Dear Reviewer 1,

Thank you for your thoughtful and constructive comments. We have adopted nearly all of your suggestions and have provided explanations for those few instances where we have not. Both reviewers are clearly experts on the determination of mass changes using geodetic methods. We will submit a revised manuscript as soon we integrate curvature correlated bias correction to our process chain (see response to Reviewer 2's Page 1573, Line 9).

Sincerely,
Alex Gardner and coathors.

Review of Gardner et al., TCD, May 2012

SUMMARY

Gardner and co-authors take advantage of numerous recent (post-1995) surface elevation measurements (satellite DEMs, satellite and airborne laser altimetry) and compare them to older maps, mainly from the 1960s, to measure volume and mass changes of glaciers and ice caps on Baffin and Bylot islands (southern part of the Canadian Archipelago). Thus, they put the recent (2003-2009) mass losses for the same regions (Gardner and others, 2011) in a multidecadal perspective and conclude convincingly to an accelerated mass loss since the mid-1990s. The increase in mass loss is related to the increase in regional air temperature with, interestingly, different temperature sensitivity for the two main ice caps (Barnes and Penny).

GENERAL COMMENTS

The paper is well written, the methodology is up-to-date and the results will be of interest to the glaciological community (and beyond). The paper deserves to be published in TC. The aim of my review is mainly to suggest some possible improvements/clarifications.

1. Improved discussion through comparison to the published literature.

In the submitted paper, there is no comparison to published mass loss for the same region within global estimates. (Dyurgerov and Meier, 2005, his region 22, Page 103) and (Cogley, 2009) / (Church and others, 2011) have published some long-term mass balance estimates for the same region (probably, by necessity, based on sparse mass balance measurements or using some extrapolation from mass balance data even outside the study region). It could strengthen your discussion if you could compare to those previous estimates. You could also compare to (Hock and others, 2009) and maybe others global modelling effort (van de Wal?) that need to be confronted against regional assessment of mass changes (such as yours).

You did not really compare your results with (Sneed and others, 2008) who also previously reported an accelerated rate of mass loss for a single transect on Barnes Ice Cap. Here again you would strengthen your discussion.

You could also compare your **mass balance sensitivity** to temperature changes to previous estimates of this variable. In particular from (Hock and others, 2009) who can probably extract for you their parameter ST for the Barnes and Penny Ice Caps. It is important to test/validate those sensitivities because they are used to project future mass losses from glaciers (e.g., Radic and Hock, 2011). Do the authors have an explanation to the different mass balance sensitivities to temperature of the Barnes and Penny Ice Caps (although I understand this is beyond the scope of the paper to discuss this in details).

I agree with Neil Glasser (his SC) that it is relevant to compare to their study (Glasser and others, 2011) that put into a longer term perspective the recent mass loss of the Patagonian Ice Fields. (Willis and others, 2012) may also be cited for recent ice losses in Patagonia. The same holds for Alaskan glaciers where acceleration of the mass losses has been reported (e.g., Arendt and others, 2002).

Comparing our regional measurements with global modeling results, while valuable and interesting, is out of the scope of our study. Modeled glacier mass changes for Baffin and Bylot islands were included in these global analysis but accounted for less than 6% of the area modeled and were not a focus these studies. As mentioned by the Reviewer 1, there are so few measurements of glacier mass balance for this region that all global estimates would have had to rely heavily on interpolation from other regions or output from climate reanalysis. Comparison with these global studies would therefore be a validation effort of previous methods for the determination of global mass budgets and is out of the scope of the current study. We have now removed the comparison with Patagonia from the abstract and added Sneed and others, 2008 to the discussion (see comment P1565, L5).

2. Drivers of the accelerated mass loss
Apart from the albedo feedback (already discussed in the paper), the elevation feedback is sometime also invoked to explain accelerated ice loss. Could the authors also provide a 1st order estimate of this feedback? For Barnes Ice Cap, they can for example estimate the mean surface lowering of the ice cap between different time intervals and thus the corresponding rise in temperature due a lowering glacier surface (e.g., using a constant temperature lapse rate with altitude, maybe from reanalysis?).

