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Interactive comment on "Predicting subglacial lakes and meltwater drainage pathways beneath the Antarctic and Greenland ice sheets" *by* S. J. Livingstone et al.

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Short Comment on Livingstone et al., (2013) "Predicting subglacial lakes and meltwater drainage pathways beneath the Antarctic and Greenland ice sheets.", by Sasha Carter and Brad Gooch

General comments

This study couples a simple model for sub-ice sheet hydrology to an ice-sheet model and the Bedmap-2 bedrock elevation dataset to test how the hydropotential surface, and resulting distribution of water flow might be affected by the evolution of the ice sheet geometry and basal temperatures over the glacial-interglacial cycle for the Antarctic

and Greenland ice sheets (AIS and GrIS respectively). This analysis and modeling here help address important questions regarding the century – millennial scale stability of subglacial lakes and waterways under the Antarctic and Greenland ice sheets. Their works shows that modern distribution of subglacial water (e.g. Lebrocq et al., 2009) might have been very different during the last glacial maximum. This follows upon work by Anandakrishnan and Alley 1997, Evatt et al., 2006, Wright et al., 2008, and Carter and Fricker (2012), highlighting the high sensitivity of subglacial flowpaths to small changes in ice geometry. Having seen several presentations on inferring lake distribution using gridded data sets (e.g. Johnson 2002a) and predicting long term changes water routing and lake stability over millennial timescales (e.g. Mayer et al., 2010), it is nice to finally see some of the theory and associated hypotheses testing making its way to print. As with any conceptual advance getting to this stage there are some elements that still need to be hashed out a bit better:

A. Overall, the authors generally explain everything they are doing, which is great, but at times they make it seem like they are doing more than they actually are (i.e. streamflow routing does not coupled to numerical ice sheet model for either AIS or GrIS). Indeed Johnson and Fastook 2002 showed the importance of coupling between these two for reproducing the inferred evolution of the Laurentide Ice Sheet.

B. Although hinted at in the Author comment the paper would be improved if the authors took a more critical eye to the input data used. Although the Bedmap-2 ice thickness model and bedrock DEM (Fretwell et al., 2013) represents a substantial advance over its predecessors, it still suffers in a number of regions from insufficient input data and interpolation strategies that prioritized continuity and ease of reproducibility over precision. Of course work must go on despite this, but I encourage the authors to review the Fretwell et al., 2013 paper and figure out which of their results might be an artifact of limited data availability or the interpolation algorithm. Although Bedmap-2 was constructed at a 1 km resolution, but in reality the smallest feature that it resolves is actually 5 km. The reason that the modal size of many of the lakes Wright and Siegert., 2012 inventory was 5 km was more related to resolution issues than actual lake sizes.

Indeed Carter et al., (2011) showed that though careful analysis of the line data that many apparent enclosed basins in the hydropotential were actually gridding artifacts, which ultimately did have an outlet. Also it is generally understood that surface altimetry south of 86S is of lower quality do to what is known as the "pole hole" south of the southern most limit of ICESat coverage.

C. It would be useful for the authors to think a bit more deeply as to why lakes form where they. The model may not be able to address all of these issues and if so it would be useful to know what might be misses. Tabacco et al., 2006 provides a nice review on several classes of physiographic settings in which subglacial lakes form. A review of Fricker et al., 2010 and Sergienko et al., 2011 would show that an entire class of lakes can also form in the lee of areas of high basal traction or sticky spots, and originate entirely from ice dynamics.

D. Obviously the choice of F=1 or 0.75 seems a bit too arbitrary and could either use some more justification or find a way to show how varying the parameter matters over a larger spectrum of values.

Specific comments:

P1179 Line 26: What do you mean by conceptual breakthroughs?

P 1181 Line 5: a number of references predate this (Alley et al., 1989; Tulaczyk et al., 2000)

P 1181 Line 12: make sure you emphasize the sensitivity of water is 10 times more sensitive to surface slope, but bed slopes can in places be 10 times greater than the surface slope.

P 1181 Line 17: Glad to see effective pressure mentioned here

P 1182: The case made for hydraulic minima equally lakes is oversimplified. They

should expand more on this matter, specially the implementation in ArcGIS.

P1182 Line 27: Glimmer (Rutt et al., 2009) should be cited

Page 1183, the wrong figure of Pattyn, 2010 cited; they probably meant Figure 2 instead of 1b.

Page 1185: Where is the justification for the cold-bedded ice masks? Also on this page, why is 2500m of ice the delineator between ice streams and divides? Please provide citations for these seemingly arbitrary physical definitions.

Pages 1186-1187: We again stress that the ice models used and the interface flow are not coupled processes so it's hard to take what they say here as seriously as they intend.

P 1187 Line 1: You may want to look to Catania et al., 2012 and Conway et al., 1999 for a brief history of the Siple Coast as these works seem to compliment what you're doing and I don't see them referenced.

Page 1191 Line 1-5: You mention that the 3rd option is least preferable but I don't know that that would be such a bad thing. It would indicate that their technique of standard GIS watershed delineation is not appropriate for what they are doing. Not a problem in my opinion.

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