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Interactive comment on "BrAHMs V1.0: A fast, physically-based subglacial hydrology model for continental-scale application" *by* Mark Kavanagh and Lev Tarasov

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This paper presents a model for subglacial hydrology suitable for continental scale ice sheets. Here, concerns are different from the glacier scale hydrologic models that have seen significant change in recent years. Time steps are longer (days vs hours) and spatial scale is larger (tens of kilometers vs hundreds of meters). The paper is novel in that it defines a set of physical processes related to channelized and distributed flow that can be efficiently and robustly solved using numerical methods. After describing the model, the authors conduct experiments on an idealized, parabolic ice sheet on a flat bed, and conclude with a sensitivity study conducted on a reconstruction of the

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Discussion paper

North American ice sheet complex during the last glacial maximum (18 and 22 kybp).

The model represents worthwhile contribution to the literature because its lower fidelity physics are well suited to the problem ice sheet reconstruction via simulation of 100 ky glacial cycles. Before it is ready for publications, I see a number of issues for the authors to address. I believe that they are significant enough that I've called them 'major' - mostly to assure that something is done to address them. In short, my primary criticism is that I do not think that the results are reproducible because important aspects of model setup are omitted. I also think the work should be better scoped so that readers understand the distinctions between this model and other, recent works related to subglacial hydrology.

* The simulations, especially those having to do with the LGM (last glacial maximum), have to be described in more detail. Enough is missing that I'm struggling to evaluate the conclusions of the paper. In particular: How are model runs set up, and how does the ISM (ice sheet model) get to the point where hydrology is called? I hoped that citing some of the Tarasov's prior work could be used, but didn't find it. I understand that this is a 'one-way' coupling (ISM forces basal hydro), but that is not sufficient. What are the fields that force the basal hydro model (ice sheet thickness, basal temperature, and basal melt rate?) How is the melt-rate computed? Is melt on the surface of the ice sheet routed to the bed? How is basal traction determined in the absence of two-way coupling? Finally, see my next point on the stability of a nonlinear system. This is perhaps my greatest concern.

* The system of equations includes a number of strong non-linearities in terms of the key prognostic variable - w (effective water depth). Specifically we have

** Flux, Q, depends on w and K, conductivity, which has w dependence ** Water pressure, or the potential surface that water is routed down, depends on w ** A critical flux, dependent upon w, can have a rapid and strong impact on w

What is the interrelation between the time stepping of the ISM and the basal hydrology model? Does the hydrology model achieve steady state between ISM updates? If not, are the larger changes in the potential field on ISM time steps sufficient to produce shocks to the transient hydromodel? Are these shocks ‘captured’ in a numerically robust way? How does the rapid drainage mechanism, and its impact on the effective pressure, impact the system? Does it give rise to rapid oscillations in streaming behavior? Are any of these non-linear couplings and effects sensitive to the spatial/temporal discretization? If the ISM is forcing the hydromodel at each ISM time step - then how sensitive is the hydro model to the initial conditions? In particular the distribution of basal water. There are mentions of stability and robust solutions in the text, but they are just that - mentions. I’d like to see more on this, to assure the reader the results are stable across discretizations of space and time.

* Continuing with the issue of reproducibility, the code should be more accessible. Publication should include a URL repository where the code can be accessed. Tag the branch used in the publication.

* The distinctive features of this model need to be contrasted to the wealth of recent publications in the area of subglacial hydrology. (eg Schoof, Werder, Hewitt, and Hoffmann). There is a need for a continental scale model like this, but it should be established by documenting how and why other models are not suitable to this task. Similarly, the authors claim that other continental scale models do not include sub-glacial hydrology. I don’t think this is true. PISM has some accounting for basal water, and so does SICPOLIS. Pollard and DeConto treat hydrology as it relates to sediment. ISSM and Elmer ICE both have hydrology models. CISM contains an ISM, and it contains some subglacial hydrological components developed by Hoffman. It is possible that none of these are good tools for continental/glacial cycle scale studies that are the specialty of Tarasov, but this should be argued persuasively in the paper. Much more should be done here.

* Axially symmetric experiments should be presented with bivariate plots. Not much is gained by inspecting these highly symmetric solutions. Something is lost in the color

map, which might hide high frequency oscillations in the solution.

* The focus for the sensitivity study should be streaming behavior, that is the point of the hydro model. Averaging quantities across the entire ice sheet diminishes the importance of changes to parameters. Why not consider the impact of parameter changes to a set of grid cells that are characterized by low effective pressure at 18 and 22 kybp?

In general, the paper is well written and logically presented. I had some minor notes about the choice of words, but given the need for major revisions, it's probably best to wait for those revisions before picking apart the language. Concluding, I'd like to see this paper published. The work occupies a unique niche in a world where basal hydrology models continue to add complexity in the absence of observation. It's refreshing to return to fundamentals and basic physical processes. I believe my concerns are specific, and can be addressed in a reasonable time frame. I look forward to seeing a revised manuscript.

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