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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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We thank the two reviewers very much for their comments, and discuss them below. In general we were able to adopt the suggestions completely.

For Reviewer 1 (H. Rott), the main comment was that our combined data sets are overlapping, and not evenly distributed in time or coverage through the period 2001-2010, and so our period of reference for the mass balance assessment is a bit variable within this period. However, the majority of our data are within the 5-year span of higher-quality ICESat data, 2003-2008. We have revised the text to emphasize that, in several places beginning with the Abstract.

Note that in our use of ICESat data we used all possible near-repeat profile pairings available (of those with ∼integer-year separations) to smooth out possible short-term variations and to reduce errors (by differencing over multiple years). There is definitely room for future, more detailed studies of year-by-year changes in specific glacier basins, perhaps incorporating airborne altimetry or CryoSat-2 data. Our paper provides an assessment of the mass loss rates of the basins for the middle of the decade.

Further comments from Reviewer 1 (H. Rott): Elevation losses at above 1000 m for the west-flowing glacier basins: We do not see these as contradictory to the ice core data, but rather indicative of even greater loss rates in the past than what we measure in 2003-2008. The reviewer asks for some indication of the amount of data and the data quality we have for the >1000 m areas of the nAP ridge crest. To address this, and in part to address the first main comment as well, we are adding a figure to our Supplemental Online Material document that presents the count of ICESat dH/dt assessments for each of the tracks that we used (new Figure S2: see attached graphic). This shows that most of the study area had 3 or more dH/dt assessments from ICESat data.

We state that the data above 1800 meters is not significant because very little area, and therefore very little ICESat data, lie above 1800 meters elevation in the study area. The majority of the high ridge area at 1400-1900 m elevation shows slightly negative to zero elevation change.

On the quoted section of the text '. . .suggests that mass loss rates are not decreasing at this time..' we were somewhat unclear. Our interpretation of the recent McMillan et al. results using CryoSat-2 data, November 2010- September 2013, is that there is little overall change in the mass loss for the nAP between our study period (centered on 2003-2008) and the most recent estimate (2010-2013). However, a recent paper submitted to GRL by H. Rott and co-authors indicates that the Larsen A embayment is now showing slightly reduced rates of mass loss. We refer to this study (which is likely to be accepted for publication in GRL soon) as 'personal communication, H. Rott'. We

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would like to evaluate the Rott manuscript status when our paper is in proof format.

Table S5 references and other issues in the Supplemental Text numbering are fixed now.

Reviewer 2 (N. Barrand) had a series of minor comments which we shall address in sequence. We paraphrase the request or comment below with the reply.

P3238, line 6: Please describe the fraction of the nAP covered by the study The 33 basin areas represent nearly 100% of the land and ice area shown in Figure 1, which is all the nAP mainland north of 66° S and several of the large nearby offshore islands. The study areas are outlined in white in Figure 1. The only significant land mass not covered within the Figure 1 region is Trinity Island (∼100 km2). A few larger islands further northwest of the nAP mainland, for example, Livingston Island and King George Island, are not inlculed. These may have some additional ice mass loss (negative mass balance is likely) that is not counted. We have adjusted the text in the Abstract and Figure 1 caption.

P3238, line 22 Note that the cited past studies do not generally infer processes that are responsible for ice mass loss patterns Done, with a new sentence in the Introduction

P3239, line 15 Address inexact descriptions of climate-related effects in the study area Done, by referring to the changes as 'climate-related changes' rather than 'climate change effects', and other adjustments.

P3239, line 3 What is meant by 'severe assumptions' in the Introduction? Text is changed to 'broad assumptions'. The assumptions were that a regular grid of dH/dt measurements were derived from ICESat track-derived data, and were interpolated to form a complete mapping. This implies that the Antarctic Peninsula is described by a smoothly varying dH/dt field at the scale of 10s of kilometers, whereas in fact there are very large variations in elevation change from basin to basin.

P3239 line 23 Would 'surface elevation change' be a better word choice than 'vertical

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movement' Done, and the correction for 'the' Advanced Spaceborne.. is also done.

P3240, line 6 and elsewhere Address the multiple periods of elevation change measurement This was also mentioned by Reviewer 1 (H. Rott). Because of the overlap of the ICESat data period, and the use of multiple laser campaign pairs for a given ICESat track site, the best detail we can provide for our assessment is 'centered on the period of the highest-accuracy ICESat data, September 2003 – March 2008'. All available estimates were combined, from both dDEM dH/dt data and from ICESat profile comparisons. We have adjusted the text in several places to emphasize this, and in Table 1 and Table S2.

P3240 line 16 Suggest change 'to migrate the measurement track data..' Done

P3241 line 2 Suggest labeling islands and other features in Figure 1 In fact, Figure 1 is intended as a locator map for the measurement basins, and the names in the figure refer to the Table S2 entries. Addition of further place names would only complicate this figure further. We believe the Figure is more useful as it is. The only islands not clearly named (but instead are abbreviated) are James Ross Island, Dundee Island, and D'Urville Island. These names are spelled out in the Table 1 and 2 (and S2, S3) footnotes. No changes were made to Figure 1.