To assess the possible impact of the elevation feedback on accelerated ice loss we assume that the dh/dt gradient with elevation is constant over a 50-year period and compare volume change estimates using the original glacier area hypsometry and one that is modified to reflect the elevation changes that would have occurred over this time. We find that changes in elevation over a 50-year period could account for no more than a 0.2 Gt a^{-1} increase in ablation for either ice cap. This has now been added to the Discussion section of the document.

3. Sampling from sparse altimetry
Because the authors have complete maps of elevation changes (at least for Barnes), they can more thoroughly examined how well sparse elevation measurements can be used to infer the total volume change. They could sample the map of the elevation change (SPOT5-CDED) where (i) they have ATM dh/dt and (ii) they have ICESat dh/dt and compare the total volume loss.

They would thus only look at sampling effect without influence of the accuracy of the different measurements. In others words, when the mass loss for Barnes is compared -2.9 Gt/yr (SPOT5, 1960-2010) and -2.5 Gt/yr (ICESAT, 1960-2006), the differences are due (i) sampling but also (ii) different time stamp for the recent survey and (iii) altimetric difference between SPOT5 and

ICESat so that it is not an unambiguous demonstration of the lack of sampling bias. Regarding (ii) it is stated in the paper that: "most of the difference is due to the sampling interval". Could you be more convincing? If this holds, you need at 4-year 2006-2010 mass loss of 7.5 Gt/yr (larger but not far from your 6.2 Gt/yr for 2005-2011)

This was done in the original manuscript for the ICESat and ATM datasets to characterize the uncertainty in the extrapolation of the ATM data. We have now examined the subset of SPOT-CDED data within a 1 km distance of the ICESat tracks and determined a slight difference in the estimated mass budget of the Barnes Ice Cap of +0.2 Gt/yr but negligible elsewhere. We have now included the following sentence:

"Merging results from SPOT Barnes_B V2 and Barnes_A V1 DEMs, we estimated that the Barnes Ice Cap lost mass at a rate of -2.9 ± 0.3 Gt a^{-1} between 1960 and 2010. This compares well with a rate of -2.5 ± 0.3 Gt a^{-1} as estimated from ICESat for the period 1960 and 2006. About half of the difference between estimates is likely due to differences in spatial sampling. The subset of SPOT-CDED data within 1km distance the ICESat tracks give a mass budget of 2.7 Gt a^{-1}."

And later one we include:
"With the exception of the Barnes Ice Cap as noted earlier, analysis of the subset of SPOT-CDED data within a 1 km distance of the ICESat tracks indicates no bias due to differences in spatial sampling between the ICESat-CDED and SPOT-CDED analysis."

4. Seasonal correction

The DEMs are stated as "generated from late summer imagery". 7 July is not exactly the end of the summer in the arctic… Cannot you use the GRACE time series (10-year mean seasonal cycle) to propose an error estimate due to the fact that all data are not acquired at the end of the melt season? I foresee a small error due to the long time separation but, for completeness, it would be nice to see this issue address in the paper. What about the 10 March 2010 DEM? Did you do any seasonal correction?

Noise in GRACE seasonal signal is too high to use as a time-of-year correction for DEM acquisitions. In any case, errors due to time-of-year differences on the order of months will introduce negligible uncertainty into our estimates. The worst-case scenario for the error introduced in regional DEM dh/dt estimates is < 0.03 m/yr.

5. No Need for updating GRACE analysis

I do not see the point in updating GRACE estimate. It makes the paper much longer (two pages in the method are dedicated to GRACE) and the mass loss for 2003-2011 (-23.8 Å} 3.1 Gt/yr) equal the -24 Å} 7 Gt/yr value from (Gardner and others, 2011). By the way, it is not explained why the error bar is lowered by a factor of two. I strongly recommend shortening the paper by not updating the GRACE time's series.

As suggested by Reviewer 2 we have decided to retain the GRACE analysis. More important than showing that the mass budget of the regions itself is the comparison of the GRACE results with other methods which confirms its usability for regions outside of the ice sheets.

SPECIFIC and TECHNICAL COMMENTS

P1564
Title. Probably too long and "long-term" is vague (multi-decadal?). Do you need to enumerate the different datasets?

