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The Cryosphere
Discussions

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,

Thank you for your review of this manuscript, we are pleased that you find the study scientifically useful and appreciate your constructive comments. 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, and those detailed in our responses to referees 1 and 2. We hope that you will approve of the measures we have taken to improve our submission. Author comments are repeated in italics and our response is given in bold.

Kind Regards,

Amber Leeson

The main point of concern I have that has not been mentioned by previous reviewers is in the statistical methods used to determine the success or otherwise of the lake location forecasts. The authors use two scores, the Odds Ratio (OR) and the Peirce Skill Score (PSS). The OR gives a result that sounds impressive but should probably not be used since it is so heavily skewed by the overwhelming number of correct non-event forecasts (type d forecasts in a 2 by 2 contingency table). The Peirce Skill Score does better, but again the large number of type d forecasts renders the numbers produced less meaningful. Other tests could be applied that would not give such apparently good results: the Heidke Skill Score (HSS) is similar to the PSS but because of the bias in the data towards over prediction of lakes it comes out with a lower score of about 0.34. The Threat score is considered better for forecasting of rare events, i.e. when type d forecasts are very numerous, and may be more applicable here. It, and the related Gilbert Skill Score (GSS), give lower scores of 0.23 and 0.21 respectively. Perhaps some more sophisticated test which incorporates a spatial dimension would be the best. I would recommend the authors discuss their data with a statistician with expertise in this type of problem.

We used the Odds Ratio and Peirce Skill Score to evaluate our model skill in locating lakes in an attempt to introduce a spatial element to the test (i.e. by testing each cell rather than each lake). Perhaps in seeking to add clarity we have instead, overcomplicated things. On a lake-by lake basis we find that of all the lakes that are observed in 2003, we simulate 59% in the same locations (54% of all observed lakes larger than 0.125 km2).

We know that the observations we use for evaluation are not themselves

without error and by comparison with the original satellite image we see the automated classification scheme used to identify lakes in the MODIS images does not correctly identify small or ice covered lakes in several places where these lakes are represented in the model. Consequently we could be mis-classifying a modelled lake cell with no corresponding observation as a 'miss' erroneously. In addition, this spatial analysis is skewed by how well our model reproduces observed lake area, another metric with which we interpret model skill.

We propose that we will revisit this aspect of our analysis and reconsider the statistical test we use to assess model skill in locating lake cells. We will extend the discussion of applying these statistical test given the uncertainty in the observations and also quote the lake-by-lake figure above to give a more rounded consideration of model skill in locating lakes.

The revised manuscript will focus our consideration of model performance in locating lakes onto a lake-by lake comparison rather than a cell wise comparison. We will include figures for a range of skill scores (PSS, HSS and Threat) and discuss their values and limitations as applied in this case.

The paper would also benefit from a much clearer statement about the rationale for this type of modelling. The abstract ends with a statement about the authors optimism that their work will be developed further but there is little in the paper itself which gives a clear statement of what the next steps in this development should be. This is an opportunity that they should take. If the long-term objective is to be a step towards coupled melt and ice flow models then perhaps this should be stated. Large scale ice sheets models are unlikely to run at the spatial resolution of the DEM used here and so some analyses of the model sensitivity to a coarser spatial resolution of DEM would be useful.

The principle objective of the work described in this manuscript is to provide some insight into the contemporary behaviour of supra-glacial lakes and to inform modelling strategies concerning them. This has two wider applications: principally we hope that forward simulations in time will be possible with this model in order to investigate changes in e.g. distribution and onset under climate change scenarios and secondly we hope to contribute to the consideration of supra-glacial lakes in ice dynamic models, which currently lack provision for lakes. It is unclear at this stage whether the most appropriate form of the latter would be a direct coupling or a parametrisation.

We will certainly consider the performance of our model at coarser resolutions (specifically 1 km) in our future work but since our current focus is on how our model might be used to make predictions for the future we feel that this is beyond the scope of this paper.

We will provide, as supplementary material, a preliminary analysis of model dependence on DEM resolution using the same DEM posted at 1km resolution.

Little said about sensitivity of the model to uncertainty in the vertical resolution of the DEM. Radar backscatter from the ice sheet surface varies across different ice sheet snow facies. At the large scale of the ice sheet this is a small error but, what effect would an uncertainty of 1-2 m in identifying the surface have on the area and volume of supra-glacial lakes given that they are typically quite shallow features?

We consider this point in our response to reviewer 1, comment 1. Supra-glacial lakes form in depressions which are lower lying than the surrounding surface. We feel that errors associated with the vertical resolution of the DEM apply not only to the lake but also to the surrounding ice and so the lake will be resolved with respect to the local topography of the ice sheet.

The assumption is made that the DEM from 1996 is a valid basis for lake prediction a decade or more later. This may be questionable but is probably unavoidable, however this uncertainty should be considered consistently throughout the paper. It is probably not strictly defensible to claim that the error in the input DEM is the reason for the forecasting of near misses (p1321 lines 14-16) but implicitly assume that it works well when predicting a "hit".

We make this assumption on the basis that surface topography is largely controlled by the transmission of basal conditions (Gudmundsson, 2003) and so we would not expect the inter-annual variability of surface topography to be significant. It has also been observed that many supra-glacial lakes are intransient features (Echelmeyer et al, 1991, Selmes et al 2011) which supports this claim and validates our use of the 1996 DEM.

We simulate lakes displaced with respect to observations primarily at lower elevations. We have most confidence in the DEM at higher elevations as reduced flow speed and a lesser abundance of crevasses at lower elevations may serve the dual purpose of rendering the DEM less accurate in representing the 1996 ice sheet surface and enabling the topography to change between 1996 and 2003. Higher up the ice sheet when there is less basal lubrication we can be more confident that the ice sheet surface represents topography at the base and so is less likely to exhibit inter-annual variability. See also response to reviewer 1, comment 15 for a comment on transient differences in lake basin topography. In the revised manuscript we will ensure that this assumption is considered throughout our study.

p1317 ln5, p1317 ln14-16 p1324 Ln6: It's not obvious early on whether the PSS scores relate to the whole study area or just the area between 1000 m and 1600 m

elevation. Need to clarify this.

The PSS scores quoted in the manuscript refer to the area between 1000 and 1600m a.s.l. The revised manuscript will clarify, when quoting statistics, which region of the ice sheet they refer to.

p1318 ln15: Some might think that an overestimation of 51 percent does not mean the model has performed "well" in predicting cumulative lake area. Rephrase this?

Please see our response to reviewer1, comment 13. The revised manuscript will clarify our intent in suggesting a reasonable model performance and rephrase accordingly.

p1324 ln24 to 1322 ln 6: this is an example of a section that is very repetitive of earlier sections and there is scope for reducing the length. This general point has been made by other reviewers.

The revised manuscript will be clearer and more concise.

References

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