Below we reply to the referee comments (RC) with author comments (AC):

Referee #1: Interactive comment on **"The modelled surface mass balance of the Antarctic Peninsula at 5.5 km horizontal resolution"** by J. M. van Wessem et al. X. Fettweis (Referee)

RC: This paper presents very high resolution (5.5km) RACMO SMB results over the Antarctic Peninsula (AP) by comparing these outputs with observations and by discussing afterwards the spatial/temporal variability of these last ones over AP. To my knowledge, it is the first time that a SMB simulation at such a resolution is performed over AP using a robust snow energy balance model fully coupled with a RCM. The paper is pleasant to read while the discussion about the spatial/temporal variability does not bring something really new in respect to previous IMAU's papers using RACMO-27km (the SMB reference simulation for the international community) over the whole Antarctica. This paper fits well with TC and I recommend to accept it.
AC: We thank X. Fettweis for the positive and constructive comments. We will address all referee comments below.

RC: However, before publication, a comparison with RACMO2.3-27km is needed for me to really see the interest of using a resolution of 5.5 km over AP, independently of the RACMO-5.5km results discussion. A resolution of 5.5 km is needed for AP or 27 km is enough ? Only some comparisons with ERA-Int (75km) are shown. The authors suggest that a resolution of 5.5km is not enough fine to resolve the AP topography. OK, but in respect to RACMO-27km, what are the advantages of RACMO-5.5km ? Results from RACMO-27km should be included in Fig2 and Fig3 (It is likely that for some observations, RACMO-27km is better due to errors compensations!).

The equivalent of Fig 6 for RACMO-27 km should also be added. I don't think that including some RACMO-27km results in this paper is a big job for the authors.
AC: We agree that a comparison with RACMO2.3-27km is illustrative and an interesting addition to the paper. We added a comparison with 27 km in Figures 2-3, and included an SMB map at 27 km in Fig. 6.
These additions show that, due to error compensation, some bins are better represented by RAMCO2.3-27km (Fig. 2), and the correlation is similar (Fig. 3). However, for instance, the spine SMB is significantly overestimated at 27 km (Fig. 6). We have added discussions in the text where applicable, noting that the main advantage of RACMO2.3-5.5km over RACMO2.3-27km is the better representation of the SMB-gradient, the (SMB over) detailed topography such as the spine, and the small features (ice shelves, islands) of the AP ice sheet.

RC: Franco et al. (TC, 2012) shown that increasing resolution with MAR does not impact the interannual variability and that the trends are the same.
Is it also the case between RACMO-27km vs RACMO-5.5km (Figs 7-9). Or, a resolution of 5.5 km is needed to better capture the interannual variability over AP knowing that the melt is likely better represented in RACMO-5km?
AC: We have shown trends of temperature and wind in the previous RACMO2.3-PEN055 paper (Van Wessem et al., 2015) and compared them to ERA-Interim. In that paper we showed that trends are very similar to those of ERA-Interim, and , as snowmelt is closely related to temperature, expect the same to hold for snowmelt and likely for the SMB-components as well. Only over some topographically detailed regions (in the case of the aforementioned paper: over Alexander Island/George VI)

were differences in temperature and wind between RACMO2.3 and the re-analysis found. We therefore did not include a more detailed analysis/comparison of trends/variability in this study.

RC: Finally, ERA-Interim (75km) seems to be used to directly force RACMO at 5km. Forcing RACMO-5km with RACMO-27km has been tested ? In such area, where ERA-Interim is not constrained a lot, using the RACMO-27km atmospheric fields as forcing could improve the RACMO-5km results.

AC : We have tested this for Greenland once, and found for that specific situation a deterioration of the results. Furthermore, further nesting results in a further drifting away of the large scale circulation patterns in RACMO with respect to ERA-Interim. Since RACMO has no data assimilation, RACMO cannot increase the quality of the representation of large scale circulation, only decrease. Moreover, we mainly run RACMO in order to improve the representation of the interaction between the atmosphere and the surface and topography. In order to achieve this, high resolution is required, but an extended free model domain is not. In any case the differences are minimal, found when we directly compare model output at the location of the lateral boundaries (where no topography is present) between the different model resolutions.

Referee #2: Interactive comment on **"The modelled surface mass balance of the Antarctic Peninsula at 5.5 km horizontal resolution"** by J. M. van Wessem et al.
Anonymous Referee #2

RC: The paper presents surface mass balance of the Antarctic Peninsula using RACMO 2.3 coupled with a Firn Densification Model. The resolution of RACMO is greatly improved from 27 km to 5.5 km and it is able to resolve finer details of the spatial variability in SMB. The modeled results are compared with in-situ measurements.
AC: We thank the referee for the constructive comments and the positive review. Below we address the referee's concerns.

