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

Anonymous Referee #2

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Summary

In the context of the current proliferation of subglacial hydrology models that largely replicate one another or make incremental changes to existing models, this paper outlines a contribution that seems a potentially worthy advancement. The model formulation is uniform across the domain and permits all the physical processes normally invoked within the spectrum of "distributed" to "channelized" drainage, with a smooth transition between laminar and turbulent flow regimes. This formulation is conceptually simple and allows fast and slow drainage regimes to evolve in an organic and intuitive fashion. The authors have incorporated the model into an established ice-flow model, making it in principal readily available to the wider community.

General comments

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Discussion paper

As elaborated below, I think the description of this new model has the potential to make a strong contribution to GMD if the authors consider the following revisions (roughly in order of importance):

(1) Adding technical model detail commensurate with (assumed) expectations for a journal focused on model development, including a more thorough elaboration of model boundary conditions, implementation and numerics; (2) Amplifying the description of the conceptual model and more thoroughly justifying the choices made in model formulation; (3) Reporting on the results of basic model testing: model convergence, consistency, efficiency, grid refinement (done to an extent already), etc. and presenting quantitative evidence of model performance (e.g. runtimes); (4) Addressing issues that plague many models of subglacial hydrology and being up-front about the shortcomings of the current model (or better showcasing the successes). Examples of these issues are: (a) low winter water pressures in contrast to observations, (b) englacial storage motivated by numerical need, (c) extreme sensitivity to initial and boundary conditions, (d) maintaining saturated conditions, (e) water-mass conservation when pressures are capped at overburden (f) convergence in the presence of substantial bed topography (g) fundamental continuum assumptions and omission of the unconnected bed (h) prescription of constant sliding speed and omission of two-way coupling (5) Streamlining the introductory material and omitting or condensing content that anyone reading this paper with the intention of using the model should already know very well; (6) Dialing back some of the stated advances of SHaKTI over existing models;

I would consider some amount of revision in response to items (1)-(4) to be essential (see details below), and revisions in response to (5) and (6) desireable.

Detailed comments (page.line)

SHaKTI: Not sure exactly what “kinetic transient interactions” are and why this phrase forms an essential part of the model name. “kinetic transient interactions” sounds more like a biochemistry term. It would help the readers if the authors could use the full model name in a sentence to make it clear why this acronym was chosen, aside from its perhaps appealing phonetic similarity to “chakra”.

1.5 “changes the governing physics under different flow regimes” If this were a clunky IF-THEN sort of statement in standard models, then I see the point. Models like those of Hewitt (2013) and Werder et al (2013) have all the governing physics but simply apply the appropriate governing equations to different parts of the model mesh (edges versus cells). Perhaps the point to emphasize here is that SHaKTI, in principal, may capture intermediate flow regimes with the laminar-turbulent transition. One could argue, however, that the other models also do this by having channels and cavity systems operating simultaneously and in spatial proximity, thus together forming intermediate flow regimes.

1.13-14 “supporting the notion that. . .” delete. Too obvious.

1-2. Suggest condensing introduction and omitting textbook-level content, e.g., lines 16-18.

2.28-3.10. Suggest omitting or highly condensing this very basic background material. See Flowers (Proceedings of the Royal Society A, 2015) for a convenient citation to replace much of this content. Ditto for most of page 3.

4.16. I’m not sure most glaciologists would agree that using different governing equations for fundamentally distributed and channelized drainage systems is questionable. Perhaps emphasize the lack of intermediate flow regimes as in the next sentence. Here I think the drawback of existing models are overstated.

4.26. Replace “it is satisfying”

4.28. Not clear how this model allows “high-resolution” exploration in particular.

5.1-8. Please give an overview of the conceptual model here. How is the drainage element envisioned? How does this relate to the fracture-flow formulation of q?

5.10-11. Conservation of water AND ice mass? “basal water flux” => “horizontal water flux”; define “internal melt generation”. Not clear if that would be englacial melt that makes its way to the basal drainage system or something else.

5.12-13. I struggle to see how SHaKTI “can be viewed as an approximation to a multi-dimensional generalization of the governing equations for glacial conduits described by Spring and Hutter (1981) and Clarke (2003).” These references describe only channel physics, not opening by sliding as in cavities. Clarke uses conduit distensibility in the governing equations and accounts for thermal advection, in contrast to SHaKTI. Easiest just to omit this text. I don’t think trying to explain the statement would add much.

5.15. It looks like the theory is developed for fully saturated flow, so this should be stated explicitly.

5.18. “input rate” = “internal melt rate” above?

5.20 (Eqn 2). Better described as “evolution of gap height” than “gap dynamics”? State what these terms are before the end of the paragraph, ideally before the equation. Eqn (2) would appear to allow for creep opening, not just creep closure. Is this intentional? If not, why write the creep term in this way? Is creep opening permitted in the numerical implementation of the model? If so, it should be justified.

5.26 (Eqn 3). State that this is the formulation for fracture flow, or how this formulation came to be adopted. Define Re here as in table, else the laminar-turbulent transition doesn’t make sense. Here the reader really needs to know what the conceptual model is in order to make sense of the flux formulation.

6. This reader is wondering how b and h are going to be related in the model, as the treatment of gap height and water pressure/hydraulic head forms a key difference in various models. Perhaps mention this early on when saturation conditions are noted.

