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The Cryosphere Discussions

Interactive comment on "Twelve years of ice velocity change in Antarctica observed by RADARSAT-1 and -2 satellite radar interferometry" *by* B. Scheuchl et al.

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We would like to thank the anonymous reviewer for the detailed review and for the encouraging feedback on our work. We appreciate the evaluation and the specific comments. Detailed responses are provided below.

The paper reports on ice velocity (speed, really) changes for two major ice shelves, and some of the adjacent grounded ice, based on a pair of InSAR mappings in 1997 (associated with the RAMP AMM-1 mapping) and 2009 (a part of the GYPSY multi-sensor mapping for IPY, in this case using Radarsat-2). The paper focuses on the

description of the observations, without a significant analysis of causes, or an in-depth review of the previous discussions of some of these changes.
We provide responses to your specific recommendations below.

Overall, the paper presents a useful mapping of ice sheet and ice shelf changes, and should be published. I recommend, however, a serious and thoughtful re-writing, taking more time with existing literature on the regions and some attempt to describe the causes of the observed 2 or 3 major changes in more detail.
We will include an extended discussion of Siple Coast in the discussion section. See also our response to your specific comment on section 5; structure, speed, and history of ice changes on the Ross Shelf.

Abstract is a bit unfocussed, just a listing of measurements, without a connecting theme. Leading with the rift-associated ice front speed-ups is a case of 'burying the lead' of more interesting changes. Also, for the Ross especially, there is little attempt to unify the changes in the context of the Kamb cessation, Whillans slowdown, and subsequent adjustments by the other ice streams. Noting a cause of the Whillans/Mercer decline would be good – at least mentioning those that have been proposed. Perhaps the extended area of slowing upstream and downstream that is observed here leads to an insight as to causality.
We will revise the abstract based on the recommendations.

Section 3.3 - the section is a bit strident, the case was made in the earlier paper GRL paper – moreover, this paper and the GRL paper still have the same flaw; the grounding line is incomplete.
The grounding line shown is based on differential SAR interferometry. Measured grounding line is shown only. For each point, we can provide sensor information and acquisition dates. Two interferograms are necessary to form the difference

interferogram. Gaps in the grounding line are the result of lack of data correlation in one of the interferograms (or both), which is not immediately apparent from the velocity map, which used both interferograms combined and therefore provides better coverage (Rignot et al., 2011a).

[What I think would be an interesting addition here – since the data set for the 2009 DInSAR is so consistent – would be an initial examination of the grounding zone width, and a comparison of that with the ice thickness in areas where thickness is known (the tidal range would also be a component of the analysis). To first order, the width of the flexure zone should be a function of ice thickness, with ice flow speed and ice temperature involved as well. I suppose this would be best placed in a separate paper.]
An analysis of the grounding zone width would be a worthwhile effort but as the reviewer mentioned, it is beyond the scope of this manuscript.

Section 4 Results – I agree with the earlier review that more space than is needed is devoted to the tidal correction. It does not provide further science insight. (But, yes, it is worth noting in the Methods section that it works well and enables the change detection. Two or three sentences, and reference, is all that is required in my opinion).
We will remove Figure 1 and text referring to it.

Section 5 Discussion – this section rambles from one brief summary observation and hypotheses to another, and is of little value to a reader. Each of these changes has had relevant material published about it before. I recommend that the authors completely re-write the discussion, and focus on one or two areas of change (Mercer-Whillans and Ross; and the general slowdown of the RonneFilcher) and provide some useful analysis integrated with a review of the existing literature. Either make it a paper useful for understanding ice dynamics, or strip it of speculation and present it as a ice speed

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change data set with a cursory and short discussion of the method and observational highlights.
We will refocus the discussion. See also our response to your specific comment on section 5; structure, speed, and history of ice changes on the Ross Shelf.

