Response to Anonymous Referee #3 – Second Response

Thank you kindly for this second review

Please find:

- Reviewer comments in back
- Responses in blue
- *Proposed changes to manuscript in italics*

Review of: Increased West Antarctic ice discharge and East Antarctic stability over the last seven years. Gardner et al., 2017
Summary
The authors have significantly revised this manuscript and incorporated most of the original reviewer requests. This effort is appreciated and the manuscript is very much improved because of it. However, there are some outstanding points where I do not feel that the authors response is satisfactory. I recommend minor changes to the manuscript to address these few remaining items.

Specific Edits
L36 – Mass change can be measured by multiple techniques with high precision and accuracy, e.g. gravimetry and altimetry in addition to velocity. Edit sentence to reflect that one technique, not all, require ice velocity to measure mass change.
While we agree that there are several measurement approaches for determining ice sheet mass change, this sentence specifically refers to the "difficulty in resolving continent- wide ice discharge". To the authors' knowledge, velocity is required to directly measure ice discharge separate from other sources of mass change.

The original comment still stands because although the authors response says their statement refers to mass discharge, the sentence in the paper is in not discharge specific. Its trivial to edit the paper to clarify this.

We have modified this sentence to read:
A major hurdle for improved attribution of mass changes determined from gravimetry and/or altimetry, and in determining mass changes themselves from the mass balance approach, is the difficulty in resolving continent-wide changes in ice discharge at high precision and accuracy for multiple epochs.

L349 – Nilsson et al 2016 was not the first publication to apply the surface fit solver to Cryosat data, therefore the authors should edit text to cite previous publications where this technique was developed. Moreover, the Nilsson et al 2016 paper documents a method for estimating altimetry mass change of Greenland, not Antarctica, where the firn processes and therefore processing challenges associated with it, are not the same,

as shown by Nilsson et al 2015. The Antarctica method should be explained in full, or an appropriate citation should be provided.
We agree with the reviewer that Nilsson et al was not the first to apply surface fits to CryoSat-2 data. There are however many approaches to applying surface fits. More importantly Nilsson et al. (2016) describe the full process chain used to go from the ESA L1b waveform data to the JPL L2 elevations and elevation changes. For this reason we feel that Nilsson et al. (2016) is the most relevant citation. Citations to other approaches of extracting elevation changes from CS2 data can be found in Nilsson et al. (2016).
Fine that the lower level processing is documented in the Nilsson et al 2016 paper, but the method used to go from elevation change to mass change from altimetry in a dry snow Antarctic environment is not documented at all in this paper. A citation must be provided for this step of the methodology, or if the authors haven't followed a previously published method, their technique for this part of the processing chain must be documented in full. The methods used for radar altimetry in Greenland have very different challenges from Antarctica, so it is not good enough to just cite a Greenland methods paper here.

We apply the methods as described in the Nilsson et al 2016 paper. We understand the reviewers concern that it is more challenging to extract elevation changes for low-density glacier surfaces where changes in volume scattering can contaminate the elevation change signals derived from radar altimetry. This is especially true with conventional radar altimeters and in the interior of the ice sheets. This is not as much of a concern in our application since we are only using elevation change result for the periphery of the ice sheet (between GL0 and FG1) where CryoSat-2 operates in SARIn mode, change signals are larger and volume scattering effects are reduced. In addition, our retracking approach is much less sensitive to changes in volume scattering than other waveform retrackers (See Figure 2 in Nilsson et al., 2016) and is similar to the methodology used by *Helm et al.*, 2014 to process CryoSat-2 data over both the Antarctic and Greenland Ice Sheets. It should be noted that *Helm et al.*, 2014 did not apply any ice sheet specific changes to their methodology nor any empirical correction for changes in waveform shape. In addition our results and conclusions are insensitive to centimeter scale errors in the estimated elevation and volume changes. For these reasons we argue that we have provided sufficient documentation of our elevation change analysis.

L353 – Edit the manuscript to explain how the authors have extrapolated elevation change at the ice sheet margins, where interpolation between two data points isn't possible. It's in this area that the highest rates of elevation change are located, therefore although the area is small, the numbers are significant, particularly given the way the authors are using this result in this paper.
We are not sure we fully understand the reviewer's request. This paragraph states that "The edited data was then interpolated onto a 1 km grid using the weighted average of the 16 closest grid points, weighted by their standard error from the least squares solution and distance.)

