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Interactive comment on “Simulating the growth of supra-glacial lakes at the western margin of the Greenland ice sheet” by A. A. Leeson et al.

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Dear Reviewer,

We thank you for your constructive comments on our manuscript. We have attempted to provide further clarification where requested and we propose to revise the manuscript in order to address your comments by making the changes detailed below. We hope that you will approve of the measures we have taken to improve our submission. The review comments are repeated below in italics with our responses in bold text.

Kind Regards,

C1032

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While not explicitly stated, it seems that the initial condition imposed for ice sheet surface is an ice sheet surface completely devoid of any lakes (i.e. just empty "sink" points). Thus, all lakes are modelled as filling from empty within one melt season. In actuality, substantial volumes of lake water overwinter in supraglacial lakes in western Greenland. Thus, at the beginning of a melt season the majority of lakes already contain substantial water, and only a minority of lakes are empty following late melt season drainage events. The initial condition therefore does not seem consistent with the observed seasonality characterized in many of the references cited. If my interpretation is correct, it would imply that the paper presents a maximum upper bound on the lake volume generated and retained within a single season, when in reality a much smaller fraction of annual melt would be retained in lakes that already contain melt from many years. As a corollary, that would mean the paper presents a lower limit for runoff.

We do assume that the ice sheet is 'empty' at the start of the melt season and agree that this initial condition may be a source of error in our results. We know from observations that lakes freeze over at the end of the melt season (Sundal, Shepherd et al. 2009; Selmes, Murray et al. 2011). If lakes do not freeze completely then we can assume that there is an ice 'lid' covering the lake. If the ice lid is thin, the radar is reflected at the lid/lake boundary; radar cannot penetrate water. Since the DEM represents a scattering horizon at depth rather than the true ice surface and if the ice lid is thick (1-3 m), the surface of the lake and the surrounding ice in the DEM will be slightly below their absolute value, but the gradient between them will be the same.

The DEM we use was formed from InSAR data collected in the winter of

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1996. An InSAR DEM of an ice sheet surface represents a scattering horizon at depth rather than the true surface. Rignot et al (2001) showed that for ice, specifically the surface of Jakobshavn Isbrae which is relatively close to our study area, the radar penetration can be as shallow as 0m (typically 1m (+/- 2 m)). For firn this depth was found to be closer to 10m however much of our DEM (95%) lies below the permanent snowline (1600 m a.s.l in MAR).

It is clear therefore, that the DEM will include existing supra-glacial lakes, although their surface elevation may be 1-3 m below the absolute value. In order to ascertain which of the sinks in the DEM contain frozen lakes we would need observations of lakes from the 1995 melt season, including a record of which lakes drained and which re-froze. Since the observational datasets of sufficient temporal resolution are obtained from MODIS imagery (which are only available from 2001) we are unable to quantify this uncertainty.

Regardless of the uncertainty in the DEM, the topographic limitation we propose in our study still exists. We suggest that our estimate of 12% therefore seems reasonable, and can be used to represent the amount of runoff that can be stored in lakes in addition to water which has overwintered in depressions in the ice sheet.

The revised manuscript will present the initial condition clearly in the method section and will discuss the potential impact imposing this condition this has on our results in the discussion.

The dependency of the agreement between simulated and observed maximum cumulative lake area on model time-step seems more like a concern, rather than a result, to me (P1319 L9). It is my understanding that neither modelled discharge nor head should significantly differ with the choice of time-step in hydrological modelling.

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When this happens, it typically indicates a shortcoming in the numerical method used to solve the system of equations describing node-coupled fluxes (i.e. not true mass conservation of fluxes). In fact, producing an identical secondary simulation with a time-step 1/10th the size of the time-step used in a primary simulation is considered a good way to demonstrate sound numerical implementation. The paper presently suggests the opposite. While not stated in the paper, the relatively small time-steps imposed lead me to believe an explicit numerical method was implemented (i.e. Euler forward?), as opposed to an implicit numerical method which would likely run on hourly or daily time-steps. Explicit numerical models are widely recognized to be highly time-step sensitive. For example, it is easy to imagine how discharge differences can arise between runs in which the free-surface gradient is maintained a single 60-sec time-step, rather than allowed to gradually decay over 60 1-sec time-steps. I suppose more clarification is required on the numerical method, but it is quite possible that an implicit numerical model method have to be implemented to convince the reader there are no numerical artefacts.

