Review of: *The role of basal hydrology in the surging of the Laurentide Ice Sheet*

by William H. G. Roberts, Antony J. Payne, and Paul J. Valdes

General Impression

This article uses a model setup that - how I understand it – has been already applied to the West Antarctic ice sheet (LeBroq et al., 2009) and now has been adapted to investigate ice-sheet instabilities of the Laurentide ice sheet in the context of Heinrich events. The article is well structured and the necessary details to understand the model assumptions are clearly laid out. Figures and tables

I am aware that in the case of dealing with simulations of such long time scales, the choice of models is limited by the needed approximations to keep the computational costs in a manageable range. Nevertheless, I think you picked the wrong approximation. I have the suspicion that a SIA model is an over-simplification to the problem, simply by the undeniable fact that it is based on assumptions that are opposite to the physics of fast ice outlets and that a model accounting for longitudinal stresses would have been the correct choice. I elaborate these points of criticism in the following section.

I think, that in the end it is about judging the relative importance of the (apparently to the mechanical problem of fast flow wrong) ice mechanics as well as (to a lower degree of importance) the displacement of the lithosphere under the load signal in relation to the hydrology model (which itself would demand a deeper discussion) in this coupled system. If the authors can come up with an improved chain of arguments justifying the combination of partly severe model assumptions, I see no problem of having this article published. The easiest way to show it would be to include higher order effects (i.e., use an improved ice-flow model) and compare to this solution.

Major points of critics

My major concern is about the choice of the ice sheet model used in this investigation. You use a model that is based on the shallow ice approximation (SIA). SIA basically only is capable of correctly representing flow situations linked to creep flow with little to no sliding in absence of longitudinal and transversal stresses – a situation completely opposite to ice streams. Throughout the text you frequently correctly point out the issues linked to this approximation in connection to fast flow features including citation of the findings by Hindmarsh (2009) that summarize those concerns. On **page 5** starting from **line 13** you state:

> The SIA neglects longitudinal stress gradients. Although these stresses are negligible in the interior of a slow moving ice sheet, they are important at the margins where they are integral to ice shelf and ice stream dynamics. Furthermore, in regions where horizontal shearing is important, for example at the boundary between slow moving ice and fast moving ice streams, longitudinal stresses are not negligible (Hindmarsh, 2009). The lack of longitudinal stresses in regions of high horizontal shear is of concern since we would expect such areas of high shear to occur during surging events when parts of the ice sheet are moving at relatively high velocities whilst surrounded by areas of much more slowly moving ice. We acknowledge this omission but must neglect it since using higher order approximations make the long model integrations that we need to perform computationally impossible.

I do not agree on the justification in the last sentence of this paragraph. Adopting the common nomenclature of the expression “higher order model” (=“anything else exceeding SIA in complexity”) in literature, there are examples that contradict your statement. There are known studies stretching over at least similar time-scales using hybrid models that swap the SIA with the shallow shelf approximation (SSA) in areas with dominating horizontal stresses. The latter being a better approximation (using a still on shallowness based assumption of plug-like ice flow) to streaming ice flow that provides a computationally relatively low-cost implementation. Besides the application using GRISLI cited within the article by yourself (Álvarez-Solas et al., 2011), I instantly could mention Pollard and DeConto (2013) who applied such a model (including a simplified model for grounding line migration) to the Antarctic ice sheet and the article by Bindschadler et al. (2013) which contains long-time integration of shelfy-stream models for the Greenland ice sheet. Even a spin-off from Glimmer, Glimmer-CISM (Lipscomb et al., 2013), would have the ability to better represent the dynamics of fast flow features in the ice sheet. Also L1L2-type of models, such as BISICLES (Cornford et al., 2013), might be able to handle simulations of such times spans. In short: **I not to all extend understand the choice of the model (in terms of physics), and have troubles to accept the justification for doing so**.

The second topic that would need elaboration is the **physical concept of the hydrology model** and not restricting yourself to studies on the parameters within. Is the solution for laminar flow between two plates an adequate description for the water transfer through aquifers in sediments? This part at least demands some discussion.

Lastly, but this might not be a big issue after all, but at least demands clarification: On **page 8, line 15** you state (mind also the typo: *…, we use~~s~~ …)*:

> Although Glimmer does allow for the use of a lithosphere model beneath the ice sheet, in order that we can make direct comparisons between the different runs in the suite of sensitivity tests, we uses a topography beneath the ice sheet that does not vary in time. For this we use the ice5g topography for 21ka (Peltier, 2004)

If you have the ability to account for **isostasy** (presumably this is what you meant by lithosphere model), **why not taking it in?** If you have something like a LDRA model at hand, then this should be relatively cheap to include such a run in the sensitivity analysis to exclude any influence of an assumed fixed bedrock. Despite the fact that by Shreve’s assumption the gradient of the ice-sheet surface has a to the bedrock gradient by an order-of-magnitude stronger influence on the hydro-potential, which manifests itself in (the corrected version of) equation (5), it would be beneficial to explicitly mention (perhaps supported by a reference) that, compared to changes in ice surface gradients, changes in bedrock elevation gradients due to changes in ice-load over the whole simulation area as well as time-period have no significant influence on hydrology. In particular as you write yourself about the sensitivity of surges with respect to bedrock resolution (**page 15, line 18**:)

> up the entire width of the fast sliding region. The structure of the surge within Hudson Bay itself is more complicated, this is likely due to the more detailed bottom topography that the higher resolutions allow.

