Reply to the Referee #2's comments

Point-by-Point Responses

1. Q. …As already pointed out by reviewer number 1: Gardner et al (ref no tc-2017-75) use pretty much the same source data and methods but come to nearly opposite conclusions: flow acceleration in West Antarctica and 'remarkably' stable glaciers in the East. I am sure the reader/scientific community is eager for an explanation of these contradicting conclusions and I strongly support reviewer's 1 suggestion for both authors to provide an open comment to the other paper.

R. This comment was just proposed by reviewer 1, and we have carefully answered the question. Please check our response 1 to Referee #1. Thanks.

2. Q. My foremost concern, however, and also pointed out by reviewer number 1, is the fact that the velocity, discharge and mass balance comparisons are done with a reference dataset that is, frankly speaking, not suitable for the purpose for a number of reasons. The used dataset (Measures; Rignot et al., 2011) is an assembly of ice velocity maps acquired from multiple satellite missions with a temporal range covering more than a decade (see metadata description). While the velocity map is a nice looking and nearly gapless product useful for various purposes and can for instance be used as a rough indication of rapidly changing areas, the large temporal span precludes the use of it for change detection pinpointed to a single year as is done in the study.

R. We agree your point of view on SAR ice velocity, but it is difficult to discern the exact observation date for SAR ice velocity. We used the reference year for 2006 which are based on the published paper by Rignot et al.(Rignot et al ,NGeo, 2008). In this paper, they analyzed mass balance and its change between 1996 and 2006. Mouginot et al (2012) indicated the spare coverage in 1996 by SAR images. These lead us to select 2006 as reference year. Although the Rignot group just released updated SAR ice velocities on 25, April, 2017, since the released date is later to our submission date, we didn't use the new data. In fact, we used the InSAR velocity data covering 2007, 2008 and 2009 from Rignot et al. (2011), so '2006' should be changed into '2008'. The single year '2008' used is just for convenience, it actually denotes 2007, 2008 and 2009. These would be indicated in the text.

3. Q. Additionally, the relative coarse grid spacing, which is not so much of concern for the large ice streams in EAIS and WAIS, is not suitable for comparisons of ice velocity of the many small glaciers that are found in the (northern) Antarctic Peninsula and that are not well resolved. Slightly different processing settings could lead to very different results when inter-comparing with the newly derived L8 maps.

R. We found the change of ice velocity in northwest Antarctic Peninsula is very large due to low estimates of SAR velocities in 2008 while the low estimates of SAR velocities may be caused by relative coarse grid in SAR data as you mentioned. Gardner et al thought that they are caused by interpolation. Because the narrow outlet glaciers can still be found in that product of Rignot et al. (2011), interpolation may be not an only cause. A detailed comparison between our velocity and SAR velocity can be found in Response 20. Taking into account the large uncertainties of SAR velocity data, SMB and thickness data, although we estimated the ice discharges, mass balances and the changes in the northwest Antarctic Peninsula we did not draw conclusions there.

4. Q. Furthermore, focusing again on the Antarctic Peninsula as the region has one of the largest uncertainties in mass balance, the discharge and mass balance calculations here are also hampered by a lack of suitable ice thickness data.

R. As you said, the Antarctic Peninsula has large uncertainty caused by multiple factors, such as SMB, ice thickness and ice velocity (spatial resolution), surface runoff, and basal melting, etc. So we didn't draw a definitive conclusions for the Antarctic Peninsula. In the manuscript, we showed a positive mass balance mainly due to the new high-resolution SMB, we also analyzed the potential causes in main text.

5. Q. For many of the glaciers BEDMAP ice thickness is too coarse or based on interpolation without actual RES data requiring a careful check for each and every single flux gate with other sources of ice thickness. While it is a straight forward calculation using these datasets, calculating discharge or mass balance (and changes) here requires accurate, detailed and well-defined gates and velocities inpointed to distinct time periods, otherwise it is a rather meaningless number and not the improvement that is actually needed. I have the major worry that many of the surprising and rather extraordinary results (e.g. tens of glaciers in the Peninsula are reported to have accelerated by more than 600%, fig. 4, line 315) are simply the result of the issues described above

R. We agree that the BEDMAP ice thickness is relatively coarse, in order to eliminate or suppress its effects, we also used the ERS data, but the ERS data only occupy ~19% of all grounding lines. If as Gardner did in TC Discussion, the grounding lines are moved intentionally toward inland for much more coverage of ERS, the ice discharge maybe influenced. Based on our experiment in Bryd glaciers, the ice discharge could changes as much as 10-20% (~2-4Gt/yr) even if 5-14km inland movement (see Figure R1, GL0, FL1~FL6) and the ERS data are used.