Looking at the data sets in Figures 3 and 4, there are many more artifacts than the Methods section implies – a general splotchy-ness especially near the edges of the data. I note that on the van der Veen arm of Whillans Ice Stream, there is a thin line of speed-up on the southern grounded ice, and slowdown on the northern ice stream margin – is this related to a geo-location error? What is to be trusted? Re other spots of small extent on the grounded ice – what would sub-glacial lake infill and drainage look like in the Figure 3 and 4 representation?
We believe that the artefacts mentioned in the Whillans ice stream area could be related to a slight mis-registration. A possible slight migration of the ice stream margins cannot be confirmed with the data at hand. One of the reasons is that the quality of the orbit information for RADARSAT-2 is better than that of RADARSAT-1 due to improvements in the satellite design (Luscombe, 2009).
The data set used in this study is not sufficient to provide a detailed evaluation of subglacial lakes. Drainage or in-fill of subglacial lakes results in a change of surface elevation. Depending on the fill level difference between the two campaigns, this would cause a signature in the velocity difference map. A detailed evaluation of a specific lake would require a more extensive data set (i.e. a time series) than is available here.

Substantial work has been done on structure, speed, and history of ice changes on the Ross Shelf, the ice plain, and its glaciers, by Catania, Hulbe, Fahnestock, Stearns, Bindschadler.. It would seem that a longer summary of this well-known slowdown in the context of this past work is in order.
We will add the following discussion to Section 5 Discussion:

The evaluation of the Siple Coast Ice Streams over the last 1000 years or so are known based on the analysis of satellite imagery and ground penetrating radar. A detailed summary of the current knowledge is provided in Catania et al. (2012). The aspects most relevant for this study are a shutdown of Whillans Ice Stream about 850 years ago (Catania et al., 2010) and the subsequent restart about 450 years ago (Hulbe and Fahnestock, 2007). Our results and earlier studies in the region (Joughin et al., 2002, 2005) show that dynamic changes in the region are ongoing. Stearns et al. (2005) suggest a change of basal conditions as the cause of velocity changes in upper Whillans Ice Stream. The authors speculate that the depletion of meltwater at the base of the ice stream is most likely responsible for the observed changes. This is the same scenario described by (Retzlaff and Bentley, 1993) for Kamb Ice Stream. This ice stream started to decline output about 440 years ago (Catania et al., 2010) and shut down about 140 years ago (Fahnestock et al., 2000). The shutdown pattern is described as a wave of stagnation that started near the grounding zone and propagated upstream (Retzlaff and Bentley, 1993). One of the conclusions of an analysis of streaklines is that interactions between the downstream reaches of adjacent ice streams is important to ice stream discharge variability (Hulbe and Fahnestock, 2007). Dynamic changes on Kamb, Whillans, and Mercer Ice Streams are therefore likely linked.

Another complete shutdown of Whillans Ice Stream in the near future is possible given the observed trends over the last 40 years. An analysis of the mass budget for the Ross Ice Shelf region including our data shows a change from near balance in 1975 to growth in 2009 (Thomas et al., 2012). The authors predict a stagnation by around 2070. Our results also suggest an upward moving wave of stagnation similar to the pattern reported for Kamb Ice Stream (Retzlaff and Bentley, 1993).

Note, there is a new paper using ICESat data to map the slowdown and compare it to RIGGS measurements in Remote Sensing of Environment (Lee et al., 2012). Does this new mapping match this one?

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The new paper by Lee et al. (2012) was not available at the time of manuscript submission. We have reviewed it and included the reference. The authors describe a method to measure ice velocity using ICESat laser altimetry at crossover points. The method uses existing ice velocity maps for initialization and will therefore benefit from the InSAR velocity map for initialization (the manuscript was first submitted before the InSAR map was available at NSIDC (Rignot et al., 2011d)). The method is less accurate compared to InSAR and measurements collected are less dense. Once ICESat-2 is available this method will prove a valuable complement to InSAR, particularly in central Antarctica, where crossovers are dense and InSAR campaigns are less frequent (as described in this manuscript). We will add a comparison of the results in the results section.
Our results generally correspond with results derived using ICESat data (Lee et al., 2012). Their 2006-1997 velocity differences for specific coordinates near Crary Ice Rise appear to be about 20 $\mathrm{m\,yr^{-1}}$ lower than our 2009–1997 velocity differences (i.e. ICESat estimates higher velocities compared to InSAR, a fact that the study authors state as well (Lee et al., 2012)). Assuming continued deceleration between 2006 and 2009 both results are comparable within the error bars of the methods.

