

Response to Anonymous Referee #3

Thank you kindly for taking the time to provide a review of our manuscript.
Please find:

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

This paper is poorly written and uninteresting to read, largely due to the tedious methods section where two very similar feature tracking techniques are documented at great length. This could be of value if a robust inter-comparison between the two approaches was performed, or if the differences between the two results was analysed in detail, however, given that this work isn't done, there is really no scientific justification for presenting the two Landsat methods in this paper. Overall the paper would greatly benefit from a thorough re-write, which should mainly consist of condensing the methods text, which is unnecessarily long and often repetitive. In addition to writing style, this manuscript must be edited to properly cite previous publications. I have noted throughout the methods and results sections that the authors have done a very cursory job of this, with many directly relevant papers not acknowledged in the text. Aside from reporting the new dataset, this paper doesn't deliver any novel science about the spatial pattern or magnitude of ice velocity changes in Antarctica, because regional case studies covering the present day time period have already been published in areas experiencing the largest change. This is however, is the first time Antarctic ice speed for the 2013-2015 period has been presented, along with ice sheet wide velocity change since 2008, so these results are novel. Again the discussion of these new results would be considerably improved if the continent wide signal was assessed in the context of previously published regional case studies.

We paraphrase the reviewer concerns as:

1. Lengthy methods section that describes separate methods for independent but similar velocity processing chains are described.
To address this concerns we have greatly reduced the length of the methodology text and removed redundancy. Most importantly we have added text clarifying that multiple velocity products were included to assess the sensitivity of the results to choice of processing methodology. We have also provided a more in-depth discussion of the differences between products and given justification for choosing to focus results to a single mapping.
2. Properly cite previous publications
We have added additional citations where appropriate.

Specific Edits L1 – Ice sheet instability and imbalance are not the same thing. The authors have shown that East Antarctica is not negatively out of balance during their study period, but their results don't prove stability. Replace this word in the title, and

check use of the word ‘stability’ throughout the rest of the paper.

The title of our manuscript was “Increased West Antarctic ice discharge and East Antarctic stability over the last seven years”. We understand the reviewers concern with the usage of the term “stability”, particularly that it could be taken out of context to infer that the mass balance of East Antarctic Ice Sheet will be resistant to future environmental change. We felt that we had provided sufficient context for correct interpretation of “stability” by indicating the quantity (discharge) and period of time (7 years) that “stability” is referring to. Given the reviewer’s comment, and the earlier comments of reviewer 2, *we have changed the title to “Increased West Antarctic and unchanged East Antarctic ice discharge over the last seven years”. We have also modified text in the main manuscript changing “stable” to “steady”, “constant”, and “unchanged”.*

L17 – New velocity map does not provide complete inland coverage of ice velocity as there is a data gap south of 82.4 mass change. Edit wording in abstract to be factually correct.

Thank you for catching this.

Text will be changed to: “inland coverage of ice velocity north of 82.4°S”

L20 – In the abstract, Marguerite Bay, West Antarctic Peninsula is flagged up as a key region with one of the most rapid velocity change, however the velocity change for this full region isn’t visible in Figure 8. Edit fig 8 to show velocity change map in zoom for this region.

The original Figure 8 did include a zoom of Marguerite Bay just above the frame showing the Amundsen Sea Sector. *We have made the Marguerite Bay frame larger in the revised figure.*

L22 –Incorrect use of term stable. Edit throughout paper.

We have modified text in the main manuscript changing “stable” to “steady”, “constant”, and “unchanged”.

L32 – Check sentence wording. Doesn’t read smoothly.

Agreed. We’ve changed this to “Recent studies indicate significant mass loss from the Antarctic Ice Sheet that is likely accelerating”

L36 – Mass change can be measured by multiple techniques with high precision and accuracy, e.g. gravimetry and altimetry in addition to velocity. Edit sentence to reflect that one technique, not all, require ice velocity to measure

While we agree that there are several measurement approaches for determining ice sheet mass change, this sentence specifically refers to the “difficulty in resolving continent-wide ice discharge”. To the authors’ knowledge, velocity is required to directly measure ice discharge separate from other sources of mass change.

L45 – Edit sentence to reflect that Landsat-8 measurements are only acquired during the summer. Use of the word annual implies that it is a true yearly average, when it is in fact just a summer mean so the speeds could be biased high. If the authors believe their measurements are representative of the annual mean, then evidence should be presented to support this.

