Thank you to both reviewers for your constructive comments on our submission to The Cryophere Discussion, which have substantially improved our manuscript. Here and in our reply to the second reviewer, we hope we have satisfactorily addressed all issues raised by the reviewers. For clarity and brevity, we have not included an author comment for straightforward edits that did not require explanation.

Reviewer #1

General comments

- Thank you for pointing out these artefacts in the differenced DEMs. In the case of GB, this is an area of dynamic thinning as the result of a surge on the glacier to the south of GB, Fridtjovbreen (see Murray et al., in press). This feature should not be included in the change statistics in Tables 2 and 4, nor in Figures 3, 4 and 5. We have made the adjustment but the difference is only very small and is well within the quoted error margins. In the case of GF, part of this area should also not be included in the change statistics as this is an area of no data in the lidar data set which had been incorrectly extrapolated. The figure has been corrected and these changes made only negligible difference to the tables and figures.
- We believe that the phenomenon discussed in Gardelle et al. (2012) does not apply to our study. First, for the glaciers that have steep headwalls (several do not), the area of these regions is very small compared to the area where the enhanced thinning has been recorded and will not be strongly represented in the 10 m resolution DEMs. Second, the comparison of the 2003 and 2005 lidar DEMs at ML, which have similar resolution, show the enhanced thinning most strongly so we believe this signal in our data is real and not the result of an altitude dependent bias.
- We agree that the variability between our glaciers is significant and that this makes a strong statement about the regional variability in Svalbard and the danger of using index glaciers to represent regional mass balance. We have now made note of this variability in the results and have raised this more explicitly in the discussion. However, we feel that the variability in Svalbard has been well documented (e.g. Hagen et al., 2003; Bamber et al., 2005; Nuth et al. 2007; and Nuth et al., 2010) and, being beyond the scope of this study, we would not like to delve into this in any detail in the absence of the meteorological and field data required to do so properly.
- We agree that our 6 sites are not likely representative of the archipelago's volume/mass changes and we are careful to make no such claim. We explicitly state in the paper that we performed the upscaling for comparison purposes only which is important for placing our results in context of previous research. We have strengthened this statement and have included a comment in the conclusion that our results point to caution when upscaling a small sample to represent regional changes.
- We can supply an alternative to Fig. 7 that includes the combined hypsometry of our sites but we feel it will be misleading since the hysometry for the whole archipelago is generated from very coarse data and will thus be smoothed whereas that from our sites would be

based on the high resolution DEMs and thus will be comparatively noisy. We feel it will clutter the figure and does not add to the paper. We welcome advice from the Editor.

- Ideally, the photographs used for measuring geodetic volume changes should be taken at the end of the ablation season. Since this precise timing is never practical in flying remote sites, we would need to apply a correction to both sets of images requiring detailed knowledge of local meteorology which are not available. Therefore, as the difference in dates between sets of photographs are negligible compared to elevation changes over the long periods of study (as in Nuth et al., 2007). We have not attempted to account for these errors. We have made a statement to this effect in the method section of the manuscript.
- Both reviewers have raised issue with our discussion of thinning, surface mass balance and assumptions related to accumulation area. We take this point on board and have attempted to be more accurate in our statements.

Specific comments

P1086

- We have strengthened the abstract with key results, including the mean thinning rates, as suggested. We have also rewritten the abstract to reflect better reflect current literature.
- L2 - We agree that the thermosteric component is the most significant contributor to global sea level. However, here we refer to 'eustatic sea-level' which excludes thermal expansion. We believe the use of this term is appropriate with usage examples in Fleming et al. (1988) and Meier et al. (2007).
- L5 – we removed this misleading sentence.
- L12 – Because we are dealing with some periods of only 15 years, we feel more comfortable referring to these data as meteorological data rather than climate data since climate is usually based on a longer term average. We are happy to change this if the editor is in agreement.
- L18 – When we say 'decadal variability' here we are referring to the meteorological record, not the decadal variability of elevation changes. We have made this clearer in the text.

P1087

- L5-10 - We prefer to cite the Church and White (2006) study here because it covers the full 20th century which includes the little ice age of the northern hemisphere and this fits better the context of our study. Similarly, the Kaser et al. (2006) paper covers time period which agree very well with the dates of the photographs used in this study. However, we have added a reference to Church et al. (2011) at the beginning of the section.

