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The Cryosphere
Discussions

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 #1

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This is a review for the manuscript: Antarctic high-resolution ice flow mapping and increased mass loss in Wilkes Land, East Antarctica during 2006–2015.

The paper deals with two ice velocity maps for the years 2014, 2015 based on Landsat-8 data and an assessment of the mass balance change compared to 2006 (based on an earlier ice velocity map). The work in itself is publishable in principle, however, I have three major concerns in the matter.

(1) There is a second manuscript currently under discussion in The Cryosphere. It was submitted a bit later but both discussion papers are dealing with the exact same topic and are using much of the same data sets:

Gardner, A. S., Moholdt, G., Scambos, T., Fahnstock, M., Ligtenberg, S., van den Broeke, M., and Nilsson, J.: Increased West Antarctic ice discharge and East Antarctic stability over the last seven years, The Cryosphere Discuss., doi:10.5194/tc-2017-75, in review, 2017. The group's velocity data are publicly available (http://nsidc.org/data/NSIDC-0710), though not necessarily as presented in the paper.

My primary concern is that the authors of these two manuscripts seem to come to different conclusions in terms of changes in Antarctica. Each group could not have known about the other, but due to the fact that both groups are publishing in 'The Cryosphere', I encourage the editors to initiate a detailed comparison of key elements of the two manuscripts. Maybe the main authors could be invited to provide an open comment to the other paper.

(2) The authors (and Gardner et al. 2017, in review for that matter) use Rignot et al. 2011 as a reference year map for 2006 and 2008, respectively. The MEaSUREs Antarctica ice velocity map (version 1.1 was used, version 2 is now available) is an undated product that aims to provide continent wide coverage for use in ice sheet modeling. The product description states that data acquired over multiple years were included to maximize coverage. While the focus was on acquisitions around IPY, data acquired as early as 1996 were included. I therefore view a quantitative comparison of this map with a product based on data from a single year as problematic. This is true for both manuscripts currently in review in 'The cryosphere'. Both groups alleviate the problem to some degree by utilizing time series data for areas where they are available (i.e. Amundsen sea). In this context, a MEaSUREs product has since become available that provides annual maps from 2005-2016 (http://nsidc.org/data/nsidc-0720). See also the next comment.

(3) Processing ice velocity form Landsat is not new. In addition to references cited in the manuscript, a recent publication presents the Landsat-8–based ice velocity maps and goes on to integrate the data with data from other sensors to present a much more comprehensive, annual time series for Antarctica spanning a time span of 11 years.

Also, Mouginot et al. 2017 is a published manuscript as opposed to a discussion paper. The maps are publicly available at NSIDC: http://nsidc.org/data/nsidc-0720 and are much better suited for the comparative work w.r.t. mass balance.

Mouginot, J., Rignot, E., Scheuchl, B. and Millan, R., 2017. Comprehensive Annual Ice Sheet Velocity Mapping Using Landsat-8, Sentinel-1, and RADARSAT-2 Data. Remote Sensing, 9(4), p.364, doi:10.3390/rs9040364.

Given the close proximity in time of the public availability of these three manuscripts, I consider the continent-wide processing of Landsat-8 data a publication worthy contribution. The novelty of this product is not particularly great, though. The Mass balance assessment is of interest, but the different conclusions of the two papers currently in discussion is confusing. This issue needs to be addressed in some form.

Specific comments:

Line 59: grounded based – > ground based

Line 69-71: While I consider this a contribution worth publishing, please see the two manuscripts mentioned earlier dealing with the same data set.

Line 72: Rignot et al. 2011 is used to estimate the mass discharge in 2006 See my comment above on this topic.

Line 93: "Compared to the satellite interferometric SAR data, the L8 panchromatic imagery is more suitable to estimate ice motion in fast-flowing regions for several reasons,..." While I agree that Landsat-8 is a valuable resource for ice velocity monitoring, I challenge the statement made here based on Mouginot et al. 2017, who show that the error for a single image pair is smaller for SAR when compared to optical.

Line 94, 95: Statement (1) nadir look: Ice velocity from SAR AND optical are generated from data pairs acquired in the same viewing geometry. If the authors mean that velocity maps from spatially adjacent scenes need to be carefully combined because the viewing geometry changes, the statement needs to be clarified. Topographic artifacts

in SAR based ice velocities in Antarctica are regionally limited to mountainous areas like the Antarctic Peninsula and the Transantarctic Mountains. Over large glaciers and ice streams this is less of an issue.

Line96 ff: Statement (2) Non-cloud free sensor. Change 'non-cloud free' to 'cloud cover sensitive' The quality and coverage of the map is owed to a very generous acquisition plan by USGS (i.e. near continuous Landsat-8 acquisitions), so cloud covered images can be discarded and there is still enough data available to provide a near full coverage of the observed area if data from a full year are combined. The fact that more data are available does not support the statement that Landsat-8 is better suited for ice velocity mapping (see my comment above and the assessment in Mouginot et al. 2017)

Line 101 ff: Statement (4) Feature tracking vs speckle tracking vs InSAR phase analysis Speckle Tracking and SAR: Range and azimuth resolution is variable in SAR, but the statement that azimuth is generally lower is not true. In fact, this is mode dependent (see RADARSAT-2 vs Sentinel-1). InSAR phase analysis and SAR: While it is correct the InSAR phase is only sensitive to line of sight displacement, it has been shown that the combination of ascending and descending data leads to a superior result. Combining SAR data from multiple angles even allows a reconstruction of the 3-d flow. Joughin, I. (2002), Ice sheet velocity mapping: A combined interferometric and speckle tracking approach, Ann. Glaciol., 34, 195–201. Gray, L. (2011), Using multiple RADARSAT InSAR pairs to estimate a full three-dimensional solution for glacial ice movement, Geophys. Res. Lett., 38, L05502, doi:10.1029/2010GL046484.

