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# *Interactive comment on* "Antarctic high-resolution ice flow mapping and increased mass loss in Wilkes Land, East Antarctica during 2006–2015" *by* Qiang Shen et al.

**Anonymous Referee #2**

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Review of: Antarctic high-resolution ice flow mapping and increased mass loss in Wilkes Land, East Antarctica during 2006–2015

By Shen et al. for The Cryosphere Discuss. (Ms. Ref. No tc-2017-34)

General Comment

This paper by Shen et al. presents and provides an analysis of two new Antarctic ice sheet wide ice velocity maps derived from an archive of Landsat 8 images acquired in the period 2013-2016. The ice velocity is computed using an optimised automated feature tracking and mosaicking technique. The new velocity maps, together with a previously published InSAR derived velocity map, are combined with ice thickness data from



BEDMAP and modelled surface mass balance (SMB) from RACMO2.3 to calculate ice discharge rates and mass balance for different sectors and individual glaciers. The authors report an increased mass discharge for one sector of East Antarctica (Wilkes Land), which they attribute to widespread accelerating glaciers, while no significant changes are observed in the other 'oceanic' regions. According to the study the same region in East Antarctica also shows the largest change in mass balance, suggesting the risk of irreversible destabilisation of this region.

While the authors did a fine job in creating these new ice velocity maps, which provide a welcome update of previously derived ice velocity maps and which highlight the complex (temporal) patterns of ice flow dynamics in Antarctica, there are several major issues that require careful consideration and major revisions and re-analysis of the data. 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.

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. 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. 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. 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 pinpointed 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. I would recommend focusing on comparing smaller areas for which accurate gate cross sections are available and with ice velocity with well-defined short periods. Below follow some additional comments.

Specific Comments

Ln 78: “the decadal changes can be easily found” – not with this dataset (see above)

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?

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?

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

Ln 156: “100-meter resolution” -> I think grid spacing is meant here.

Ln 162: “or rise” -> What is a rise in this context?

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.

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

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

Ln 236: “resolution” - grid size

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

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

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.

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?

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?

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.

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

Line 356-360: "In the Antarctic Peninsula, there was a positive mass balance (33$\pm$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%!

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

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?

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

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

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.

SM Ln 174: Drygalski Ice Tongue not 'ice shelf'

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

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