Did the authors consider mapping any flow directional changes? If not, and if they are not discussed somewhere (in terms of sensitivity, error, effect on speed changes), then nearly all of the places where the word 'velocity' is used should properly be replaced with 'speed'. Also - noticing some odd features on the Ronne Shelf and Berkner Island - would firn collapse events (sometimes called firnquakes) affect the differencing in InSAR? These collapses can extend across the surface for a few km, with a vertical movement of a few cm (They result from a collapse of a weak snow hoar crystal layer at depth).
We indeed measure ice velocity. The sensitivity and error of flow direction measurements is discussed in (Mouginot et al., 2012). We will mention this aspect and carefully review the terminology used.

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Firn collapse: A surface change of few cm due to a firn collapse should not cause a significant signal. However, if the firn collapse results in a structural change of the surface layer, therefore changing the signal penetration depth (1 to 9 m, depending on conditions (Rignot et al., 2001)) by more than a few cm, the resulting signal could be detectable. This would need to be tested with in-situ measurements.

The yellow arrow on Figure 4 is not easily seen at first, could be clearer – or perhaps a yellow circle around the area in question. Also, on Figure 4 I note some long sinuous features, a trail of ice slowing, on Recovery Glacier. The feature is mapped as a deceleration, but is narrow and follows the flowlines of the glacier. This should be mentioned. A similar feature may be present on Foundation Ice Stream.
We will make changes to Figure 4 as recommended.
On Recovery Glacier, a thin blue line can be observed running parallel to the flow line. This line represents the northern edge of the fast flowing portion ($>100\,\mathrm{m\,yr^{-1}}$) of the trunk. Similar to the trunk edge effect on the Van der Veen arm of Whillans Ice Stream (see our response above), it is possible that this is the result of a geolocation error.

p2 line 11 and elsewhere: 'slow down' and 'speed up' - these are too colloquial, use 'reduction in speed' or 'deceleration'; 'increase in speed', or 'acceleration'.
We will address this change of terminology in the revision

p2 line 18-19 awkward sentence, no significance to 'the 8% level', is there?
We will revise the abstract and take this comment into consideration.

p2 line 22-23 - for these glaciers, a small change _is_ discussed in the text. Please re-word this line.
We will revise the abstract and take this specific comment into consideration.

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Interactive Comment

p2 line 23 'On the Filchner Ice Shelf itself..' would be better.
We will change all occurrences of "... ice shelf proper ..." to "... ice shelf itself ..."

p2 line 25 -end. Really? it would seem that slowing is fairly common on these glaciers. I think a quantitative assessment is warranted here – or, instead, focus the paper more generally on the historical changes and their causes for the two shelves.
Our results show that dynamic changes are present in the region of interest. Central Antarctica is a region that deserves continued observation, with primary focus on Siple Coast. A 25% reduction in ice velocity of Whillans Ice Stream significantly affects the mass balance of this ice stream, a similar conclusion can be drawn for Mercer Ice Stream. Changes are significant and point to a possible shutdown of these ice streams if the trend continues. Mass balance changes of West Antarctica have shown to be driven by losses on glaciers in the Bellingshausen and Amundsen seas region and on the Peninsula (?). We do want to make the point that the changes observed here remain relatively small compared to changes reported for other areas of West Antarctica.
We will rephrase the sentence in question to make our point clear.

p4 line 13 - '. . .limited data collection..'
We will change: "limited data collect" to "limited data collection"

p6 line 14 - no need for the dashes separating out 'or the interferometric phase'.
We will remove the dashes

p7 line 20 - '. . .was not effected in the best manner.' this is a bit awkward.
We will change the sentence to:

Track boundaries of difference maps are highly sensitive to calibration or mosaicking errors even if these issues are not visible in the original velocity maps.

p8 line 6 - ' central Antarctica north of Titan Dome..' this needs a lat-long location to define it, and the phrase should not be capitalized.
We will provide the coordinates and change the spelling.