Although SMB and elevation change in the gap area between flux gate and grounding lines can be used to correct the ice discharge from inland flux gate, the total contribution from elevation change and SMB is found to be only ~0.04Gt/yr for the gap between flux gate and grounding line, which is largely less than the magnitude of ice discharge variation due to gate placement movement. So the method moving the gate is skeptical. In addition, theoretically, the elevation change is not completely attributed to dynamic volume change, it is also caused by multiple factors such as firn densification, SMB variation and snow drift by wind, and etc. New error sources are thus also involved, such as for SMB and firn densification. The large acceleration in northwest Antarctic Peninsula is discussed in Response 3.

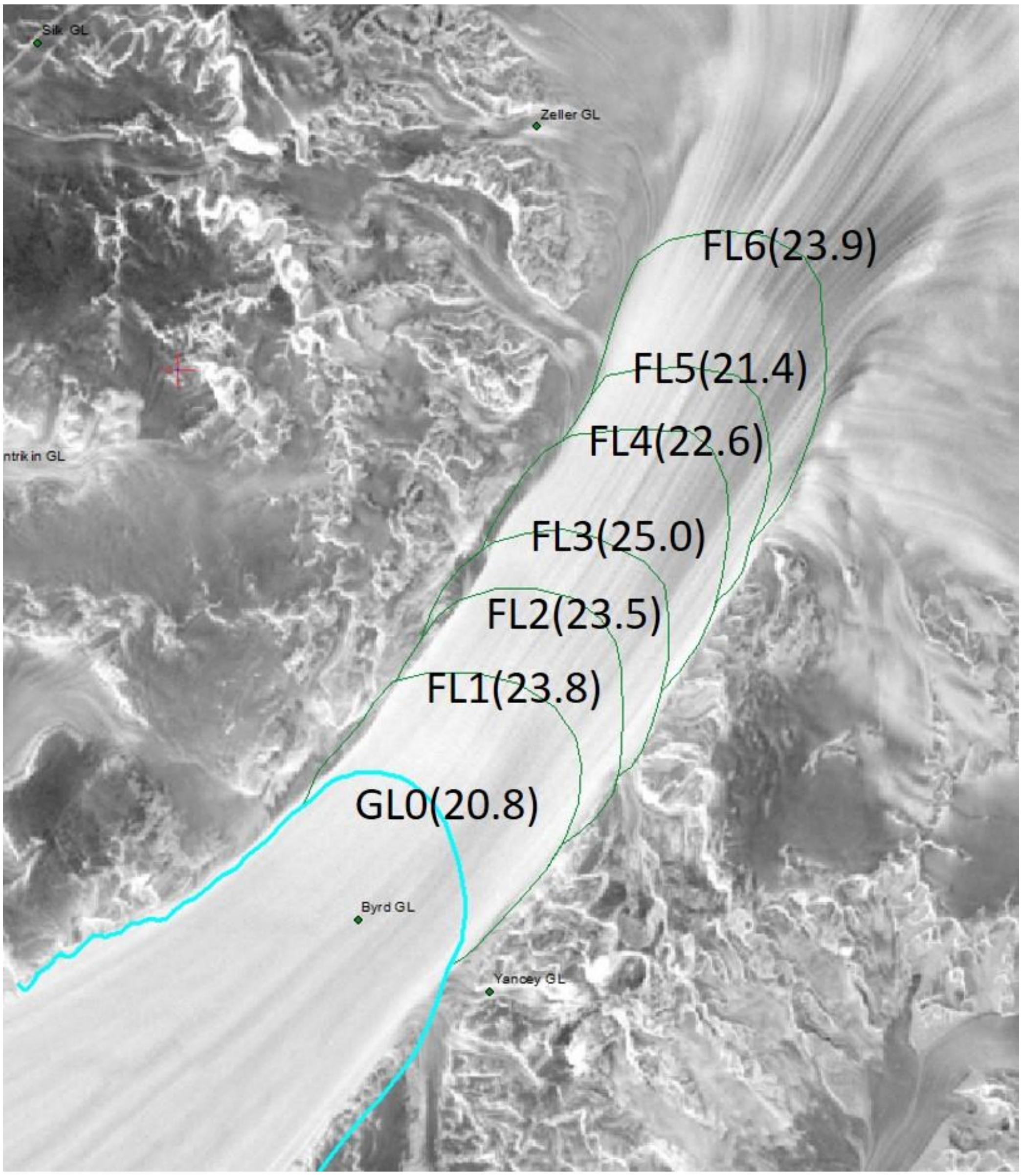

Figure R1. Grounding line and flux gate placements in Byrd Glacier. (GL0: grounding lines, FL (1-6): the locations of flux gate for sensitivity experiments. The values of ice discharge are shown in braces).

Point-by-Point Responses Specific Comments

6.Q. Ln 78: "the decadal changes can be easily found" – not with this dataset (see above)

R. the sentence is changed to "the changes of mass balances in the period can be easily found". The InSAR-derived ice velocity has been mentioned in former sentences.

7. Q. Ln 93-104: This paragraph seems a bit biased listing only positive aspects of optical feature tracking versus the limitations of using SAR data. One could just as well argue SAR data is more suitable being an all-weather, year-round technique (important for

time series), the possibility of detecting sub-surface features and a capability to derive true 3D velocities. Also, what about slower moving terrain?

R. As responded to Referee #1, we agree your point of view. We delete the description of the comparison between Landsat8 and SAR. And the descriptions on characteristics of Landsat 8 are now involved into the former paragraph.

8. Q. Ln 123-131: The accuracy assessment seems rather limited using only GPS (and other data) in slow moving terrain and also from a (in some cases) much earlier period. What does this say about the accuracy in fast flowing terrain at the margins, crucial for accuracy assessment of the IOM method applied in the study? Where any comparisons on faster flowing ice performed with contemporaneous data sets. What about sensor cross-comparisons, assessment of velocities on ice free terrain etc?

R. We originally used all field-surveying measurements in Velmap project to investigate the reliability of our results because it is very difficult find the contemporaneous data sets and the fast flow glaciers may have the velocity change in the time interval. In order to response your concerns, all data compiled in velmap project (hereafter using velmap velocity) and detailed comparison will be given in supplement file. Here, we show some cases. The first one (see Figure R2), in Ronne Ice Shelf close to the vicinity of Evans glacier, in the Label aaa/bbb, the 'aaa' strands for differences between InSAR-derived and Velmap velocities and 'bbb' is the differences between Landsat 8 2015 and velmap velocities. No matter for the high shear regions (blue ellipse), or other regions, the results for aaa and bbb are in general close each other, the high shearing area probably shown continuous slowdown of ice velocity since 2008. This showed the agreement between our L8 velocity and the Velmap velocity is similar to the case for InSAR velocity. This is also found in the north of Larsen B (Figure R3) and the downstream of Bindschadler glacier (Figure R4)

