensive work on alpine glaciers. The focus on outlet glaciers and sea level rise in the introduction is somewhat of an aside.

2. The motivation of the manuscript is somewhat unclear if the reader is un-indoctrinated into the world of subglacial hydrology. It would be useful to include a through description of viscous dissipation and why it hasn't been included in previous subglacial models in section 1.2 and clearly describe - before the model description - the goals of this modeling effort.

3. The basal flux parameterization (Line 25) needs to be more carefully documented. There are several line notes to this effect, but essentially, the addition of the Reynolds number requires the selection of characteristic length scales and dimensionless parameters - reasoning behind how these values are assigned should be included in order to enhance the usefulness of this manuscript.

4. There should be a 'model limitations' section that includes a more through discussion of grid dimension sensitivity and perhaps a narrow sensitivity study on other parameters such as gap height initialization. In this section it might also be beneficial to include some discussion about how to determine the most appropriate grid scale - i.e. is there an ideal scale depending on ice thickness, roughness, coupling with an ice dynamics model, etc.

5. The citations/references should be checked. There are a few citations that are not in the references section and vice versa.

Specific comments Page 1 Line 1. I am not sure "poorly understood" is the best phrase to use here. There is an extensive body of literature exploring the state and evolution of the subglacial hydrologic system and its representation in current models, while not perfect, are able to replicate many features of ice velocity fields. We know that the link between melt and ice motion is the subglacial system; however, there are parameters and parameterizations which are not well constrained.

1-2. The wording of this sentence is awkward.

9. Much of the manuscript switches between 'channel' and 'efficient' drainage. Consider using something like '. . .over a wide range of drainage efficiencies. . .' or inefficient and efficient drainage to eliminate the "channel".

15-22. While understanding ice sheet dynamics is important for the characterization of future sea level rise, it might be more correct to acknowledge that basal lubrication alone may not be a major uncertainty in sea level rise predictions (e.g., IPCC, sea level change chapter, pages 1168-1169; Shannon et al., 2013).

21-22. Consider citing the chapter instead of the whole 'Physical Basis' document.

Page 2 7. There are a number of other publications that could be cited along with Cowton et al. (2013), including Bartholomew et al. (2010), Chandler et al. (2013), and Andrews et al. (2014).

8-9. This sentence seems out of place and doesn't provide much information.

12-13. It seems that a description of the unknowns would logically follow this sentence instead of a description of a description of how the subglacial system works. It might be useful to remove references to unknowns - while this is certainly true - the main purpose of the manuscript is to rectify a persistent known problem - that models of subglacial hydrology tend to only represent 2 endmembers of the continuum of possible configurations.

23-26. Reference the section numbers

27. This section would benefit from explaining the motivation for subglacial hydrology model development as well. See Flowers (2015) for a great review of the topic.

Page 3 3-4. This relates to the previous comment - it would be nice to detangle why the community ended up focusing on these two endmembers.

13. See comments regarding line p2L12-13 and p1L1.

34 - p4L5. These sentences start to feel rushed. Also consider including more recent work by Rada and Schoof (2018) and Downs et al. (2018).

Page 4 7-8. This sentence is a direct repeat of a sentence in the abstract. Consider revising.

13-15. The instability that arises with the viscous dissipation has been discussed by a number of studies (Hewitt et al., 2012; Hoffman and Price, 2014; Kamb, 1987; Schoof, 2010; Schoof et al., 2012; Walder, 1986; Werder et al., 2013). In addition, Flowers (2015) has a nice summary of the reasoning behind and numerical approaches to switching between drainage elements. Because the primary contribution of this work is the inclusion of the viscous dissipation term and the representation of both turbulent and laminar flow, it is important to thoroughly discuss the reasoning and justification and numerics used in previous modeling work. This summary could readily follow lines 13-15.