P1088

- L20 - We did not cite Kääb (2008) here because this paper focusses on Edgeøya, whereas the other examples to which we refer in this paragraph are more pan-archipelago studies.

P1089

- L16 – ‘Topographic modelling’ is a standard term and indeed we give a lecture in digital terrain modelling every year at Swansea. We would prefer to use this terminology if the editor doesn’t think that it will cause confusion.

P1090

- L3 – Geodetic mass balance measures can only be used to measure mass fluxes from a surge event if the photos closely bracket the surge event. Otherwise, it is difficult if not impossible to differentiate between surface mass changes and surge dynamic changes. Due to the nature of the data, this paper focusses on long-term changes. Including glaciers that had experienced a surge would skew these results, although we have used these methods to characterise a surge in a recent paper, Murray et al. (in press). We have attempted to make this case stronger in the text.
- L18 – We agree that this term is vague and we had clarified the text.
- L28 – we have corrected this reference to the comparison of lidar to dGPS in Barrand et al. (2010)

P1091

- L4 – The comparison of lidar data to a flat surface like the airport runway is done to isolate error in the laser ranging and GPS position (which are the largest sources of error in a lidar position). This is common practice before and after a lidar survey to judge the baseline quality of the data. The influence of surface roughness and reflectivity is another dimension that can only be assessed with comparison to coincident ground truth (which is often unavailable). We have edited this section to make this clearer in the text.

P1092

- L8 – We have added these details to the text but as the upscaling (and mass changes) are a discussion point rather than a result of the paper, we have included details of the upscaling in the discussion.
- L16 – We believe it is accepted that over long periods, where DEM quality is good and elevation changes are orders of magnitude larger than errors from density differences, the geodetic method is more accurate than the glaciological method. We have rephrased this sentence to qualify this statement.
- L22 – While we think ice flux is an appropriate term to use here (as in Nuth et al. 2007) we have changed this to ‘flow’ to be more clear.

P1093

- L16 – The propagation of the error was done in quadrature as per standard error propagation theory.
- L17 – We agree that terminal retreat is not independently suggestive of mass loss but we would like to highlight the combination of terminal retreat and elevation loss which is strong evidence that this is a climate response not a dynamic response. Terminal retreat with elevation gain or indeed elevation loss with terminal advance are indicators of surge behaviour and not a response to warming climate.

P1094

- L1-5 – We have attempted to reword this paragraph to improve clarity.
- L7 – We believe it is generally understood that snow/ice penetration of laser ranging in IR wavelengths in insignificant. While this has been largely anecdotal (i.e. from tech crew), we have found two incidences of this documented in the literature (Sun et al. 2006; Prokop, 2008).
- L7 – We have changed the wording here to not refer to these areas of elevation loss as former accumulation areas. We address the reviewer's comment about the sole use of the Longyearbyen Met station in our reply to referee 2 below.

P1095

- L1-4 – Warmer temperatures affect glacier thinning according to the lapse rate which means the degree of warming decreases with altitude (as demonstrated in Schwitter and Raymond, 1993). Therefore, this non-linear relationship between elevation and thinning rates cannot be explained by warming. We have made this clearer in the text.
- L19 – We do not understand what is meant by absolute value of precipitation when discussing a long term trend. The trend here is a decrease in precipitation which is indicated by the negative. We have changed this sentence to read a negative trend of 4.4 mm a^{-1}. Is this what the reviewer means?

P1096

- L1-3 – We address this above under general comments.
- L7 – We have added these references to our discussion of albedo in the revisions of this section.
- L8 – Corrected. We have explained the effect of albedo on the accumulation area in the text.
- L16 – These feedbacks are interconnected because decreasing albedo and surface lowering enhance melting and act as a positive feedback. As albedo decreases, melting is enhanced and the surface lowers. Conversely, lowering the surface increases melt (due to the lapse rate), melt is enhanced which decreases albedo. We have clarified this in the text.
- L26-28 – We have attempted to address this when revising this section.

P1097

- L9 – we have provided this in terms of AAR.
- L8 – We don't think converting the 15 m of mean thinning is meaningful since this does not all occur above the ELA.
- L14 – See above.
- L16 – We have now indicated the location of Nordaustlandet and Kvitøya earlier in the manuscript.
- L18 – The upscaling of our results is carried out solely for comparison and discussion purposes with respect to other studies and we draw no conclusions from these results (this

is discussed above). We have stated this more clearly in the text. The approach we use is described on P1097 but we have expanded on this somewhat to provide more detail. We have changed the density used to 917 kg m^{-3} as recommended but this does not change our results.

