Response to Anonymous Referee #1

Thank you greatly for taking the time to provide a thorough review of our manuscript.

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

General comments
The manuscript shows the present-day Antarctic-wide surface velocities using Landsat7/8 images and an assessment of mass discharge change compared to earlier ice velocity map inferred from synthetic aperture radar. The work itself is of significance for the glaciology community to help to understand the present-day situation of Antarctic ice sheet. But the manuscript do not provide new insight to scientific community.

We're sorry that you did not glean any new insights form the paper. Here we highlight some of the scientific insights that we are excited to share with the broader community:

Despite numerous papers on the mass budget of the ice sheet (e.g. *Harig and Simons*, 2015; *Shepherd et al.*, 2012; *Velicogna*, 2009; *Wouters et al.*, 2015; *Zwally et al.*, 2015) and its total discharge (*Depoorter et al.*, 2013; *Rignot et al.*, 2008; *Rignot et al.*, 2013), its change in ice flow has not yet been directly measured on a continental scale. We feel that this in itself is a significant result as it provides the first comprehensive measurement of the dynamic response of the ice sheet that is primarily the result of changes in the rate of ocean melting and/or changes in buttressing (ice shelf back stress). Such a result allows for the disaggregation of the plethora of mass change results into the primary mechanisms of change: ice flow (ocean influenced) and precipitation (atmosphere influenced). All earlier studies attempting to do this (e.g. Harig and Simons, 2015; *Shepherd et al.*, 2012) have had to rely on large assumptions to infer such separation.

Our analysis reveals several previously undocumented and large changes in ice sheet flow. One major finding is that the glaciers feeding into the Getz Ice shelf have accelerated in response to recent ice shelf thinning and are now losing mass at a rapid rate as a result of a sharp increase ice discharge, this has not been documented elsewhere. Or analysis also reveals that the fastest speed-up of any Antarctic glacier is observed for the set of glaciers feeding into Marguerite Bay and is highly suggestive of enhanced ocean forcing in that area. Likely the most significant and novel result is our finding of a highly stable East Antarctic Ice Sheet over the period of study with virtually no change in discharge between 2008 and

2015. While recent stability has been inferred from gravity and volume change studies it has never been measured directly. Our study provides strong evidence that despite large glacier response to changes in ocean in the Amundsen Sea sector the East Antarctic has seen none. On top of all of this we also confirm the continued slowing in the rate of acceleration of the Pine Island and acceleration of Thwaites glaciers previously documented by *Mouginot et al.* (2014), that Totten glacier has not seen any recent increase in discharge as previoulsy proposed by *Li et al.*, 2016), and that the much discussed Bellingshausen Sea sector of the ice sheet has experienced only highly localized increases in flow with little change in total ice discharge over the 2008-2015 period in close agreement with *Hogg et al.*, 2017.

It is our opinion that these results will be of interest to the atmospheric and snow communities that study changes in Antarctic precipitation and to the oceanic community that is interested in studying the response of the ice sheet to changes in ocean circulation and temperature. This result is also invaluable to the ice sheet modeling community whose focus is on projecting changes in ice flow into the future.

I have five major concerns in the matter.

1. There is another manuscript in discussion in TC submitted earlier. Both papers are discussing the same issue. Although some results seem to be similar, my concern is that the both seem to draw different conclusions in term of ice discharge change in Antarctic ice sheet. The causes of the differences should be discussed in details. Shen, Q., Wang, H., Shum, C.-K., Jiang, L., Hsu, H. T., and Dong, J.: Antarctic high-resolution ice flow mapping and increased mass loss in Wilkes Land, East Antarctica during 2006–2015, The Cryosphere Discuss., and https://doi.org/10.5194/tc-2017-34

We closely followed the submission by Shen et al. who did a similar analysis to the one presented here. That paper was recently rejected so we do not feel that it is appropriate to comment on the specifics of their findings. That said, large differences in flux estimates typically result from:

a. Differences in the velocity fields. Despite using much of the same imagery to generate the offset fields, Landsat velocity fields are highly sensitive to outlier rejection criteria and to the determination of the image pair geolocation corrections.

b. Definition of the flux gate. Estimates of total flux are sensitive to the definition of the flux-gate cross-section. In particular, errors grow quickly with increasing reliance on interpolated estimates of ice thickness.