Model outputs for lake onset date, filling rate and location are not found to be time-step sensitive although we acknowledge that simulated lake area does show significant time-step sensitivity.

We have investigated this further since submission of the manuscript and are testing a fourth order Runge Kutta (RK4) approximation with which to integrate the flow. We have also modified the method by which water is accumulated into lakes. Initial tests suggest that these measures act to reduce the time step sensitivity, although they do not completely remove it. We find that in some locations, because of the length scales involved, free surface gradients are such that even using the RK4 approximation, a proportion of cells experience water displacement greater than their water contents. The only way to reduce this error is to adopt a smaller timestep.

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We chose to use the RK4 approximation over a full solution in order to retain the model's simplicity, which we believe to be a desirable attribute. In addition the water is now accumulated into lakes iteratively at each timestep; the water in a depression is accumulated until it is all incorporated into a lake, rather than on a cell-wise basis. As this is an early study, and one of the first attempts to model supra-glacial lake evolution, we feel that these actions adequately address the highlighted concern over timestep dependence, within the scope of this paper.

The revised manuscript will discuss model results obtained using an RK4 method of integration and a modified accumulation scheme. The method section in the revised manuscript will include details on this new approach and its implementation and we will discuss the limitations that the use of an RK4 over a full solution imposes on our results in our consideration of the study with particular reference to maximum lake covered area.

Supraglacial lakes tend to form at higher elevations on the ice sheet, where firn is present (year-round by definition). Routing meltwater in these high elevation regions without even a crude parameterization for firn effects seems to be quite limiting. For the early portion of the melt season, Darcy porous flow is far better for describing the horizontal movement of meltwater than Manning's open channel flow. Indeed, even at the end of the melt season, the vast majority of the ice sheet surface above 1200 m is still covered by firn as opposed to open channels. It is easy to imagine firn effects (such as runoff delay and/or refreezing retention) as primarily responsible for the discrepancy between observed and modelled lake area growth. Similarly, the absence of a parameterization for lake refreezing or draining at the end of the melt season limits the predictive power of the model. I can appreciate that the authors have limited their title to modelling the onset of supraglacial lakes, but the introduction of the paper

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couches the paper in the importance of what happens at the end of the melt season.

We use runoff simulated by the MAR (Modele Atmospherique Regional) to force our model which features a comprehensive snow model including retention and refreezing. Most of the runoff simulated by MAR occurs over bare ice, however runoff can occur in snow when the snow pack has a liquid water content of 6%.

Since our submission of this manuscript we have modified our model to include a treatment of flow through snow. The new version of the model has a modified velocity subroutine which first uses MAR simulated snow depth to identify if snow is present. If snow depth is equal to zero, flow velocity is calculated using Manning's formula. If snow is present then velocity is calculated using Darcy's Law. We use an equation derived by Shimizu (1969) to calculate porosity (a requirement of Darcy flow) using snow density simulated by MAR and an assumed snow grain size of 1 mm. We have performed a sensitivity analysis against this value.

The results discussed in the revised manuscript will be made using Darcy and Manning style flow, where appropriate. The method section will also be modified to include this addition to the water flow scheme.

The presentation of lake location as an independent model output seems to be slightly misleading, as lake locations are primarily the result of the DEM accuracy (i.e. the DEM determines the sink locations, the hydrology model just fills them). Perhaps the good prediction of lake location should be presented primarily as an endorsement of the DEM and secondarily evidence of realistic routing?

We agree that lakes only occur in locations where there are depressions

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in the DEM, however the timing of their appearance in the depressions is independent of the DEM. We find, for lakes that are coincident in both the model and observations, that the correlation between the simulated and observed onset date (day of first appearance) has a Pearson Correlation Co-efficient of 0.71.

In this study; locating lakes in the right place at the right time is a function of runoff production and realistic water routing as well as DEM accuracy. We propose therefore, that the skill our model displays in predicting supraglacial lake location and onset is a result of uniting these three factors.

In our revised manuscript we will extend our evaluation of the model to include a comparison between observed and simulated lake onset date. We will attribute the locating of observed lakes to DEM accuracy and attribute the timing of lake onset to runoff production and routing.

