Review of James et al., The Cryopshere Discussion, May 2012

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SUMMARY and OPINION

James and co-authors compared digital elevation models (DEMs) derived from aerial photographs and from recent Lidar aerial surveys to assess volume change of 6 glaciers in Svalbard during the last 4-5 decades. Their analysis confirms the previously reported acceleration in thinning of glaciers in this region after 1990 and suggests that thinning is now also strong in the upper reaches of 4-5 of these 6 glaciers. This trend is then related to fluctuations in climate variables (T and P, in particular at the decadal time scale) and the authors suggest that changes (=reduction) in surface albedo may be needed to explain the thinning observed in the upper reaches of glaciers.

Technically, the paper is strong. The authors rely on a good set of elevation data and, elegantly, used the precise X,Y,Z information contained in the recent Lidar dataset to unlock the archive of stereo-photographs. Their error analysis is conservative. The discussion could certainly be improved, in particular with a stronger comparison to previously published regional elevation change assessments. The section on the role of albedo change is not really convincing and need also to be strengthened. Still, I recommend future publication in the Cryosphere after the comments below have been thoroughly addressed.

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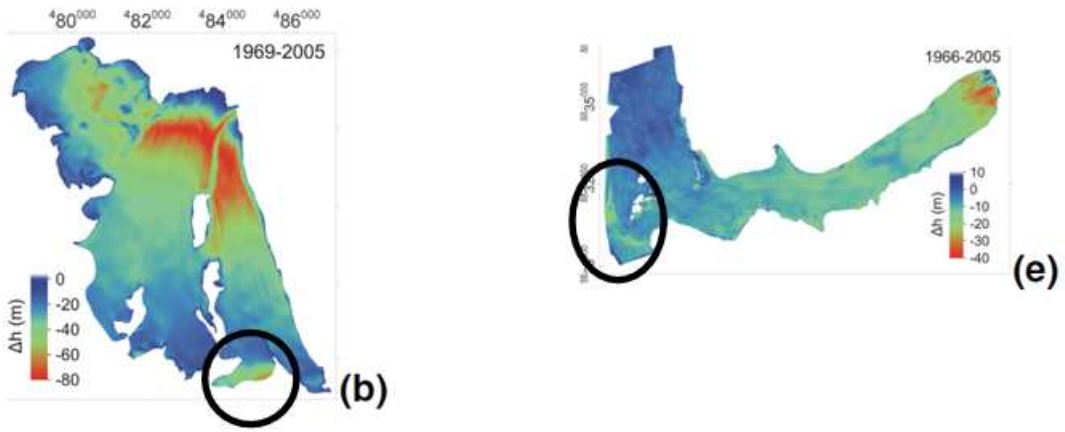
GENERAL COMMENTS

High elevation thinning. This is indeed an important finding, if confirmed (as I expect). In this respect, Svalbard glaciers would join the Patagonian, the Yukatat Icefields (and others icefields or icecaps) among the regions on Earth were thinning is also now strong in the upper reaches of glaciers. I do not challenge the finding, it seems to be robust. However, for at least two glaciers the maps of elevation changes reveal some unexpected pattern in their upper parts that could partly bias the estimate of thinning. This is the southernmost part of GB and the Southern part of the accumulation area of AL (Figure 2, edited below). Do the authors have a good understanding of those “artefacts”? Are they real elevation changes or errors in one of the DEMs?



Do

Additionally, a recent study has shown that when DEMs of different resolutions are compared, biases in the elevation changes can be induced and are particularly important close to the glacier headwall

where the surface slope is changing rapidly (Gardelle and others, 2012). Could this sort of bias exist between the very high resolution Lidar DEM and the (probably) coarser DEMs derived from aerial photographs and lead, artificially, to exaggerate thinning in the upper reaches of the glaciers?

The authors have measured distinctly different mass balances for their 6 glaciers. They focused mainly on the acceleration of the thinning rate and the decadal variability but more could probably be said about the variability between glaciers. It is noteworthy that the mass balance can differ by a factor of 4 between them. It illustrates the danger of using one or two "index" glaciers as representative of the whole archipelago ice and the importance of surveying a large sample of glaciers (if not all) to have an unbiased estimate of the mass balance. You should thus discuss more whether your sample of 6 small glaciers is representative when you extrapolate the mass loss to the whole archipelago. You could show in Figure 7b, the mean hypsometry of your 6 glaciers so that the reader can judge if your sample is representative of the rest of Svalbard. One or two sentences on this aspect would be welcome. The difference in glacier mass balance seems, at least partly, to be due to their different locations in Svalbard. This needs to be better discussed in particular in comparison to previous papers on the topic (Nuth and others, 2007; Nuth and others, 2010).