Access to the component velocities and the flux gate definitions used to determine the flux are required to identify the exact cause of the discrepancy, this can not be determined from the results presented in the paper alone.

2. The estimation of uncertainties of ice discharge changes were not rigorously based on error propagation law by an intentional and non-scientific method so that small estimates were obtained. Accordingly, uncertainties of ice discharge changes were obviously underestimated (see Table 2). For example, the uncertainty for all of Antarctica should be ±56 Gt/yr (sqrt(41^2+38^2)), not ± 15 Gt/yr (see Table 2) as stated in the paper. The uncertainties for individual basins and sectors (East Antarctica, West Antarctica and AP) were also underestimated. The estimate of the ice discharge change in the Antarctica should be 35±56 Gt/yr. Therefore, it is incorrect to conclude that there was a certain increased ice discharge since ~2008. There are no significant acceleration of ice discharge in West Antarctica using a correct uncertainty estimate for ice discharge change, rather than increase ice discharge as mentioned in the title.

We can understand the Reviewer's confusion. The appropriate propagation of errors in geophysics is non-trivial task and requires some degree of understanding of the correlation of each error term that are not always well known. For two fully independent estimates of mass flux, having normally distributed and random errors, the difference between the two terms is simply the Root Sum of Squares (RSS) of the individual errors, as suggested by the reviewer. However, when a single definition of ice thickness is used, estimates of flux have large systematic errors that mostly cancel when differenced.

Here we provide an illustrative example of why errors should not be propagated as suggested by the reviewer.

The change in flux between times t1 and t2 and its uncertainty are a function of the change in the velocity and not the velocity magnitudes. Assuming an ice thickness of 1.0 km +/- 0.1km, a gate width of 1km, and that ice thickness is the only source of uncertainty, a depth averaged velocity increase of 1 km/yr normal to the gate cross-section would result in a 1km^3/yr. increase in flux with an uncertainty of +/- 0.1 km^3/yr .

Now if the velocity at t1 and t2 were 10km/yr. and 11km/yr., respectively, the total flux at t1 would be 10 +/- 1 km3/yr. and 11 +/- 1.1 km3/yr. at t2. Taking the RSS of the flux magnitude uncertainties (sqrt(1.0^2 + 1.1^2) = 1.5 km^3/yr) overestimates the error in the change in flux by an order of magnitude.

We detail the propagation of errors in Appendix A with uncertainty in the changes in discharge specifically detailed in Section 1.2A. Hopefully this example helps to explain why errors for the change in discharge are smaller than errors in total discharge, a concern revisited by the reviewer throughout his/her comments.

3. The paper stated that in the calculation of ice discharge, the uncertainties were apparently reduced due to the extensive use of RES data. But I do not think that the use of RES can really reduce the uncertainties. At first, the uncertainties of dynamic

volume and surface mass balance were not shown in the tables of the manuscript. In general, the uncertainty of firn densification model is relatively large during the transfer between elevation change and mass change but was not shown. Additionally, the elevation change was directly considered as the dynamic volume which is problematic, because there are many driven factors of elevation change of ice glacier/sheet, for example, firn densification, the snowfall change, basal melting etc. The surface mass balance is another large error source to the uncertainty of ice discharge using the FG2. Furthermore, the small estimates of uncertainty may result from the large number of statistical units as much as 27. For example, in the paper, the uncertainty of total ice discharge were calculated based on 27 basins, while Depoorter et al. (Nature, 2013) estimated the uncertainty based on six oceanic sectors, and Rignot et al. (Science 2013) summed the uncertainties of each calculation units (ice shelf). More importantly, calculation of ice discharge is highly sensitive to the definition of the flux gate, the intentional movement of grounding line could cause the over 20% error in individual ice discharge even if the RES data are used. Therefore, the method for the calculation of ice discharge needs to be rigorously validated before use although this method was previously proposed by other authors.

