The role of basal hydrology in the surging of the Laurentide Ice Sheet: response to reviewers

We thank the reviewers for their comments. We begin our response with a general defence of use of the Shallow Ice Approximation model. We then address the specific issues of each reviewer.
We reproduce *the reviewers comments in blue italics*, with our comments in black.

General Comments

Both reviewers note, as do we in our manuscript, that the use of the Shallow Ice Approximation (SIA) may affect our results because of the omission of longitudinal stresses.

The first thing to note is that because of the horizontal grid resolution that we use (chosen to allow us to use available computer resources to probe the parameter space) the effect of longitudinal stresses are greatly diminished. Following Paterson (1983) we may assume that basal shear stress can be approximated by the gravitational driving stresses alone when averaging over distances greater than around 20 times the ice thickness. With our chosen resolution, 50 km, we are well within this limit over much of the ice sheet.

In practice the effect of the longitudinal stresses is to smooth the stress field, and thus velocities, in comparison to the SIA. In order to mimic this effect on the ice sheet we undertook a large number of tests to approximate this effect by applying a smoother to the temperature field in the model (not shown in the original m/s). This smoother followed Beuler et al. (2007) and applied a Gaussian smoothing kernel to the temperature field calculated by the model. We found that this smoother had no effect on the surging behaviour in the model: regardless of how much smoothing was applied the model surged (see attached Figure 1).

Finally, while we must accept that when the ice is surging the SIA does not encapsulate the physics of this behaviour, it has been shown (Hindmarsh 2006, Kyrke-Smith et al 2013) that more complete stress balance models do simulate the onset of surging in a manner consistent with our model. Thus, although while surging our chosen model is insufficient, it can simulate the transition into the surges.

Comments to Reviewer Lev Tarasov.

Glimmer has higher order physics options. Could this not be turned on for a short (eg15 kyr) test?

We fear that 15kyr would be insufficient time for the model to equilibrate to the different stress balance. Furthermore, it would not allow us to simulate more than one event; we would not feel comfortable reporting findings from such a limited number of events.

The other issue is the choice of water depth dependence as opposed to basal water pressure.

We agree that there is a discrepancy between the mm scale water depths we simulate and the metre scale roughness at the bed of ice sheets. However, we note that at the grid resolution that we are simulating such features are far below the resolution that can be resolved. To address the motivation for using water depth rather than pressure we will include the following text in Section 2.1 where we discuss the sliding scheme. “The use of water depth as the control upon fast sliding has been suggested to be a better representation than water pressure because it is the water content of the till that determines the sliding (Le Brocq et al. 2009). This parametersation, although reasonable, is, however, an empirical relationship. At present, fully process based hydrology models are not yet suitable for long-term continental scale integration and are thus unsuitable for our purposes.”

The exact value of the water depth can not be known *a priori* and this was the motivation for thoroughly investigating the parameter space around the different water thresholds. Practically, until detailed hydrology models are incorporated into ice sheet models approximations such as the one that we use must be made. As an analogy: climate models must parameterise convection as it occurs at scales below that of the grid resolution, and it is only recently that simulations have begun to resolve convective processes. Let us hope that we do not have to wait as long for ice sheet models to reach this stage!

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.

See comments above.

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). ###########

We shall rewrite this to read: “As a closure we shall assume that the effective pressure is zero (see, e.g., Budd and Jenssen, 1987; Alley, 1996)

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 agree with both of these comments. Our aim in this short section was to try and construct an argument to explain why you might expect to see increased freshwater flux/calving from an ice sheet when the temperatures are anomalously cold, such as before Heinrich Events. This is an necessary argument if one believes that changes in the AMOC are linked with Heinrich Events. The literature, currently lacks such an argument, so in the interests of fairly proposing the externally forced Heinrich Event hypothesis, we attempted to construct such an argument. As is pointed out there are flaws and inconsistencies in our argument, which rather highlights the difficulty in making this link. Due to the vagueness of this argument we suggest that we remove the sentences beginning “If temperatures are anomalously cold” ending “changes in the ice shelf thickness have not been simulated (Hulbe,1997).” This paragraph will now read:

“Uncertainties surrounding an external trigger include the ultimate reason for the warming beneath the assumed ice shelf covering parts of the Labrador Sea. Although changes in the Atlantic Meridional Overturning Circulation (AMOC) have been implicated as the cause for the warming (e.g. Marcott et al., 2011; Menviel et al., 2014), it is not clear why the AMOC is itself reduced. If we assume that AMOC reduction goes hand in hand with Dansgaard/Oeschger (D/O) events, which is itself by no means certain (Dokken et al., 2013), it must be explained why the AMOC is more reduced during some D/O events than others such that a HE does not occur for each D/O event. This could arise from the link between the coldest stadials and HE. Other key features required for an external trigger also remain, so far, unobserved. Not all HEs are observed to have an associated subsurface warming, although this is due to a lack of observations rather than an evidence of absence (Marcott et al., 2011). There is also no evidence for an ice shelf in the Labrador Sea. The geography of the Labrador Sea makes it likely that an ice shelf would form there, however its size and therefore capacity to buttress the ice sheet is unknown. Observations of this ice shelf are key to supporting this mechanism.”

We acknowledge this omission but must neglect it since using higher order approximations make the long model integrations that we need to perform computationallyimpossible.
 # 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? ###########

This experiment would be ~10 times more computationally expensive than a 25km run (2x2 times more intensive due to horizontal resolution, ~2 times more intensive due time step changes). 10 times more computer time than for the 25km runs (we did more than the one simulation presented to ensure that the results were robust) we feel is not justifiable. Furthermore we would be uncomfortable with basing our results on a single 40kyr run at 12.5km.

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. ###########

Indeed basal temperature is a result of energy conservation. The temperature *gradient,* however, is due to the geothermal heat flux and heating due to friction (this is a boundary condition to the model's temperature equation).

Re the second point: the two sentences read:
“The occurrence of the events is the result of a slow warming at the base of the ice sheet that gradually brings the ice sheet bed to pressure melting point, at which time a layer of water can form at the base of the ice sheet. This warming is the result of the geothermal heat flux and, especially in the Hudson Strait region, the strain heating.”

Fig 5. shows exactly, this. Before the event the two terms that are significant are the Strain Heating and the Other Terms. Once the event is under way (from 28 kyr on) the strain heating term is indeed negligible. We note that for clarity in the figures we include the geothermal heat flux in the other terms. We shall note this is the figure caption.

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 ###########

We thank the reviewer for pointing this out. We shall rewrite the sentence to read:
“Previous models *looking at ice sheet surging* have taken as the switch the temperature at the bed of the ice sheet (Calov et al., 2002, 2010; Papa et al., 2005)” and add at the end of the paragraph:
“Water depth has been shown to be important in simulating the slow evolution of ice sheets over a glacial cycle (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. ###########

See the attached Figure 2 which we shall include in the supplement.

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

Not surprising, but it is also not a given that this would be the case.

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) ###########

Water flow beneath the ice sheet is determined by the melt rate at the ice sheet base which in turn is determined by the convergence of energy in the bottom model layer. There is no explicit switch that blocks the water flow when the ice is at pressure melting point.
We shall clarify the sentence to read: “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 begin to melt the ice at the ice sheet bed”

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. ###########

This is good to have documented, but unfortunately we are unaware of any other such dataset. Since it was used merely for mapping regions with high or low sliding we do not feel it contributed any errors. If we had used it to define our sliding parameter this would have been a far bigger concern.

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. ###########

Rewrriten: “Following Le Brocq et al. (2009) we model the onset of sliding as a tanh function of water depth which has been used to simulate sliding at the base of the present day West Antarctic Ice Sheet”

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

We shall add these to the caption

Anon Reviewer:

SIA model comments

For justification of use of the SIA see our opening comments. We will emphasise, that although we would have liked to use models with higher order physics we chose to probe the highly uncertain parameter space relating to the sliding parameterisation and the hydrology schemes rather than investigate higher order physics, whose effect is negligible at the model resolution we chose.

Water scheme model comments.
We reproduce the comments made above about the water scheme

We agree that there is a slight discrepancy between the mm scale water depths we simulate and the metre scale roughness at the bed of ice sheets. However, we note that at the grid resolution that we are simulating such features are far below the resolution that can be resolved. To address the motivation for using water depth rather than pressure we shall include the following text in Section 2.1 where we discuss the sliding scheme. “The use of water depth as the control upon fast sliding has been suggested to be a better representation than water pressure because it is the water content of the till that determines the sliding (Le Brocq et al. 2009). This parametersation, although reasonable, is however an empirical relationship. At present, fully process based hydrology models are not yet suitable for long-term continental scale integration and are thus unsuitable for our purposes.”
The exact value of the water depth can not be known *a priori* and this was the motivation for thoroughly investigating the parameter space around the different water thresholds.
Practically, until detailed hydrology models are incorporated into ice sheet models approximations such as the one that we use must be made. As an analogy: climate models must parameterise convection as it occurs at scales below that of the grid resolution, and it is only recently that simulations have begun to resolve convective processes. Let us hope that we do not have to wait as long for ice sheet models to reach this stage!