The authors do not account for the fact that the dates of aerial photos or Lidar surveys are not separated by an integer number of years. Yet, they indicate that this rapid summer elevation change made it difficult to assess the precision of the Lidar DEMs (hard to be with a GPS in the field exactly on the same day). I suspect that the up to 1.5-month difference in dates (see 1990 and 2005 surveys for AB and ML, in Table 1) will have little influence on the final mass balance (given the 15-yr time separation) but it would be an improvement if the authors could discuss this and, if significant, includes it in their error bars.

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SPECIFIC and TECHNICAL COMMENTS

P1086
Abstract. Maybe provide more results / quantitative values in the abstract: e.g., mean mass balance of the 6 glaciers, typical (mean) recent thinning rates observed in the accumulation areas...
L2. I though that on the long term (half century time scale), the thermosteric component was the major component of sea level rise (SLR). Can you check (Church and others, 2011) for an updated reference on this?
L5. There are now many regions where the SLR contribution of glaciers has been estimated from geodetic surveys (Alaska, Patagonia, Canadian archipelago). So this part of the introduction was true 10 years ago, not now. SLR estimates do not only rely on field measurements. See (Cogley, 2009).
L11. "before and after 1990" -> "after 1990" (clearer)
L12. Climate is probably best here than meteorological
L18. "When" repeated twice. Delete the second one?
L18. I think that other glaciologists publishing a mass balance estimate for let's say a 40-year period have in mind that this is a mean value and that it hides some inter-annual and decadal variability. So the statement is a bit weak. Rather focus on the variability you documented.
L 25. reference to Berthier et al. 2010 is not relevant here (regional scale study). (Church and others, 2011) is a more appropriate reference.

P1087
L5-10. update values using recent papers. (Church and others, 2011)
L16. Can you provide the total ice-covered area in Svalbard? I think it was never given in the paper.

P1088
L5. it does not account -> it does not necessarily account
L20. The paper by (Kääb, 2008) could probably also be cited here.

P1089
L16. “modelling”. Yes, you are creating digital elevation ‘models’. But some readers may be confused by the use of “modelling” here. “Topographic measurements” is probably preferable.

P1090
L3. Although I understand the choice to avoid surging glaciers, I do not think it is very well justified. Technically, sequential DEMs can also be used to map the transfer of mass during a surging and/or the total mass balance of a surging glacier…
L9. included -> were
L17 field -> airborne?
L18. Long baseline GPS processing. Enigmatic for me. Which method do you refer to here? PPP? Provide a reference if possible.
L28. 0.16. What do you show here? The mean elevation difference (then why +/-)? If yes, can you also provide the standard deviation?

P1091
L1. “influenced by the changing ice surface” -> “influenced by the changing elevation of the ice surface”
L4. A flat airport runaway is indeed useful to assess the quality of a DEM. However, care must be taken before extrapolating this error to the glacier surface which is much rougher. DEMs are errors are know to increase with slope (e.g., Toutin, 2008) so that the DEMs errors may be larger on the glacier than on the runaway (and even larger on the steep relief surrounding glaciers). For example, errors for SRTM measured on airport runaway (+/- 2 m see (Becek, 2008)) are not representative of the error in the DEMs (generally quoted as +/- 10 to 15 m in rougher topographic mountain terrain).
L27. Their surface can change -> their surface elevation can change
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P1092
L8. You need to say here by which surface you divide the total volume loss to obtain the mass balance. The mean of the glacier area at the start and at the end of the period of interest?
L12. An photographs -> a photograph
L16. The most accurate. This is arguable and certainly not always true. It will really depend on the accuracy of the DEMs and the time separation between them… and this method also need some assumptions about the density of the material gained or lost.
L22. “Ice fluxes” unclear what you mean here. Calving flux?
In the method section, you need to explain how the frontal retreat has been measured. Along a centreline? Or using the average of a set of frontal retreat measurements done across the whole glacier front?