OK, there is a lot to cover in the response to this comment. To be clear we do our best to account for errors resulting from uncertainties in surface mass balance, firn air content (depth averaged density), change in firn air content, surface velocity, elevation change, and the assumption that surface velocities equal depth averaged velocities. We try here to address each of the reviewer's specific comments:

I do not think that the use of RES can really reduce the uncertainties
Use of Radar Echo Sounding (RES) measurements to define ice thickness greatly reduce errors in flux gate areas relative to interpolated estimates such as BedMap2 (cf. *Li et al.*, 2016; *Rignot and Kanagaratnam*, 2006; *Rignot et al.*, 2011). Improvements come from lower errors in estimates of ice thickness (as shown Figure 4b) and higher resolution data that reduces resolution dependent systematic biases in flux (as shown in Figure 5).

At first, the uncertainties of dynamic volume and surface mass balance were not shown in the tables of the manuscript. In general, the uncertainty of firn densification model is relatively large during the transfer between elevation change and mass change but was not shown. Additionally, the elevation change was directly considered as the dynamic volume which is problematic, because there are many driven factors of elevation change of ice glacier/sheet, for example, firn densification, the snowfall change, basal melting etc.

All error sources mentioned by the reviewer were taken into account in the initial submission. One point of note is that in applying the mass conservation approach we only rely on estimates of surface mass budget, dynamic thinning, and firn compaction for the area between the grounding line and the upstream flux gate. While uncertainties in surface mass balance (20%), elevation change (0.1 m yr^{-1} or

30% of the rate of change, whichever is larger) and firn change (see Appendix) are large; estimates are integrated over a small total area (6% of the total area of the ice sheet). To address reviewer's point that not all elevation change measured between the flux gate and the grounding line can be attributed dynamic volume change is valid. To deal with this we make the assumption that only elevation change occurring over ice with a velocity > 200m/y is counted as dynamic volume change. Overall this correction is very small and insensitive to the cutoff velocity.

For resubmission we will add an additional table to the Appendix detailing all error terms included in the analysis. We hope that this will address the review's criticisms and make our propagation of errors more transparent.

The surface mass balance is another large error source to the uncertainty of ice discharge using the FG2. Furthermore, the small estimates of uncertainty may result from the large number of statistical units as much as 27. For example, in the paper, the uncertainty of total ice discharge were calculated based on 27 basins, while Depoorter et al. (Nature, 2013) estimated the uncertainty based on six oceanic sectors, and Rignot et al. (Science 2013) summed the uncertainties of each calculation units (ice shelf).

We felt that an error in surface mass balance of 20% that was fully correlated (systematic) within each basin was a conservative estimate. Unfortunately there is no definitive practice for estimating and propagating modeled SMB errors as there are not enough in situ observations to rigorously quantify uncertainties in SMB over the large scales relevant to this study. To calculate errors appropriately some assumption must be made as to the correlation length of the modeled estimates. Depoorter er al. 2014 estimated the error for an earlier version of RACMO2 over ice shelves by comparing to in situ observations (primarily over Ross and Filchner-Ronne) with no assessment of correlation length. In that analysis they determined an average "local" ice shelf SMB uncertainty of 28%, slight larger than the 20% grounded ice uncertainty applied here. For unsurveyed grounded ice basins (see Table S1 of their manuscript) they assign an 11% uncertainty to the climatological SMB for each of their 6 sectors. The study of *Rignot et al.*, 2008 used output from an earlier version of RACMO2 for which they estimated absolute errors in accumulation varying from 10% in dry, large basins to 30% in wet, small coastal basins. In that study the Antarctic wide error was estimated to be 6% (compared to 5% estimated in this study). *Rignot et al.*, 2013 also use output from an older version of RACMO2 and report basin scale errors ranging from 7% to 25%, with an Antarctic wide error of 14%. *Shepherd et al.*, 2012 report modeled SMB errors of 5% to 20% depending on basin size and location. Considering the improvements in modeled Antarctic SMB by RACMO2.3 (*van Wessem et al.*, 2014) and the inclusion of higher resolution output for the Antarctic Peninsula (*van Wessem et al.*, 2016) we feel that the errors applied in this study are consistent with earlier studies and well justified.

More importantly, calculation of ice discharge is highly sensitive to the definition of the flux gate, the intentional movement of grounding line could cause the over 20% error in individual ice discharge even if the RES data are used. Therefore, the method for the calculation of ice discharge needs to be rigorously validated before use although this method was previously proposed by other authors.

