

Dear Reviewer 2,

Thank you for your constructive comments, they have resulted in a much-improved manuscript. Both reviewers are clearly experts on the determination of mass changes using geodetic methods. All of your suggestions were very straightforward and helpful. We have adopted nearly all, see below. We are still working to include a curvature dependent bias correction into our process chain and will submit a revised manuscript as soon as that is done (see comment Page 1573, Line 9).

Sincerely,  
Alex Gardner and coauthors

Review of Gardner et al., (2012) “Long-term contributions of Baffin and Bylot Island Glaciers to sea level rise: an integrated approach using airborne and satellite laser altimetry, stereoscopic imagery and satellite gravimetry.” *The Cryosphere*.

### **General Comments**

The paper is very clearly written and provides a comprehensive explanation of the methods used to calculate both long and short-term mass loss rates for the glaciers on Baffin and Bylot islands from a comprehensive suite of elevation data. The paper provides a substantial new source of observations for the Penny and Barnes ice caps and the southeastern portion of the Canadian Arctic Archipelago. It provides compelling evidence that the recent regional rates of mass loss have accelerated compared to the long-term rates. I recommend that the manuscript be accepted for publication in *The Cryosphere*.

### **Specific Comments**

I am not very familiar with previous studies on the region. Similarly to reviewer one, I think the paper only needs some minor tweaks before it goes forward.

I have no specific comments, other than I disagree with reviewer one, and feel the GRACE analysis should be retained – it is interesting to see the agreement between the gravimetric and geometric methods, deriving about the same numbers in an independent manner.

### **Technical Corrections**

The title to the paper is too long, and the paper covers timescales of various lengths, not just long term. It may also be worth noting the location of the study in Canada in the title to aid those unfamiliar with the locations involved.

We've now changed the title to: **Accelerated contributions of Canada's Baffin and Bylot Island glaciers to sea level rise over the past 50 years.**

**Page 1564, Line 18.**

I don't think the comparison to Patagonia is particularly useful (these are two very different climate and glacial environments), maybe a comparison to Svalbard would be more appropriate?

We have removed reference to Patagonian glaciers.

**Page 1564, Line 25.**

I think it might be worth using the figures for Baffin and Bylot for 2006---2009 instead of the whole archipelago here ( $24 \pm 7$  Gt/yr, based on Gardner et al., 2011)

We have left this reference as is because it helps to place our new study in the context of the recent Canadian Arctic assessment of Gardner et al. (2011).

**Page 1566, Lines 1---4.**

Should cite Andrews et al., (2002). Page 1568, Line 12. Data are plural. Page 1568, Line 17.

Added citation

**From Page 1568, Line 21.**

Elevation data on a map with mixed inputs from optical stereo photogrammetry and radar interferometry can be problematic due to biases in the radar data. Please report the area filled in using the interferometry

This was also mentioned by the first reviewer. 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.

**Page 1569, Line 27.**

The date range involved is not particularly late summer. Maybe adding the dates to the actual acquisitions on the previous lines would be better. Page 1573, Line 3. Differencing.

We have removed this sentence and added exact dates of acquisition for all DEMs.

**Page 1573, Line 9.**

Please explore the effect of curvature as well as spatial---, slope--- and elevation---biases. See Gardelle et al, 2012. Journal of Glaciology. 58 (208), 419---422.

Initial examination shows some (not overly large) correlated bias with maximum curvature. We will work to include this in our process chain to better determine if the biases are significant and require correction. This will take some time to do.

**Page 1573, Line 11.**

..corrected for. These biases can result from...

changed

**Page 1575.**

I agree with the comment by reviewer one. Are the medians and means for each bin similar? Please more clearly define “Regional” on line 11, too. Were the “regional rates of volume change” completed on an ice cap by ice cap, or glacier complex basis?

Our estimates are insensitive to the use of elevation interval means versus medians for determination of  $dV/dt$ , all estimates agree within  $\pm 0.1 \text{ km}^3 \text{ a}^{-1}$ . This has been added to section 3.4.1.