Line 122: See comment for Line 72

Line 156: 100 m resolution product. Does this refer to 100 m posting (i.e. a calculated value every 100 m), instead?

Line 167: These filters factor into the resolution of your product (see comment above).

Line 189, 190: 'In fact, the offset tuning is often called absolute calibration of the ice velocity data.' Is often called . . . by whom? Please provide reference(s).

Lines 207, 208; Equation 1: I would have expected a weighted average of speeds here. The equation as written will provide more weight to data with longer time separation, but this could be presented more clearly in my opinion (i.e. specify the weights). Also, as written, the authors do not provide an option for spatial adjustment of weights depending on glacier speed for example. Longer time separation will provide advantages in areas of slow flow, but are less suitable in areas of fast flow (see Mouginot et al. 2017)

Equations 2 and 3 are derived from Equation 1. A clearer formulation of the weighting scheme used would necessitate a revision of these equations.

Chapter 4: Decadal glacier dynamics

The primary concern here is that Rignot et al. 2011 is an undated reference map for the period of IPY (definitely not 2006), where data from 1996 were used to provide increased coverage (this aspect is described in Rignot et al. 2011 as well as in the product description at NSIDC). A better comparison here would be using the recently published annual maps provided by the group (see initial comment for access).

Chapter 5: Decadal variations of mass discharge and mass balance

How do the authors account for surface elevation change or ice sheet thinning? e.g. Pritchard, H.D., Arthern, R.J., Vaughan, D.G. and Edwards, L.A., 2009. Extensive dynamic thinning on the margins of the Greenland and Antarctic ice sheets. Nature, 461(7266), pp.971-975.

Do the authors account for basal melting? This is an issue where the grounding line used is outdated and too far downstream.

The authors use multiple sources for grounding lines, but do not appear to have selected a flux gate for estimates well upstream of the GL to improve the flux estimate. This method is described in Mouginot, J., Rignot, E. and Scheuchl, B., 2014. Sustained

increase in ice discharge from the Amundsen Sea Embayment, West Antarctica, from 1973 to 2013. Geophysical Research Letters, 41(5), pp.1576-1584.

Bedmap-2 is a somewhat unreliable source for ice thickness for the purpose of flux measurements due to interpolation issues. The authors seem to access also underlying ground penetrating radar flight lines, however, the choice of gates (i.e. use specific grounding line products) indicates that Bedmap thickness is used. This aspect should be clarified and may require an update of the uncertainties.

The differences to Gardner et al. 2017 (in review) should be investigated further. Both papers deal with the same topic, use roughly the same data sets, but come to different conclusions.

Conclusions: Lines 438,439: The two maps generated do not cover all of Antarctica, the first sentence is therefore misleading.

Figures

Figure 1: Caption should indicate that the InSAR derived velocity is previously published work from someone else. The 2015-2014 difference map shows large differences in several areas where none are expected. This, in my mind, indicates that the overall quality estimate for this data set is likely overly optimistic.

Figure 2: Caption should indicate that the InSAR derived velocity is previously published work from someone else.

Figure 3: Upper inset (this work) shows larger deviations from in situ data than lower inset (Rignot et al. 2011). This runs counter an argument the authors make in lines 93-104, despite the fact that apparently more data pairs were available in 2015 (Landsat-8) compared to 2006 (InSAR). Reference Year (2006) is not a good choice (see comment above).

Figure 4: See chapter 4 comments.

Figure 5: See chapter 5 comments. Also, please compare to Figure 9 from Gardner et al. 2017 (in review) Differences are striking given that essentially the same data were used to generate the results.

Figure 6: Need to cite the sources for data if not generated as part of this work

Figure 7: Given that the velocity difference map presented in Figure 1 shows some massive differences over Ross and Ronne Ice Shelves, I question the accuracy of the data over ice shelves. One way would be to provide a separate quality assessment for ice shelves and provide new error estimates for the region. Absolute differences are interesting, however, I would suggest that the changes are also provided in % of the speed of the glacier/shelf.

Supplementary material: ——————————-

Tables S1 to S4 require a caption. Information sourced from other papers, data sets or sources need to be credited as such.

Line 152 of SM_v16 points to a footnote that does not exist. It seems to be a citation in a wrong format.

Line 225 contains a wrong reference

Table S1, Table S2: How is the velocity measured? Through a single data point, averaged over the gate, or through some other average? More details need to be provided. The naming convention could be described somewhere. Some of the names used could be shown in the figures of the supplementary materials for better orientation.

Table S4: Needs further clarification. There are multiple entries of the same reference showing different values. At the very least provide a comment column. The referenced papers used for this table do not appear in the SOM reference list.

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