Reviewer #2

General comments

- Title – We agree that this title might be misleading with the use of 'widespread' and have opted to change the title more appropriately to "Observations of enhanced thinning in the upper reaches of Svalbard glaciers" which we hope is more appropriate. We have decided to use the term 'enhanced thinning' instead of 'accelerated thinning' which we think is more appropriate given the availability of only two periods.
- We have added more detail to the methodology as detailed below.
- We address the reviewers comment about the use of the Longyearbyen station below.
- We have emphasised the spatial variability in our results and have included a paragraph in the discussion. However, we would like to reiterate that we believe the regional mass balance variability is well documented (see reply to Reviewer 1) and that our purpose here was to neither document nor explain the regional variability.
- We have edited the discussion section on albedo in order to strengthening our argument as suggested. We this this has significantly improved the discussion and hope our changes are satisfactory to both reviewers.
- We address the issue of upscaling our results to the archipelago below.
- This issues with respect to our discussion of thinning, surface mass balance and assumptions related to accumulation area was also raised by Reviewer 1. We have attempted to be more accurate in our statements.

Specific comments

Section 2.1

- We reference James et al. (2006) and Barrand et al. (2009) which contain full details of the photogrammetric approach used here. However, we have increased the level of detail in our methodology to report better the key steps in the DEM processing and sequential DEM analysis which we hope is satisfactory.
- While this is common practice, we do not use the image correlation as an indicator of DEM point quality. The image correlation statistic simply reports the probably that a point is correctly matched. It is possible to have both a highly correlated match that is inaccurate (especially where there are repeated patterns like crevasses) and a poorly correlated match that is accurate (as in areas of poor contrast).
- This comment from the reviewer highlights the importance of DEM quality assessment in sequential DEM analysis. This is not a straightforward problem when working with historical imagery where acquiring independent check data is impossible. We address the different components to the referee's comment below:
    - The airport test site was used only to assess the quality of the lidar GPS processing and ranging, not the photogrammetric DEMs.
    - The rationale behind our data quality assessment is as follows: The quality of the photogrammetric adjustment of the photographs was assessed mainly with the

output from the photogrammetric processing (standard practice) which included the root mean square error (RMSE) of the ground control points (GCP) coordinates and image coordinates. For sequential DEM analysis, absolute accuracy of a photogrammetric model is of secondary importance to relative error between the DEMs that are to be differenced. Since all DEMs were controlled using the same lidar DEM and same GCPs, the relative systematic error between the models is small. However, this provides no information on the quality of the stereo matching process which generates the DEM elevations. This can only be assessed with coincident validation data which is not available on ice for the historical DEMs. The initially obvious alternative is to use the off-ice area validation against the lidar DEM. However, this requires two assumptions: (i) that the area has not changed over the period; and (ii) that the stereo matching quality off-ice is the same on-ice. Unfortunately, neither of these assumptions can easily be made. In terms of (i), the terrain around a glacier can be very dynamic (steep mountain slopes, ice cored moraines, outwash plains). In terms of (i), the bare ice surface is typically ideal for stereo matching (good image texture, relatively gentle slopes) whereas the surrounding landscape is typically not, with dark, low contrast and steeply sloped surfaces. Also, a considerable amount of time goes into manually editing errors of the on-ice DEM which is not invested in the off-ice DEM. Therefore, a blanket comparison of off-ice elevations to the lidar would not provide a good representation of the on-ice elevations. We find in Svalbard that the stable (i.e. vegetated) outwash plains are the most realistic surface for comparison with the on ice DEM.

  - Our off-ice test is based on three test sites: one between AB and ML (4100 points); one at SB (2800 points); and one at GF (6900 points). Because of its size, the GF site was not manually edited. We concluded that no suitable test sites were available at AL and GB. We have now provided results of this test in greater detail in the text.

Section 2.2

- We agree with the reviewer, that the higher elevations tend to have the largest errors. However, as stated above, we do not rely very heavily on correlations statistics. The areas of low image texture were measured manually (and thus have no correlation statistic) and only rarely were there cases where image texture was so poor that a human operator could not visualise the surface. We have edited this paragraph and addressed the lower reliability of elevation measurements at higher elevations in Section 2.1.