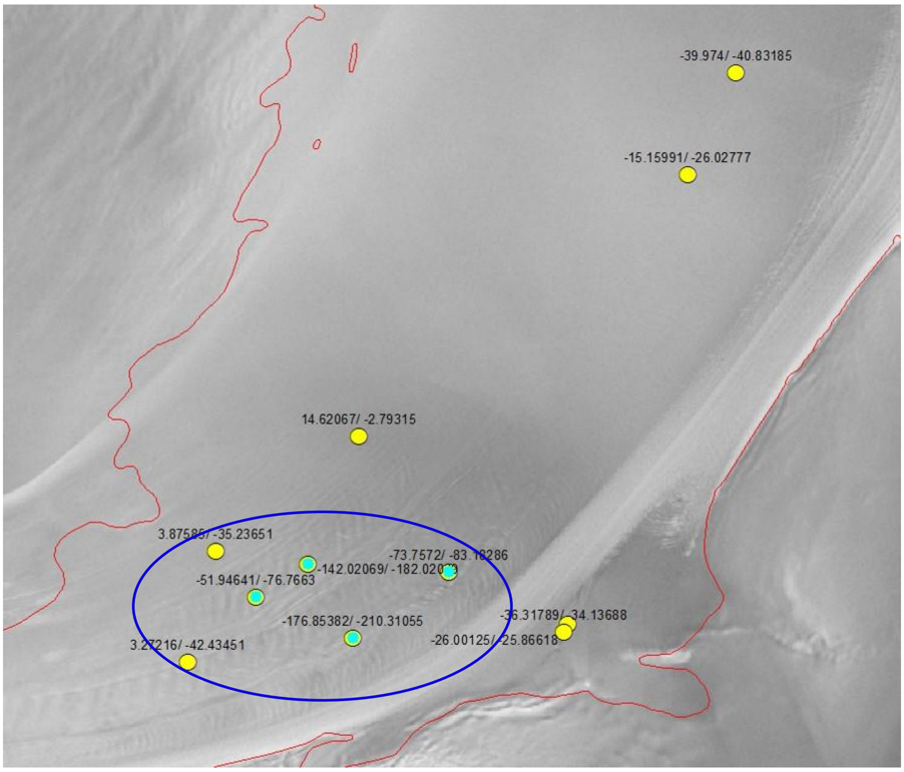

Figure R2. Ronne ice shelf in the vicinity of Evans glacier

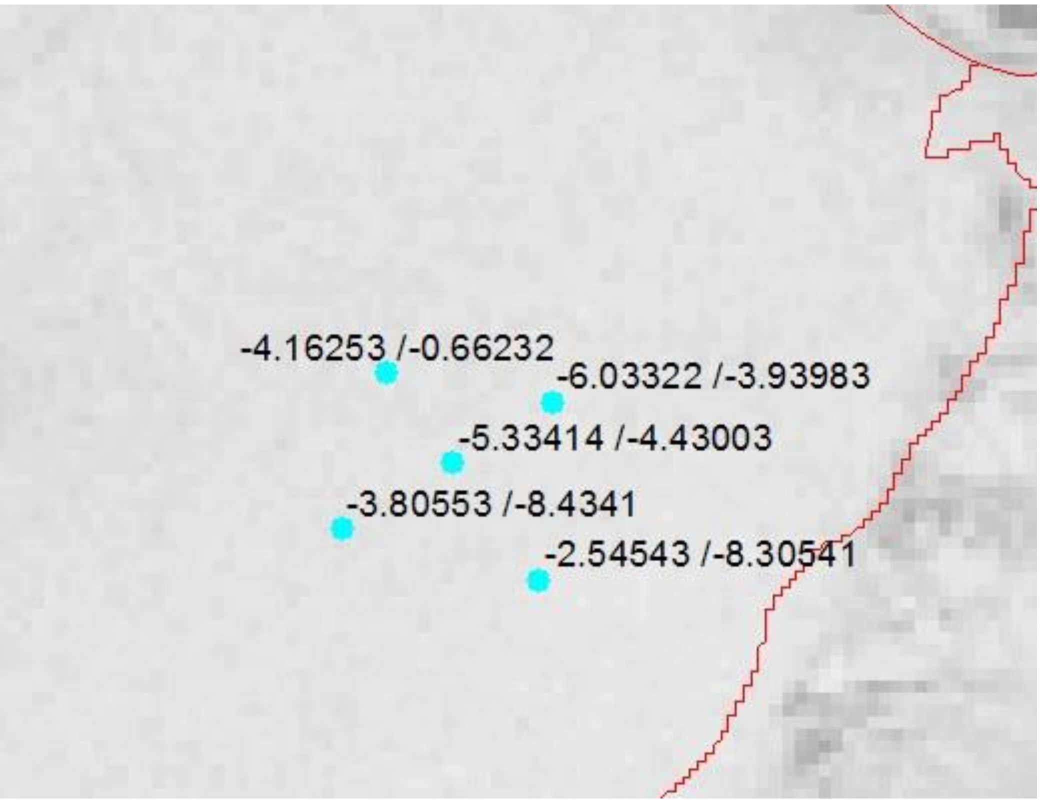

Figure R3. The north of Larsen B

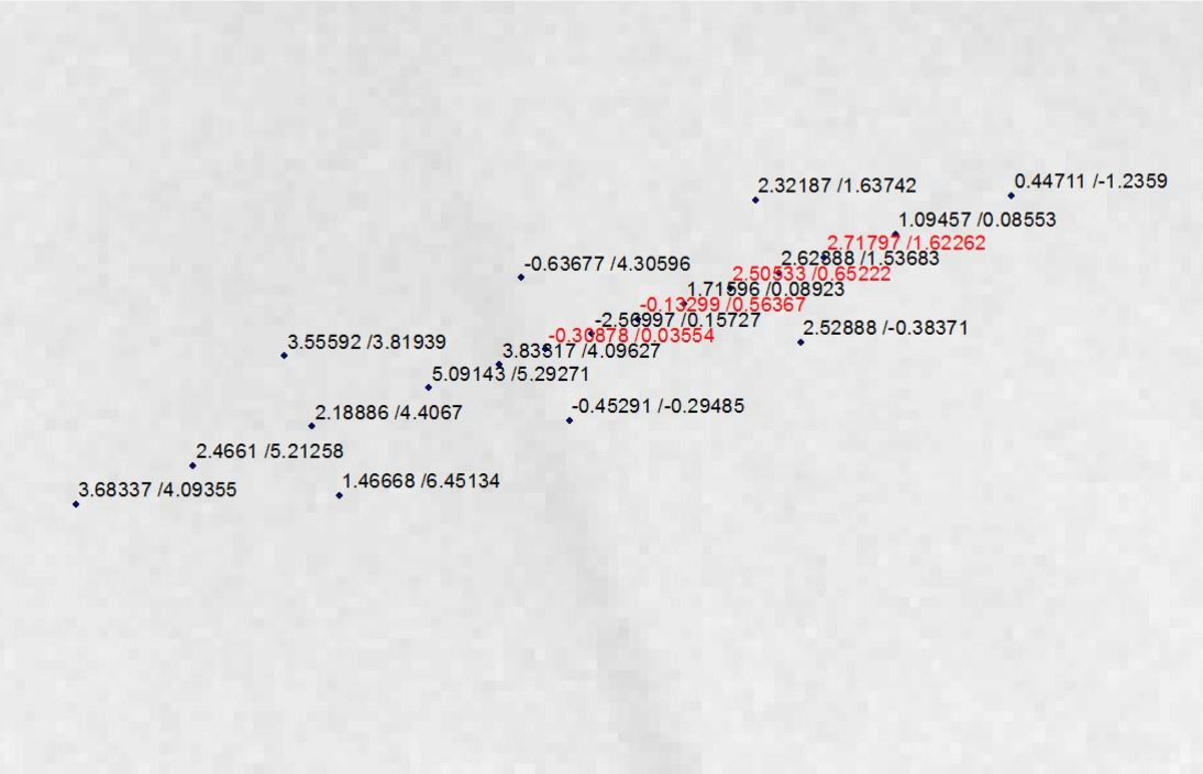

Figure R4. The downstream of Bindschadler glacier (in Ross ice shelf).

9. Q. Ln 132-157, section 2.2: Missing any mention of co-registration here. Was this performed/why not?

R. Because we used the released COSI-Corr software to obtain the displacement vector. We didn't discuss the details of the method. The procedure is based on the co-registration in frequency domain. The characteristics of the method are only shown in the SI.

10. Q. Ln 156: "100-meter resolution" -> I think grid spacing is meant here

R. we get displacement vector with 100-meter resolution since the displacement vector is expressed in image model of 100m resolution. Therefore, our ice velocity mosaic has a resolution of the 100-meter.

11. Q. Ln 162: "or rise" -> What is a rise in this context?

R. We mean the rise is similar to island but with smaller size.

12. Q. Ln 183-184: "The : : : experiments" unclear what is meant here and actually how the pairs are selected to ensure we still have a distinct "2014" and "2015" map

R. At first, we did experiments for some glacier areas, we found that the co-registration would fail if time separation is larger than two seasons (for example 2013-2014 summer, and 2014-2015 summer). The '2014' and '2015' are defined in caption of Figure 1, showing the start and end date. To check one couple of image pairs if they fall into a specific year, the time interval must lie between the start and end date of the year. For example, if the displacement vectors are inferred from the start image on

December, 2013, and end image on December, 2014, the result for ice velocity should be incorporated into ‘2014’.

13. Q. Ln 222: “spatial resolution”- not the same as grid spacing

R. Please see Response 10.

14. Q. Ln 229-231: “In : : : Antarctica”: better move to methods section, it is not a result.

R. Thanks, the sentence has been moved to method section.

15. Q. Ln 236: “resolution” - grid size

R. yes, corrected

16. Q. Ln 245: “conservatively set to be 1/25” - This seems not so conservative. Was this checked e.g. in stable terrain, or just assumed?