I would encourage the authors to also include sensitivity analyses of additional key variables such as the Manning coefficient and DEM accuracy.

A sensitivity analysis of simulated lakes to the value of Manning's 'n' has been performed using values of 0.01, 0.011 and 0.012; a range of values derived experimentally (Lotter 1932). We compared a time series of daily lake area for all five values and found no range in onset date and a negligible (0.14

We also investigate the impact of the DEM smoothing window on simulated lakes by applying smoothing windows of 3, 5 and 7 cells to the DEM and repeating the analysis detailed in this manuscript.

The revised manuscript will describe our Manning sensitivity study and we

will provide, as supplementary material, a time series of the cumulative lake area anomaly with respect of the observations for all three values of n . We will also provide, as supplementary material, a comparison table detailing the number of lakes that are coincident in both the model output and the observations, the correlation co-efficient between their onset dates and the maximum simulated lake area.

P1308 L2: "routing" seems to be more frequently used scientific literature than "routeing".

Instances of 'routeing' will be replaced with 'routing' throughout.

P1308 L10: The 17 % presumably refers to fast drainages observed by Selmes et al.(2011)... after a quick look at Selmes et al. (2011), it seems to me that number is specific to NE Greenland?

In quoting 17%, we refer to figure 2 in Selmes et al (2011) which suggests that in SW Greenland, averaged over all years considered, 17% of total lake area experiences fast drainage. In the revised manuscript, the phrase '17% of lakes' will be replaced with '17% of lake area'.

P1310 L8: Should that be "Sole et al."?

'Sole' will be replaced with 'Sole et al'

P1311 L10: I imagine it is supraglacial lake surface, rather than supraglacial lake bed, that is incorporated in this ice sheet surface DEM? Presumably the majority of the true ice sheet "sinks" were already filled with lakes at the time of DEM acquisition... is the m-scale vertical offset between lake bottom and lake surface important in

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100m-scale horizontal routing? I would think this may make your model overestimate lake area, as the modelled lakes have shallower and broader sink points in which to accumulate, in comparison to the actual ice sheet surface / lake bed topography.

Supra-glacial lakes range in diameter from tens of metres to kilometres. In terms of the impact of supra-glacial lakes on ice dynamics, the larger lakes are the most important as smaller lakes hold too little volume of water to drain through hydrofracture (Krawczynski, Behn et al. 2009) and indeed some studies (Selmes, Murray et al. 2011) disregard any lakes smaller than 0.125 Km² which corresponds to a diameter of 400m for a circular lake. We agree that using a DEM posted at 100 m may overestimate the area of individual lakes, however we suggest that this over estimate is a small fraction of the size of the lakes of interest. When considering an area average, overestimating the size of the larger lakes will to some extent be compensated for by the omission of sub-grid scale lakes. In our revised manuscript, we will restrict our analysis to only consider those lakes (observed and modelled) which are larger than 0.125 Km².

P1312 L8: "additional physics are".

Ok. 'additional physics is' will be replaced by 'additional physics are'.

P1312 L25: From where does this assumed 12% ASTER-MODIS discrepancy come? Sundal et al. (2009) suggest it is only 4% (see their figure 2 and section 3.2).

Sundal et al do discuss a 4% discrepancy, but this is to do with the digitisation of the 15 m resolution ASTER imagery. In Section 3.2 paragraph 4, Sundal et al (2009) discuss the fact that the 250 m resolution of the MODIS instrument means that it cannot resolve lakes smaller than this. By comparison with the finer resolution ASTER imagery this discrepancy was found to be 12%.

P1313 L11: Rather than "a dynamical model of water flow...", can you say "a fully transient 2D hydrology model..."? At present it is left to the reader to assume the model is 2D and can be run in both steady-state and transient modes.

We do run the model in both steady-state and transient modes however we never explicitly state that the model is 2-dimensional. The revised manuscript will explicitly state that the model is 2D. Where results from a transient run are presented the model will be termed accordingly.

P1318 L15: Comparing the red solid (6.5 %) and red dotted (4.0 %), it looks like more of a 63% overestimate $((6.5-4.0)/4.0)$ to me, rather than the 51 % stated. In either case, it certainly tests the bounds of "reasonable" agreement.

