

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.

Anonymous Referee #1

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I will start with some general comments and then address the questions posed on the website under m/s evaluation criteria. Minor typos, grammatical errors and poor sentence structure and use of unscientific or inappropriate terms are highlighted in the marked up pdf.

The paper addresses an important and noteworthy topic: dynamic stability and changes in dynamics of glaciers feeding the two largest ice shelves in Antarctica. The results reached are sufficient to warrant publication in TC and my concerns relate primarily to the structure of the paper, how it is written and the figures. I will address each of these topics next.

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The introduction, methods and results sections were largely OK. The discussion appears to be an almost seamless continuation of the results section. I could discern nothing about the discussion and conclusions that was not in same way part of the results. The authors need to have a clearer idea in their own minds about what the key take home messages from this study are. For example, Fig 1. is used largely to inform the reader that a tidal correction is needed to produce decent velocities on the ice shelves. This is not new or novel or a surprise. Why devote a whole panel of figures to this? Two sentences explaining that horizontal vels are sensitive to vert vels and that the tide model handles these mostly well would be entirely sufficient.

Where the discussion becomes informative and goes beyond describing the results is at l8 of p1729 on Byrd glacier. At least here they place their results in the context of other observations. The section prior is a continuation of the results. The authors need to make more effort and give more thought to section 5 and 6. For example, they could have contrasted the behaviour of these shelves with smaller ones in the same sectors or elsewhere from Pritchard et al 2012 and so on.

The figures. Other than Fig 1 being largely superfluous, all the figures are much, much too small. Even at 400% I could not see the apparent yellow-black dashed GL from MOA on Slessor that they claimed was there. Nor could I see they “markers” every 100 km. The “yellow arrow” in Fig 4 is completely unintelligible at 100%. I realise that most people will be viewing the PDF file on a screen but a printed version of these figures will be almost useless. At the very minimum they need to separate the dv image from the graphs of dv for Figs 3 and 4.

Minor comments not in the pdf a) Use English not US spelling. b) Avoid non scientific terminology like “flaps up and down” for describing vert motion of ice shelves. c) Surge has a specific meaning in glaciology and you do not mean Byrd surged. Replace this word throughout and use speed-up or acceleration d) Quite a few sentences were poorly structured such as the use of i.e. twice in one sentence.

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M/s evaluation questions: 1. Does the paper address relevant scientific questions within the scope of TC? Yes. The topic is of relevance to the journal.

2. Does the paper present novel concepts, ideas, tools, or data? The data and results are, as far as I am aware, new and although this group has published a number of studies of Antarctic velocities and their changes, I believe this is the first detailed assessment of glaciers feeding the two largest ice shelves. However, the earlier paper by [Rignot et al., 2011] appears to include annually resolved discharge values from 1992-2010 for the whole of Antarctica. This implies that annually resolved velocities for these ice streams exist for a longer period than has been presented here.

3. Are substantial conclusions reached? To some extent. Based on my comments above, it seems likely that an even longer annual record of discharge could, and probably will, be published for these sectors of Antarctica. Then again, from the discussion in this m/s it seems that SAR data this far south are limited primarily to two RADARSAT acquisition periods so perhaps the earlier study [Rignot et al., 2011] interpolated in time or used feature tracking or who knows what. I found the conclusions to be a little bland and confused. On the one hand it is stated that the sector studied shows little change and is therefore stable. On the other hand, they report a slow down of 25% which is significant for mass balance. This is for just one ice stream (Whillans) but it does presumably represent a ~25% reduction in discharge.

4. Are the scientific methods and assumptions valid and clearly outlined? The methods are described adequately for anyone who is relatively familiar with InSAR processing and make sufficient use of references where needed. InSAR and DinSAR are well established approaches.

5. Are the results sufficient to support the interpretations and conclusions? Yes, as far as the conclusions go.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

7. Does the title clearly reflect the contents of the paper? No. The title says nothing about the glaciological results/conclusions, region studied. It is all about the data used. It needs revising to reflect the science not the data.

Rignot, E., I. Velicogna, M. van den Broeke, A. Monaghan, and J. Lenaerts (2011), Acceleration of the contribution of the Greenland and Antarctic Ice Sheets to sea level rise, *Geophys. Res. Lett.*

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/6/C776/2012/tcd-6-C776-2012-supplement.pdf>

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

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