In regions of imbalance, the rate of elevation change typically increases from its maximum level at the grounding line, to decreasing magnitude inland. At the ice sheet edge, if the 16 closest pixels are inland, as is often the case because steeply sloping topography at the grounding line, filling the margin with the mean of the inland data points will lead to an underestimation of the real thinning rate at the grounding line. Consequently, interpolating across data gaps where the 16 closest pixels are distributed all around the gap, should ideally be handled in a different way where the data gap exists to one side of the 16 closest measurements. The authors should account for this in their method to avoid a marginal underestimation of the elevation change rates.

The reviewer is concerned that steep gradients in elevation changes near the grounding line are not being properly resolved with the distance weighted interpolation of the 16 nearest points. This is a classic problem of interpolation and is not easily satisfied, as changes in elevation are not always well correlated with ancillary information that might be used to improve the interpolation (e.g. mean velocity as used by *Hurkmans et al.*, 2012). Here we have chosen to use a distance + error weighted approach using the 16 closest neighbors which we feel is a reasonable approach that does not introduce significant uncertainty into our analysis as the distribution of SARIn elevations are relatively dense compared to earlier conventional altimetry that had more difficulty in high-slope areas. While imperfect, we feel that our approach is sufficient for our purposes and can likely be improved upon in future investigations. As can be seen in Table A1 the dynamic volume change correction for the entire Antarctic is 16 Gt/yr or less than 1% of total discharge so any further improvement in the estimate of dynamic volume change will have minimal impact on our results. To provide the reviewer with a better sense of what the elevation change patterns look like we have included a figure here showing a close up of the area with the majority of the dynamic volume change (Amundsen Sea Sector):