Good point, will change “annual” to “yearly”. For some regions imagery can be matched with 1 year of separation making it a truly annual measurement of ice velocity. Time of acquisition may bias estimates of ice flow but unlike Greenland it is unclear if there is a significant seasonal cycle in Antarctic ice discharge.

L67 – Attribution of author contribution to the paper should be listed in the acknowledgements, not the main body of a paper.

Thanks for catching this. This will be removed.

L80 – The authors have clearly stated their adaptive window size used for velocity tracking. Add sentence to also state the step size.

We have clarified this in the following sentence:

*Results from the sparse search guide a dense search with search centers spaced such that there is no overlap between adjacent template search chips (*i.e. distance between template centers is equal to the template size*).*

L84 – State the method used to correct the scale distortion, and provide some statistics evidencing that the error has been reduced.

Scale distortion is an artifact of all map-projected data and is simply an artifact of projecting a warped surface onto a plane. Corrections are derived from the projection equations.

We now include a reference to projection equations:

We corrected for this scale distortion when converting from pixel displacement to velocity following the equations presented in Snyder, 1987

L86 – Is the variability of the ice speeds measured with all window sizes, less than the stated accuracy of the velocity measurements, (i.e.10 m/yr)?

The stated uncertainty average of 10 m/yr. is for the merged product only and is more dependent on number of cloud free image acquisition and persistence of surface features than on the selected chip size.

L92 –State the threshold ice speed that was used to identify ‘stable’ (or rather stationary/slow flowing) ice surfaces. Were all areas classed as stationary used to improve the image co-registration, or was it a subset? If the later please edit the text to clarify rational for selecting ground control sites. Again the authors should also re-evaluate their use of the term ‘stable’.

All data was used to generate the “reference velocity” to which the annual mosaics were registered. We have clarified this sentence as follows:

This was done by stacking all time-normalized displacements (velocities), co-registering them over stationary/slow flowing surfaces with low variability as described in the next section...

L135 – The authors have used a shorter epoch for the raw data used as input to the NSIDC LISA processing technique. Why do this? If the purpose of the paper is to provide a present day assessment of Antarctic ice speeds and compare this with historical data, then only one processing technique is required. If alternatively, the authors aim to inter-compare multiple techniques to assess their respective merits, then the study period has to be the same for any meaningful inter-comparison to be performed. Either process data over the same time period or remove the poorer Landsat-8 method info and results. *The aim here was simply to demonstrate the robustness of the discharge results. Using different epochs, different processing methodologies (though similar), different chip sizes, different sample spacing and different resolutions has minimal impact on the conclusion of the manuscript (see original Figure 6). We have added text clarifying the purpose of including multiple velocity products and greatly shortened the methods section.*

L145 – Again state the step size used.
Now included.

L145 – The authors used chip sizes ranging from 16 to 128 pixels in the JPL method, and 20 pixels in the NSIDC method. This will have a measurable impact on the output velocity measurements, as ice speed derived from larger window sizes will be biased lower than if a smaller window size was used on the same image pair. The authors should demonstrate how they have accounted for this. *The goal here was not to homogenize datasets and approached but rather to demonstrate the robustness of the discharge estimates using two independent approaches to measuring velocity. Using different chip sizes does not seem to have any impact on the discharge results (see original Figure 6).*

L150 – Quantify ‘fairly strong’, or amend writing style.
This paragraph has been re-written.

L152 – Edit double full stop.
Fixed, thanks.

L166 – State the maximum temporal baseline used for the image pairs.
Now provided.

L170 – Justify why different post-processing methods have been applied to the output from the JPL and NSIDC velocity processing chains once the velocity measurements have been obtained.
Please see response to L135 comment.

L177 – Provide the statistics for this intercomparison with Rignot et al 2011 for all three surface types, (rock, zero flow, slow flow).

This paragraph describes the offset corrections applied to the PyCorr velocity fields. The Rignot et al 2011 data is used to determine the magnitude of the correction, not to assess the quality or differences between products. This paragraph was largely redundant so has now been merged with the preceding paragraph.

L180 – The paragraph structure in this paper must be reorganised, it's completely arbitrary in its current form. For example, why have the authors introduced v_r , v_z and v_l in the previous paragraph (which started off describing the NSIDC post processing method), and then discussed use of these variables in the following two short paragraphs? It's not great throughout the rest of the paper, but its particularly infuriating in the methods section as the paper would read much more clearly if each paragraph did a specific job, i.e. explained a distinct aspect of the work.

This section has been re-written.