R. The value is larger than the suggested value (1/50) by Leprince et al. (2007) and 1/128 pixel for SAR velocity by Rignot (2011). Because it is very difficult to get the errors for each grid (pixel), here, we assumed there are same co-registration errors for each displacement scenes. The assumption is reasonable after the check in stable terrain (see section 3.3 in Main text).

17 Q. Ln 239-264: The uncertainty analysis seems to describe only mis-registration, what about other sources of error?

R. The other sources of errors included those for ice thickness, grounding line fluctuation, etc. and are described in SI.

18. Q. Ln 270-276: See comment above. Only slow-moving areas are used, could be very different on faster ice streams and glaciers used for the discharge/mass balance analysis.

R. Please see Response 8.

19. Q. Ln 280-281: Just looking at the histogram the agreement appears to be better/less skewed for InSAR derived velocity. Referring back to paragraph Ln 93-104 how should we interpret this?

R. The difference between our observation and InSAR-derived velocity is probably due to the real surface change in the long period, or the uncertainties of our observation. Same as the Response 7, we delete the description of the comparison between Landsat8 and SAR. And the descriptions on characteristics of Landsat 8 are involved into the former paragraph.

20. Q. Ln 292-295: A majority of the glaciers accelerated by more than 200% in the northern part of the Western AP: If true, this is a major finding that requires more careful checking. Did you check cross profiles for individual glaciers? Is it not just an artefact from the coarser gridding or different algorithm settings?

R. As the response 3, we compared the individual glaciers. InSAR-derived ice velocity is very small, especially in the grounding lines. In the next, we present a case in a glacier in WAP, the outlet glacier generally move fast in the front of the glacier and show in high-shearing area (See Figure R5). In InSAR-derived ice velocity, the velocity in the vicinity of grounding lines is unrealistically low (see Figure R6) while our observation shows more realistic.

