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Climate of the Past Discussions

Interactive comment on "The role of basal hydrology in the surging of the Laurentide Ice Sheet" *by* William H. G. Roberts et al.

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This submission examines the possible role of basal hydrology in Hudson Strait ice stream discharge cycling (aka Heinrich Event source). The dynamical source of Heinrich Events is a major ongoing question for the paleo community as well as a challenge for the modelling community.

Though the duration and periodicity of the Hudson Stream surging are somewhat off, the surge cycling modelled in this submission is otherwise of reasonably magnitude. A number of relevant model uncertainties are examined via sensitivity tests: parametric (sliding speed scaling parameter and dependencies on basal water depth), and to a lesser extent, model resolution. The results show the response to be relatively robust which is a significant modelling accomplishment. The paper is overall wellwritten/edited (I did not find any typos), with generally appropriate figures and tables and supplement. There is some thoughtful analysis ("Anatomy ...") examining the details of surge activation and deactivation.

It would have been nice to see a 10km resolution test, as a factor 2 total range for the sensitivity test is quite limited. Also, there needs to be corresponding time-series and table entries for resolution response as is provided for the parametric sensitivity tests.

The study did leave me with two outstanding questions. At the beginning, it reiterates that standard understanding that the shallow ice approximation (SIA) for ice dynamics is inappropriate for ice streams but then ignores this throughout the remainder of the text. There should at least be some discussion of what the potential impacts would be from including a higher order representation, or hybrid/membrane approximation for the ice dynamics (eg with reference to the MISMIP comparisons...). The study obtains Heinrich type cycling with a model that uses a representation of ice streaming that is not physically defensible. The relative robustness of the cycling response to tested model parameters and even to the complexity of the hydrology model offers some confidence in the model. But with confirmation from an appropriate ice dynamics representation, there would be significantly more confidence in the physical validity of the results.

Glimmer has higher order physics options. Could this not be turned on for a short (eg 15 kyr) test? PISM with membrane approximation could be run at 25 km resolution over a glacial cycle as could Dave Pollard's hybrid model. I know this is non-trivial, and therefore would not use this as grounds for rejection of this submission but I urge some exploration of time feasibility of this.

The other issue is the choice of water depth dependence as opposed to basal water pressure. There is no clear physical model of why a 2-4mm basal water depth would be a reasonable threshold for fast basal sliding, especially consider actually topography/surface roughness at say 1 m scale... Computationally, water depth offers an easier to implement model and I can speculate on some physical motivation. There is

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Discussion paper

some discussion of this in Le Brocq et al., 2009, and it would help this paper to provide further justification.

The final discussions/conclusions would also benefit from more attention to remaining uncertainties and implicit assumptions. What impact might higher order physics have on the surge cycling duration and periodicity?

Specific comments

The depth of water beneath ice sheets has been argued to be intimately related to the speed with the overlying ice can slide (Budd and Jenssen, 1987; Le Brocq et al., 2009).

Yes but the 2nd reference also raises the issue of how to reconcile mm scale water depth with potentially metre scale water storage in subglacial sediment. It needs to be made clear that this parametrization as of yet has no clear physical basis.

We assume here that the effective pressure is zero (see, e.g., Budd and Jenssen, 1987; Alley, 1996). Although we would expect the effective pressure to have an impact upon the rate of sliding we neglect this effect as it is small.

#I see no basis for either of the above claims (depending what "close" means, presumably small enough to be ignored) given current literature and understanding (eg Cuffey and Patterson, 2010, for a broad review). ###########

If temperatures are anomalously cold we would expect a reduction in the mass lost from the ice sheet from surface melt but an increase in the mass lost due to calving.

The later does not follow necessarily. Perennial landfast sea ice could choke up the system as presently observed seasonally for tidewater glaciers in the Arctic. Cold conditions could also reduce thermal forcings of calving.

An increase in the calving could make it easier for the freshwater from the ice sheet to impact the AMOC, but it will undoubtedly also increase the ice shelf's thickness making it more resistant to melt and a better buttress.

Do you mean thickness at the calving front? I would expect thickness at the grounding line to decrease with increasing calving (with some time lag) due to less buttressing from less shelf extent

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.

Could you at least do a 40 kyr integration at 10 km, interpolating a restart file from the 25 km run to avoid the spin-up?

At the base of the ice sheet the vertical gradient of temperature, contained in the vertical advection and diffusion terms is a result of heating by the geothermal heat flux and heating due to friction at the bed.

This warming is the result of the geothermal heat flux and, especially in the Hudson Strait region, the strain heating

Incorrect. Basal temperature is the result of energy conservation, and is therefore due to all terms. Your figure 5 shows that "other terms" contributes more than strain heating.

Previous models have taken as the switch the temperature at the bed of the ice sheet (Calov et al., 2002, 2010; Papa et al., 2005).

Not all models, eg Johnson and Fastook, 2002

The behaviour of the events are broadly similar, with events being of similar size and duration. This is strongly indicative of the robustness of the events to resolution.

"similar size and duration" with presentation of the actual results does not provide any evidence of robustness. Provide a time series comparison.

This compares well with the ice5g distribution that the model was initialized with, which has an area of 1.68×107 km2.

Not surprising, if ice5g was used as the boundary condition for the FAMOUS run

At some time the base of the ice sheet will warm sufficiently that the gradient in ice sheet surface, and its associated strain heating, can warm the interior of the ice sheet above pressure melting point.

Is the basal water flow blockage switch off/on at the pressure melting point? This would not be physical as a 50 km square block of ice won't freeze or get warm-based simultaneously across its base and the experiment should be repeated again with a smooth ramped transition over some range O(0.1 to 0.5 K)

As the water depth increases the sliding speed increases and thus the heating rate from friction can increase.

Physically, increasing water depth decreases effective basal drag to permit increased sliding speed, so its not clear if the heating rate from increasing water depth should necessarily increase though it's clear why it does in the current model.

These two regions are determined using a global sediment thickness map (Laske and Masters, 1997).

Caveat, this thickness map was created for a seismology context and has numerous errors for a glaciology context.

reasonably simulate sliding at the base of the present day West Antarctic Ice Sheet

vague claims such as the above are common within the ice sheet modelling community, but indefensible. Be more precise.

#FIGURES:###########

figure 10, need to label plots (a=, b=) so that the reader can decipher without opening up another page

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