Section 3

- This paper has not aimed to account for the variability in the thinning rates of our six sites. We feel that the regional variability in Svalbard is well documented and has been satisfactorily linked to strong temperature and precipitation gradients in the archipelago (see Nuth et al., 2010; Bamber et al., 2005; Hagen et al. 2003; etc). A detailed assessment of the causes of the variability would require climatological data of a spatial resolution that is not available. However, we agree that the variability should not be glossed over and we have referred to the high variability explicitly in the results and have included a paragraph in the discussion to address this.

- P1094 L6-8 – we have changed our wording of this sentence.
- We are talking about increases in thinning rates here, not absolute thinning rates. The greatest thinning rates are clearly at the glaciers' termini but our results show that the greatest increase in thinning rates are indeed happening at altitude. This is clear from the curves in Fig 4. Additionally, the differencing of the two lidar DEMs at ML in 2003/2005, where data quality is arguably of better quality in the colder higher regions of the glaicers' (where there is less surface moisture) show the same pattern of change in thinning rates. While we agree that errors are higher in the glaciers' upper reaches, we are confident that our data is showing a real signal here.
- Photogrammetric methods do not work where there are not stable surfaces from which ground control can be measured. Both AL and GF extend to ice caps where elevations cannot be reliably extracted photogrammetrically. While it is not uncommon for poorly trained photogrammetrist to extend a DEM beyond the range of the ground control, it is not good practice and can result in large systematic errors. Therefore, we had to limit the height to which our DEMs extended at these sites. This is discussed as a footnote in Table 2 and we have now mentioned it in the methods section.

Section 4.1

- As advised, we have made reference to both these publications in the text.
- PDD is a standard approach to quantifying the potential for ablation of a melt and is calculated using the sum of mean daily temperature as standard (Paterson, 1994). We feel that using the max temperature for calculating PDD would be inappropriate.
- The conclusion of our paper is not that there is regional variability in Svalbard and we apologise if this is not clear. This variability is already well documented (see above). More importantly, we have found that despite this variability, our sites (with the exception of the NE site, GF) show increases in thinning rates before and after 1990 with enhanced thinning in the glaciers' upper reaches. We agree that the regional meteorological variability would be better characterised using all the stations available in Svalbard (there are several with relatively long records) but we do not feel this is necessary for the purposes of our study. As shown in Føreland and Hanssen-Bauer (2003) the temperature trends at Svalbard Airport (Longyearbyen), Ny Alesund and even Bjornoya are highly correlated over the longer term (see Fig. 2 in Føreland and Hanssen-Bauer, 2003). This is less so for precipitation but the Longyearbyen record is the only one that covers our time span. For higher temporal resolution mass change data, we agree that the use of the other met stations would be required but for our purposes, since we are talking about uniform trends over a decade or more, we concluded that the Svalbard Airport station was sufficient. However, we agree that readers may pose the same question and therefore we have made a statement in the text to address this.
- We have attempted to make the last sentence clearer.

Section 4.3

- We agree with the reviewer that the upscaling is difficult to justify. Our 6 glaciers are not representative of all of Svalbard and we make no such claim in the paper. We have also

highlighted the lack of tide water glacier and ice cap representation in the data. Applying our measured changes to the archipelago's hypsometry is for comparison of our results to other studies only and we draw no conclusions based on this number. Applying these results to tide water glaciers cannot account for any dynamics which we state in the paper. This has been an issue for both reviewers and we have revised the text to ensure that this is understood.

- We agree that it is difficult comparing our results to those derived from GRACE but there is always difficulty in such comparisons and we think there is value looking at these results in the context of the GRACE mission. We have attempted to be less speculative.

Section 5

- We have endeavoured to restate our conclusions in line with the referees' comments. However, we stand by our original conclusions as justified above. We hope that our explanations above and edits to the manuscript have satisfactorily addressed the referee's concerns.

Technical Corrections

- P1090 – L2-4 – it is not possible for DEM differencing to account for dynamic changes without a detailed history of velocity. We have attempted to make this clearer in the text.
- P1091 - L17 – Pixel brightness correlation between images in photogrammetry is often referred to as 'autocorrelation'. I think it is a reference to the automation as opposed to manual correlation carried out in 3D by a human operator. In retrospect, this is confusing and we have opted to use 'correlation' as suggested.
- Fig 2e – The linear features in this differenced image were the results of a scan line error in the digital images. We have corrected this problem as best as possible (by minimizing the use of the image with the error. We have reproduced the figure and found that the error and correction have negligible effect on the results other figures.

References

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