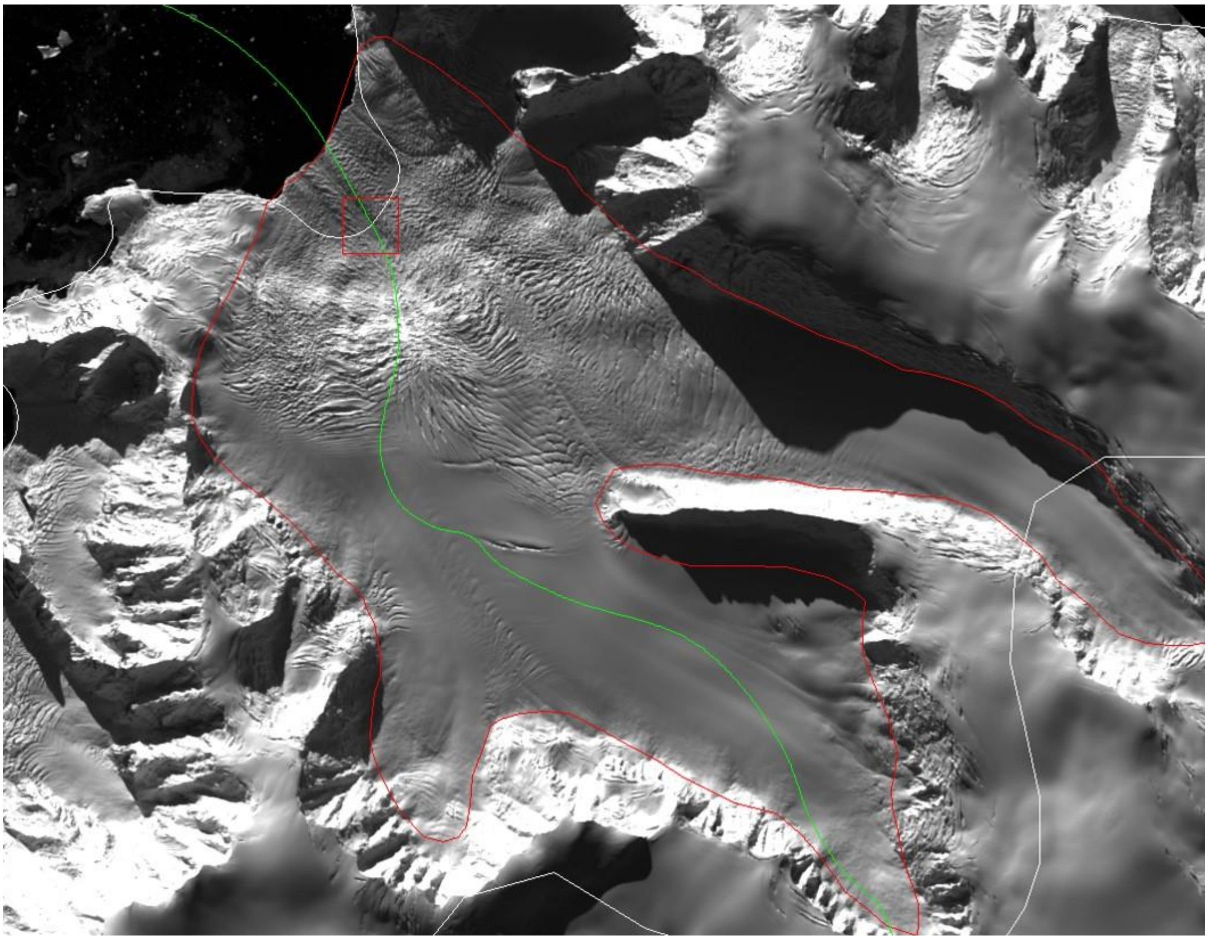

Figure R5. A glacier in WAP (white solid line: grounding lines; red lines: the area of the glacier; green line: profile (from upstream to outlet). The red box shows the crossover between grounding line and profiles.