RC: General comments: The abstract needs to be tightened. Currently, it is too long. Please focus on what is new and the important numbers.
AC: We tightened the abstract by focussing more on the integrated estimates, removing lines 15-19.

RC: The paper is easy to read, but it fails to capture the reader's attention. The figures do not appear in the text in order. For e.g. Fig 11 after Figure 4 in 5107 while Fig 5 is called for the first time in 5108.
AC: This is a mistake, a label was used that referred to the wrong figure. This has been fixed.

RC: The text lacks organization. At least two of the figures can be combined together.
AC: We have combined Figures 8 and 9 into one figure, with subfigures a,b,c,d. We do not think it is possible to combine other figures.

RC: At several places, the paper mentions increase in surface melt and temperature but modeled results show otherwise. This needs to be clarified.
AC: We do not understand this comment. Where do we mention an increase in surface melt? Only in the introduction we mention, with regard to past climate, an increase in snowmelt. In all other sections in the manuscript, we mention a 'decrease' (or negative trend) of snowmelt, with regard to the model time-span (36 years), which is related to a negative trend in temperature that was reported in Van Wessem et al. 2015. All figures and text mentions are consistent with this statement. We have therefore not changed the text.

RC: This paper is important because it presents modeled surface mass balance estimates at a significantly high resolution over the Antarctic Peninsula. However, there are several points that need clarification and the text needs to be better organized.
Specific comments: I am not a big fan of how the data are binned in elevation ranges in this study, if I understand it correctly. If possible, please provide a separate figure of the bins and the observations included within them. It will also be helpful to have the years of the in-situ measurements.
AC: We think the current way the bins are presented is as detailed as it can be. We have tried to add additional colours to figure 1 to show in which bin every observation belongs, but this made the Figure too complicated. Furthermore, a more detailed

discussion of this plot worsens the focus of the manuscript, that is mainly about the new SMB results.

RC: Throughout the text, both 'Fig' and 'Figure' are used.
AC: We have adopted the TC format about usage of these words throughout the manuscript: If a sentence is started with this word, we use the complete word 'Figure'. If it's in the middle of a sentence (or within brackets) we use the abbreviation.

RC: What is erosion of drifting snow?
AC: Erosion of drifting snow is divergence/convergence of drifting snow. We have better explained the definition of erosion of drifting snow in the data/methods.

RC: Abstract Ln 15: Sublimation is considered to be the largest ablation term with 100 mm we yr-1. Snowmelt is high but refreezes but still considerable 200 mm we yr-1. This is confusing because the snowmelt term seems to be larger than sublimation. If that is not so, then you need to clean up the sentence.
AC: Snowmelt is no ablation term as long as it refreezes in the snowpack. Nevertheless, in line with the previous comment of the referee, we have removed this part of the abstract and focus more on the integrated numbers.

RC: Line 25: Snowmelt has a decreasing trend? You mean less snowmelt even when temperature is increasing? Please clarify why this is so.
AC: For the model time-span, there is a decreasing trend both in snowmelt, and of temperature (see Van Wessem et al. 2015 for the latter). We do not mention a current positive trend in temperature in the manuscript. Of course there is an increasing trend in temperature and hence snowmelt for the last century, but our study considers only the last 36 years, suggesting that the trend has dropped and has become negative in the recent decades.

RC: Introduction: Line 20: Please find a better way to define 'towards/away'? Better to use gain or loss perhaps?
AC: We have changed this as suggested.

RC: Page 5101: Ln 17: thickness change –do you mean elevation change?
AC: Indeed. We have corrected this as suggested.

RC: Page 5102: Ln 5: It would be nice to know what exactly is different in the 27 km and the 5.5 km here in terms of any changes in the Physics. You refer to Lenaerts paper, but it is useful to have it here too. Did the 27 km have the FDM coupled to it? You can summarize these things in one place maybe in section 2 and shorten the introduction if necessary.
AC: Previous manuscripts (Van Wessem et al. 2014a,b) discuss the changes in the physics between RACMO2.1 (the model of Lenaerts et al 2012) and RACMO2.3 at 27 km resolution. The physics used in the 5.5 km version of RACMO2.3 is exactly similar to the ones used in those studies. We have only increased the resolution to 5.5 km, and have a slightly different usage of the internal snow model. As explained in the text, for this study, given the importance of the snowpack conditions for an accurate runoff calculation, we used a higher-resolution FDM (that, in terms of physics, is similar to the FDM used in the other studies).

