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. 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

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.

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.
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.
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.

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.

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.

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.

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)) .

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.

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.

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.

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.

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

Ln 120: the threshold was set too large.

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

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 234-235. ‘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.

Ln 250. Authors didn’t provide the SI.

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.

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.
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.
Ln 351. ‘the magnitude larger than ±15 m/yr were culled’. Why did the authors use the threshold?
Ln 372, see general comment 3.
Ln 444, see general comment 2.
Ln 466. Figure 7 may be wrong, should be Figure 8?
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.
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.
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.
The AP seems to have a positive net mass changes (+11 Gt/yr) in Table 2, because SMB value is larger than ice discharge.
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.