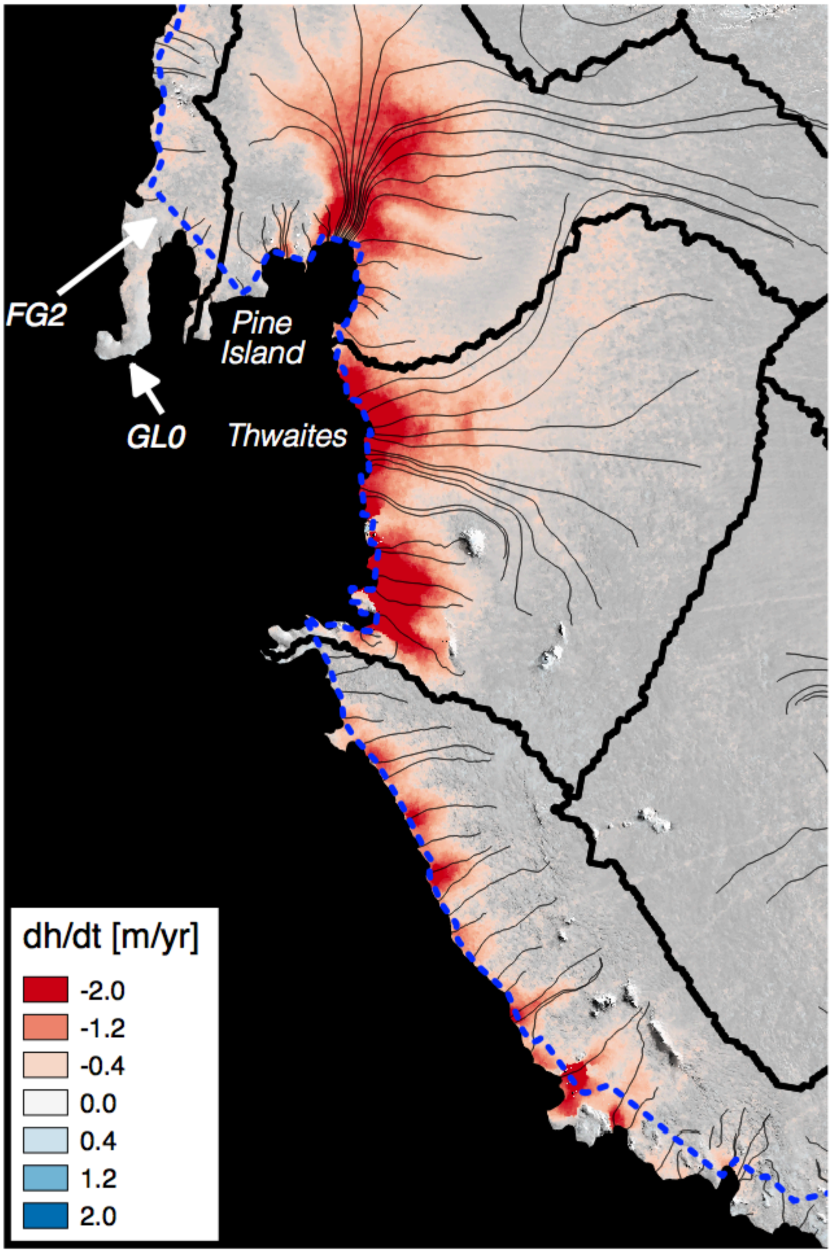

Figure 1: Zoom of Amundsen Sea Sector elevation changes derived from CryoSat-2 data. The integrated area between *GL0* and *FG2* having ice velocities > 200 m/yr were used to compute the dynamic volume change correction.

L437 – The spatial pattern of speedup on Law dome looks like its associated with the spatial distribution of image tracks. Can the authors demonstrate that this speedup is not just an artefact caused by a processing error?

Point well taken. The radar tracks are clearly visible in the Figure 8 inset image for this region. Underwood and Bond glaciers acceleration (~40 m/yr. in places) signals exceed the radar errors that are like due to residual ionosphere effects (~20 m/yr.). Even so we have added cautionary language to account for the increased error in this region: "The region to the west of Law Dome, including Underwood and Bond glaciers,

shows evidence of some increased flow speed and ice discharge, though the signal is near the limit of detection."
This isn't sufficient. The processing artefact isn't in the error estimate, so it wouldn't be possible for users of the data to filter bad data out based on the available quality information. Along with this, the magnitude of this velocity error is the same, and in some cases larger, than velocity change elsewhere that the reader is expected to trust is real. So the author's statement that the signal is at the limit of detection is wrong, unless other 'real' signal is below the limit of detection. The authors should remove known errors in their dataset, or transparently state in the paper that their data shouldn't be trusted in this region.

The stripes in the velocity differencing are largely a result of artifacts in the previously published radar velocity mosaic of Rignot et al. 2011. This dataset is provided with an error estimate but "these estimates should be used more as an indication of relative quality rather than absolute error". Maximum errors are all < 20 m/yr. with larger errors concentrated over Wilkes Land (Figure 2 below). Even so, acceleration patters are evident at the fronts of both Bond and Underwood glaciers, this becomes more evident when the color scale is changed (See Figure 3 below). We have modified the main text slightly to be more explicit:

The region to the west of Law Dome, including Underwood and Bond glaciers, shows subtle evidence of some increased flow speed and ice discharge, though the signal is near the limit of detection in part due to larger errors in the earlier radar mosaic for this region.

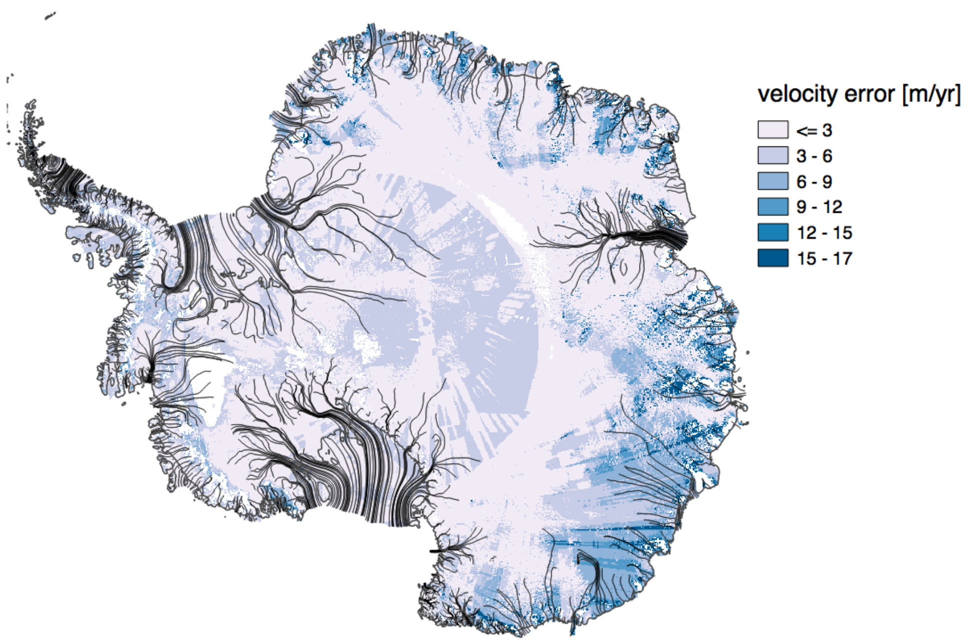

Figure 2: Error estimate provided with radar velocity mosaic (Rignot et al., 2011)

