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The Cryosphere Discussions

Interactive comment on "Response of the large-scale subglacial drainage system of North East Greenland to surface elevation changes" *by* N. B. Karlsson and D. Dahl-Jensen

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General comments

The paper describes the application of an ice flow model to investigate the impact of changes in the ice sheet surface on the subglacial hydrologic system of the North East Greenland Ice Stream. The topic is of interest and is well justified in the introduction. The paper uses a simple ice flow model and assumes the water is at overburden pressure. The simplicity of the approach allows an easy understanding of what is going on in the system, so while some of the assumptions (e.g. using shallow ice) are perhaps known to not represent the system well, the paper is open about the deficiencies and

justifies why they shouldn't impact on the work too much.

The simplicity in the approach should allow a number of different scenarios to be investigated, however, there is really only one experiment carried out in this paper which limits the conclusions to a very general finding, which perhaps one could have reached intuitively anyway without the need for the model. The paper is largely well written, but the experiment is not clearly explained which led me to have to try and work out what had been done.

The paper is acceptable with the current level of experiments, as the paper can form a basis for further experiments, resulting in a method description paper (though may perhaps have been more suitable for the Geoscientific Model Development journal). However, it would be a much stronger paper if a more extensive range of experiments were carried out – i.e. what has to happen to get major reorganisations in the water system; is this realistic? How much water can be rerouted into other catchments under what circumstances? The option is either to leave the results as they are but introduce a discussion as to the potential of the model and further experiments that could be carried out, or to devise a more extensive range of experiments. I have detailed more specific changes below which should help clarify exactly how the model experiment works.

We have included a paragraph in the discussion outlining the potential for new experiments. The final paragraph of the discussion (starting at line 19, p. 737) now reads:

"The latest bed topography data show that some ice streams in Greenland are constrained by deep troughs (e.g. Jakobshavn Isbræ, Gogineni et al., 2014), while other Greenlandic ice streams are not strongly controlled by bed topography (Bamber et al., 2013a). We therefore hypothesise that other drainage basins in Greenland also might experience subglacial rerouting of water and corresponding fluctuations in ice-flow velocities. The model presented here is a tool that could be applied to other parts of GrIS and thus the sensitivity of the subglacial drainage pattern in different drainage basins could be assessed. Places of interest include the basin containing the Petermann and Humboldt Glaciers, and the glaciers on the northwest coast. Both of these areas could have potential for subglacial water rerouting. For example, studies have found that in Northern Greenland changes in the subglacial waterways are likely to have taken place during the last glacial maximum (cf. Bamber et al., 2013b). The method outlined in this paper could be used to investigate the change in subglacial drainage patterns as the ice sheet retreated and thinned after the last glacial maximum to its present day state. Alternatively, the model could be applied to the whole of the ice sheet; During glacial times, GrIS most likely extended out onto the continental shelf and formed an icebridge with the Laurentide Ice Sheet (e.g., Dyke 2004). The break up of this bridge most likely impacted the surface topography of the ice sheet and therefore also the subglacial water routeways. Finally, the applicability of the model might be improved with the addition of a shallow-shelf mode (e.g. MacAyeal et al., 1996) in order to better capture the ice stream dynamics. "

Specific comments

p726 section 2.2 title and elsewhere: Be consistent in the use of routes/routeways/ways for describing flow paths.

The term "routeways" is now used consistently throughout the manuscript.

p727 line 10: A note to highlight that these sorts of routing methods are sensitive to the algorithm used, grid orientation and size would be useful here – see for example Le Brocq et al. 2006, Computers & Geosciences.

The following has been added to the end of the paragraph:

"This scheme has been shown to be the most suitable for calculating fluxes across profiles, since it is consistent for different orientations and resolutions, which is not always a given for routing schemes (Le Brocq et al., 2006)."

p727, end of section 2.2: Information about the time step of the model would be useful

here, you present outflux over time, it would be good for a clearer description of how this is come by would be useful.

The paragraph below has been added (following the sentence added above to the previous comment):

"We use the routing scheme to calculate the distribution of subglacial water (for a given basal melt configuration) every 100 model years. This allows us to calculate the outflux of subglacial water at the margins of the model domain over time and thereby investigate the changes in outflux."

p727 section 2.3: This section is not clear to me to describe the model runs. Do you initialise from present day? Why do you use a low sliding coefficient to start with? Why not start with the inverted parameter and then do a set of sensitivity experiments to see what happens to the water routes under potential future scenarios of sliding change? A full outline of the model runs is needed here to prepare the reader for the results they are going to see.