The calculated values of our overestimate of maximum cumulative lake area are 55% for 1000-1200, and 30% each for 1200-1400 and 1400-1600. The overestimate for the combined 1000-1600 m a.s.l. elevation band is 33%.

We suggest that this is a reasonable agreement because a) we calculate our estimate of cumulative lake area by aggregating observations of daily lake area in turn. Since the observations we use are temporally sparse, it is likely that these observations do not capture the maximum lake extent, particularly of those that drain, since this may be reached between observations.

b) Sundal et al (2009) suggest that their observations underestimate total lake area by at least 12% due to small lakes that are not resolved by the MODIS instrument and in their supplementary material they present an estimate for error associated with the mis- categorisation of ice-covered lakes of around 14.9%. These errors cannot be applied as a general correction as they are

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based on single image case studies, and the actual values are likely to be highly temporally variable. If there is a typical error of 26.9% in the observations, then our simulated values are eminently reasonable.

In the revised manuscript we will modify the values quoted for our overestimate and ensure that figure 4 reflects these accurately.

P1318 L28: The present estimate of the observed meltwater volume is very zerothorder. From where does the assumed mean lake depth of 3.11 m come? Box and Ski (2007) provide a very wide range of (maximum) lake depths (not sure how to translate that into a mean lake depth), and Liang et al (2012) use a first-order area-to-volume conic approximation. At a minimum, with the presently employed estimation, a range of lake depths should be used to provide a range of comparable observed water volumes.

The assumed mean lake depth of 3.11 mm comes from the simulated lakes. This was merely intended as a simple comparison; however I agree that a more robust method would add value to our conclusions. The revised manuscript will quote values for observed lake volume using observed area and an area-volume conic approximation.

P1321 L5: See Colgan et al (2011) regarding crevasses in western Greenland. To be consistent with the notion that the presence of crevasses reduces the meltwater available for lake, is there a way to simulate the presence of crevasses by say reducing the melt available by routing based on an assumed crevasse fraction?

Our study area extends from the margin to 1751 m a.s.l and covers 16000 km². We can assume that because of it's spatial extent, crevasse density is highly variable in this region. There are too few observations of crevasses in our study area with which to parameterise a spatially distributed crevasse fraction

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approximation and we feel that making such observations in order to develop a parameterisation for our model is beyond the scope of this study.

P1321 L16: You just previously argued that the surface topography (and hence lake position) was stable (on P1313 L5 and again on P1322 L19)?

The fact that the majority of simulated lakes are coincident with observed lakes suggests that the surface topography is stable. However we do observe a very small number of modelled lakes offset from observed lakes are observed and we seek to understand this. One mechanism by which this would occur could be short term fluctuations in ice sheet topography; for example if a lake formed one year, and then completely refroze. This could change the local profile of the depression which could cause an offset in a subsequent year. However because we do not know if any lakes completely refreeze this point is speculative. The revised manuscript will provide clarity on this point.

P1327 L20: Duplicate reference.

The duplicate reference will be removed in the revised manuscript.

P1331 Fig1: Some confusion over whether it is a Bamber or Layberry paper being referenced.

The reference will be changed from ‘Bamber, Layberry et al’ to ‘Bamber et al’

P1333 Fig3: Having the modelled lakes the same color as the background ice is not ideal. Perhaps for easier interpretation you might consider representing the observed and modelled lakes with primary colors (i.e. red and blue), and their

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overlapping area with the corresponding secondary color (i.e. purple)?

In the revised manuscript the modelled lakes will be coloured blue, the observed in red and the coincident area in purple.

P1336 Fig6: I think the description of these results is over optimistic in the text... three of four years appear to have a 100%+ discrepancy between observed and modelled lake area coverage...

We agree that there is a large discrepancy between simulated lake area in years other than 2003 and that which is observed. We comment in the manuscript that we have reason to believe that a large degree of uncertainty exists around the value of cumulative lake area derived in those years due to the relative sparseness of data; we feel that these data are not sufficient to enable a full inter-annual variability study with respect to this metric. The revised manuscript will clarify that our interpretation of inter-annual variability using the model is limited at present due to uncertainty in the observations.

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Interactive comment on The Cryosphere Discuss., 6, 1307, 2012.

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