The Cryosphere Discuss., 9, C672–C677, 2015
www.the-cryosphere-discuss.net/9/C672/2015/

The Cryosphere Discussions
Open Access

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

N. B. Karlsson and D. Dahl-Jensen

nbkarlsson@nbi.ku.dk

Received and published: 4 May 2015

This manuscript present results from a numerical modelling study, which looks at how ice thickness changes in North East Greenland affects the re-distribution of subglacial hydrological routing, and the potential impact on ice dynamics. It makes a very interesting point, that variations observed closer to the margins may be part of an ongoing response to geometrical changes initiated farther inland, and over long timescale. This should be considered in interpreting current observational records and the causes to detected changes.

The introduction and rationale are overall well presented and clearly written. However,

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

the authors use major simplifications in their approach, and although they are explicit about them in the text, I do wonder if additional information is nonetheless needed to support further the model results. I am hoping that this could merely be addressed by making better use of the figures to display additional model output. In particular, I refer to:

1) The use of the shallow ice approximation in ice-flow model: there is not enough information to substantiate the results from the inversion. How close to observations are the thickness / velocity fields, at the end of the spin-up period? For example, Figure 1 could be used more effectively, e.g. by showing the errors between modelled and observed geometry. Similarly, contours for the modelled velocity could be added to the plot. Currently, the main justification provided (e.g., that the basal stress is equal to driving stress in most of the domain) is drawn from a study by Joughin et al., but does not apply to two particular regions (the onset of fast flow, and the fast ice plain, as stated in the manuscript), which happen to be key when measuring the outflux of ice and geometrical changes. We assume that the reviewer is referring to Figure 4, which is also mentioned in the comments below. This figure has now been extended to show the difference in surface elevation between present day elevation and the surface at the end of the spin-up run and at the end of the model run, respectively. The balance velocities have also been included. We hope this addresses the comment above.

2) It seems like the authors are using a constant basal melt rate set to 5 mm/yr. Since they have calculated the melt rate for various regions of the domain, why not routing this water instead (could they calculate a distributed melt map?)? In my opinion, a sensitivity study on this number would nicely complement the work (5 mm/yr is a lower end value, as stated in the manuscript – why not choose an average value?).

It is correct that we could have chosen an average value instead of the lower value of 5mm/yr, but the main point is the resulting variations in subglacial outflux, not the exact value of the outflux. These variations would be identical for other values (if the melt rate is constant in the drainage basin), although with different values. We prefer not to use

TCD
9, C672–C677, 2015

the upper value estimate since it is most definitely too high in some areas, for example, the area upstream of the divergence between Zacharia and Nioghalvfjerd. Here the surface slopes are small which leads to an overestimation of the sliding velocities. Instead, we have included a map of this upper limit basal melt. To this end, Figure 5 has been extended to include a figure of the subglacial water flux using the calculations described on p. 730. Furthermore, the following sentence has been inserted after line 2 on p. 731: "Figure 5a shows the flux of subglacial water using this upper limit estimate. Note how the subglacial water is routed along the shear margins. This is in agreement with field observations (Christianson et al., 2014)."

And the following sentence line 16, p. 731:

"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."

Please note that Fig. 6 has been updated. The units on the y-axes were found to be incorrect, so the calculations have been redone using a slightly different flux gate scheme. This change does not affect the conclusions of the manuscript in any way, the variations in subglacial water persist.

Other comments

There are mentions of a whole GrIS grid, at 10 km resolution, but no figures/ results relate to this. Is that grid mostly used for setting initial conditions?

Yes, in order to clarify this line 14 p. 723 has been modified:

"Data input in the model are surface and bed elevation from Bamber et al. (2013a) regridded to 5km (north-eastern drainage basin) and 10km (GrIS, used in the spin-up run), and present day. . ."

Page 729, lines 14-17: Is this based on Figure 4?

TCD
9, C672–C677, 2015

Partly, but a reference to Figure 4 has been added for clarity:

“The build up of ice volume in the central part of the NEGIS naturally corresponds to an increase in surface elevation (cf. Fig. 4).”

Page 733, Lines 6-8: It is unclear what the author are implying, in stating that cells which are sensitive to the bedrock are also associated with large errors? I suggest to make the point clearer, or to remove the statement.