Isostasy model

We attach a Fig 3 which shows time series similar to those in the m/s Fig 3 in which we use an elastic lithosphere model based on Lambeck and Nakiboglu (1980) and compare it to a simulation without the lithosphere model. As you can see the results are qualitatively indistinguishable from one another. The decision not to use an isostasy model was made to ensure that any changes in the events that we saw were the result of the parameter being investigated not from changes in the bedrock topography which we would be unable to control.

Minor issues

Page 7, equation (5):

The equation should read (rho_w – rho_i), we thank the reviewer for spotting this.

Page 8, line 21: time interval of output
The output is every 10 years (not every time step). We shall correct the m/s to reflect this.

Page 8, line 24: field rescaling
The surface mass balance fields are interpolated onto the ice sheet model grid. No lapse rate correction is applied to the temperature filed to ensure that the SMB/surface temperature field is the same for all model simulations. Thus the reported changes in the simulated surge events arise solely from the parameter being varied, not from changes in the SMB/surface temperature that might result from different ice sheet height between model runs.

Page 8, line 27: geothermal heat flux choice
We argue that any spatial/temporal variability in the geothermal heat flux is a small effect that will not change the overall nature of the surges, especially when compared to the effect of using a fundamentally different hydrology scheme. This is not to say that geothermal heat flux is not important, rather that its temporal/spatial variation is negligible.

Page 8, line 31: sliding parameters
The sliding parameter is highly uncertain, hence the numerous sensitivity tests we undertook. We will add a comment here pointing the reader to section 2.1 where we describe why we chose these values.

Page 8 calving parameterisation
We use the fixed horizontal boundary condition. We shall add a comment to this effect in Section 2.5 *Further model details*

Page 15 convergence:
We are happy to drop this part of the sentence

Typos:
Will be corrected and we shall converge on a single definition of 3-D for clarity.

Refs:

Bueler E, et al. (2007) Exact solutions to the thermomechanically coupled shallow-ice approximation: effective tools for verification. J Glaciology (53), 182

Hindmarsh RCA (2006) Stress gradient damping of thermoviscous ice flow instabilities. JGR (111) B1

Kyrke-Smith T.M. et al. (2013): Stress balances of ice streams in a vertically integrated, higher-order formulation. J Glaciology (59), 215

Le Brocq A.M. et al. (2009) A subglacial water-flow model for West Antarctica. J Glaciology. (55), 193