L185 – The 750m output grid size for the NSIDC dataset is substantially larger than the 240 m resolution of the JPL dataset, and this will impact any intercomparison between the two. The authors must state how they have accounted for this.

Using different grid resolution was simply to demonstrate the impact of differing resolutions on discharge estimates. The impact was minor as shown in the original Figure 6. Please see response to L135.

L192 – The authors have chosen flux gates based on some fairly straightforward rationale. A bit of time should be spent making the description more concise, as two sides of A4 is unnecessarily long.

We have shortened this section in the revised manuscript

L262 – There are known issues with assuming that firm corrected elevation change rates are 100% dynamic (Zwally et al 2015, Wouters et al 2013), so this assumption is not valid. Moreover, not all ice flowing at $>200\text{m/yr}$ is dynamic either, so the authors should edit the paper to state their rationale, and cite published literature that have demonstrated the complexity of this issue if there is no alternative to this assumption.

We have added the following for increased transparency:

A velocity cut off 200 m yr^{-1} was selected to separate volume changes resulting from changes surface mass balance and those resulting from changes in dynamics. This threshold is arbitrary. Even so, the dynamic volume change correction is very small and largely insensitive to the selected cut off velocity.

L265 – What's the logic for choosing 0.1m/yr or 30%? These numbers seem arbitrary, so assuming there is some justification, edit the paper to state rationale.

We have added the following for increased transparency:

Uncertainty in the dynamic volume change can not be rigorously quantified and are therefore conservatively assumed to be 0.1 m yr^{-1} times the area between the grounding line and the flux gate having a surface velocity $>200\text{ m yr}^{-1}$ or 30% of the magnitude of the estimated dynamic volume change, whichever is larger.

L275 – Relevant mass flux literature should be cited through the methods section. For example, Rignot et al, Mouginot et al 2014, Chuter et al 2017. The authors have not invented a new technique, so previous publications should be acknowledged in the text.
Additional references added.

L349 – Nilsson et al 2016 was not the first publication to apply the surface fit solver to Cryosat data, therefore the authors should edit text to cite previous publications where this technique was developed. Moreover, the Nilsson et al 2016 paper documents a method for estimating altimetry mass change of Greenland, not Antarctica, where the firm processes and therefore processing challenges associated with it, are not the same, as shown by Nilsson et al 2015. The Antarctica method should be explained in full, or an appropriate citation should be provided.

We agree with the reviewer that Nilsson et al was not the first to apply surface fits to CryoSat-2 data. There are however many approaches to applying surface fits. More importantly Nilsson et al. (2016) describe the full process chain used to go from the ESA L1b waveform data to the JPL L2 elevations and elevation changes. For this reason we feel that Nilsson et al. (2016) is the most relevant citation. Citations to other approaches of extracting elevation changes from CS2 data can be found in Nilsson et al. (2016).

L353 – Edit the manuscript to explain how the authors have extrapolated elevation change at the ice sheet margins, where interpolation between two data points isn't possible. It's in this area that the highest rates of elevation change are located, therefore although the area is small, the numbers are significant, particularly given the way the authors are using this result in this paper.

We are not sure we fully understand the reviewer's request. This paragraph states that "The edited data was then interpolated onto a 1 km grid using the weighted average of the 16 closest grid points, weighted by their standard error from the least squares solution and distance.)

L374 – The authors have presented two separate Landsat datasets, JPL and NSIDC. Please choose a nomenclature and stick to it throughout the paper as readers do not know which dataset is referred to by 'Landsat' alone. This should be edited throughout the paper.

This has been corrected throughout.

L377 – Figure 8 in this paper shows that there is large spatial variability in the velocity change parameter, therefore its not correct to assume that velocity change at FG1 is the same as at FG2. The error associated with this assumption must be sensibly quantified, or better, don't use this unsatisfactory approach at all.

We measure flux change at the FG1 grounding line and add this to the total flux estimated at FG2. This approach reduces errors by ~50%. We have added clarifying language to the methods section to better justify our approach.

L384 – This one sentence does not constitute a rigorous inter-comparison between the JPL and NSIDC datasets. Aside form the fact that the epoch covered by each datasets is

not temporally contiguous, the authors provide no discussion about the respective merits of each method, the statistical differences between the two datasets, or geographical regions over which one method might out perform the other. It is immensely frustrating to have had to read through lengthy methods description of two marginally different techniques, only to have one dataset discarded with no apparent logical basis other than the personal preference of the authors. This paper should be edited to remove the description of one of the Landsat datasets, or, the authors should to a formal inter-comparison.