Yes this tile is not all that great. We've now change it to "Accelerated contributions of Canada's Baffin and Bylot Island glaciers to sea level rise over the past 50 years."

L19. Why do you change unit for the recent and multi-decadal mass losses (mm and mm/yr)

I'm not sure why I did that.. I've now changed both to mm/yr

P1565
L1. Give the exact area also (41000 km2 is not exactly one third of 147000 km2, rather 28% so closer to one fourth)

chnaged

L5. Reference to (Sneed and others, 2008) here?

We have included discussion of Sneed et al.'s (2008) results in the results and discussion section of the manuscript. Their study does not really fit in the intro because they report elevation changes for a single transect located on the north-eastern lobe of the Barnes Ice Cap and do not provide regional estimates of mass or volume change.

I've added the following paragraph to the discussion:

"It is difficult to compare our regional estimates of glacier mass change with earlier in situ measurements that have small spatial coverage and span short time periods (Ward, 1954;Sagar, 1966;Løken and Sagar, 1967;Weaver, 1975.). The only place a reasonable comparison with pre-ATM (1995) estimates can be made is for the Barnes Ice Cap where long-term elevation differences have been measured for a summit-to-terminus transect along the north-east lobe of the ice cap (Hooke et al., 1987;Sneed et al., 2008). Using theodolites, GPS, an ASTER DEM, and ICESat altimetry, and averaging over the length of the transect, Hooke et al. (1987) and Sneed et al. (2008) estimated that the Barnes Ice Cap had an area-averaged mass budget of -0.12, -0.76, and -1.0 kg m^{-2} a^{-1} for 1970-84, 1984-2006 and 2004-06, respectively. These later values compare well with our 1995-2000 and 2000-2005 Barnes Ice Cap estimates of -0.76 and -0.94 kg m^{-2} a^{-1}, respectively, and support our finding that the rate of mass loss from the Barnes Ice Cap has accelerated in recent years."

L16-17. This sentence is "method", not needed in the intro
Removed

P1566
L12. "of the" repeated

Deleted

P1567
L3. "gradient" is unclear (maybe simply remove)

I've now added "gradients with respect to surface elevation"

P1568
L3. ":" needed in the title?

We've removed the ":"

L18-20. How did you treat the regions where gaps in CDED are filled using modern satellite data? Could you identify those unambiguously and exclude them?

These modern DEMs were not included in our analysis as we use a CDED map cutoff date of 1983, as outlined in the methods section. To make this clear for the reader we've included "and are excluded from our study" to the end of this sentence.

L23 and L26. Use same number of decimals for consistency

Changed

P1569
L8. Not sure "detection" is the best world. "measurement" instead?

Changed

L15. This is the maximum achievable ground coverage. In general case the data strip is not as long as 600 km (the 120 km swath is constant)

Added “maximum” to the sentence

L22. Berthier and others (2007) used a DEMs derived from SPOT5-HRG imagery not SPOT5-HRS so they cannot be cited about “Similar DEMs…”. A study where SPOT5-HRS DEMs were used in complex glaciated terrain: (Gardelle and others, 2012)

Berthier and others (2007) removed and Gardelle and others, 2012 added.

L23. Can you progress logically from North to South when you describe the DEMs?

Changed

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P1571

L11-12. This is on ice? (if this is on stable ground threshold on dt not needed)

Added “over ice”

L19-20. I did not understand

“Potential systematic errors that might be pertained in all data should be smaller than 0.1 m and 0.01 m a-1, respectively (Zwally et al., 2011a).”

changed to:

“Correlated errors (bias) are likely smaller than 0.1 m for individual measurement campaigns and 0.01 m a^{-1} for the derived elevation change rates (Zwally et al., 2011a).”

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P1572

L25. “cf.” not needed

Removed

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P1573

L10-14. This section is not really clear. In Fig A2, a slope dependent bias is mentioned first and then spatially dependent bias. So is a slope-dependent bias detected or not?

We’ve now removed the first sentence so L10-14 now reads “Of these three biases, only spatially correlated biases were detected and corrected for which can result from spatially varying phenomena such as air photo coverage, glacial isostatic adjustment, errors in ground control points and errors in geoid transformations.”