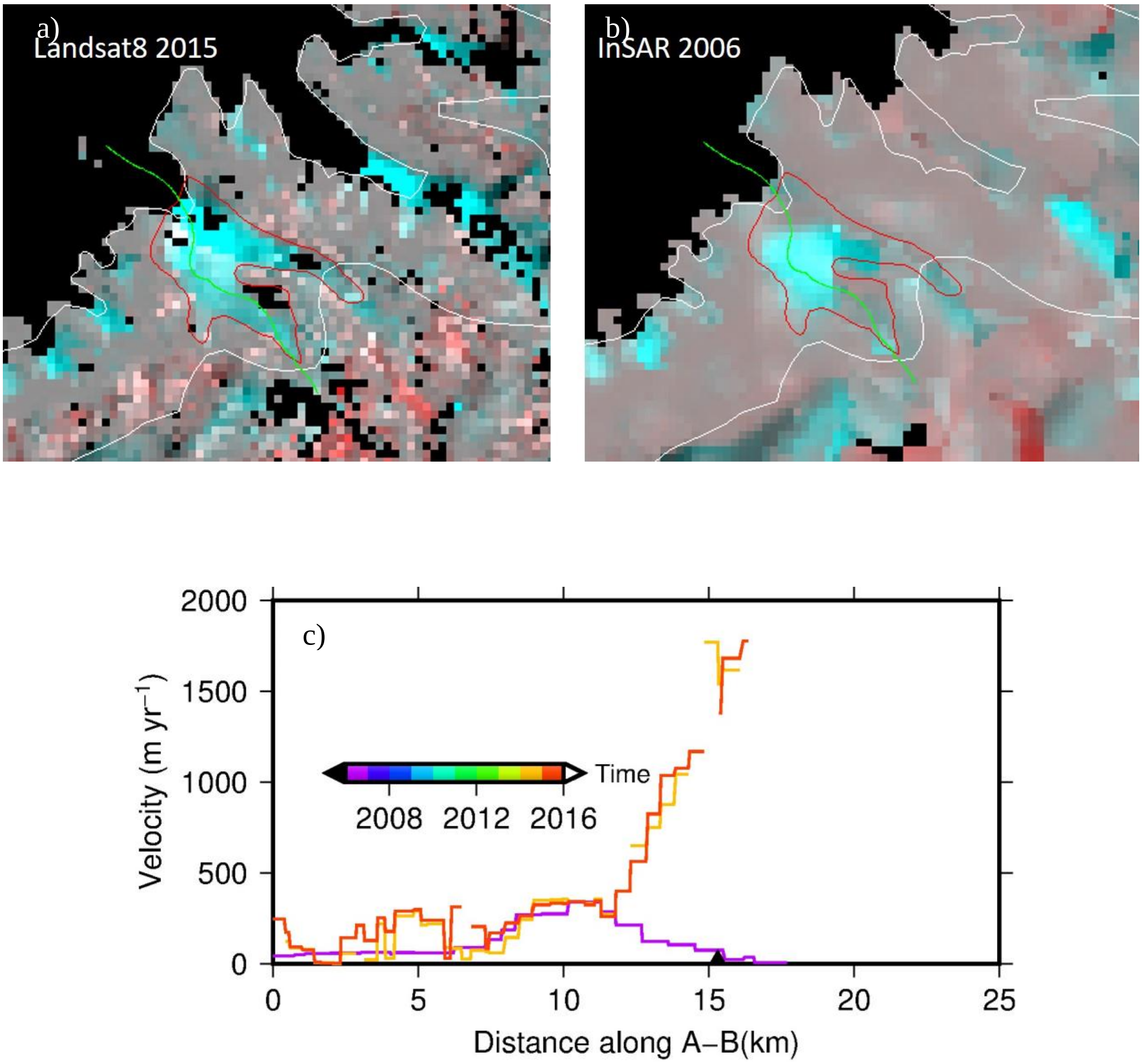

Figure R6 a) Landsat8 2015 ice velocity in the glacier, b) InSAR-derived ice velocity; c) the profiles of ice velocity from upstream to outlet (location: green line in Figure R5, and Figure 6a,6b). The black triangle shows the location of grounding line.

21. Q. Ln 315: See comment above, here it is written that most glaciers in the Western AP actually sped up by more than 600% (nearly all dots are white in this region). This really is an extraordinary result and needs to be checked/discussed in more detail.

R. Please see Response 20 for comparison. Since there are no field measurements available in the area, it is difficult to compare with field data.

22. Q. Line 322: "for 2006" - > not just 2006, see previous comments.

R. please see Response 2.

23. Q. Line 356-360: "In the Antarctic Peninsula, there was a positive mass balance (33±21 Gt yr-1) in 2015, contrary to previously studies" -> This statement requires clarification

and is in contradiction with other estimates (e.g. from GRACE). Seems impossible considering that so many glaciers have sped up by >600%!

R. Because the estimate of SMB used is larger than the previous estimate from low-resolution SMB product (27km or 52 km). It caused the positive mass balance in AP in our estimate. The previous SMB in AP is generally less than 100 Gt/yr, such as 94Gt/yr for Rignot et al. (2008) in Ngeo.

24. Q. Line 356-360: “probably due to a larger estimate of snow accumulation rate”. How much larger? Need to provide numbers to clarify the statement

R. please see Response 23.

