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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.

Anonymous Referee #1

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This paper seeks to model both the contemporary and paleo distributions of subglacial lakes beneath both ice sheets. While I am deeply sympathetic to the intent, I have substantial difficulty accepting the methodology. I feel the paper needs to present fewer data resulting from different scenarios, and focus on building a stronger caveat base around its existing assumptions for the most suitable scenario. The suggestion of substantial subglacial lakes in Greenland, up to 250 km2 in area or 190 m deep (depending on which DEM is used), is of course of great interest to the cryospheric community, but it would seem prudent to attempt to validate such an assertion when data exists to do so (i.e. ICEBridge radar).

General Comments

1. The biggest issue for a modeller to swallow is clearly the use of steady-state equipotential surfaces. While I can appreciate this was done for simplicity, if this paper is to be published, this assumption needs to be accompanied by a much larger caveats section. The limitation is most severe in Antarctica, rather than Greenland. While the authors provide a reference for near-flotation englacial water pressures on the Antarctic Ice Streams, there have of course been numerous boreholes in the ice sheet interior that are effectively "dry" (i.e. F = 0 in the cold ice encasing ice cores). Unlike Greenland, where most englacial water originates as surface melt and likely travels through the englacial pseudo "aquifer" to the bed following Shreve (1972) gradients (and perhaps cite something like Colgan et al., 2011 for F = 1 in Greenland), in Antarctica most subglacial water is produced at the bed and routed at the bed, beneath the cold overlying ice. Shreve (1972) assumed temperate ice, with surface melt being the water source. The cited work of Fricker et al. (2007) indicates small-scale pipe flow, rather than large-scale Darcy flow, is a key mechanism in routing subglacial water in the Antarctic. Although they appear to work, it is not immediately clear to me why steady-state equipotential surfaces can be expected to describe subglacial water flow in a setting so different from Shreve (1972). A second issue with steady-state implementation (from my experience at least), is that "water" can collect at an initial sink point, rather that potentially overtopping and aggregating with a larger water mass (which leads to comment two...).

2a. After searching in the text, I cannot find the minimum lake area threshold employed. From Figure 4 it appears to 1 km2, presumably because that is the pixel size? Using BEDMAP2 to infer 1 km2 features seems to be a case of "over-precision" to me. Given my familiarity with BEDMAP2, I would think 5 km is the minimum resolution which potential flowlines might be extracted. But perhaps more important than acknowledging the "effective" (rather than "nominal") horizontal resolution (and it was only in the most densely sampled areas that flight lines were 5 km spaced), is the vertical uncertainty... Figure 12 of Fretwell et al. (2012) has >50% of East Antarctica with a vertical uncertainty of >100 m. Is there any way to propagate this into lake distribution uncertainty (i.e. see which lakes persist through a large number of random perturbations in which each grid point is perturbed by, for example, a random distribution around 0 m with a standard deviation of +/- 100 m)? Talking about 1 km2 water bodies when the elevation uncertainty at that resolution is +/- 100 m seems to do a disservice to the work.

2b. I would be interested in knowing the smallest subglacial lake area observed in the Wright and Siegert (2011) dataset. Presumably there is a physical lower limit for subglacial lakes, below which the body of water is "unstable" and will prefer to migrate to a larger body of water (e.g. analogous to the "smaller conduits flowing normal to the equipotential surfaces will thus be deflected toward existing larger conduits" of Hooke, 1989). With this in mind, I would suggest that the observed minimum lake area be used as a threshold, with the analysis restricted to only simulated lakes of greater than minimum area. That of course raises the question of what to do with the discarded "water" of the micro-lakes, which presumably re-organize themselves into more substantial meso-lakes in real life... a key process that a steady-state equipotential surfaces cannot capture, and a likely reason why the authors are finding two orders of magnitude more "unconsolidated" lakes than observed "consolidated" lakes.

3. A cold ice mask is used to discard lakes that form in areas of cold-based ice. Presumably, it is assumed that any water flowing into areas of cold-based ice refreezes? While zeroth order, that is certainly a fair representation of the effect of cold versus warm ice on the output side of the authors' water budget. What about on the input side of the equation? I do not see it stated in the text, but I would think that the same cold-based ice mask should be used to remove grid cells from the pool of "source" cells. When "cells that have more than 5000 cells flowing into them were used to arbitrarily define networks of meltwater flow concentration", what happens if 4000 of those cells were in cold based areas? At present, the paper reads as though all cells are possible water sources, but only a subset are possible water sinks, resulting in an apparent mass conservation disconnect. It should be clearly implemented and articulated that the only sources and sinks for subglacial water are the subset of warm-based ice

nodes.