Our estimates are not as sensitive to the position of the flux gate as one might initially expect since we apply a mass conserving approach when extrapolating measured flux upstream of the grounding line to estimates of discharge across the grounding line. In fact, such an approach can greatly reduce errors in discharge when uncertainties in ice thickness near the grounding line are large (cf. *Li et al.*, 2016; *Rignot and Kanagaratnam*, 2006; *Rignot et al.*, 2011). These basic principles are the same as those used for reconstruction of basal topography (*Morlighem et al.*, 2011). The size of the error is solely dependent on how well the mass flux terms (primarily SMB) can be quantified between the fluxgate and the grounding line. Since the flux gate used in this study is located in close proximity to the grounding line, for most basins, errors associated with the estimation of SMB and other flux terms are smaller than the uncertainties introduced by poorly known basal topography. Taking this approach reduces the uncertainty in the total flux estimate by 64%.

We will include an additional table in the Appendix detailing the individual errors and the magnitudes.

4. The authors (and also Shen et al. in review) used first Antarctic-wide ice velocity (Rignot et al. 2011, science) as a reference map, the reference year are 2008 and 2006 respectively. The MEaSUREs Antarctic ice velocity map (v1.1) was inferred from over a long period (1996-2009) according its production statements. The data were acquired as early as 1996. Therefore the SAR-derived ice velocity map as a single year is problematic. The new MEaSUREs products have released and annual maps from 2005 to 2016 can be obtained (http://nsidc.org/data/nsidc-0720), authors should use the new products to alleviate the problem prompted.

Thank you for pointing this out. This dataset was made available after our original submission. Now that it is public we have assessed the implications of using the older dataset in our calculations and summarize our findings here.

These new data come with more precise time stamps but at the expense of reduced horizontal resolution (1km vs. 450), reduced spatial coverage and larger uncertainties. To ensure that our stated time period of cira-2008 is appropriate we resample (linear interpolation) the original MEASURES radar mosaic to 1km and compare to the error averaged 2007_2008 and 2008_2009 velocities from the new dataset. Differences are less than 2 Gt/yr. for all basins except for Basins 12, 13, and 14 that differ by -4, -5 and -6 Gt/yr. respectively and basin 24 by -4 Gt/yr. Some of the difference can be attributed to real differences in flow for differences in temporal sampling but also from differences in uncertainties between products (the original MEASURES mosaic having lower errors, particularly for the East Antarctic) and from differences in horizontal resolution.

From this analysis we concluded that the best estimate of flux for the ~2008 period is still produced by the earlier MEASURES mosaic (higher spatial resolution and the lower uncertainty) that is derived from the same underlying data contained in the annual mosaics. We also determine the period "circa-2008" well characterizes the effective date of the earlier MEASURES mosaic. This data has been used previously to estimate total Antarctic discharge in Rignot et al. 2013 with a reference date of 2007 to 2008 and in *Depoorter et al.*, 2013 with a reference date of 2007 to 2009.

We will include a paragraph in the revised manuscript summarizing our analysis.

5. The authors used different products of ice velocity (M14/15, W14/15, L750 and L124) to estimate the change in flux across FG1, but the values are apparently different. For example, there are conflicting estimates of ice discharge changes in basins 8, 12, 13, 14. In particular, the discharge is decreased for the M14/15 while it is increased for W14/15. Noted that they used the same data, only difference is that the mosaicking methods. Unfortunately, for the choice of the velocity data, the author did not present any convincible standards. The accuracy of ice velocity products should be carefully assessed using the independent surveyed data.

Yes, there are clearly differences between mappings. Much of the difference can be attributed to product errors. As shown in Figure A2, the 2015 mosaics have the lowest uncertainties (used in this study), followed by the 2014 mosaics with the LISA products have the highest uncertainties (See Figure A2). Some difference between mappings can also be expected due to real changes in ice flow between effective dates of each map. Even so, the standard deviation between all flux change estimates is below the stated uncertainty in discharge listed in Table 2 for all 27 basins.

In the revised manuscript we will include a sentence or two in the Figure 6 caption to this effect. We also noticed that we failed to include a legend for the Figure 6 bar plots. We will make sure to include this in the revised manuscript.

Specific comments:
Ln 17: ' with a mean error <10 m yr-1' . The spatial distribution of error maps should be shown, and error of ice velocity should be carefully assessed using independent data.

