

Interactive  
Comment

## ***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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### General comments

This paper presents an analysis of the differences in ice-flow velocity measured over a significant portion on the Antarctic ice sheet between 1997 and 2009 using interferometric synthetic aperture radar acquired by the Canadian satellites Radarsat-1 and -2. Several areas of apparent change are identified, on and around the Ross and Filchner-Ronne ice shelves.

Responding to the specific questions posed to reviewers. The paper is relevant and within the scope of TC, and presents some novel data. The discussion arrives at

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significant conclusions (I express some concerns below), and the methods appear to be sound, and the overall presentation of the paper is logical and complete.

While, it appears that the velocity fields presented are the product of well-considered and well executed analysis, and correction for the most important confounding issues has been undertaken with care, I believe that there are two areas of the interpretation and conclusion that I do not fully support, but which could relatively easily be addressed in a revised manuscript.

1. The radar data were acquired over 24-day satellite repeat periods, separated by 12 years, and velocity fields derived from these data are representative of these epochs. The interpretation is (implicitly) based on the assumption that the differences between these epochs are capturing persistent changes in the ice-flow over this timescale, rather than, say, shorter-term variability. I am not strongly objecting to the likely validity of this assumption, but I would like to see some effort to acknowledge, and to justify it. Ronne Ice Shelf in particular is an area of daily, fortnightly (Gudmundsson, 2006), and annual (Adalgeirsdottir et al., 2008) fluctuations.

2. A statement at the end of the abstract concerns me. “Overall, we conclude that the ice stream and ice shelves in this broad region, . . . exhibit changes in ice dynamics that have almost no impact on the overall ice balance of the region”. I think this needs clarification, and may well not be justified. Surely, if there is a change in the rate of ice-flow in an ice stream, across the grounding line or ice-front, then this has to be a direct impact on the out-going ice flux, and so in that sense has a one-to-one impact on the mass-balance of that glacier basin. Whether or not the change in flux is counter-balanced or overshadowed by larger changes in snowfall is not really the issue. This entire study is aimed at determining (successfully) the magnitude of the changes in flux, but then this statement (and similar ones in the conclusions section) suggest that these are unimportant, when they are clearly part of a larger picture of stable/changing mass-balance in these glaciers. Perhaps, the authors are trying to point out that the dynamic changes they have observed are generally “second-order” effects compared

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to those elsewhere (Amundsen Sea embayment, etc.), and perhaps that is arguable, but that is not clear in the current text.

I am certainly not calling for the dynamic changes to be “hyped-up”, but I would say that the study rather clearly shows that it is not just the gross changes in the Amundsen Sea and on the Antarctic Peninsula, that are worthy of further study. Indeed, these results taken at face value show that dynamic changes are present in many other glaciers. Overall, the language used in the text is somewhat informal, and consequently, in several areas, it is not as precise as one would hope.

The figures are generally presented in a form that is too small to review properly. This may be due to the way the authors presented the figures, or the preparation processes within the editorial offices of TCD, or possibly a function of the charging structure of the journal, but the outcome is the same – I could not see the detail on the maps that was discussed in the text.

#### Specific comments

The title seems to promise more than is delivered – the focus of the paper is two ice shelves and their hinterlands, and the title should really reflect that.

Pg 1719 line 3-4 The Bamber et al. 2007 citation seems an odd choice here, (Doake et al., 2002) certainly measured it directly.

Pg 1719 line 14 “Horizontal ice velocity is. . .”

Pg 1722 line 18 “flaps up and down” is not very precise language.

Pg 1724 line 8 Were any other mammals used apart from seals? If not, why not say “seals”?

Pg 1724 line 28 Surely this statement shows that the changes in velocity are significant to the dynamics of the entire glacier and so to the mass balance, which would contradict the last statement in the abstract.

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Pg 1725 line 8 Citation to “prior studies” is missing.

Pg 1726 line 8-9 In several places the terminology makes reading very difficult, and potentially misleading. In this example... “Mercer and Whillans...experience...slow down. Their 12 yr velocity difference... is up to 50 m yr<sup>-1</sup>...”. Here, a value indicating the magnitude of a slow-down would be positive, but the change/difference would be negative. Personally, if I was talking about a change, I would indicate the sign explicitly (+50 or -50 etc.) to make this clear.

Pg 1726 line 20 A recent paper (Brunt et al., 2010) has been through much of this identification of uncertain and erroneous errors in the MOA grounding line.

Pg 1727 line 12 “Filchner/Ronne Ice Shelves proper”, this is very informal, but it is also unclear. And, Filchner-Ronne Ice Shelf is I think usually given in the singular.

Pg 1727 line 20 Mosaicing would surely be possible, it would simply highlight the mismatch between frames.

Page 1728 line 8/9 Gudmundsson and Jenkins undertook an extensive ground-based study that showed no evidence of ongoing change on Rutford Ice Stream, over (I think) an overlapping interval. I would recommend citation of that study here, both to corroborate the findings of this survey, and give due precedence to the earlier work. (Gudmundsson and Jenkins, 2009)

Pg 1728 line 25 Ronne Ice Shelf!

Pg 1728 line 25-28 I don't see how this paragraph relates to the results of this study - the connection is not made explicitly.

Pg 1730 “ which suggest that the ice dynamics of the entire region does not have a strong impact on the mass budget of the Antarctic continent”. Related to previous comments, this sentence is really not correct as written. It may be that ice dynamics (i.e. the forces that control ice flow) has not changed significantly during this short period, but to suggest that ice dynamics (=ice-flow) is not important is surely, incorrect.

Especially, where such broad concluding statements are made, their meaning must be made quite precise.

Figure 1 – This figure seems an excessive way to demonstrate the importance of a routine correction applied in processing. I think it could be omitted, put to supplementary text, or some other technical report – unless there is citable evidence that the omission of the tidal correction by other authors has resulted in dubious results in which case those should be cited.

Finally, one picky point. Hyphenation is largely ignored in the text, but one repeated example jars on me as a reader. A “12-yr” change should surely be hyphenated.

#### References

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Interactive comment on The Cryosphere Discuss., 6, 1715, 2012.