Paterson W.S.B. (1983): The Physics of Glaciers

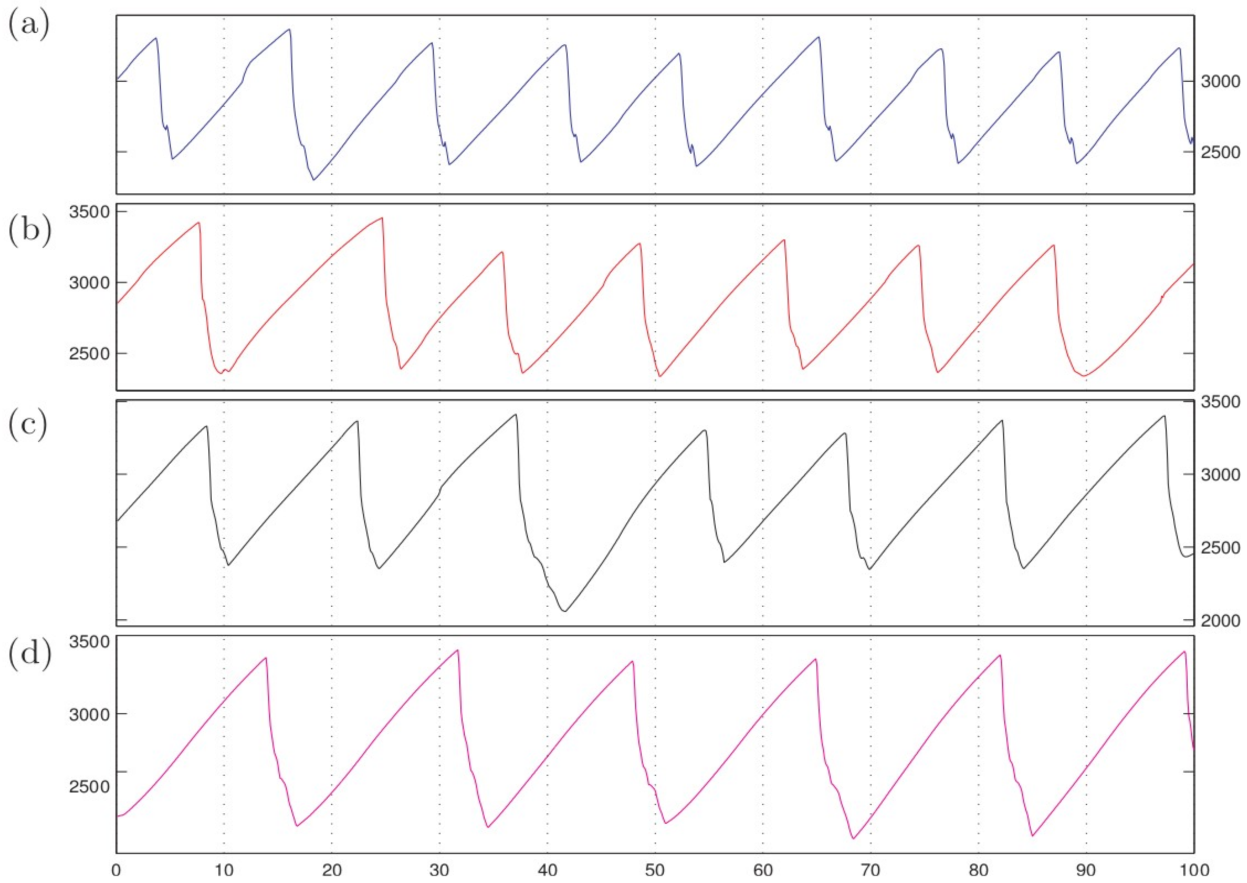
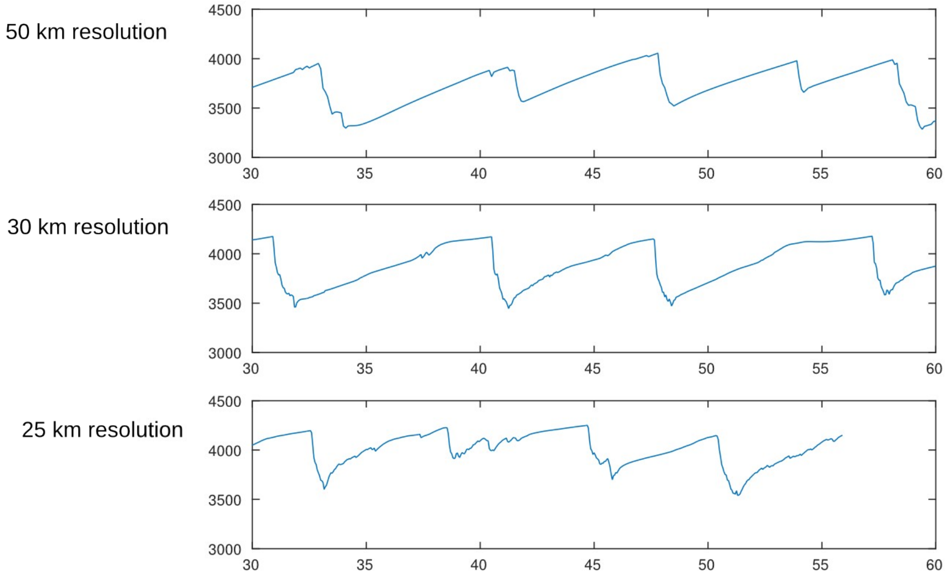
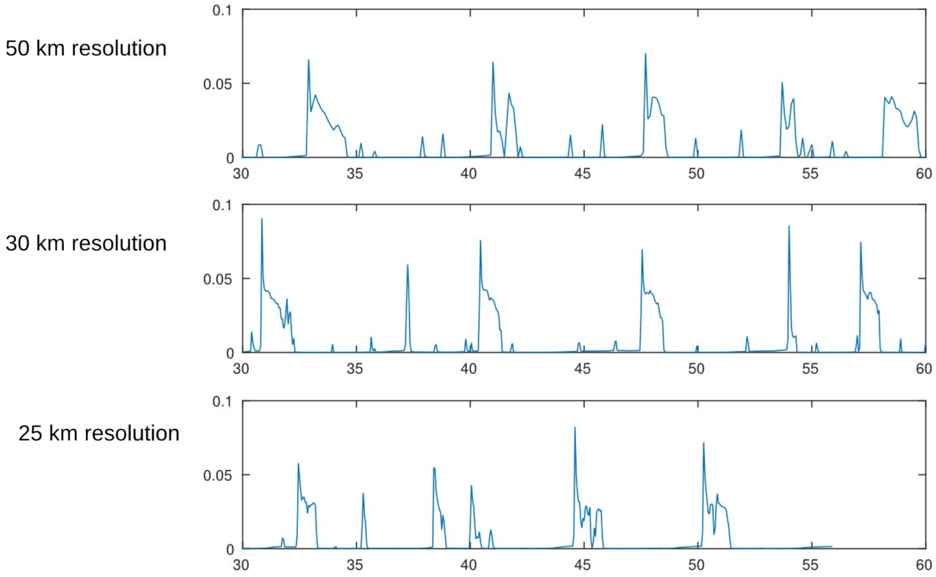