The sentence has been modified for clarity:

“In contrast, a large number of the grid cells that are sensitive to the bedrock slope coincide with areas where the bed topography is associated with large errors, implying that the calculated subglacial water routeways in this region are particularly uncertain.”

Page 734, paragraph starting line 4: Starting the paragraph with “our results indicate...” seems a bit of an overstatement, since there is no feedbacks allowed in the model between ice dynamics and subglacial system (yes?). Else, showing some plots of how ice velocity evolves together with water routing would strengthen the argumentation.

The paragraph has been reworded and now reads:

“In our model run, an increase in basal sliding generally leads to a lowering of the ice surface. We hypothesise that the formation of the ice stream could have happened as a positive feedback effect, where the introduction of subglacial water at the bed (i.e. increase in basal sliding) led to a lowering of surface elevation, leading to more subglacial water being rerouted into the system. This in turn might lead to a further lowering of the surface and thereby more subglacial water. However, since our model does not include feedbacks between the subglacial system and ice-flow, we cannot truly determine the formation mechanism. It is worth noting that we do not observe “water piracy” in the sense described by Anandakrishnan and Alley (1997) where neighbouring ice streams slow down or speed up when they exchange subglacial water. We also investigated. . .”

Page 735, lines 26-29: Sentence is unclear. Do you mean that the "routing" from grid

TCD
9, C672–C677, 2015

cells north of the onset of NEGIS is correct – while routing along the margin of NEGIS are "less" constrained?? This has now been changed for clarity:

“This makes us confident that our assessment of the sensitivity of the grid cells north of the onset of NEGIS is correct, i.e. that even for present day topography with steeper gradients in the hydropotential, they can still be sensitive to changes in surface slope. The subglacial water routeways originating at the grid cells along the margin of NEGIS are probably less sensitive to changes in surface slope.”

Figures

Figure 3: Should the RHS axis be labeled as a "change" in sliding coefficient (but if so, why is the original value set to 1 Pa-3 m2 yr-1?)? - If not, then I am not sure how to relate the value of sliding coefficient shown on that plot, to the mapped values shown on Figure 2?

Figure 3 shows how the maximum allowed value of the sliding coefficient is slowly increased, and how the ice volume changes in response to this increase. The figure caption is a bit misleading, it has now been changed to:

“Change in ice volume (blue) of the NE Greenland model domain as the sliding coefficient (black) is varied during the model run.”

In addition, line 22-23 on page 727 has been expanded to make it clearer:

“The maximum allowed sliding coefficient value ks is then increased in small steps every 1000 model years. Thus, for the spin-up run the sliding coefficient values (Fig. 2) that exceed 1*10-11 Pa-3 m2 yr-1 are set to 1*10-11 Pa-3 m2 yr-1. Then, after the spin-up run, the sliding is slowly increased, such that instead all values that exceed 1.25*10-11 Pa-3 m2 yr-1 are set to 1.25*10-11 Pa-3 m2 yr-1 and, after another 1000 years, all values that exceed 1.5*10-11 Pa-3 m2 yr-1 are set to 1.5*10-11 Pa-3 m2 yr-1 and so forth.”

Figure 4: Wouldn’t it be a more consistent/useful approach, to have two panels, one

TCD
9, C672–C677, 2015

showing the difference in surface elevation from the end of the spin-up period and observed elevation (which gives an idea of model performance). The second one showing the difference between the ice sheet surface at the end of the model run and at the end of the spin-up period. In addition, why not showing the modelled velocity, rather than the observed?

The figure has now been changed and shows two panels; one showing the difference between the spin-up surface and the surface at the end of the model run, and one showing the difference between the present day surface and the surface at the end of the model run (i.e. the original figure 4). In addition, the balance velocities have been added, however, we would like to emphasise that even present day balance velocities are below observed velocities. We have kept the observed, present velocities to aid the reader in identifying the current position of the ice stream.

Figure 5: Why is Storstrommen not clearly identified? Consider adding values to the melt rate contours?

This figure is mainly meant to illustrate the distribution of subglacial water between Zachariae and Nioghalvfjerds glaciers. For clarity the drainage system of Storstrømmen has not been added, but the water routeways indicate their drainage pattern. We leave it to the discretion of the editor if Storstrømmen should be added to the figure.

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