Response to Anonymous Referee #2

We thank the referee for their considered comments, and we provide a response to each comment below, using italics to highlight our response.

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

We are pleased the referee finds the paper well written and concise. The methods we employ have been used elsewhere, but the motivation for this paper is very much to simulate lake drainage across a wider area of the ice sheet, and to investigate the possible controls on the timing of lake drainage. As we have stated in our responses to referee 1, we can certainly change the emphasis in our conclusions to highlight the clustering of drainage which occurs in our model without any 'communication' between nearby lakes.

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.

As far as we are aware, we are the first group to use the GIMP DEM in this way, and we will make this clear. As we have discussed in our reply to referee 1, our study does form a type of validation for small-scale topography captured by the DEM. We can develop this discussion in any revised manuscript if requested. We can certainly compare our study with others; Leeson et al. 2012 model lake filling (with no drainage) for a different area of the ice sheet; they present their results differently, but overall simulate the location of 66% of lakes accurately (p1081). Thus, our results are certainly comparable, and indeed *exceed that paper for our higher pixel threshold. With due respect to the referee, therefore, as we show in the paper, the chance of having 179 out of 229 lakes located within depressions simply by chance has a vanishingly small probability given all the other possible sources of error, and is certainly a favourable result. In terms of the variation in the coefficients, and the lack of a single best-fit value, we deliberately show a range of measures which emphasise different aspects of the fit, given the difficulties inherent in comparing model results with observations, and refer the reader to a paper which discusses these issues.*

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.

We will clarify this. The coefficients we present are calculated by taking the modeled water depth for all pixels containing water on the date of each image, and comparing them with the water depths as calculated from the landsat image. Each image is treated as a distinct dataset, but we amalgamate the resulting pairs of water depths (modeled vs observed) from each image in order to calculate our overall coefficients of agreement to maximize the sample size, and hence statistical robustness of our results. We cannot compare pixels where there is water in only the model or only the visible imagery, but we could track overall lake volume in the model and in the imagery on each date, though given that we only have 9 dates, any statistical comparison would be based on a very small sample size.

In terms of examining lake drainage depths in the imagery compared with the model, this is effectively impossible given the low temporal resolution and small overall number of the visible images that are available. The bulk of lake drainage events in the model occur either before the pair of images we have for early July in 2001, or in between those and the pair for early August 2001. For 2002, the smallest time interval we have between successive images is 17 days, which again precludes any meaningful comparison.

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 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.

The assumption that all moulins can only form at the bottom of drained lakes is correct, and we will clearly state this in a revised version of the paper. This assumption follows Banwell et al (2013), which states reasons for why this is a realistic assumption. These reasons include:

1) Although additional moulins, not associated with supraglacial lakes, are likely to be present on the ice sheet surface [Catania et al., 2008; Colgan and Steffen, 2009], evidence suggests that moulins not associated with large depressions do not route large volumes of water to the ice sheet bed compared to moulins associated with depressions [Catania and Neumann, 2010; Hoffman et al., 2011].

2) The assumption that a moulin has the potential to form in the lowest grid cell of every depression gives a moulin density of 0.25 km^2, which is similar to those mapped from satellite imagery by Colgan and Steffen [2009] (0–0.89km^2) and Zwally et al. [2002] (0.2km^2) for the Paakitsoq region. Thus, our assumption that all surface depressions have the potential to contain a moulin is likely a good approximation to the actual number of moulins that are available to route large volumes of meltwater to the ice sheet bed.

In terms of the proportion of water we call supraglacial runoff, we will re-word this to make it clear that this is water which does not enter the subglacial system via drained lakes, and which could either runoff supraglacially or enter crevasses and/or moulins outside lake basins (see also our responses to referee 1 in this regard).

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.

This is a useful comment and in the next iteration of the paper we will add the meltwater volume in m^3 as well as a percentage (in brackets). The motivation for discussing the % values, and presenting the figures in those terms, is that using total water volumes rather than % values rather obscures the behaviour at the beginning of the melt season, as the water volumes in themselves are quite small (as referee 2 correctly infers).

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.

This is a valid comment; what we call total cumulative melt is in fact the total cumulative runoff produced by the surface energy balance model, that is surface melt minus refreezing or storage within the supraglacial snowpack. We will make this clear in a revised version, and could quantify this amount of supraglacial storage over our model domain if requested, through the end-of-season total will be small as most of the snowcover in our model domain has melted by the end of the modeled periods in each year. Please also see our response to Reviewer 1's comment 8.

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 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 1000-10000m^2 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 (i.e. independent of any ice geometry)?

We discuss the fact that the up-glacier progression of lake drainage events is driven by the upglacier progression of lake filling due to later melt onset at high elevations, the larger drainage threshold due to thicker ice, and the relative size of the lake catchments in some detail in our discussion section (e.g. p6155, lines 7-11; p 6157, lines 12-17). Our drainage threshold is quite simple, and we acknowledge that. We did some early testing of the model with constant drainage thresholds across the domain, in fact; the problem with a constant volume threshold is that if it is set to a small volume, to allow the drainage of the smaller lakes typical at lower elevations, lakes higher on the ice sheet drain much too soon, and never reach the size observed in the visible imagery. Setting a higher constant threshold across the whole domain to allow larger lakes to form at higher elevations means that no drainage events occur lower down on the ice sheet. We are happy to discuss this in a revised version of the manuscript if requested. Our results show that ice thickness does exert some control on the observed size of supraglacial lakes.

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.'

As explained more fully in response to Reviewer 1's comments, we will explain what we mean by the 'fracture area' in the next paper iteration and will give it units (i.e. m^2).

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

We will add this reference (Plummer, J., S. Gogineni, C. van der Veen, C. Leuschen and J. Li. (2008), Ice thickness and bed map for Jakobshavn Isbræ. Center for Remote Sensing of Ice Sheets Tech. Rep. 2008-1.)

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

This should indeed say Figure 3b instead of 4b.

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.

We agree this this comment and will reword this section so that the remote sensing versus modeling approaches are briefly compared and then described as complimentary approaches if used together (i.e. in this study we use satellite imagery to help parameterize the drainage criterion in the model).

References

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