Figure 1. Time series of ice sheet height over central Hudson Bay using the smoothing scheme with 4 different e-folding distances: (a) 240km, (b) 110km, (c) 75km, (d) 0km.

Ice sheet height over Hudson Bay



Calving Flux from Hudson Strait



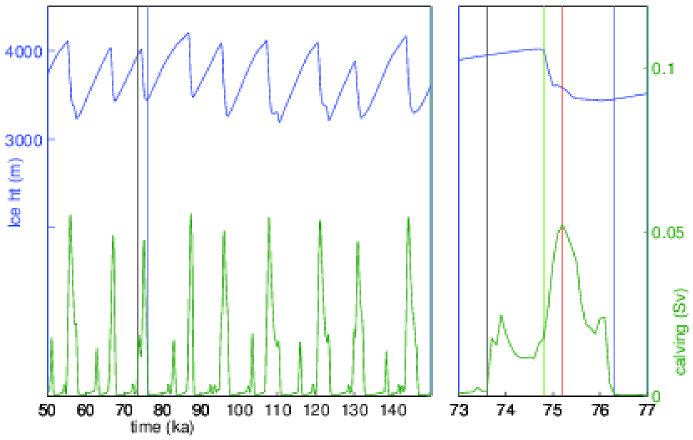
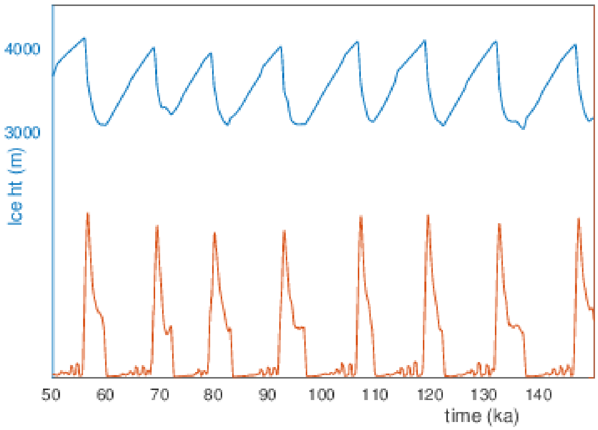


Figure 2. Time series of Hudson Bay ice sheet height and calving flux for 3 different model resolutions 50km, 30km ,25km. Fig 3 from the text reproduced for comparison.

(a)



(b)

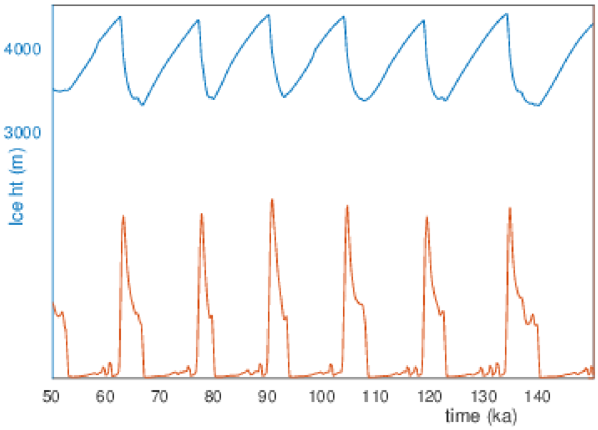


Figure 3. Comparison of model simulations with (a) and without (b) the isostasy model. Upper blue line ice sheet height over Hudson Bay. Lower red line calving flux from Hudson Strait (scales as per fig 3 in text).