While spatially distributed errors are informative they can be misleading without knowledge of the spatial correlation. Spatial errors are provided with MEASURES mosaic but the documentation states that "Error estimates for the velocity magnitude are located in the variable err; however these values should be used more as an indication of relative quality rather than absolute error". The most relevant metric to qualitatively assess error in the Landsat products is image pair count, which are displayed in Figure 1.

Most important for our study is the assessment of errors in velocity along fluxgates and how these errors are correlated with distance along the gate. For this we compare Landsat velocities (all 6 mosaics) to Radar velocities (MEASURES version 1) over East Antarctica. We make the conservative assumption that all differences can be attributed to measurement error (i.e. assuming no real change in velocity between mappings). The results of this analysis are shown in Figure A2. We conclude that point-scale differences are on the order of 30m/yr. but quickly decrease with averaging distance.

In the revised manuscript we will add a note that the image pair count can be used as a relative metric to judge quality of the velocity field. And that the assessment presented in Appendix A provides a more comprehensive assessment of absolute errors when assessing velocity change. We will also include a more explicit description of the validation analysis in the main manuscript instead of just in the Appendix.

Ln 18: 'is 1932±38'. The ice discharge estimate is obviously smaller than the previously studies. For example 2,048 ± 149 Gt/yr for Rignot et al. (2013) in Science, 2,049 ± 87 Gt/yr for Depoorter et al. (2013) in Nature. The ice discharge for 2008 is also smaller than previous studies as above. what are the causes? As mentioned in the general comments, the uncertainties were underestimated, and should be adjusted to their correct values. In addition, authors should show the differences of uncertainties using RES data or not.

Rignot et al. (2013) used the same Radar velocity mosaic combined with Operation Ice Bridge and BEDMAP-2 ice thickness data at InSAR derived grounding lines to determine a total Antarctic grounding line flux of 2048 +/- 146 Gt/yr with upscaling accounting for 352 Gt/yr. of the total flux. This compares to 1897+/-41 Gt/yr. presented in this study. We should fist note that estimates agree within stated errors. The most obvious reason for the difference in the central estimates is the definition of the flux gates. Rignot et al. (2013) mostly rely on BEDMAP-2 data while our study draws almost entirely from flight data. Another possible reason for the difference is the upscaling of results for unmeasured basins. For these basins the total flux is assumed to be the modeled climatological average surface mass balance integrated over the upstream basin. Such estimates have not been adjusted for losses due to basal melt, are sensitive to errors in the modeled SMB and to the delineation of the contributing basin area over which SMB is integrated.

Depoorter et al. (2013) estimate a total groundingline flux of 2049 +/- 86 Gt/yr. with up scaling for unmeasured areas (same as in Rignot et al. 2013) accounting for 476 Gt/yr. This study uses a different definition of groundling but otherwise uses the same data as used in Rignot et al., 2013. This estimate is significantly higher than ours. Again the definition of ice thickness and upscaling to unmeasured basins likely accounts most of the difference.

It should also be noted that Depoorter et al. (2013) and Rignot et al. (2013) both used output from an earlier version of RACMO that produced larger total SMB than the version of the model used in our study. Since SMB is used to upscale flux, this likely contributes some to the larger flux estimates. Similar conclusions were made for updated Greenland Ice Sheet discharge estimates that were lower than previous estimates (*Enderlin et al.*, 2014).

In the revised manuscript we will include a an additional paragraph discussing differences between our estimate of total discharge and those of Depoorter et al. (2013) and Rignot et al. (2013) along with likely sources of the discrepancy.

Ln 19:' 35±15 Gt/yr'. As mentioned in general comments 2. The uncertainty was apparently underestimated in Table 2. The underestimated uncertainty leads directly to a certain conclusion that there is an increased mass change since ~2008. This conclusion is obviously not convincible. It may mislead the scientific community.

Hopefully our response to general comment 2 has better explained our approach to the propagating errors.

Ln 19:'flow accelerations across the grounding lines of West ..., account for 89% of this increase' . A quantitative assessment of the uncertainties of ice velocities and their changes is required. We can not determine where there has a significant acceleration of ice flow from Figure 8 because most of the changes are less than 50m/yr in Figure 8. So the significance of flow acceleration should be first assessed under the consideration of a large uncertainty estimate for ice velocity (mean error of 10 m/yr and as high as 20-30m yr-1 (in Ln 63)) .