[Please see response to general comment 1 and to L135](#)

L390 – The time period covered by the JPL dataset is only 1 year longer than the epoch covered by more recent data in Mouginit et al 2014. Edit paper to state how these results differ from Mouginit et al paper during the time period they overlap, not just the period where they don't.

Thank you for pointing this out. I am not sure why we had omitted this in the submitted paper. *We have changed the text accordingly:*

This implies an average discharge increase of 2.4 Gt yr^{-2} for 2008-2015 that is considerably lower than the 6.5 Gt yr^{-2} previously estimated for 1994-2008 (Mouginit et al., 2014). This recent slowing in the rate of acceleration is in excellent agreement with the previously published temporally dense history of ice discharge that gave a rate of discharge increase for this region of 2.3 Gt yr^{-2} for overlapping but shorter period of 2010-2013 period (Mouginit et al., 2014).

L398 – Edit paper to comment how these Getz results compare to the Chuter et al 2017 result, and cite the relevant paper.

Thank you for suggesting this. *We have included the following:*

This result is in broad agreement with Chuter et al. (2017) that observed increases in ice velocity during the 2007-2013 period alongside 2010-2013 dynamic thinning rates of 0.7 m yr^{-1} for the glaciers feeding the Abbot and Getz ice shelves.

L405 – Edit paper to comment how these Bellingshausen results compare to the Hogg et al 2017 result, and cite the relevant paper.

Thank you for the suggestion. *We have added the following sentence:*

This result agrees with a recent investigation of longer-term (1995-2016) changes in ice discharge for this region (Hogg et al., 2017) that found that the region's glacier experienced an increase in ice discharge between 1995 and 2008 and almost no change in discharge between 2008 and 2016.

L418 – The authors state that Scar Inlet Ice Shelf has sped up, however in the lengthy methods section of this paper, there has been no mention of how tidally induced velocity changes have been removed from the new dataset. The authors should remove this statement about the cause of Leppard and Flask Glacier velocity changes, or demonstrate quantitatively in this manuscript that tidally induced velocity change has been removed from both the Landsat 8 and historical SAR dataset.

We have removed the reference to our data and instead rely on the citation to Khazendar et al. 2015.

L437 – The spatial pattern of speedup on Law dome looks like its associated with the spatial distribution of image tracks. Can the authors demonstrate that this speedup is not just an artefact caused by a processing error?

Point well taken. The radar tracks are clearly visible in the Figure 8 inset image for this region. Underwood and Bond glaciers acceleration (~40 m/yr. in places) signals exceed the radar errors that are like due to residual ionosphere effects (~20 m/yr.). Even so we have added cautionary language to account for the increased error in this region:

“The region to the west of Law Dome, including Underwood and Bond glaciers, shows evidence of some increased flow speed and ice discharge, though the signal is near the limit of detection.”

L440 – Edit increase ‘d’ L715 –
Changed

Fig 1. Change figure to show inland ice speed (in the ‘Pole hole’) in the Rignot et al 2011 full Antarctic velocity map, or explicitly state in the figure caption that this area has been masked out to fit the spatial extent of the new JPL ice velocity datasets. It is misrepresentation of the Rignot et al 2011 dataset to imply that there is large a data gap in areas where one does not exist, particularly when the authors have actually used their velocity measurements from this region in their assessment.

Agreed. This was a mistake on our part. Figure has been corrected.

L715 – Add spatially variable error map for each velocity dataset shown in Figure 1. Input data density is interesting, but the error estimate has practical value.

We now include both count and error.

L740 – Edit figure caption to state more clearly which Landsat dataset corresponds to each color in the bar charts.

Thank you for catching this. We have added a legend for the bar plots.

L760 – The spatial pattern of change in ice speed on Pine Island Glacier, shown in Figure 8, isn’t in agreement with change in speed presented elsewhere, and published in Mouginot et al (2014). The authors should discuss if the pattern, (specifically the two separate patches of high speedup), is a real signal, or if it is due to an error in one of the datasets?

The reviewer makes a very keen observation. Our map of velocity change shows an area of peak velocity change at 50 km upstream of the grounding line and a secondary peak at 110 km from the gl. We see no such peak when comparing between Landsat products, which makes us confident that the secondary peak is not an artifact of the Landsat processing. One possible non-geophysical explanation is that the radar mosaic includes data from a period significantly earlier than 2008 for this area. We have include a mention of this in the revised manuscript.