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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

Anonymous Referee #2

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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, 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.

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?).

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?

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

-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.

-Page 735, lines 26-29: Sentence is unclear. Do you mean that the "routing" from grid cells north of the onset of NEGIS is correct – while routing along the margin of NEGIS are "less" constrained??

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

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

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