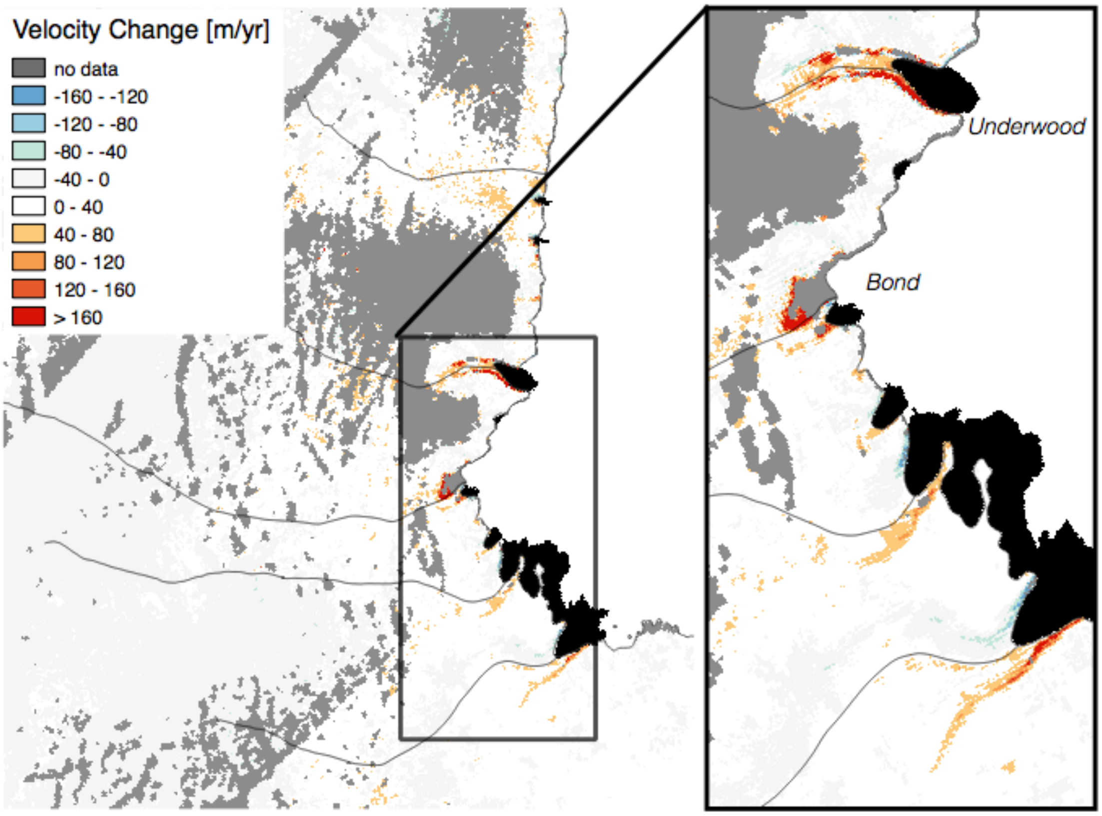

Figure 3: Change in surface velocity between circa-2008 radar mapping and 2015 Landsat mapping. Same as Figure 9 of the main manuscript but with different color scale to accentuate speedups at glacier termini.

L760 – The spatial pattern of change in ice speed on Pine Island Glacier, shown in Figure 8, isn't in agreement with change in speed presented elsewhere, and published in Mouginot et al (2014). The authors should discuss if the pattern, (specifically the two separate patches of high speedup), is a real signal, or if it is due to an error in one of the datasets?

The reviewer makes a very keen observation. Our map of velocity change shows an area of peak velocity change at 50 km upstream of the grounding line and a secondary peak at 110 km from the gl. We see no such peak when comparing between Landsat products, which makes us confident that the secondary peak is not an artifact of the Landsat processing. One possible non-geophysical explanation is that the radar mosaic includes data from a period significantly earlier than 2008 for this area. We have include a mention of this in the revised manuscript.