Hopefully this makes it less confusing as to which corrections were applied.

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P1574

L15. "from repeat measurements acquired five year apart" probably not needed

Removed

L26. Barnes (check everywhere)

Changed

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P1575

L13. The two sigma filter is dangerous. You can end up removing real measurements due to strong spatial variability within one elevation band. Did you check that it was not the case? This is especially the case if the filter is applied to all ice within a region. Or did you apply it to each glacier separately? More generally, did you test the sensitivity to this filter?

The two standard deviation filter was applied for each sub-region for each 50 m elevation interval. For most regions we found that results change little between using the filter and not, with maximum differences of about 0.1 km^3 a^-1. For the Penny Ice Cap ICESat-CDED differencing the two standard deviation filter removes a line of ICESat-CDED data that show's high-elevation (>1400 m) lowering on the order of 1-1.5 m a^-1 which deviates greatly from the other ICESat tracks and the SPOT-CDED analysis. Because the ICESat-CDED analysis for the Penny Ice Cap has sparse data coverage at high elevations a three standard deviation filter would not remove the erroneous tack of data. We did however find that the two standard deviation filter did remove some usable data from the ATM analysis due low sampling rates in some elevation bands. We have therefore increased the filter from two to three standard deviation and manually excluded the erroneous high-elevation Penny Ice Cap ICESat-CDED data.

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P1578

L15-17. Could probably be explained a bit more clearly (or illustrated with a map for one ice cap?)

We've now added a map to Appendix A (Figure A4) showing the subset of ICESat data used to assess the uncertainty in extrapolating the ATM data to determine regional volume changes.

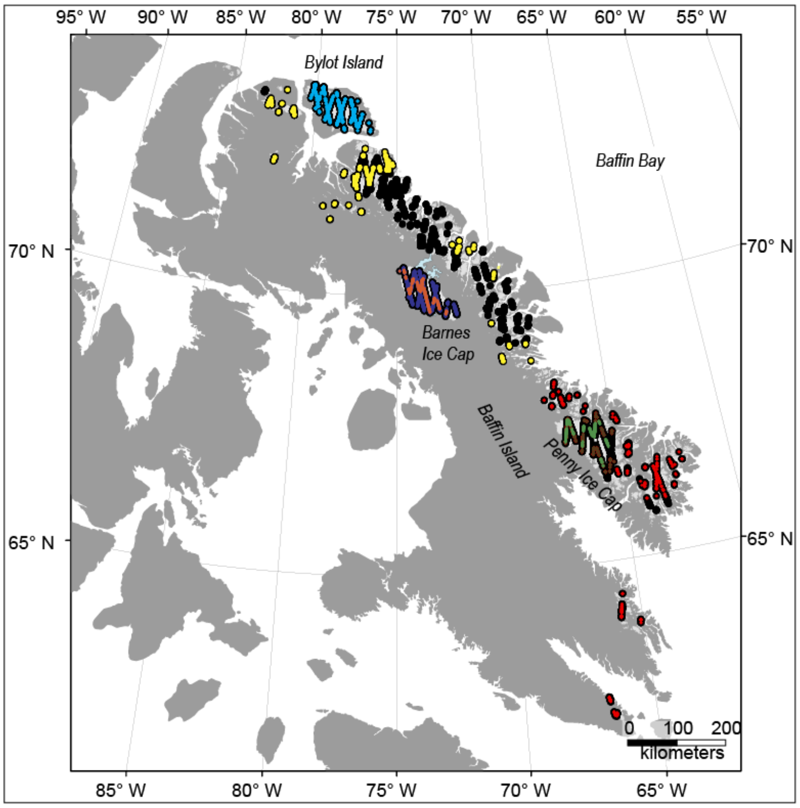

Figure A4: Locations of ICESat elevations used in study. Underlying black dots (●) show all ICESat points used for ICESat-ICESat estimation of 2003-09 elevation changes. Cyne (●), yellow (●), blue (●), red (●), and brown (●) dots show the subset of ICESat elevations used for ICESat-CDED differencing for Bylot Island, North Baffin Island, Barnes Ice Cap, South Baffin Island, and the Penny Ice Cap, respectively. Orange (●) and L28. Penny. Check everywhere

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P1579

L11. Where does +/- 0.25 come from? 0.9 + 0.25 = 0.925 > density of pure ice

In lack of data we assumed an uncertainty for the bulk density. This is now stated in the text and we have reduced our assumed uncertainty to 17 kg m^-^3 to stay within physical bounds. This change has virtually no impact on our final mass change uncertainties.