We have removed “Regional” and now just say “Volume changes were estimated ...”

**Page 1576, Line 3.**

“GRACE” could be defined earlier in the paper.

We removed the definition, as we don’t think it is needed for the TC readership.

**Page 1578, line 13.**

Just a question --- is 5% appropriate? – What are area differences between early data and more up to date imagery?

Area differences between our new area estimate and previous estimates are given in Section 3.1. Our new estimate has 2 +/- 2% and 4 +/- 3% less area than previous estimates for Baffin Island and Bylot Islands, respectively. We’ve now tested our assumption and added the following text:

“To test if the assigned 2-sigma uncertainty of  $\pm 10\%$  is appropriate we re-ran our analysis with 1.5% more ice added evenly to the lowest 400 m of glaciers within each region. We chose 1.5% as this is roughly the midpoint between area differences (see Section 3.1) and can be expected to be the net affect on time-averaged mass change estimates. Changing the glacier hypsometry in this way resulted in sub-region losses that were 4 +/- 3% more negative than our estimate. Since changes in area are not well constrained we stick with an uncertainty of  $\pm 10\%$ .”

Note that in the revised manuscript we quote uncertainties for the 2-sigma interval instead of the 1-sigma interval.

**Page 1579, line 11.**

I agree with reviewer one that  $925 \text{ kg m}^{-3}$  for ice density is problematic

This has now been changed to  $900 \pm 17 \text{ kg m}^{-3}$ . See comment to Reviewer 1.

**Page 1580, line 7.**

I agree with reviewer one, for all areas combined the uncertainty should be summed.

As explained to Reviewer 1, we assume that all individual uncertainties are correlated in space but not with each other. Therefore to calculate the uncertainty of larger regions we take the sum of the individual components (correlated) for different regions and then take the RSS of the summed individual components (not correlated) to determine the region uncertainty. We have modified the text to try and make this clearer.

**Page 1581, line 16.**

Repeats line 13 to some extent (very small trend, trend is small)

Changed to insignificant

**Page 1584, line 22.**

Remaining

Fixed

**Page 1585, line 1---5**

Kind of a big deal can you do any better at attribution?

Abdalati et al. used a very low-resolution and poor glacier mask. We think this likely lead to large errors in the hypsometry used for the extrapolation and is most likely the cause of the large disagreement. Without redoing the analysis of Abdalati et al. this is about all we can conclude.

**Page 1585, line 14.**

A significant difference in the GRACE estimates (30%!) needs more fully explained, especially when their rate is from only one less year of data. Also their rate has a  $\pm 5$  Gt/yr uncertainty on it.

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:  $-33 \pm 2.5$ ) than our corresponding GRACE (2003-2009:  $23.8 \pm 3.1$  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. We think it's worth noting that it is better to discuss the magnitude of the disagreement (~10 Gt/yr) between GRACE estimates as oppose to % differences.

In the original submission all of our errors were 1-sigma, this is the reason for the difference between our stated uncertainty for Jacob et al. and their 2-sigma value. As noted earlier we will report 2-sigma errors in the revised manuscript.

**Page 1586, line 5.**

What height does the 700 mb equate to, approximately?

~2900 m a.s.l. (added to text)

**Page 1588, line 10 onwards.**

These are not great sentences to end on. They were almost an aside in the manuscript and their effects were not fully explained (what are the large errors due to geoid transformation errors, for example?).

Removed

**Table 1.**

Month of year in the date might become useful for shorter timescale.

This table is getting pretty full so we have not included months. Exact dates of SPOT DEMs are, however, now included in the text and SI.

**Figure 3.**

Unfortunately in your color scheme, the lakes are gaining elevation and the ocean is neutral.

We've now changed the color of the lakes to light grey

**Figure 4.**

Move the Navy Board Inlet label from behind the legend. There is a hint of thickening at higher elevation, but it is very hard to see due to all the data gaps. If you can make no---data a shade of grey, does your image still work? The elevation gain at the front of D78 is not obvious due to the colour of the adjacent lake.

We've now changed the color of the lakes to light grey but kept no-data as black.