P3241 line 18 Provide map of ICESat data locations This is now provided as Figure S2. Note, however, that ICESat data are only mildly weighted relative to dDEM data, because of the very large number of grid cells for each dDEM. As noted in the text, the idea was to allow ICESat data to have some impact in regions where nearly zero dDEM data exist. In any region where dDEM coverage is spatially continuous, these data dominate the assessment.

P3241 line23 Address the multiple periods of elevation change measurement (again), and discuss our assumptions for the ice-front-loss estimates This is now addressed from the earlier comment. The time-period cited in this instance was a typo. For the ice front loss assumptions, we are applying the closest measured ice dH/dt rate we

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have to the loss area (reviewer agrees with this) and then assuming that on average the ice was lost mid-way through our period of observations (generally, using a single image pair). This is to avoid the complexity of determining and handling partial retreats occurring sequentially for a glacier over the study period. We note that the overall impact of the grounded-ice retreats is a small part of the signal we measure.

P3242 line6 change text to 'individual glacier basins' (basins, plural) Done

P3243 line3 address statement on variation in timing of dDEM and ICESat observations in Table 1 Done, we have improved the description of the timing of the various measurements within the text.

P3244 line6 and line16 word edits Done.

P3244 line25 question on whether we observed, or inferred, kinematic wave propagation The statement includes citations for two earlier papers and our work here. Taken together, the kinematic wave propagation is 'observed'. However, 'we observe' is an issue since the authorship of the earlier works is not the same as this paper. We adjusted the wording to reflect this better.

P3246 line28 Recognize the difference between mass accumulation and surface mass balance Changed to 'Surface mass balance (SMB)...'

P3258, Fig1 caption: adjust wording for 'climate ice core'. Also, do backgrounds need to be black? Changed wording to 'ice core'. Re background color, given the variety of colors used, black is the best choice. A blue background would interfere with the color bar colors and the ice shelf extents shown. Grey would be hard to distinguish from the image data. White would be a problem for the basin outlines.

P3259, Fig2 Color changes suggested for ice shelf areas The ice shelf area color is now grey-blue, and clearly not on the color bar.

P3259, Fig2 and elsewhere: dh/dt or dH/dt? It is not systematic, it is a function of the span of time spent in compiling the paper and the figures (that is, we forgot earlier

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conventions and used new ones in more recent work). Since our measurement is of the change in surface height, and not ice thickness, I think the proper convention is: dH/dt. We have converted to this throughout now.

P3256 Table 1 comments Errors in area determination for ice front retreats are not included specifically in the overall error budget. However, such errors would be far below the cited error. Elevation band for the ice front retreat area is from the first 50 m elevation bin above the lost ice, for all sites. Changed text in footnote c. Footnote h: this is a note to facilitate the readers understanding of how we determined the column values. The reference text is near the citation of Table 1. The additional footnotes actually save space and are more precise than a text description. I believe most readers would feel that Figures 1 and 2 are complex enough.

SOM comments Page 2:We have replaced the footnote indicator (footnote was missing) with the URL for the SPIRIT archive.

Table S2 comment We did not undertake this very time-consuming suggestion. Most readers are interested in the mass balance, and we have placed that in the left column near the basin name.

However, we re-checked the values in all tables as part of this review. There were a handful of minor changes.

Table S3 comment Short-term variations in mass balance (e.g., when fewer years are averaged) could lead to patterns in the imbalance ratio that do not really indicate long-term glacier 'health'. Granted, in this region everything is changing (SMB, ice flow speed, and elevation) but we believe that the long-term mean mass input is the best one to compare to for an imbalance ratio.

Figure S2 (now S3) caption: Done, added clearer axis labels and standardized on "dH/dt".

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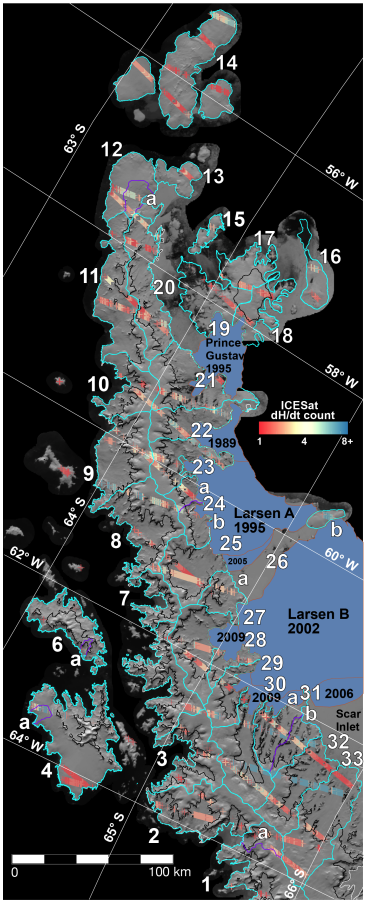

Fig. 1.

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