Brief Communication: Expansion of meltwater lakes on the Greenland Ice Sheet by I. M. Howat et al.

Response to Reviewer 1

We thank the reviewer for his or her thorough evaluation. Original comments are below in italics, followed by our response.

Overview: The notion that there is no physical limit on the recent upwards migration of supraglacial lakes to higher elevations in Greenland is important. The authors have assembled a dataset to demonstrate this fairly convincingly. The manuscript would benefit from improving the discussion of their algorithm sensitivity and other sources of uncertainty. This would more unequivocally demonstrate their hypothesis is not influenced by biases, such as a decrease in their effective threshold size over time due to a growth in lake area over time.

These issues are addressed in response to the specific comments below.

P4448 L13: An order of magnitude guess of the total number of supraglacial lakes in Greenland is probably well beyond 10ˆ4 and closer to 10ˆ5, rather than "thousands".

Revised to: "Tens to hundreds of thousands..."

P4448 L15: It is not immediately apparent to me the elevation "where seasonal meltwater runoff is less than annual snow accumulation" on the ice sheet. Perhaps you could use the zone facie vocabulary of Benson (1961)? It almost sounds like you are describing the equilibrium line, which is of course far below the upper elevation limit of lakes in Greenland.

Revised to: "...covering the zone of bare ice and extending through elevations where the firn layer is seasonally saturated (i.e. the wet snow zone). Above this elevation, meltwater production is low enough, and the firn layer thick enough, that all meltwater permeates into the firn rather than collecting on the surface."

P4448 L20: If I recall correctly, Zwally et al. (2002) do not really discuss the warming effect of meltwater on velocity, perhaps you need an additional citation.

For clarity, the reference to the effect on viscosity is removed here and introduced separately in the next paragraph.

P4449 L4: You are blending the meltwater mechanisms of increased deformational and basal sliding velocities together. Meltwater increases deformational velocity by warming ice and lowering "effective viscosity" (not a true "viscosity" because ice has a non-linear fluid rheology). Meltwater increases basal sliding velocity by reducing effective basal pressure, by "floating" part of the overburden pressure of the ice, and hence reducing basal friction. You need to clearly articulate that these are two different processes.

Revised to: "However, if warming causes lakes to form at higher elevations, water may reach areas of previously frozen bed, increasing the speed and annual flux of ice to the margin. Increased penetration of meltwater would also heat the ice, reducing its effective viscosity and potentially increasing its flow speed."

P4449 L7: So you are indeed calling the "elevation at which meltwater runoff equals the snow accumulation" the equilibrium line altitude (ELA)? At first glance that almost sounds correct, but upon reflection I suppose there are the additional surface mass balance (SMB) mass loss sinks of evaporation and sublimation that need to be considered. So technically, the ELA would be equivalent to the location where all mass loss (runoff, sublimation, evaporation) is equivalent to accumulation on an annual basis.

Replaced "meltwater runoff" with "ablation"

Also, how does this square with your usage of the phrase to denote the upper elevation limit?

This has been clarified as per the comment on that line.

P4449 L10: Actually, the Liang et al. (2012) population of lakes in West Greenland that was shown to shift to higher elevations in warmer years extended quite high into the wet snow (possibly percolation?) zone. They actually found that lakes formed every year in the bare ice zone, so there was no significant relation between temperature and the lowest fraction of their population's elevation.

Revised to: "Previous work has shown that the distribution of lake surface area does shift to higher elevations in warmer years (Liang et al., 2012)."

P4449 L15: How many images total?

Added: "Our analysis utilized a total of 402 scenes from 244 satellite passes over our regions of interest."

P4450 L6: Perhaps it would be more accurate to say "similar to the methodology of..."as you are introducing a novel aspect of inverting pixel values and then applying an exponential stretch. It is not immediately clear to me what this preconditioning does to the sensitivity of an algorithm that depends on the slope of a distribution tail. Presumably the algorithm is already quite sensitive to tail slope, and applying an exponential stretch might make it more so? In any case, it might be good to state what the inversion aims to accomplish, and what the exponential stretch aims to accomplish.

This paragraph has been completely revised. The stretch was simply for visual inspection and quality control purposes and does not impact the Liang et al algorithm. This has been made clear.

P4450 L15: When you state "this area threshold" it is not explicitly clear what parameter you are talking about. Presumably it's the 0.1 km^2 state in the preceding sentence? In any case, I think you need to demonstrate to the reader that the uncertainty in area returned by your algorithm is significantly less than this arbitrarily chosen threshold. I *suppose that means quantifying the uncertainty associated with identifying lake pictures from the histogram tail. And maybe it would be good to state the absolute total elevation sensitivity of your algorithm and include it as vertical error bars in your Figure 2?*

As suggested, we now include error bars on our elevation estimates, as determined from the uncertainty in the classification threshold, and as explained in lines 86-89 of the revised paper. We also more clearly state that we tested areas ½ and 2x as big with no substantial change to the results.