L17. space missing

added

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P1580

L6. Could be clarified. For all ice area I would not have done the RSS sum but simply the sum of the individual uncertainties. Am I wrong?

For the total uncertainty of all ice area we sum each of the separate sources of uncertainty for each region then take the RSS of the summed individual components to determine the total uncertainty. This assumes that individual sources of uncertainty are correlated in space but not with each other. We have modified the text to try and make this clearer.

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P1581

L23. If ever GRACE longer time series was retained in the revised paper, could you provide at the end of the paragraph the total uncertainty and explain the difference with Gardner et al. 2011?

We now refer to Gardner et al.'s (2011) Supplementary Figure 3 and our Figure 2 which show the uncertainty in the monthly GRACE solution.

L25 (to L3). Unnecessary repetition.

removed

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P1582

L19. "from"

Changed throughout

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P1583

L8. "a strong pattern of low elevation ablation". You do not measure ablation but elevation changes this is (really!) different.

Agreed. We've changed this to: "shows a strong pattern of elevation lowering that is most pronounced at lower elevations"

L15-17. I do not think the explanation is needed for TC readership.

Removed

L26. "smaller" is always ambiguous for negative values (a solution would be to add "absolute" before elevation change)

Changed to "the magnitudes of glacier elevation change rates are smaller"

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P1584

L3. "Barnes"

Changed

L21-24. Why do you extrapolate from measurements on "Penny Ice Cap" only?

In Section 3.5.1 we found that extrapolating the Penny ATM data to determine the mass change of the remaining ice performs better than using the Barnes data. This was determined by extrapolating a subset of ICESat data within 5 kms of the ATM tracks and comparing results to the full ICESat dataset. We've now added the following text to clarify this:

"Extrapolating elevation changes measured over the Penny Ice Cap to the reaming glacier cover, which performs better than extrapolating Barnes Ice Cap data (see Section 3.5.1), gives a total mass loss for the region of -14.9 ± 3.6 Gt a^{-1} and -25.3 ± 6.4 Gt a^{-1} for the periods 1995-2000 and 2000-2005, respectively"

P1585

L14. Did you compare to Jacob et al for the exact same period? Disturbing nearly 50% difference between the two estimates…

We've now added the following to the discussion:

"The second regional estimate of mass change is from a recent GRACE study (Jacob et al., 2012) that shows higher rate of loss for Baffin and Bylot Island Glaciers (2003-2010: -33 ± 2.5) than our corresponding GRACE (2003-2009: 23.8 ± 3.1 Gt a-1) but agree within error bounds. We have reexamined both estimates and it appears that differences in GIA and terrestrial water storage corrections, time interval, and the method used to estimate mass changes (end-of-melts-season vs. trend of full time series) could only account for a small fraction of the difference (0-2 Gt a-1) between GRACE estimates. Other possible sources for the disagreement are differences in domains, how signals outside the target regions are treated, and the partitioning of mass changes between Northern and Southern Canadian Arctic regions/mascons. Again, both GRACE estimates agree within error bounds, but more in-depth examination would still be valuable to identify the source of the disagreement."

Bert Wouters will be moving to Boulder, Colorado within the next couple of months to work with John Wahr so hopefully we'll be better able to peg down the sources of the disagreement in future work. It's worth noting that it is better to discuss the magnitude of the disagreement (~10 Gt/yr) between GRACE estimates as appose to % differences.

L19. The fact that inter-annual variability is controlled by ablation (and thus temperature) does not necessarily imply that the decadal variability is controlled by temperature. There are known example where the (pluri-) decadal mass balance variability has been found to be related to precipitation (e.g., Vincent and others, 2005)

Good point, We've removed "therefore" from the sentence

P1587

L13-15. Can you clarify the difference between "accelerated rates" of mass loss and "increases" losses?