P1098

- L1 – We have not separated our sites into subregions in this paper because we do not have enough sites to identify or represent any climatic subgroups like those identified in Hagen et al. (2003). Our purpose in this paper is not to upscale since we do not argue that our sites are representative of the archipelago. However, we feel it is impossible to compare our results with those of other studies unless we perform the same upscaling.
- L3-5 – Here we are comparing our results with those of other studies. Our results disagree with those of Nuth et al. (2010), likely because of our exclusion of the eastern ice caps. It is difficult to compare individual studies because of the differences in methodology, study area etc. However, the agreement in magnitude of our results to the global trend (which are comparable) does lend credibility our results and suggests that we are seeing a similar mechanism reported therein.
- L25 – we meant 'meteorological' here. This has been corrected. We have amended the conclusion to suggest future research direction.

P1099

- L6 – We have attempted to make this statement more clear.
- L8 - We argue that our results show that it is very difficult to draw conclusions on long term trends from geodetic mass balance over long time periods because of the sensitivity of the analysis to the start and end point. We have attempted to address this more explicitly rather than simply saying that it 'complicates' the interpretation of changes in glacier geometry.
- L9 – We agree that our 6 glaciers will not have a significant implication on global sea level sea level and we have not implied this in the manuscript. We are arguing that the mechanism that is causing high elevation acceleration of thinning, which might be albedo-feedback driven, will have an important impact on regional mass balance (especially for somewhere with a sensitive ELA), and potentially on sea level if wide spread. We have attempted to make this clearer in the text.
- L15 – In the conclusion edits, we have made it clear in the paper that we are not drawing any conclusions on the role of albedo in our results.
- Table 3 – caption amended to include description of column statistics. We would like to keep the volume change stats in the table in case these are useful in gravimetry studies like Memin et al. (2011)

References

Church, J. A., and White, N. J.: A 20th century acceleration in global sea-level rise, Geophys. Res. Lett., 33, 10.1029/2005GL024826, 2006.

Fleming, K., Johnston, P., Zwartz, D., Yokoyama, Y., Lambeck, K., and Chappell, J.: Refining the eustaticsea-level curve since the Last Glacial Maximum using far- and intermediate-field sites, Earth and Planetary Science Letters, 163, 327-342, 1998.

Gardelle, J., Berthier, E., and Arnaud, Y.: Impact of resolution and radar penetration on glacier elevation changes computed from DEM differencing, J. Glaciol., 58, 419-422, 2012.

Meier, M. F., Dyurgerov, M. B., Rick, U. K., O'Neel, S., Pfeffer, W. T., Anderson, R. S., Anderson, S. P., and Glazovsky, A. F.: Glaciers dominate Eustatic sea-level rise in the 21st century, Science, 317, 1064-1067, 10.1126/SCIENCE.1143906, 2007.

Mémin, A., Rogister, Y., Hinderer, J., Omang, O. C., and Luck, B.: Secular gravity variation at Svalbard (Norway) from ground observations and GRACE satellite data, Geophys. J. Int., 184, 1119-1130, doi: 10.1111/j.1365-246X.2010.04922.x, 2011.

Murray, T., James, T. D., Macheret, Y., Lavrentiev, I., Glazovsky, A., and Sykes, H. J.: Geometric changes in a tidewater glacier in Svalbard during its surge cycle, Arct. Antarct. Alp. Res., in press.

Nuth, C., Kohler, J., Aas, H. F., Brandt, O., and Hagen, J. O.: Glacier geometry and elevation changes on Svalbard (1936-90): a baseline dataset, Ann. Glaciol., 46, 106-116, 2007.

Prokop, A.: Assessing the applicability of terrestrial laser scanning for spatial snow depth measurements, Cold Regions Science and Technology, 54, 155-163, doi:10.1016/j.coldregions.2008.07.002, 2008.

Sun, X. X., Cooper, J. W., Hom, M. G., Shuman, C. A., Harding, D. J.: Measurements of snow and ice surface reflectance and penetration to short laser pulses at zero phase angles and 532 and 1064-nm wavelengths. American Geophysical Union, Fall Meeting 2006, abstract #C21A-1121. 2006.