A quantitative assessment of velocities and correlation lengths is provided in Figure A2. All error terms are defined in Appendix A and are propagated according to the equations presented in Appendix A.

To make our propagation of errors more transparent we will include an additional table in the Appendix detailing the individual errors and their magnitudes.

Ln 63-64 : ' as high as 20-30m/yr locally but ...(see Appendix A for validation of the velocity fields).' Authors should show where is the area with the large uncertainties of ice velocity, in other words, should show error maps for all products. How did the authors get the conclusion of 'largely uncorrelated at basin scales'? Additionally, the Appendix A didn't show any validation of ice velocity fields, except only for ice discharge.

Please see response to Specific comment "Ln 17"

Ln 68 ‘collection0 LT1 images’. In Antarctica, the majority of images are in the processing level of L1GT, not L1T, except for some region in Antarctic Peninsula. The details of Landsat processing level can be found in the site https://landsat.usgs.gov/landsat-processing-details. ‘LT1’ is wrong, should be L1T.

Thank you fro catching this.

We will update appropriately in the revised manuscript.

Ln 94:‘all x and y displacements that fell outside of the range ... were culled from the dataset’. From the formula. It seems to all displacements were involved to estimate ice velocity, because Q3 equal closely to 95% (3sigma) and IQR equal to Q3-Q1, which closely equal to 2sigma, and T value is set to 3. Additionally, the method is possible to exclude the valid displacements, which inferred images acquired from a longer period. A longer period, and a larger displacement is expected. Furthermore, the cloud contamination is key problem in post-processing, the authors didn’t show how to deal with the issue.

Our use of the word “displacements” was incorrect. Interquartile Range filtering was applied to time normalized displacements (i.e. velocities).

Time normalized displacements are filtered using 3 then 1.5 times the inter quartile range (IQR = Q3 - Q1). If the data is normally distribute the IQR ≅ 1.3 sigma. This means that we reject data outsize of the 4- then 2- sigma range. This approach removes ~8-10% of the data. This filtering strategy is aggressive due to the low SNR of the dataset. We hope this addresses the reviewers concerns.

For cloud filtering, or more generally for filtering areas of without matchable features, we apply a Normalized Displacement Coherence (NDC) Filter.

In the revised manuscript we will replace “displacements” with “velocities” we will also provide a description of the cloud filtering.

Ln 106:’ with median velocities <10m/yr and with >100 valid retrievals’. The threshold may be set too large as reference velocity. Additionally, the use of image-pair velocity itself to define the static reference velocity fields may be problematic.

The reference velocity is assumed to be moving at a constant rate (not changing over time), it is not assumed to be stagnant. For this assumption to cause errors in our velocity mosaic there would need to be large areas of ice with very slow velocities that either had large trends or secular changes in velocity. We are not aware of any evidence that this assumption should cause concern for the Antarctic Ice Sheet. *Mouginot et al.*, 2017 use 20% of the lowest velocities within each displacement field. Given the vast area of Antarctic ice moving at >10 m/yr. this assumption is likely not radically different from our approach.

Ln 112:'have velocities <50m/yr and ...'. Same as above, the reference velocity may be set to large.

We find no evidence that this approach introduces significant error into the auto-RIFT velocity mosaic in a way that affects our results. This was determined through comparison to NSIDC's LISA mosaics (Figure 6), which does not adopt this criterion, and to MEASURES velocities over the East Antarctic Ice Sheet (Figure A2).

Ln 120: the threshold was set too large.

Please see response to previous comment

Ln 186. Why did the authors use only four weighting factors?

This was done for simplicity. Nearly all data used in the LISA mosaic is from image pairs separated in time by 64 days or less. As such this simplification has negligible impact on the final velocity fields.

Ln 226-227. 'We found that FG1 was the most suitable flux gate line for estimating changes...'. why did the authors use FG2 for ice discharge change in Table2. Ln 234-

Please see next comment (Ln 235)

235. 'We used this flux gate line to estimate absolute discharge ..., but not for assessing temporal changes in discharge'. In my view, in table2, authors used the flux gate to estimate the absolute discharge and its changes.