P4450 L27: "In East Greenland and above Humboldt Glacier (area I) in the far northwest, however, lake elevations have not kept pace with the rise in the ELA, suggesting a physical limit." – Looking at Figure 2, I would disagree with this statement. It appears that all the time series from East Greenland do exhibit positive slopes post-2000, which supports the notion that maximum lake elevation is increasing with ELA. Study region I does seem to have a bit of a post-2005 "plateau", but that is hardly conclusive. Given that you have 11 of 12 study areas exhibiting broad trends over the observational period, I think your big finding is that there appears to be no upper limit on lake elevation across most of the ice sheet. To make a statistically robust claim, however, I think you would need to perform a regression between maximum lake elevation and ELA in each study area. Then you could say maximum lake elevation is related to ELA with a chosen degree of confidence.

We have softened our wording with regards to this result. We now point out that the region I and the East Greenland regions have not risen as quickly in recent years as the west Greenland regions, and offer a hypothesis for this. We do not conduct a regression between lake elevation and ELA due to the small sample size and irregular sampling, both of which limit our analysis to a qualitative discussion. However, the qualitative patterns are strong enough that we feel this conclusion is justified.

Other Thoughts: Is RACMO2 known to over-estimate ELA? In Figure 2 the maximum lake elevations appear to be very close to the modelled ELA, when in fact anyone who has worked in Greenland knows that lakes are found well above the ELA. The pre-2000 ELA of 1650 m shown in Figure 2 for Southwest Greenland seems exceptionally high. I think a bias in RACMO2 output is fine, as long as it is acknowledged, since the task at hand is comparing trends in observed maximum lake elevation to trends in modeled ELA.

As explained in the text, the ELA's shown in Figure 2 are the average over the entire region: the average of 1650 m for the ELA of the southwest is the average of the far south, where the ELA is well over 2000 m, and the central west, where the ELA is closer to 1300 m. We only show the average to minimize the clutter on the plot, since only the temporal variability and not the absolute magnitude of the ELA is important to our result. Of course, the juxtaposition of ELA and maximum lake extent would be an interesting further study, but would require more in-depth treatment of model uncertainty, which is beyond the scope of this initial study.

Presumably there is some intra-annual variability in maximum lake elevation at each site. For example, is the highest lake in an early June image at the same elevation as the *highest lake in a late August image? Please assess and discuss this source of uncertainty in maximum lake elevation.*

We now include a discussion of how we mitigate this effect and provide a test of the correlation between sampling dates and our results (no correlation is found).

Another sticky thought: Is there some way to control for (presumably?) more lake pixels during warmer years? Given that you are using a maximum area threshold (0.1 km^2), I would imagine that the apparent maximum elevation is higher in a year with say 100 km^2 of lake pixels, than a year with say 50 km^2 lake pixels, simply because 0.1 km^2 represents a smaller fraction of the former case. Would perhaps using a proportional threshold (i.e. 1 % of lake pixels) address an underlying increasing trend in the absolute number of lake pixels?

We have made this presentation more clear with regards to the fact that we test other area sizes and why we do so. It is unclear, however, how a proportional metric would impact the central conclusion that there is more water at greater elevations. As explained in the text, our area threshold is designed to capture the upper limit of lake formation, and thus we seek to minimize the effect of lower elevation lake expansion. We do this by selecting a small upper area to sample, so that most of the change in elevation is due to the change in extent (In other words, if we had 100% confidence in our classification, we would simply measure the elevation of the highest water pixel each year. However, since there is error, we sample an area of pixels to improve confidence in the results).

Figures: 1 – "above" in caption. Should their perhaps be dates of image acquisition shown? If the aim is to show an upward progression in lake extent, it would probably help if the images were more evenly spaced in time, rather than having two pairs of years together.

Corrected caption and revised figure to show images from 2000, 2005 and 2011, as suggested, and the normal, rather inverted, grayscale is shown.

2 – If the ELA is a five-year average, should the average be "lagged", rather than(presumably?) "centered" on each year? For example, the year 2000 maximum lake elevation is more likely to better reflect the 1996-2000 mean ELA, rather than the 1998-2002 mean ELA (something to keep in mind if you do indeed do regressions, but then the smoothing affects the degrees of freedom).

Changed to a 5-year retrospective mean as suggested.