18. If the isolated/weakly connected system is the primary scientific motivation behind the inclusion of viscous dissipation, then it would be beneficial to expand upon this topic (and include the body of work from alpine glaciers) (e.g., Andrews et al., 2014; Gordon et al., 1998; Hodge, 1979; Murray and Clarke, 1995), perhaps in a separate section or paragraph. However, the manuscript should also note that a through modeling effort to explore this future work.

Page 5 4-5. It would be useful to expand on the representation of channels in this model compared to other models because they are very different - previous models represent channels along element edges (e.g., Hewitt, 2013; Schoof, 2010; Werder et al., 2013).

5-6. Is two-way coupling implemented between ShaKTI and ISSM?

21-22. Clearly define that β is a function of bedrock bump height and spacing and that it goes to zero when the gap height exceeds the bedrock bump height. This is essentially the delineation between 'cavity type' opening and 'channel type' opening and shouldn't be relegated to the Tables alone.

26. What does ω represent, more than simply the 'Parameter controlling nonlinear transition between laminar and turbulent flow'? In order to be useful to readers, some information about how it is chosen needs to be provided.

26. What is the characteristic length scale used in the calculation of the Reynolds number? This length scale should be associated with bedrock bump spacing and the gap height though some sort of hydraulic radius. How this is characterized and justification should be discussed. In this vein, q in Table 1 should probably have an equation associated with it.

Page 6 10-29. It would be useful to mention that the internal dissipation term in Equation 10 is not included in the Werder et al. (2013) formulation and perhaps nod to previous discussion of the inclusion (or lack thereof) of this term in previous modeling

efforts (see comment on p4L13-14).

Page 7 3. Awkward phrasing.

13-15. The over/under pressure problem is complex (Hewitt et al. (2012) and Schoof et al. (2012) only solve it in one dimension). It may be best to temper this statement and simply explain why subglacial pressures are constrained and how the forces are balanced.

15. Extra ';'

Page 8 19-20. The grid scale and the duration of the model run should be mentioned.

30-31. Rather than stating that the head and gap heights show a clear channelization structure, why not plot the 'degree of channelization'? This will remove any ambiguity.

Page 9 1. consider using the term 'arborescent'.

5-8. This should be moved and expanded into a model limitations section and the supplementary figure should move to the main text.

7-8. Quite similar might be an overstatement, particularly because differences in the vicinity of channels is +30 meters - which is $\sim$10% of the total ice thickness and $\sim$50% of the total diurnal head variation measured by Andrews et al. (2014).

23-24. What low distributed input value? Does the choice of initial subglacial gap height affect the spin up time?

28. Though the meltwater input during the winter is low, it really isn't realistic. Is there a model stability reason for having winter meltwater input?

Page 10 6-8. These sentences imply that the model is fully coupled with ISSM. Unless this is the case, consider adding citations to delineate that the described ice velocity behavior is what would be expected in the coupled model, or rephrase the sentences.

16-18. The last sentence in this paragraph is a bit out of place.

Page 11 7-9. This sentence should have a citation to minimize confusion between the model results here and the link between ice velocity and the subglacial system.

9-11. It makes sense to try and relate this work to observational work on outlet glaciers since those are the glaciers most likely to impact sea level rise, but the boundary conditions and the model domain presented here are more realistic for land-terminating regions of the ice sheet.

15-20. This paragraph should be expanded into a 'model limitations' section. It would also be nice to see some discussion of an ideal length scale. I imagine that when coupled with an ice dynamical model, there will be some grid size after which, a finer mesh won't improve modeling results due to modeled ice characteristics.

Figures Figure 2. It would useful to see the 'degree of channelization'. Also consider using a non-linear color scale for gap height and flux.

Figure 3. Can the gap height panels be plotted on the same scale? It would also be useful to see the 'degree of channelization'

Figure 4. 'Box on' for panels b and c. Panel labels are also needed. Consider adding 'degree of channelization'. Instead of using the log of gap height, consider just using a nonlinear color bar (for this and all other figures).

SI figure. This figure should move to the main text and include difference plots of channelization and possibly gap height.

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