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Interactive comment on "Melting over the East Antarctic Peninsula (1999–2009): evaluation of a high-resolution regional climate model" *by* Rajashree T. Datta et al.

Anonymous Referee #1

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Summary:

This paper focuses on melting over the East Antarctic Peninsula, with specific focus on the Larsen C ice shelf using the Modele Atmospherique Regionale (MAR) model. Model results are evaluated by satellite estimates of melt from passive and active microwave sensors, and from automatic weather station observations of near-surface variables. The abstract and introduction point to an assessment of the impact of westerly föhn flow on the melting, however this doesn't seem to have the same prominence in the results and discussion. My main concern is that the focus of the paper is to evaluate the MAR model in terms of melting, however there appears to be relatively

little discussion on the impact of the chosen model options, such as the horizontal and vertical resolution, which limits the readers understanding of how efficient MAR is at reproducing melt on the ice shelf, as seen in satellite observations. The potential impact of model choice, and model physics is most clear in the results and discussion of the wind direction, where there is a large discrepancy between the model and observations. I think this work is of sufficient merit and in terms of comparison of the model with both AWS and satellite observations, it takes a novel approach. Similarly, this (according to the authors) is the first time that MAR has been applied and evaluated over the Antarctic Peninsula, as opposed to Greenland, and therefore the results of this paper could influence model development by testing it in complex regions. As a model evaluation paper, it uses multiple data sources over a relatively long-time period. However, the model resolution is rather coarse over this region, as higher resolution model studies have been performed previously. An additional novel aspect which this paper does not mention is the length of this modelling study. Output for the model are for 15 years, which is a long study period over this region. Previous melt-föhn studies have largely focused on case studies, or shorter time periods (e.g Elvidge et al 2015, Grosvenor et al (2014), King et al 2017). More emphasis could be put on the length of study, as this is of importance. In my opinion, this work is sufficient for publication, but major revisions are required beforehand.

Major concerns:

The major concerns are mostly centred around the choice of model, and the justification for this model. There are a number of possible reasons why this model (in its current state) should not be used for this area (coarse resolution, hydrostatic assumption, low vertical resolution). However, if the goal of this study is to evaluate MAR to assess whether it is useful to model melt in this area (and particularly the wind-driven melt), then it needs to be phrased differently in the abstract and introduction. If an objective is to test whether hydrostatic models can capture the wind-induced melting and wind processes (as opposed to non-hydrostatic which have been largely used), then this needs to be outlined. Similarly, if an objective is to assess whether you need to go to much lower resolutions, or whether you can model melt at coarser resolutions, this should be stated. Currently, the paper reads that you have reservations about the potential of the model before you start, but continue to use it anyway. In the discussion, you would also need to compare this study to ones of higher resolution or different models, to say whether MAR has been successful at capturing wind-induced melt. The following points, break down the concerns with the model set up.

The hydrostatic assumption and horizontal resolution of MAR. In the abstract, authors state that "melting in the East AP can be initiated by both sporadic westerly föhn flow over the AP and by northerly winds advecting warm air from lower latitudes. To assess MAR's ability to simulate these physical processes, this study..." (line Pg 1, 24-27). Then later in the discussion you state that MAR can't accurately represent the wind direction and föhn processes due to the model's hydrostatic assumption, and state that a non-hydrostatic model would do better (pg 15 line 23-27). Models which have previously, successfully captured the föhn characteristics (WRF and UM) are non-hydrostatic, and this appears to be known to the authors prior to the study as in the introduction (Pg3, line 33), they discuss the (non-hydrostatic) RACMO study, and justify their use of a coarser resolution due to the hydrostatic assumption. If a large part of the study is to assess the impact of wind on the melting, why chose a model which can't represent the dominant westerly flow (and subsequent downward föhn flow) over the AP? If an objective of this study was to attempt to model this type of flow using a model with hydrostatic assumption, then this should be made clearer, and authors should note any previous studies of this kind.

The above comment is also linked to the relatively coarse (for this region and this topic) horizontal resolution used here. Previous föhn studies use much finer resolution (5km, 1.5km) and suggest that this resolution is required for adequate föhn representation. Authors discuss the Van Wessem et al (2015) study which suggests higher resolution than 5.5km. However the authors appear to use the following statement: "where hydro-

static assumption is preserved (such as this model run), higher resolutions may inhibit flow in the model..." (pg 3, line 35/36) to justify using a lower horizontal resolution. To address this issue, I think a sensitivity study using higher resolution is required. It doesn't have to be for the full-time period, but should capture at least a season of melt to assess whether the spatial resolution could improve the results, and whether the breakdown of the hydrostatic equation does limit air flow. You should also add more to the discussion about this. The spatial resolution is not mentioned at all in the discussion, and as found in other papers (Van Wessem et al 2015, Turton et al 2017, Elvidge et al 2015), higher resolution runs do capture föhn winds.