P1093
L6. (Schwitter and Raymond, 1993) may be cited here

L16. Can you clarify how the averaging is done? Simple average of the 6 glacier-wide mass balances? Clarify where +/- 0.04 comes from as it is not the standard deviation of the sample (N=6) which is much larger.
L17. It is not terminal retreat but elevation losses that suggest negative mass balance. But saying that elevation losses suggest negative mass balance is so obvious (pleonasm) that it does no need to be stated... (density is never a negative value…!) The sentence can thus be deleted.
L23. Can you indicate how many glaciers were measured by Kohler et al?

P1094
L1-5. I think the wording could be improved to make it easier for the reader to understand directly what you mean here.
L7. Something crucial to reach your conclusion is that the wavelengths used here to acquire the DEMs do not penetrate into snow and ice. Probably to mention somewhere in the paper.
L7. “former snow accumulation”. See also comment below (P1097 L14). Same awful confusion.
Longyearbyen: Is there only one station with a long climate record in Svalbard? No others to look at the regional variability?

P1095
L1-4. Can you clarify why the warming alone cannot explain the thinning at high elevations? Not really demonstrated in the paper.
L8. Make it clear that the PDD are not calculated at the glacier elevations but at the altitude of the weather station. Right?
L12. Provide references.
L14. “These results”. Clarify what are these? Your results? The difference in mass balance from one glacier to another?
L19. Providing absolute value of precipitation would be best.

P1096
L1-3. Not really convincing. Again it seems that you are confusing surface mass balance and elevation changes which are two distinctive quantities.
L7. Important missing references for the albedo feedback on mountain glaciers are (among others) (Oerlemans and others, 2009; Wagnon and others, 2001).
L8. Did you mean “decrease” in albedo (not increase…)? Can you clarify why a change in albedo would play a role mainly on the upper reaches?
L16. Why the two feedbacks are inter-connected? They seem rather independent for me.
L24. Can you locate Ny Alesund in Fig 1?
L26-28. As it is, the sentence is a bit enigmatic and seems to discard the previous statement of “lower concentration between 1979-1989”. Can you reach any conclusion or not regarding black carbon?

P1097
L5. Space missing.
L9. Could you give also the change in AAR?
L8. The mean thinning from your set of glaciers is roughly 15 m since 1990. Can you quantify the corresponding change in AAR?
L14. Very weak statement as if thinning = ablation!!! Those glaciers probably still have an accumulation area although they are thinning across their complete altitude range. Only point mass balance measurement (and not elevation change) can tell us if they still have an accumulation area. End of summer snowline can also contribute to this.

L16-17 What are Nordaustlandet and Kvitoya? Refer to figure 1 and add the latter name.
L18. The extrapolation to the whole Svalbard seems a bit speculative and at least lack some explanation/discussion. How did you compute the mean curve of elevation change with altitude given that there is a "strong regionality in the area". The simple mean? The use of a density of 918 kg m^{-3} is surprising as a density of 917 kg m^{-3} is more often quoted and most of us (?) are using a density of 900 kg m^{-3}, probably to take into account the fact that the material is not pure ice.

P1098
L1. A comparison for the sub-regions would be useful.
L3-5. Why "however"? Do you imply that the agreement with the global trend is a proof that your values are more reliable? I do not see any reason why.
L25 "Metrological": a bit vague. Could you provide also some future research direction to assess the role played by change in albedo?

P1099
L6. unclear what you mean by "the net effect will be a loss of a significant proportion of the former snow accumulation area to summer ablation"
L8. Meteorological is vague "temperature and precipitation variability" is probably best.
L9. I do not think it "complicates". Just a measurement over a long period will not resolve the intra-period variability (by definition). Nothing complex.
L13. I do not think your results have such vast implications on global sea level rise... You are only surveying 6 rather small glaciers in one of the numerous glaciated regions on Earth.
L15. The role played by the albedo feedback is suggested in your paper but not demonstrated at all.

Table 2. Improve the caption. The table does not show "Site terminus position" rather "Rate of terminus retreat"

Table 3.
Provide the meaning of each column in the caption
I do not think the total volume change (and their rate) are necessary (last two columns). The areas and mean thinning are sufficient.

Fig 2. Impressive low noise level of the measurements, nice figures!

Fig 5. Keep the same style for this caption as for others

Fig 7. In the text you said you excluded Kvitoya also.

REFERENCE for my review

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