This is clearly an error on my part. I was referring to the second derivative of the mass change rate as acceleration, this is obviously wrong. Thank you for pointing out my mistake. I have corrected the sentence

L18. Barnes

Corrected

L19. The number was never quoted earlier in the text.

We have now added specific mass budget to Table 1.

L21-22. Probably not needed in the conclusion (?)

We have decided to keep this in because it highlights the complexity of the flow regime of the Barnes Ice Cap.

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P1588

L1. (Arendt and others, 2006) have published a thorough and useful analysis on different methods of extrapolating centerline elevation changes measurements. Given that the sampling by centerline altimetry itself has been challenged subsequently (Berthier and others, 2010), I am not sure this is an appropriate reference here. I do not disagree with your conclusion that discontinuous/sparse measurements of elevation changes can provide a reasonable estimate of the regional volume loss but one property is that the glacier complex needs to be randomly sampled. This is probably often the case with ICESat (as soon as the study area is large enough so that the number of tracks is sufficient). This is less obvious with a centerline sampling of a selected number of glaciers.

We agree that extrapolation of centerline measurements to determine region wide glacier mass changes has lead to biases in regional estimates for Alaska so we have removed the reference to Arendt et al., 2006. It is sill not clear if this approach would introduce biases in other regions.

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Tables

Table 1. Maybe add a column with the glacier wide mass balance (a unit that is more useful than total loss to compare different glaciers/glacier complex) Maybe it would be useful for the reader if the 5 regions Barnes, Bylot, Penny, North, South were outlined in Figure 1 (South and North is not really obvious)

Table A1. Indicate exact date of survey YYYYMMDD. Did you define C.P. somewhere?

Added YYYYMMDD and defined C.P.

Table A2. I would have expected close to 0 mean difference “after”. Not really the case for Penny, C.P., Baffin. Reason? 1-year time difference with ICESAT? (Maybe add an explanation in the legend?)

Good question. We have now added the following text to the table caption: “The mean difference between ICESat/SPOT versus CDED after co-registration is not always zero because products are co-registered using a cubic spline fit to the median bias of 100 km by 100 km grid cells (see Figures A2 and A3).”

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Figures

Figure 1. Could you zoom in? The western part of the figure does not seem to be useful.

We’ve left Figure 1 unchanged so that we can continue to use it to show that locations of the weather stations.

Figure 2 (if retained). Make the vertical axis larger and the blue dots thicker

Modified

Figure 3. Add ICESat and ATM location where repeat measurements are available (in particular if you follow my General Comments #3).

We thought about doing this in the original submission but we decided that it was better to keep the figure clean and simple. This way it can be used by others, without modification, as an example elsewhere. For this reason we have not added the ICESat or ATM profiles to Figure 3. ATM profiles for Baffin are shown in Figure 7 and ICESat profiles are shown in Figures 1 and A4 (admittedly only at a large scale). See comment P1578, L15-1.

Figure 6 and 7. Avoid transparency for the insets. With a white background they would be more readable (those two insets are important results probably more important than the background map). On a right axis you could show the corresponding mass balance values (also true for Fig. 10).

Transparency removed from insets in Figures 6 and 7. We tried adding corresponding mass balance values to insets but the figure became too cluttered. Instead we have added the conversion between Gt/a and kg/m2/a to the figure captions. We also added second axis to Figure 10 showing mass change in units of kg /m2 /a

Figure A1.
Panes _ Planes

Corrected

Hard to understand the whole “bubble”: “filter dH by SPOT correlation score”
Filter out outliers. Here 3 sigma. It is for each altitude interval separately? Otherwise you risk filtering out rapidly thinning or thickening glacier tongues?

While this can be the case for other regions, especially when differencing over short time periods, we double-checked and found that the 3-sigma filter does not remove any usable data. This would likely not be the case for regions with rapid tidewater glacier thinning such as Alaska and a 3-sigma filter would certainly not work for Greenland or Antarctica.