25. Q. Ln 377-379: “likely linked to the incursion of warm CDW“ - Any further evidence to support this claim? From figure 7 the PTM in this area doesn’t strike me as being particularly high. Is there a relation between glacier thickness, speed up and ocean temperature data here?

R. For plotting, we used a PTM data beneath 200m and the data was obtained at a long time ago. This may not represent the current state of ocean, especially in EIS sector, but PTM data can show a warm oceanic environment in WAP. However, the recent measurement on Totten glacier may support our claim. Australian’s scientists reported warm water reaches the glacier and may be driving melt of the glacier from below. The new observation has been referenced in our manuscript. The more information for the link.
http://www.abc.net.au/news/2015-01-26/sea-water-melting-totten-glacier-in-antarctica-from-below/6047076

26. Q. Fig. 7: PTM color scale very unclear – seems to have 2 parts with greenish colors.

R. We tried many color schemes for high contrast between grounded ice and ocean. Although the bounder between ocean and grounded ice is not easily to discern because the two color scales have same color component, fortunately, the majority of boundary between grounded ice and ocean are filled by cold color scheme (blue and dark blue), which is helpful for the plot’s clarity.

27. Q. SM Ln 52: SMB is not the same as surface snow accumulation

R. corrected. Thanks.

28. Q. SM Ln 69: “total SMB of the Antarctic ice sheet is 1,901 Gt yr-1” – for which year or long-term average – needs clarification

R. we used the long-term average value of SMB.

29. Q. SM Ln 174: Drygalski Ice Tongue not ‘ice shelf

R. corrected, Thanks.

30. Q. SM Ln 298: Talev glacier is West coast and not in the Larsen B catchment.

R. corrected to Crane glacier, Thanks.