RC: Why does the period start from 1979?

AC: This is because the satellite era, and hence ERA-Interim, began in 1979, and the quality of re-analyses significantly increased since. We believe that the sentence in the Introduction “the period for which reliable forcing data are available,” properly explains this, and have not changed the text.

RC: 5103, Line 11: Das et al., 2015 and 2013 are more recent references to show direct evidence of sublimation over steep slopes.
AC: The reference used here is related to the hydrostatic balance assumption. The mentioned sublimation is just an example of a variable that might be affected by the hydrostatic assumption.

RC: 5104: Ln 13: What are the minor issues in the internal snow model? Please be specific. 5105:
AC: There was an error in the code, and we have rerun the FDM offline. We did not want to go into this in too much detail, since the snowpack conditions appeared to be of minor importance throughout our research. We changed the explanation in the text slightly for clarification. Except for the resolution, both models are identical.

RC: Ln 8: Not sure what is meant by time of the measurement not known? You mean date is not known or the hour of a day is not known? How many datasets are there with this problem? Should such observations be used at all?
AC: There are several in-situ observations in the Turner et al. database that did not have a date of measurement. However, they were quality controlled and also included in the Favier 2013 data-set. Which was our criterium for usage in the analysis.

RC:5105: Ln 10: Here and elsewhere spell out Sect. as Section. Or does the journal recommend this?
AC: As noted in the comment about Fig./Figure, it is indeed recommended by the journal.

RC: 5106: Figure 2: In some of the bins, RACMO has a lower spread in values than observed. I ask this because the resolution is high. Is it topography? How are the observations spreading out in the locations?
AC: We think that you mention the lower spread of RACMO in bin W7? The RACMO spread is indeed remarkably low, likely due to most of these higher elevations located in a region of relatively weak variability in the southwestern Palmer Land mountains. The green (total RACMO) variability is much higher, because of the addition of northern high elevation locations in this bin. Most likely, the spread of the observations is too high as well, as these observations, which are located at high elevations, are scarce and they generally cover short (<5 years) timespans.

RC: 5107: Ln 7: Here it clearly shows you need to have a figure for the 27 km comparison as well.
AC: We agree. As also noted in the reply to the other referee, we have added a 27 km analysis. A comparison with RACMO2.3-27km is illustrative and an interesting addition to the paper. We added a comparison with 27 km in Figures 2-3, and included an SMB map at 27 km in Fig. 6.

RC: 5113: Ln 16: I did not quite get why a persistent surface inversion would favor surface deposition. Please clarify.
AC: With a stable surface temperature inversion comes an increasing humidity mixing ration with increasing height as well. Because of these stable conditions, deposition is the dominant process (albeit small), while drifting snow sublimation is relatively small. See for instance King et al. 1996. We clarified this in the text.

RC: Comments on the figures: The captions need to be more concise. Please do not include methodology in the captions.
AC: We have removed most of the methodology from the captions, and noted it in the text only.

RC: Figure 1: I think you should use a gray scale for elevation and a different color for the floating ice shelves. It is very hard to see the in-situ locations.
AC: We have changed the elevation color scaling to gray scale in Fig. 1.

RC: Figure 2: Fig 4: A small location figure would be helpful.
AC: We already have location maps in Figure 1, Figure 7 and Figure 11. We have added some clearer references to these Figures in the captions.

RC: Figure 6: The comparison between the modeled results and in-situ measurements is a little hard to understand because the color scale is not linear. For e.g the green on the color scale covers a range of 200-700. Although I am impressed by the comparison in most cases, there could be important differences that may become more apparent with the right choice of color bar. It would be nice to use a linear scale and break up the regions into different panels if necessary.
AC: We chose this color scale to be consistent with earlier studies (and color scales) with RACMO. Moreover, SMB is largely non-linear, which is why this non-linear scale is chosen. We think this gives an adequate overview of observed and modelled spatial patterns. For comparisons of spatial coherence, we refer to Figure 3.

RC: Fig 7: A comparison with 27km RACMO will be useful.
AC: Yes. As noted, we have included this comparison.

RC: Figure 11: What is the point of this figure? Should there be any correlation between an ice core at James Ross with other regions far away from it?
AC: In addition to what is already mentioned in the text: The point of this figure, and placing it at the end of the manuscript, is that such a correlation map is an interesting case study and application of RACMO data. Moreover, it further emphasizes the clear differences between the WAP and the EAP which we believe is one of the prominent climate features of the AP.