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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 H. Fricker and her group for their comments and questions as well as for the encouraging feedback on our work. Detailed responses are provided below.
Congratulations on an impressive piece of work. Here are some brief comments and questions from my group. We hope that you find them helpful.
The comments and questions are very helpful. In this document we provide each comment in italics and answer directly below.

General comments
The language is quite colloquial in places and the writing could do with significant tightening up throughout. For example Page 1722, Line 18 "an ice shelf flaps up and down with changes in the oceanic tide."
We will work on tightening the writing in accordance to all reviewer comments. In particular, the phrase "flaps up and down" will be changed to "moves up and down".

It should be made clear (starting with the title) that these are two snapshots acquired 12 years apart i.e. this is not a 12-year time series.
We will change the title to:
Ice velocity changes in the Ross and Ronne sectors observed using satellite radar data from 1997 and 2009.

The paper covers only the FRIS and Ross ice streams, and this should be made clear in the title. Why is it not all of Antarctica? Or are these results being saved for another paper?
We focused on data from the two left looking campaigns (RADARSAT-1 in 1997 and RADARSAT-2 in 2009). The results represent the maximum extent of the coincident area.

Methods
The description of how the velocity measurements were derived is too short and some more explanation would be helpful. I realise that this has been spelled out before in previous papers, but it would help to include the highlights of these details, so that this paper stands alone.
The methods used are described in detail in several other papers that we cite. To provide more information we will include some highlights of the velocity generation method in Section 3.1 (pg 1719 line 13 of discussion paper):

We use a speckle tracking technique (Michel and Rignot, 1999) to derive slant range and azimuth displacements from the InSAR data. The quality of the result is further improved for areas of slow flow where the unwrapped interferometric phase of tracks can be used in range instead of range offsets from speckle tracking (Rignot et al., 2011b). Assuming surface parallel flow, we use a DEM (Bamber et al., 2009) to calculate the two-dimensional displacement field. To obtain the two-dimensional ice velocity we apply tide correction (per track) and velocity calibration (per track and using multiple tracks together) as discussed below. A detailed description of our method is provided in Mouginot et al. (2012). Three data cycles are available in 2009, we therefore combine the resulting two velocity products per track to reduce data noise.

My group raised the following questions about the methods:
1a. What is the uncertainty in the measurements?
The discussion paper contains an estimate on the precision based on the variation over stagnant areas (pg 1722 lines 5-13). A more realistic error estimate is provided below. We will include this text at the end of section 3.2 Velocity difference estimate (pg 1722 lines 12):
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.

1b. What are the sources of uncertainty with phase unwrapping vs speckle tracking?
Sources of uncertainty for speckle tracking include changes between the two acquisitions causing decorrelation, and ionospheric perturbations. Sources of uncertainty for interferometric phase analysis include all sources mentioned above. It should be mentioned that the interferometric phase is more sensitive than speckle tracking to decorrelation (i.e. changes between the acquisitions). In addition, if the offset magnitude is too large, the resulting fringe pattern may become too dense to unwrap correctly.

2. How is the correction to surface parallel flow performed? Using an external DEM or differential InSAR data? How valid is this correction over ice shelves which are floating on flat water and can have undulating surfaces? This can for example be checked if overlapping data are available from different satellite look directions.
We use an external DEM for correction to surface parallel flow (Bamber et al., 2009). The error of this DEM is smallest over Ross and Filchner/Ronne ice shelves. We therefore expect no significant error contribution on the shelves. We are estimating a 2-d velocity field for each satellite track. Our calibration scheme uses overlapping tracks where available and allows the consideration of all available overlapping tracks. The various tracks, once calibrated, are therefore not independent. This is particularly true for tracks covering ice shelves (Mouginot et al., 2012).

3. Is there any potential for unresolved movement perpendicular to the satellite look direction? Or are there generally multiple acquisitions from different angles such that InSAR data with unfortunate orientation can be excluded? e.g. when the flow axis deviates by more than 70 deg. from the look direction.
We use speckle tracking to measure ice velocity. Speckle tracking provides 2-d velocity estimates (range and azimuth), we therefore have no unresolved movement component in azimuth. Our data coverage is described in detail in Rignot et al. (2011b). The method described in the question is used for interferometric data with a short revisit time (e.g. ERS-Tandem), where the interferometric phase in range is

analyzed and no azimuth component of the motion is available. Indeed, for this case coverage from both ascending and descending orbits are a requirement to resolve 2d velocity fields.

4. Was a correction applied for the inverse barometer effect?
We did not apply a direct correction for the IBE. The resulting difference map based on our calibration concept (Mouginot et al., 2012) after tide correction does not show visible track boundaries. Any IBE signal present therefore appears largely suppressed.

Page 1723 Lines 8-9: Do you have references and examples for the width of the flexure zone varying over the tidal cycle (Section 3.3)? Our analyses of repeat-track ICESat data over the grounding zones at multiple phases of the ocean tide suggest that the width of the flexure zone does not change significantly with tidal state, unless there is an ice plain (see Figure 3 of Fricker & Padman, GRL, 2006, and Brunt et al., 2009, Annals of Glaciology).
A detailed discussion of the differential SAR interferometry based method to detect the grounding line is provided in Rignot et al. (2011a). Some examples of differential interferograms are shown in Rignot (1998). The visco-elastic deformation of an ice shelf in the grounding zone is discussed in Schmeltz et al. (2002); Reeh et al. (2003).

Page 1725, Line 22: Have you compared these estimates for rates of slowdown with the timescale for ice stream shutdown? (e.g. Retzlaff and Bentley, 1993; Hulbe and Fahnestock, 2007).
We will include a discussion of the history of Siple Coast in the discussion section. Here, we will also discuss the timescale for ice stream shutdown (Section 5 Discussuion (pg 1727 line 24 of the discussion paper)):
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 are 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, that includes our data, shows a change from near balance in 1975 to growth in 2009 (Thomas et al., 2012). The authors predict 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).
...discussion cont.

Page 1729, Lines 1-7: The confirmation of continued slowdown of WIS is a very worth- while result; however; Pritchard et al., (2009) actually reported that lower WIS thickened between 2003 and 2009. Upper WIS indeed thinned at the rates you posted.
We will clarify that we refer to upper WIS.

Page 1729, Lines 8 – 17: This is an interesting result. Although Byrd Glacier is the only place within your study area where there is any reported connection between lake drainage and ice velocity, there are many other locations within your study area where lake drainage occurs. These include: WIS (Fricker et al., 2007); MacIS (Fricker et al, 2010); Slessor IS (Smith et al., 2009); and Recovery Glacier/Slessor IS (Smith et al., 2009; Bell et al., 2007). At a minimum, it may be worthwhile to mention that there are lakes elsewhere in the study area.

We will mention the presence of subglacial lakes in the study area following the discussion of the results on Byrd (pg 1729 line 16 of the discussion paper):

Byrd Glacier is a well-documented example of temporary acceleration due to a subglacial flood. The presence of subglacial lakes has been proven in other areas of the study area (Smith et al., 2009). Specifically, lakes have been studied for some Ross Ice Shelf tributaries (Fricker et al., 2007, 2011) and for Recovery Glacier (Bell et al., 2007). 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.

Figures In Figure 1, why is the green line (the GL) not continuous? Figure 2, the individual maps appear too small to be easily readable. In Figure 3, is this meters per year, or meters per 12 years?

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).

Note that Figure 1 will be removed from the revised manuscript.

We will rearrange the Figure 2 to show larger maps.

We will separate maps and graphs for Figures 3 and 4.

Figure 3,4: meters per 12 years

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