4. I think the authors should use conventional terminology of "false-positives", "true-positives" etc, in their accuracy description. Continual use of phrases like "predictions that correspond to a known subglacial lake location" are just plain clunky. Liang et al. (2012) provide a fairly comprehensive statistical accuracy summary of lake hits and misses, with mathematically defined quantities like "precision" and "recall" that seem to be useful to the reader. Table 1 would definitely benefit from some of this clean language, as well as discarding the two 1 km columns and possibly the ALBMAP columns as well. It is not entirely clear to me what purpose the ALBMAP comparison serves, as it receives no mention beyond P1185 L22. I would think sufficient competing lake number estimates are contained within the paper without further confusion from the foray into ALBMAP.

5. I found the entire thread dealing with lake genesis during deglaciation to be somewhat tenuous. My impression is that the authors are using the observed DEMs of Greenland and Antarctica for the "present day" snapshot (Table 2), and then modeled ice geometry for the historical epochs. The direct comparison of modeled and observed ice sheet geometries is not permissible for these purposes. Observed ice sheet geometries have significantly more structure at the ice sheet surface than modeled ice sheet geometries, as models have a tendency to exchange bumps and valleys in the ice sheet surface with relatively clean parabolic profiles. As a result, there effectively is far less surface structure in the historical epochs than the present-day epoch. Given that equipotential sink points are sensitive to surface structure, it is therefore not entirely unexpected that your observed DEM produces more lakes than all of our modelled DEMs. I suppose to make the comparison entirely fair, the present-day DEMs should also be modelled (i.e. arbitrarily "smoothing" the BEDMAP2 DEM is not the same). But deglaciation DEM aside, what the deglaciation cold-based ice mask employed? Presumably epoch specific masks from Pattyn (2010) rather than assuming the ice temperature distribution has not changed in the past 20 ka?

6. The apparent lack of any validation for the Greenland modelled supraglacial lake locations is somewhat surprising (galling even). While a subglacial lake inventory does exist for Antarctica, in many ways Greenland is a better validation target as it is smaller and better observed, and its glaciological setting is more closely aligned with the assumptions unpinning Shreve (1972), and hence the steady-state model employed in this study. While the observed proglacial hydrological outlets of Lewis and Smith (2009) are a charming addition, the authors do not provide any insight where subglacial water has been observed in the interior (aside from citing Oswald and Gogineni (2008)). There is abundant radar data available for Greenland which has the potential to constrain the modeled lake inventory... why not at least try to demonstrate the existence of the inferred 250 km2 Greenland subglacial lake that will be new to science?

Specific Comments

Title – "Predict" implies the subglacial pathways of the "future" to me. Perhaps plain old "modelling" is more appropriate?

P1178 L6 – This "two orders" of magnitude discrepancy with observations throws up a flags at the very start of your writing.

P1181 L13 – Pw and Pi reversal?

P1182 L24 – Some description is needed of this smoothing (i.e. filter type / width)

Section 2.3 – Was there no thermal mask employed for Greenland? I see the authors have posted a comment on replacing the (now seriously) outdated Bamber et al. (2001) DEM with the new Ice2Sea DEM (Bamber et al., 2013) instead, which has been available to the community in alpha version for two years. This appears to change their results and discussion of the Greenland Ice Sheet significantly.

The use of non-scientific terms like "success rate" (P 1185 L26), "hit"/"miss" (Figure 1) "cut" (Figure 4) could really be phased out in place for proper statistical terminology.

P1186 L7 – I think this pseudo Monte Carlo needs to be bolstered into a real Monte

Carlo that also takes uncertainty in bedrock elevation into account during your random simulations.

P1190 L11 – Or perhaps the lack of difference whether or not the depth of the lake is included in the input DEM merely confirms that the horizontal dimension of subglacial lakes far exceeds the vertical dimension (i.e. they are wide and flat, so it doesn't matter much whether or not DEMs are corrected for them).

Is my interpretation of Figure 4 correct if you are saying there are >5 undiscovered subglacial lakes that are $\sim$100 km2, and one 250 km2 subglacial lake, beneath the Greenland Ice Sheet? I find it difficult to accept that researchers have so far failed to find a 10 by 25 km water body beneath the Greenland Ice Sheet...

Table 1 presents seven estimates for the number of lakes beneath the AIS and obliges the reader to wade through the text to figure out which DEM/resolution/temperature mask is most suitable. I think those assumptions should be clearly articulated in the text and a more concise package of data presented to the reader here.

Section 6 – Going into bullet form gives this final portion of the manuscript an unfinished appearance. I believe the convention in TC is to use paragraph structure.

Figures – The structure, quality and ease of reading are quite variable.

References – Why do so many references have a string of hyperlinked page numbers following them? Is this a new TC feature?

Technical corrections

None at this time.

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