To give an example where neglecting lithosphere dynamics might be an issue: Bedrock gradients can affect if they apply in regions with $\nabla\Phi \approx 0$, i.e., over regions of low surface slope, which usually coincide with regions of largest ice thickness and hence largest bedrock displacements (and consequently largest changes in displacement) to decide on the principal direction of the water routing (I recall on the findings of AGAP around Dome A in Antarctica!).

Minor issues

Page 7, equation (5):

$$\nabla\Phi = \rho_i g \nabla S + (\rho_w + \rho_i) g \nabla h. \quad (5)$$

I think the brackets should contain the difference rather than the sum of the densities, hence $(\rho_w - \rho_i)$ as else the bedrock influence on the hydro-potential would be about twice the one of the free ice-surface – in reality it is 1/10-th.

Page 8, line 21:

is made. Diagnostic fields are output every 100 years. However when diagnosing the processes responsible for the surges we use output derived every time step.

The last sentence would need a definition of your time-step size, for convenience in the same paragraph.

Page 8, line 24:

the FAMOUS climate model. The surface mass balance used by the ice sheet model is calculated using the precipitation and temperature fields from the climate model and use a simple positive degree day scheme (Reeh, 1991; Rutt et al., 2009). The

If the resolution of your ice sheet does not coincide with the climate model, please drop a line on how you interpolate/downscale your forcing fields. Do you account for elevation lapse rates?

Page 8, line 27:

also shown, in Fig. 1(b). The base of the ice sheet is forced with a spatially and temporally constant geothermal heat flux that takes a value of $4.2\times10^{-2} W m^{-2}$.

The choice of a spatial and temporal constant numeric value of the heat flux deserves a justification.

Page 8, line 31:

tion uses the Water Sheet Scheme, a sediment sliding parameter of 0.1 m Pa^{-1} yr^{-1}, and a hard bed sliding parameter of 0.005 m Pa^{-1} yr^{-1}. Firstly we describe the mean state of the ice sheet.

Same as before. Do these value somehow link to something in literature or are they tuned parameters? Having read further, I conclude they might be chosen due to findings from other studies – perhaps mention that already here.

Page 8, missing information:

You are not revealing details on the boundary **condition** imposed at the **ice-sheet/ocean boundary**. This is in particular of interest as you report on different calving fluxes in the discussion later on. What does “calving flux” in terms of your setup mean? The two options you have with SIA is fixed horizontal boundary (and calving is equal to the ice flux through this boundary) or that you allow to advance the ice sheet along the sea-floor, describing ice loss (a.k.a. calving) below a certain depth. In view of some of the HE theories involving ice-shelf dynamics, I would ask to have the consequences imposed by the ocean boundary of the ice-sheet model discussed (missing buttressing, no grounding line migration, etc.).

Page 15, sub-section 4.4:

> The striking similarity between the structure of the surges is indicative that the surges are not a numerical artefact ~~but rather they are a physically based process~~.

This very much links to the major point of criticism on model choice. At the best you can say that by your series of runs presented in Fig. 11 you give indication of **numerical convergence** of your setup. Still, this applies only on the level of your discretized model, which might or might not represent physics. Hence, I suggest to drop the last part of the sentence. If you could compare the results of Fig. 11 to a reference solution (e.g., from another model with a suitable mechanical representation of ice flow, such as a SSA model), this then could give some indication on physics. **For the moment all you can claim is that you have indications (not even the proof) to have a converging numerical model that is capable of producing cyclic behaviour, but nothing beyond that.**

Typos, etc.

Page 1, line 5: the period is missing at the end of the sentence:

> contrast to many previous studies where surges only occur for rather specific cases. The robustness of the surges is likely due to the use of water as the switch mechanism for sliding The statistics of the binge-purge cycles resemble observed Heinrich

Page 2, line 33:

> if the ice sheet must be forced we need to find another external trigger mechanism. In particular, such external forcing would require ice shelves to exist in other sectors of the ice sheet, areas which may be less conducive to their formation that the Labrador Sea.

than?

Page 4, line28 (and on different other places):

> bed being kept at pressure melting. Thus, although 3D ice sheet models have been coupled to basal hydrology models, their configuration has never been realistic enough to show the importance of water in ice sheet surges.
> In this study we shall simulate HEs in a 3-D ice sheet model, Glimmer (Rutt et al., 2009), using such a water scheme. We

Very minor issue, but I suggest to consistently use either 3-D or 3D.

References

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