We see how this can be confusing. FG2 provides the cross-sectional area with the lowest uncertainty and is most appropriate for estimating the total discharge, even after having to account for additional mass input between the gate and the grounding line. FG1 strikes a balance between proximity to the grounding line (GL0) and the distance from ice thickness observations. This gate is best suited for estimating changes in ice discharge. Our best estimate of total discharge is computed using the 2015 autoRIFT velocities, FG2 and estimated mass flux between FG2 and GL0. We then compute the change in discharge between the 2015 and 2008 period at FG1 and subtract this form our best estimate of total discharge, accounting for dynamic volume change and changes in ice thickness between periods. Taking this approach we reduce errors in estimates of ice discharge by 64% compared to estimating ice discharge at the grounding line (GL0).

For the revision we will add clarifying text describing the suitability of the three gate definitions for determining ice discharge.

Ln 250. Authors didn't provide the SI.

In the original submission we did not include shapefiles with fluxgate definitions and attributes. We will make sure to include these with the final submission.

Ln 253-255. The error of grounding line could cause that ice flow don't drain outside in some nodes in the estimate of ice discharge in some areas, so that, the directions of grounding line and ice flow vectors should also be considered.

As long as flux is calculated using the component velocities (vx and vy), the ice flow vector is not needed. Here we defined the flux gate following polygon convention with the upstream side of the flux gate being defined as to the right hand side of the polygon gate vector as one move from node n to node n+1. The flux in the x-direction is then simply the flow in the x-direction multiplied by the width of the gate projected on the x-axis and the ice thickness all multiplied by the direction sign of the flux gate. The flux in the y direction is calculated following the same approach. The total flux is then the sum of the flow in the x and y directions.

In the revised manuscript we will expand the equation shown on L255 to include the summation of the component velocities as discussed here.

Ln 260-265. Authors should give the differences of ice discharges using GL0 and FG2 grounding lines respectively. As mentioned in general comment, in FG2 ice discharge, authors used SMB and cryostat-2 elevation change to correct the FG2 ice discharge. In my view, at first, the elevation change used to estimate the dynamic volume change is problematic. Because the elevation change do not result from the ice flow convergence, but from snowfall, firn densification, etc. secondly, the acceleration of elevation change in the gap region should be less than 10 Gt/yr because mass balance of Antarctic ice sheet is only about -70Gt/yr. The absolute ice discharge estimates in the paper is obviously smaller than those of previous studies. the possible cause for the matter is the two terms (SMB and elevation change) could not compensated the unmeasured ice flux due to the movement inland for grounding lines.

We are not confident that we follow the reviewers comment. Accounting for dynamic volume change and SMB between the upstream gate and the grounding line does introduce additional error into our estimate of ice discharge. Since these corrections are integrated over relatively small areas, their contribution to the total discharge error term is relatively small for most basins. It is also true that ice sheet volume change is a combination of dynamic mass change, surface mass balance anomalies and changes in the firn air content. We have separated dynamic volume change from SMB related volume change by applying a 200 m/yr. threshold to our CryoSat-2 elevation change results. Because this is an imperfect assumption we add a large uncertainty to the dynamic volume change of (0.1 m yr^{-1} or 30% of the correction, whichever is larger).

In the revised manuscript we will provide an additional table in the Appendix listing all correction magnitudes and associated uncertainties for all basins. We will also include a comparison of total discharge estimates using the three different flux gate definitions.

Ln 349. 'surface elevation changes and rates of acceleration were ...'. We are skeptical over how to estimate the acceleration of elevation change because the short period (from 2011 to 2015) and the acceleration must be obvious in the time series of elevation measurements, rather than the only mathematical analysis method.

We do not have altimetry data for the 2008-2010 period so we need to extrapolate rates of elevation change determined from CryoSat-2 data to the period of study. Our choice is to either apply the constant rate measured over the 2010-2015 period or to include some estimate of the change in rate through time (acceleration). Here we chose to apply a linear rate of acceleration since acceleration is expected in areas of rapid dynamics. Including an acceleration term has negligible impact on our total flux estimates since data is only being extrapolated over the 2-year period from 2008-2010. We hope that this better justifies our use of an acceleration term when estimating the dynamic volume change.

Ln 351. 'the magnitude larger than ±15 m/yr were culled'. Why did the authors use the threshold?