I still don't have any confidence in the velocity change signal on pine island glacier. In my opinion, it looks like an error in the data. It is critical that the authors correct the data, or prove using an independent dataset or technique that this is not the case, because their result currently contradicts previously published results (Mouginot et al, 2014). The fact that there is no peak difference between the two Landsat products, doesn't rule out the more likely scenario that there is an error in both velocity products given the lack of independence between the input data or processing technique. The problem area on Pine Island Glacier is a particularly

challenging area for the feature tracking technique to perform well because in recent years, a series of very regularly spaced, arch shaped crevasses have formed on this section of the ice stream. The uniformity of the features may lead to the feature tracking algorithm miss identifying, and overestimating ice speed. This problem is not present in historical velocity datasets because the arched crevasses were not nearly so pronounced. If the authors find that their secondary patch of high velocity difference is located over these crevasses, then I strongly recommend they provide independent evidence to prove that the speed change is real. If this additional work isn't done I think there is a very real risk that they will publish a result that is incorrect.
Given the high interest in Pine Island Glacier, this is likely to be one of the most used areas of their dataset, therefore its particularly important that the results are correct in this region.

Unfortunately there is not much more we can do to address the reviewers concern, as the spatial pattern in question is a result of the pre-existing radar mosaic used in the analysis. To demonstrate this we difference the 2014_2015 radar/optical velocities presented in *Mouginot et al.*, 2017 with the circa-2008 radar mosaic (Figure 4 below). This results in a nearly identical spatial pattern as the one presented in Figure 9 of the manuscript, which gives us confidence that the peculiarity of the spatial pattern does not originate from the Landsat dataset generated as part of this study.

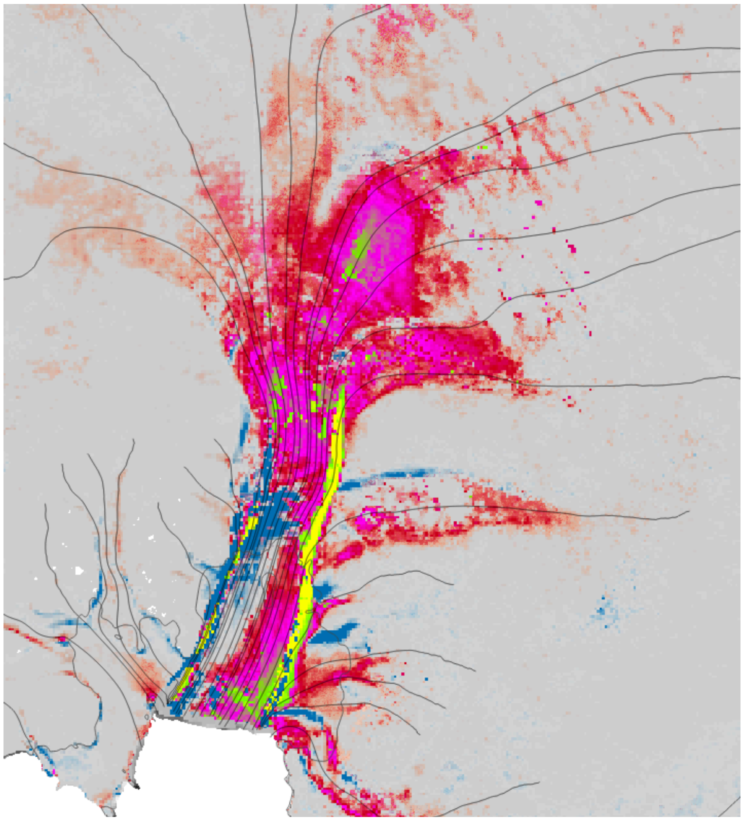

Figure 4: 2014-2015 radar derived velocities (Mouginot et al, 2017) minus circa-2008 radar velocity mosaic (Rignot et al, 2011). Same color scale as used in Figure 9.

References:

Helm, V., A. Humbert, and H. Miller (2014), Elevation and elevation change of Greenland and Antarctica derived from CryoSat-2, *The Cryosphere*, *8*(4), 1539-1559, doi:10.5194/tc-8-1539-2014.

Hurkmans, R. T. W. L., J. L. Bamber, L. S. Sørensen, I. R. Joughin, C. H. Davis, and W. B. Krabill (2012), Spatiotemporal interpolation of elevation changes derived from satellite altimetry for Jakobshavn Isbræ, Greenland, *Journal of Geophysical Research: Earth Surface*, *117*(F3), F03001, doi:10.1029/2011JF002072.

Mouginot, J., E. Rignot, B. Scheuchl, and R. Millan (2017), Comprehensive Annual Ice Sheet Velocity Mapping Using Landsat-8, Sentinel-1, and RADARSAT-2 Data, *Remote Sensing*, *9*(4), 364.