p8 line 11 - 'and the precision of our detected changes is about. . .'
We will change the sentence to reflect the recommendation.

p8 line 12 - Looking over the velocity change maps, it is clear that the last sentence is an understatement, and more has to be said about the level of accuracy in the presented data. In the Ross (Figure 3) there are abrupt changes near Crary Ice Rise with no clear explanation; striping effects are (to my eye) at least 10 to 20 m/yr scale, and there are mottled areas along the southwestern edge of the DInSAR coverage that are not related to real velocity changes, but some error effect. On the Ronne, similar things are observed, well away from the grounding line and at a scale far smaller than tidal fluctuations.. this needs to be addressed. There's no doubt that the overall signal is real, but a more realistic estimate of the error would be +/- 15 m/yr change, and some of the blotchy-ness should be addressed.
Our change detection map is the product of combining data from more than 100 tracks for each of the two years. Residual errors and artifacts are unavoidable and most likely associated with data noise due to ionospheric effects. These residual artifacts have no impact on the general conclusions of this paper.

We will add the following text to the methods section (Section 3.2 velocity difference estimate): This estimate is rather optimistic compared to previously published error estimates. The most significant contribution to velocity estimation uncertainty is the Ionosphere (Rignot et al., 2011b; Mouginot et al., 2012). The error for velocity estimates using RADARSAT-1 and RADARSAT-2 is estimated as $\pm 6\,\mathrm{m\,yr^{-1}}$ (Rignot et al., 2011b) (SOM). This leads to an error estimate for the difference product of $\pm 8.5\,\mathrm{m\,yr^{-1}}$ in areas where only a single coverage is available from both sensors (i.e. no track overlap, decorrelation in one of the two RADARSAT-2 pairs). Locally, errors may exceed these values.

p9 line 18 - in fact, the MOA grounding line shows a large island iceplain in front of Slessor glacier that is correctly aligned with the InSAR determined grounding line one – this would minimize the effects on the tidal correction. Further, there are similar-scale issues elsewhere in the Ronne that are unlikely to be a part of grounding line location errors, including some sharp variations on grounded ice (Berkner island) and sharp small-scale problems within the Filchner. Showing the MOA grounding line over the rest of the image in Figure 4 would reveal the level of consistency between the two determinations, and the completeness of the MOA line.
The ICESat-based grounding line reveals some areas of uncertainty or erroneous areas in a MOA based grounding line (Brunt et al., 2010), a detailed comparison between MOA-, ICESat-, and DInSAR-based grounding line is provided in Rignot et al. (2011a). These differences are relevant as the tide correction used is based on the MOA-based grounding line.

Our analysis showed that applying tide correction on one satellite track covering roughly the area between the DInSAR grounding line and the MOA grounding line results in an erroneous signal for the trunk of Slessor Glacier. Discussions with Laurie Padman (personal communication) revealed that the bathymetry used for the tide model in the FRIS region is also flawed as discussed in the manuscript. As a result the tide model will be updated with a more accurate grounding line and bathymetry where needed. Our point here is that these errors affect tide correction in the region and have an impact on our results.

p9 line 18 - 'flaps up and down' - that is a bit much, perhaps just 'moves up and down'.
We will change the sentence to reflect the recommendation.

p10 line 1-3 - end the sentence at the words '. . . ice streams' The rest is not needed, can be misinterpreted (the source of the slowdown or the source of the tributary? and the noise floor is not the source of the tributary)
We will change the sentence to reflect the recommendation.

p10 line 11 '. . .that happened after 1997.'
We will change the sentence to reflect the recommendation.

p12 line 17 ' 24 days ' (not 'day', and no hyphen)
We will change the sentence to reflect the recommendation.

p14 line 20-25 - the magnitude is real. . . not well-put, you mean the magnitude and ex- tent and the uniformity of the speed change is unlikely to be an artifact of tide, noise, or errors in processing, and so is most likely explained by a real change in flow speed. The most reasonable explanation is (the one you cite). . . other explanations include. . .(cite some more).
We will change the sentence according to the recommendation.

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