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Interactive comment on "Detailed ice loss pattern in the northern Antarctic Peninsula: widespread decline driven by ice front retreats" *by* T. A. Scambos et al.

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Review of Scambos et al. 'Detailed ice loss pattern in the northern Antarctic Peninsula: widespread decline driven by ice front retreats.' The Cryosphere Discussions, 8, 3237-3261, 2014. [Nick Barrand, 25-08-2014]

This paper provides a comprehensive account of the recent mass balance of the northern Antarctic Peninsula ice sheet and outlet glaciers using geodetic methods. The techniques of repeat altimetry and DEM differencing provide measurements of surface elevation change which are combined with firn layer thickness modelling and regional climate modelling of surface mass balance. The paper makes a substantive contribu-

tion towards understanding the recent volume evolution of Antarctic Peninsula glaciers. I recommend that the paper be accepted for publication in The Cryosphere providing the following minor revisions are addressed. The suggestions below aim primarily to provide clarity to some textual descriptions and in the presentation of results.

- p3238, line 6: What proportion of the total area of the nAP do these 33 drainage basins represent? This is not stated anywhere in the manuscript. Figure 1 suggests that it may be 100%, but it would be useful to know if any areas are not represented.

- p3238, line 22: these papers have also typically not attempted to explain mechanisms of ice loss; perhaps this could be added here? (given that this paper suggests several processes that could be causing the observed patterns of loss).

- p3238, line 26: McMillan et al. 2014 should be added to these citations for 'altimetry-based studies'.

- p3239, line 15: 'air warming ' is a somewhat inexact phrase, how about 'increased surface air temperatures'? Or refer to elsewhere if you don't mean the surface / near-surface. Equally, I'm not sure it's quite correct to refer to 'regional sea-ice decline' as a 'known climate change' – isn't this a result of climate change rather than a climate change itself?

- p3239, line 3: what is meant by 'severe assumptions'? Perhaps its better to mention what these are so the reader can fully understand why this work is such an improvement on previous. line 4/5: it could also be mentioned here that existing mass flux estimates cover only âĹij50% of AP drainage basins, which may go some way to explaining why they cannot yet be reconciled with independent (altimetric and gravimetric) assessments.

- p3239, line 23/24: would an assessment of 'surface elevation change' be a better description than 'vertical movement'? line 25: . . .from the Advanced Spaceborne. . .

- p3240, line 6, Table 1, and throughout: The ICESat measurement period is Septem-

ber 2003 to March 2008, yet the results presented in Table 1 refer to 2001-2010. Table S1 states that the DEM data (and thus, differences) were collected in (and calculated between) 2004-2010, 2001-2006, and 2003-2008. More detail should be provided if temporal averaging or scaling has taken place. This should be justified if so, given that large changes may have occurred outside these volume change epochs. If this cannot be justified, then volume and mass changes must be specified for the appropriate epoch of measurement, rather than simply 2001-2010.

- p3240, line 16: should this be 'To migrate the measurement track data..'?

- p3241, line 2 / Figure 1: Not every one of the islands mentioned in the text are labelled, or leglible, in the map text of Figure 1. Please add labels to Figure if place names are mentioned in the main manuscript text.

- p3241, line 18: Given that dh/dt is weighted so heavily compared to dDEM measurements, it would be useful to see how extensive the actual ground track data are. How well are outlet glaciers represented given the slope filtering? It seems that these data are shown in some elevation change profiles in Figure 2, but it might be useful to show the elevation change data distribution (as points) in Figure 1.

- p3241, line 23: now the study period is '2002-2010'. What about 2001 (see above, Table 1, etc)? What ice shelf and ice front retreats occurred during 2001 and are these likely to affect the results? Lines 24-27: The first part of this approach seems reasonable (that the rate of surface elevation change of ice just upstream of a loss area would thin at the same rate as the loss area ice). Is there any physical basis for the second assumption, though? (that the ice was lost midway through the study period). Is this assumption made as ice front data are only available during 2002 and 2010 (and no time inbetween)?

- p3242, line 6: '. . .individual glacier basins. . .'

- p3243, lines 3-4: 'variations in the time span of measurements for dDEM and ICESat

measurements' should be accounted for in the description of results / Table 1 – see comment above.

- p3244, line 6: '. . .decreasing elevation change with time. . .' line 16: 'shown' instead of 'suggested' here? line 25: isn't the proposed mechanism of propagation of kinematic waves of thinning up glacier inferred here, rather than observed?

- p3246, line 28: 'Mass accumulation from the model (surface mass balance, or SMB)' – I'm being a stickler here, but mass accumulation is not the same as mass balance. If it's a positive balance anomaly then say 'mass accumulation', if it's positive mass balance then say 'positive mass balance' – but it's not 'mass accumulation (surface mass balance)' as one is not the same as the other, especially in the Peninsula where surface melting takes place.

- p3258, Figure 1 caption: I'm not sure what a 'climate ice core' is. Climate variables may be inferred by proxy from an ice core, but are not measured. Figures 1, 2, and 4: do their backgrounds need to be black?

- p3259, Figure 2: The colour scale and ice shelf delineation is confusing. The shelves being marked bright blue suggests (following the colour scale) that they have gained 10 m or more of surface elevation per year. I doubt this is the case. The shelves, if colour delineated, should be assigned a colour that does not appear on the dh/dt colour scale.

P3259, Figure 2 caption line 1 and elsewhere: throughout the majority of the main manuscript text, a single measurement of surface elevation change over time is expressed as 'dH/dt'. However, in a couple of places, and throughout the tables, it is expressed as 'dh/dt'. Is this capitalisation random, or systematic? Does lower case h signify a single measurement, while upper case H specifies an area-average? This should be made clear if so, or standardised throughout the manuscript, Figures, Tables, and SOM, if not.

- p3256, Table 1: Footnote a. 'kg m3'. Footnote b. Are the area errors or uncertainty

from area determined at different time intervals included in the total measurement error budget? Footnote c. How much above? Was this standard and used for all sites? Footnote h. is there a reference for these reported uncertainties – or a pointer to the appropriate derivation in the main text? Footnotes i. to p. these could be summarised in outlines plotted onto Figure 1, which would save this table from its rather extensive list of footnotes.

- SOM, page 2: '. . .SPIRIT database2. . .' – what does this superscript refer to?

- SOM, Table S2 (and relevent to all Tables in the entire manuscript): I wonder if it wouldn't make more sense to order the columns in these tables according to the approximate order of derivation of measurements and calculations? First Area, then dh/dt, then dV/dt, then dM/dt.

- SOM, Table S3: Given the observed changes in surface mass balance over time (especially in the Peninsula region) why is the 1979-2011 averaged surface mass balance used to calculate the imbalance ratio, rather than SMB modelled over the time span of the remote sensing measurements? (2001-2010, or whatever the time span of change actually is – see comment above). Presumably this would be relatively easy to recalculate.

- SOM, Figure S2: there's an even different way of expressing elevation change over time here : 'Dh/Dt' – make consistent will all symbology within the manuscript (see comment above).

Interactive comment on The Cryosphere Discuss., 8, 3237, 2014.