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Interactive comment on "High-resolution modelling of the seasonal evolution of surface water storage on the Greenland Ice Sheet" *by* N. S. Arnold et al.

Anonymous Referee #2

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This paper describes the use of a model to predict the formation of supraglacial lakes, their capacity for melt water storage, and the timing of potential drainage events. The paper uses a model that has been described and tested elsewhere, but applied to a larger area of the Greenland Ice Sheet, and it compares the results with Landsat imagery, concluding that the model does a favourable job.

The paper is well written and concise. Many of the results seem rather obvious, especially given the simplicity of the lake drainage criterion and the fact that the model is (as far as I could tell) the same as presented elsewhere. One important conclusion, however, is the potential for clustering of nearby lake drainage events even when there is no 'communication' between lakes in the model - this seems to be an important ob-

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servation to bear in mind if ascribing such clustering to a direct mechanical connection.

I have a few comments that I think the authors should consider.

1. The model is deemed to be doing a good job, but there is little to compare with to assess this assertion. On reading in section 3.1 that 252/505 or 179/229 lakes were consistent with surface depressions in the model, I thought that this didn't sound so good. Similarly, the correlation of the lake volumes does not necessarily seem all that convincing given that very different slopes are found for different values of Fa but the correlation coefficients do not very clearly differentiate which is best. Could some indication be given of how this compares with the results of Banwell et al 2012, for instance, and of other authors? Are there any other studies that have used the GIMP data set to predict lake locations, with which this study could be compared? If not, it would be worth emphasizing that this is the first study to try to do that, and to conclude with more pros and cons, rather than simply stating that the performance is good.

2. The method of comparing lake volumes could be explained a bit more clearly. Are the volumes of individual lakes being correlated (ie. satellite derived volume vs modelled volume)? Assuming so, what is done for lakes that are not consistent between satellite imagery and model? Are the lakes in each different image treated as distinct data points? (It seems the correlation makes use of all 4 images in each year 2001/2002). Related to this is the testing of the drainage threshold, which seems to be achieved by looking at the lake volume correlation rather than by looking at whether the modelled lake drainages are consistent with the satellite imagery. I'd have thought that a better test would be to evaluate, on an individual lake basis, whether the lake drainage is successfully predicted or not (within the temporal resolution of the images), and to take some bulk statistics of the model performance from that.

3. Throughout, there seems to be an implicit assumption that all moulins are at the bottom of drained lakes, and these are the only route for water to drain subglacially. This seems quite unlikely to me, and I wonder if some evidence could be given to

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back this up. It is quite common for lakes to overtop and spill into a nearby moulin (eg Tedesco et al 2013), for moulins to remain persistent from year to year (eg Catania & Neumann 2010), and for water to drain through crevasse fields. On the same note, the amount of meltwater that apparently runs off supraglacially (40-50%) seems very high - this ought to be visible as enormous surface streams (?). Is there other evidence to suggest that these numbers are correct. More comparison with other runoff estimates / experience on the ground would be good.

4. In the figures, and throughout much of the discussion, many of the meltwater quantities are expressed as percentages of total cumulative melt. I think it would be better to show/discuss raw numbers - certainly in the figures, it would be best to show the actual cumulative amount of water stored in the lakes, routed subglacially and routed supraglacially (a separate panel could also show the same as percentage of total cumulative melt, since this wouldn't take much space). For instance, the apparent 'large' storage in the lakes at the beginning of the melt season is a bit misleading since this is a large percentage of a small number - the actual quantity of water stored in the lakes is presumably still rising for much of the early melt season.

It also needs to be made much clearer what is meant by 'total cumulative melt' - is this only meltwater that runs off / fills lakes, or does it include meltwater that subsequently refreezes in the snow pack? Adding up the contributions to figure 2b by eye, it looks like there is not much left to include any storage in snow. This is especially important to clarify given current interest in the amount / potential for water storage in firn as the ablation area grows.

5. Lake drainage events and the lake draining criterion. The criterion is based on lake volume reaching a constant multiple of the ice depth, so that lakes over thicker ice are required to become larger before they drain than those on thinner ice. This is argued to be the reason for the up-glacier progression of lake draining events over the season, but this assumes that all lakes fill at the same rate. A potentially dominant reason for the upglacier progression is the reduced surface melt rate, and increased surface storage

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capacity at higher elevations (though the size of catchment basins must also come into it). Given that individual lake drainage events are not tested - and that the method of choosing the Fa parameter is somewhat indirect (see point 2 above), it is difficult to see whether the drainage criterion is doing a good job. On the face of it, the assumption of a constant cross-sectional area for the fracture (which at 1000-10000mˆ2 is quite large) seems rather arbitrary. Has any effort been made to test if this does 'better' than using an even simpler fixed volume threshold, for instance (ie independent of any ice geometry)?

Minor comments

6. Page 6144, Line 17 - The lake volume threshold numbers described in the abstract need some units, and it needs to be clarified what units the ice thickness is measured in. I'd suggest being more explicit, e.g. 'The threshold volume is V = A H, where H is the ice thickness, and A the potential fracture area. The performance is maximized for A in the range 4000-7500m.'

7. Page 6148, Line 26 - Reference to Plummer et al 2008 appears to be missing.

8. Page 6154, line 28 - Figure 3b?

9. The early part of the paper appears to draw quite a battle line between remote sensing and modelling approaches to dealing with lake drainage, which I found a bit odd. Both fairly obviously have advantages and drawbacks, but presumably the 'best' method is to make use of both. I don't think such a binary picture needs to be drawn.

References

Catania, G.A. and Neumann, T.A. 2010 Persistent englacial drainage features in the Greenland Ice Sheet, GRL 37

Interactive comment on The Cryosphere Discuss., 7, 6143, 2013.