6.6. fracture flow: this is a description of the conceptual model that should appear

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Discussion paper

earlier.

6.7 “Most” => “Many”

6.14-15. “heat consumed due to changes in water pressure” More physically based to explain that it is the heat consumed or released in maintaining the water at the pressure-melting temperature in the presence of changing water pressure.

6.17-18. Good place to cite Clarke (2003) for heat advection and Creyts and Clarke (2010) for supercooling.

6.21. Please state rationale for including englacial storage. Werder et al (2013) do this, but is it needed here for numerical stability?

6.28-30. Expressing K as a tensor here, given that it is assumed isotropic, seems needlessly complicated. An even more compact way to write the first term in Eqn (9) is \nable \dot q.

7. Section 2.2. Boundary conditions are key for model implementation. It would seem to make sense to articulate them mathematically. I think Werder et al (2013) set a nice example of the balance between the mathematical and descriptive exposition of a model, including boundary conditions and method of solution.

7.9. So, negative water pressures are permitted in the model? If so, how big are they? Do they have a significant influence on creep closure?

7.12. How is the Pw=Pi restriction implemented without violating conservation of mass? If it’s not, it would be good to report the amount of mass-conservation violation this restriction imposes.

7.17. “Euler-Backward” => “backward Euler” seems more conventional, unless this means something else.

7.17. Picard iteration. This is a common methodology, but one not known for its speed. Though not mentioned in the manuscript, I surmise that a major advantage of this modeling approach (unified physics applied everywhere) over others could be its efficiency, but perhaps not, depending on the numerical implementation. This reader would be very interested to known if the model formulation had the potential to be fast, and whether the numerical implementation was designed with this in mind. Given the disparity in timescales between ice flow and water flow which typically necessitates comparatively small timesteps for hydrological models, many model users will be looking for hydrological models that do not add unnecessary computational burden to their ice-sheet models.

7.19-20. It sounds like gap height and hydraulic head are not solved simultaneously (or iteratively). Why not? Explicit time-stepping is simple but can lead to large errors. Can the authors reassure the readers that this has been investigated and propose corresponding limits on the time step?

8.1. Is the model convergence sensitive to prescribed initial gap height?

8.7. Curious why closure is not included in “degree of channelization”. If closure balances opening at small gap heights for any opening mechanism, it seems channelization would be supressed.

8.11-12. The software-style description seems a little strange. I guess the key thing here is that the output is ascii, not binary or something else? It seems like output from any model could be visualized in contour plots, timeseries, etc, and in any software.

8.15. Somewhere above this it should be noted that the mesh is irregular.

8.16. “Application”. Here I was expecting to see some multi-faceted demonstration of the model performance (e.g. accuracy, consistency, convergence, efficiency) prior to the demonstration that the model produces qualitatively familiar results in some basic tests. Model performance metrics are not often reported in journal articles focused on model applications, but I expected this would be different in Geoscientific Model Development. The Editor can decide if this suggestion is misguided; it could be misinterpreting the purpose and expectations of the journal.

8-9. Subglacial hydrology models frequently have trouble in the presence of bed topography. The tests presented here omit bed topography with the exception of a gentle slope. It would be useful to know if this model does better than others in the presence of realistic bed topography. It's ok if it doesn't.

9.3. "drainage configuration ... affected by ... bed topography" Except in this test the bed is flat. Is this just a general statement?

9.4 "unstructured mesh" Please mention this when model implementation is described.

9.10. The test domains seem very small. It would be useful to report something on model runtimes. Is it practical to run this model coupled to an iceflow model for a large catchment?

10.3. "do not include storage term" Meaning englacial storage?

10.6-7. Paper should make clear that ub is prescribed and constant, thus there is no two-way coupling with sliding, meaning the negative feedback associated with sliding is absent (Hoffman and Price, 2014?)

10.9. This sounds like a problem that plagues most models (c.f. Downs et al, 2018: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017JF004522), so should be noted as a common shortcoming with a citation or two.

10.21/ "impose potential channel locations" Indeed this is a limitation in some models, but here the "channel" locations are a function of the mesh, just as they are in the models of Hewitt, Schoof and Werder. In the latter case, the channels may lie anywhere along the mesh edge. In this model, they may lie anywhere in the mesh elements. In the models of Hewitt, Schoof, Werder, increases in grid size mean small channels cannot be represented; in the current model, increases in grid size mean channels become unrealistically wide. Both are limitations in different ways.

10.25-26. This seems like a big deal, and a true potential advantage over the other models out there.

11.10-11. “Supports the notion” Too obvious. Suggest deleting.

11.13-14. Arguably the unconnected bed requires additional model physics, but this regime has been parameterized by Hoffman et al (2016) and Downs et al (2018).

11.25 suggest “channels” => “pathways”. Reword “sorts itself out”.

Editorial (page.line)

2.22. “the model…, a model formulation” => “we describe the model formulation of SHaKTI, which allows for…”

5.19 and 5.25. These lines and the text that follows them do not form sentences.

6.9. ditto above

References: more than 15 of the references are incomplete. Authors should check the list thoroughly. “Truffer” is missing an “r”. “et al” is used where it probably shouldn’t be. Sometimes journal titles are written out, sometimes they are not.

Tables: check superscripts. I have great respect for SI, but please give ub in m/a also.

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