This is done to remove gross outliers (*Nilsson et al.*, 2016). 5-year elevation changes rates are not expected to exceed this threshold.

Ln 372, see general comment 3. Ln 444, see general comment 2.
Response provided above.

Ln 466. Figure 7 may be wrong, should be Figure 8?
Thank you for spotting this.

We will change this to Figure 8 in the revised manuscript.

Ln 740. Figure 6. The authors used three grounding lines for ice discharges, which make us confusing. In the figure, the FG1 ice discharges were used, while FG2 ice discharges were also used. This makes it is difficult to determine which grounding line is appropriate to estimate ice discharge. Additionally, as mentioned in general comments, the conflicting results of ice discharge change in basins 5,8,12,13,14,15,23 make it is difficult which ice flow product is correct, especially, in East Antarctica.

We see how this can be confusing. FG2 provides the cross-sectional area with the lowest uncertainty and is most appropriate for estimating the total discharge, even after having to account for additional mass input between the gate and the grounding line. FG1 strikes a balance between proximity to the true grounding line (GL0) and the distance from ice thickness observations. This gate is best suited for estimating changes in ice discharge. Our best estimate of total discharge is computed using the 2015 autoRIFT velocities, FG2 and computed mass fluxes between FG2 and GL0. We then compute the change in discharge between the 2015 and 2008 period at FG1 and subtract this form our best estimate of total discharge, accounting for dynamic volume change and changes in ice thickness. This is why results for FG1 are shown in Figure 6 (assessment of change in discharge). Taking this approach greatly reduces errors in estimates of total discharge (error of 5.6% for *GL0*, 4.4% for *FG1*, and 2.0% for *FG2*). GL0 is only used to determine the area between the gate and the grounding line for which corrections need to be applied. Temporal changes in the position of the groundling line only affect the area for which flux gate to grounding line corrections are determined. For the 7-year period of this study changes in in grounding line position have negligible impact on our results.

Our response to general comment 5 addresses the rest of these concerns.

For the revision we will add clarifying text describing the suitability of the three gate definitions for determining ice discharge.

Ln 760. Figure 7. A mis-coregistration between the L8 ice velocity and SAR-derived ice velocity is obvious because there are apparently positive/negative pattern of change in surface velocity, especially in Marguerite Bay, Getz ice shelf. The mis-coregistraton will affect the result of ice discharge and its change.

Geolocation errors that most likely originate in the radar data introduce noise into our analysis but are unlikely to significant biases our estimates of flux or flux change because: 1. Errors will somewhat cancel when integrated across the entire glacier cross section (speedup has corresponding slowdown on opposite side of glacier), 2. flux-gate nodes located in problematic areas are assigned a Landsat flux value and are assumed to be constant between time periods (see Figure 2), 3. Geolocation errors are represented in our assessment of the velocity error (see Figure A2).

We will include a statement to this effect in the revised manuscript.

Ln780-290. Table2 . Why did the authors used only the two JPL 2015 Landsat 8 velocity maps for 2015 ice discharge estimate. Why the other results were not included. The most important thing is that the uncertainties of ice discharge changes seems to intentionally underestimate. Although the authors attempted to give an explanation in the appendix A. The uncertainties of the changes should be estimated using the uncertainties of absolute ice discharges in 2008 and 2015. The concerns have been mentioned in general comments.

Hopefully our response to general comments 2 & 5 has addressed these concerns.

The AP seems to have a positive net mass changes (+11 Gt/yr) in Table 2, because SMB value is larger than ice discharge.

Knowledge of ice thickness, circa-2008 ice velocities and SMB all have large uncertainties for the AP. For this reason we do not use estimates of flux and SMB to determine the mass balance of the AP and instead rely on earlier estimates derived from ice volume change estimates (*Scambos et al.*, 2014) that we correct for measured changes in discharge. Our assumption of stable rates of mass loss for the AP is supported by repeat gravity measurements from the GRACE satellites as presented in Figure B1. For a detailed description of how the AP mass change was estimated we refer the reader to Appendix B.

We will include clarifying text in the caption of Table 2 to address this confusion.

Ln 860-875. The uncertainty in Flux-change estimates should be directly calculated from the uncertainties of ice flux in 2008 and 2015, rather than another method. Hopefully our response to general comment 2 has better explained our approach to the propagating errors.

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