We use a spin-up run for several reasons. Firstly, if we initialise the model from present day topography using the inverted sliding parameter, the model will need a relaxation time in order for the numerical scheme to reflect the surface topography. During this relaxation phase, any change in surface topography could be due to the relaxation and not the response of the ice surface to changes in basal conditions. Secondly, we wish to avoid giving the impression that we are trying to forecast the behaviour of the system. As stated elsewhere in the manuscript our simple model cannot capture the present dynamic state of the drainage basin. By presenting future scenarios our results would take on a character of forecasting, which is not in line with this work. We hope that by rewriting the section highlighted by the reviewer, it is clearer exactly what the model does:

"The aim of the ice flow model is to obtain realistic changes in ice surface elevation for changing basal sliding values. In order to achieve this, we wish to start with an ice-sheet configuration that is not influenced by basal sliding. Using the present day surface topography, we do a spin-up run with a low sliding coefficient. Thereby we obtain a simulated steady-state ice-sheet whose shape is in agreement with the numerical scheme of our model, and not influenced by basal sliding. When the basal sliding is increased in the subsequent model run, we can then assume that the changes in surface elevation directly reflect the response of the model domain to the changes in basal conditions.

Specifically, during the spin-up the ice flow model is run on two grids; a 10km grid for the entire GrIS and a 5km grid for the model domain encompassing the North East Greenland basin (shown in colours in Fig. 1). At every model year the grid cells along the drainage basin boundary are updated with the result from the 10km model downscaled to the 5km grid by linear interpolation. The drainage basin boundaries are assumed to not shift position over time. The spin-up is run for 20kyr with a constant sliding coefficient of 1*10-11 Pa-3 m2 yr-1.

We then perform our simulation of changing basal conditions starting from the steady-state ice-sheet configuration obtained from the spin-up. We decouple the nested regional 5km model from the 10km resolution ice-sheet model and the surface elevation is now kept constant along the basin boundary. The maximum allowed sliding coefficient value ks is now increased in small steps every 1000 model years. This model simulation is run for 20kyr. Finally, after 20kyr the sliding coefficient is kept constant and the model is run for another 10kyr. This last stage of the model simulation is what we refer to, when we use the term "no external forcing", because no further changes are imposed on the basal conditions, although margin loss and mass balance field are still applied."

p728, line 3: Change to 'Using the simple inversion technique described in Appendix A, ...' This has been changed as suggested.

p728, line 13: How does Budd et al (1979) come by his value – is it transferable to

Greenland? Is it from fieldwork or lab work?

The sentence has been changed to:

"Our values are within the range of values typically found from models of subglacial settings, and also comparable to laboratory experiments, that suggest a value of ks = 1.8*10-11 Pa-3 m2 yr-1 (Budd et al., 1979). This experimental value has been found to agree well with observations from real glaciers (Bindschadler, 1983), although it is likely very variable for different glacier settings. Even so, our results indicate a high degree of basal sliding."

p731, line 15: Again, this needs a bit more explanation how you come about the outflux over time. We hope that by modifying the paragraph on p. 727 (as suggested by the reviewer, see above), it is now clearer how the outflux is calculated. In addition the following sentence is added:

"Figure 6 shows the change in outflux over time for the three major glacier outlets of NEGIS, calculated using a routing scheme (as described above). Please note that in the following discussion of variations in the flux over time the constant basal melt rates of 5mm/yr was used. We setup three flux gates at the glacier outlets close to the margin such that all subglacial water that passes through the flux gates continue towards the margin, and exits at the outlet. The figure shows the total volume of water that passes through a given flux gate. We have further assumed that changes in water transport are instantaneous compared to the time-scale of ice-flow"

p732, lines 3-5: The mechanism of feedback which causes changes in the ice surface needs explaining here.

We are unsure of what the reviewer is asking here. The changes in surface elevation are not a feedback process. The small changes indicate that the model is approaching steady state. For clarity we have added the following sentence to line 5:

"...after a few thousand years the ice-flow model shows surface elevation changes of the order of 10-1 m/yr or less. We interpret this as a sign that the model is approaching steady state. However, even after several…"

Figures

Figure 2 – this dataset looks like it has been smoothed/interpolated by the plotting software? It would be better to present the raw data which would help to demonstrate the resolution of the model. The labels on the scale bar also need to be tidied to have superscript for the power of 10. The smoothing is not due the plotting software, but stems from the solution to the inverse method. The sliding coefficient is inverted on a 1km resolution dataset. In order to impose a degree of smoothness the solution is regridded to a 5 km resolution (corresponding to the resolution of the ice-flow model), and in order to further avoid large spatial variations in the sliding coefficient, the result from the inversion has been smoothed by a running mean using 2 neighbouring points (in all directions).

This has now been clarified in line 11 on p726:

"The inversions are performed on the 1 km resolution topography data and subsequently regridded." And in line 13 on p. 728:

"The results from inverting for the sliding coefficient ks are shown in Fig. 2. Note that the solution has been regridded to 5km from the original 1 km solution (see also above) and smoothed with a running mean to impose a degree of smoothness."

Interactive comment on The Cryosphere Discuss., 9, 719, 2015.