In the abstract and introduction, a fair amount of emphasis is put on the role of föhn winds and northwesterly winds e.g (Pg 1, line 25, 32, 33, 35, Pg 3, line 5-21, 34-37, Pg 4, line 1, 20). However, in the results and discussion, this is not discussed thoroughly, either in the context of other studies, or how well MAR can model these features. More discussion of the föhn characteristics and melt related effects needs to be included in your discussion to have such a prevalence in the earlier sections.

The abstract states that increased spatial resolution and topographic resolution could improve the output from MAR, but there is no mention of this in the discussion or results. You should not include statements in the abstract which do not reflect the results of the study. Either address whether changing the spatial or topographical resolution does impact the modelled melt or near-surface conditions, or remove this from the abstract.

The vertical resolution of MAR's atmosphere is very coarse, especially to have the lowest model level at 2m above the surface (pg 5, lines 10 and pg7 line 29/30). What is the vertical discretisation of your levels? The WRF model for instance has difficulties if there is over 1km between model levels, or if the stretching factor is greater than 20%. Similar to the first major comment, a sensitivity study is required to assess the impact of this vertical resolution on the representation of the near-surface conditions and the wind. This is much coarser than many studies of this kind and studies using

MAR (see for example, Gallee et al 2015 or Wyard et al 2016 who both use 60 vertical levels). Again, this doesn't need to be the full period (and shouldn't be as this would be a huge/long undertaking) but a full season should be tested using a number of higher vertical levels.

Minor comments:

Lots of sentences start with 'Because'... Perhaps vary this a little.

Coordinates are needed on Figure 1 and 2.

The insert for Figures 3 and 4 is very small and hard to read. Could it maybe be included in Figure 1 alongside the other map? Or move Figure 8 earlier, where the insert is clearer.

There needs to be a discussion of why MAR is unable to capture the wind direction, as presented in Section 4.2. Is it getting the synoptic situation wrong? Or near-surface conditions? Is this related to the relatively coarse resolution? This section is important for assessing the impact of the wind direction in MAR, but it should also be stated that if MAR is getting something like large scale flow wrong, it might be getting other processes wrong due to this.

Detailed comments:

Abstract:

Line 34: Authors state that reducing the underestimation of flow may be obtained by increasing the spatial resolution, but this is not given much discussion later in the paper. Either remove it and focus on hydrostatic assumption, or include changes to the spatial resolution in the discussion- either results from the suggested sensitivity study, or by discussing other studies.

Line 35: You mention reducing the underestimation of flow may be obtained by using higher-resolution topography, but this is not mentioned anywhere else in the paper.

Similarly, you do not state what topography is used in the model, or what resolution it is.

Introduction:

Pg 2 Line 7: remove 'finally' .

Pg 2 Line 23: 'suggested' should be 'suggest' .

Pg 3 Line 17: Remove 'during recent warming' at the end of the sentence.

Pg 3 Line 20: Is there a citation for this? 'East AP is as vulnerable to wind dynamics as it is to temperature change'. Has a study quantified the difference in vulnerability? What vulnerability mean in this context?

Pg 3/4 : Some citations are missing which may need including here, such as King et al 2017 and Elvidge et al 2015 which discuss föhn and melting on Larsen C.

Pg 4 Line 2/3: 'These last studies taken together' doesn't read well. Perhaps change to 'Both of these studies, along with others by Elvidge et al 2015 and King et al 2017, discuss both the atmospheric...'. This would include the previous comment also.

Pg 4 Line 10: AWS is not defined yet (but is later defined on line 13/14).

Pg 4 Line 14: Which satellites? Just give their names/abbreviations here.

Pg 4 Line 15 and 20: Be consistent with use of abbreviations or names. AP for example.

Pg 4: Line 24: from what date to 2014?

Data and Methods:

Pg 4 Line 27/28: MAR and AWS have been defined earlier.

Pg 5 Line 5: Which part of Antarctica?

Pg 5, section 2.1: Where can readers can get more information about MAR, such as physics set up? Include a citation for this. What is the model top? 23 Sigma layers is very coarse (see major comments). Why was this vertical resolution used? Only 1 domain or is it nested? What is the resolution of the topography, and what dataset is used? BEDMAP2 for instance?

Pg 5 Line 17: what mask? Land use? Land/sea?

Pg 5 Line 20-26: reorder this paragraph to make it clearer what each notation is. For example, line 20-23, both notations are stated, but more emphasis is put on LWC0.4. It could be split into 2 sentences, one for LWC0.4 and one for MF0.4.

Pg 5 Line 25: What is the justification for this condition? The same as LWC0.4 (Tedesco et al 2007)?

Pg 5 Line 30: change 'microwave sensors are weakly affected…' to 'microwave sensors are only weakly affected'.

Pg 5 line 31: after citation, change 'where' to 'whereas'.

Pg 6, line 6/7: 'used extensively' is stated, but there is only 1 citation. Are there other important citations? The Drinkwater and Liu (2000) reference only looks at Antarctica, not Greenland.

Pg 6, equation 1: what is Tc?

Pg 6: active and passive microwave: what are the spatial resolutions of the satellites? To allow some comparison with the 10km resolution of MAR.

Pg 7, line 2: confused what 'here' means in this context. For this location?

Pg 7, line 3: 'zwa is based on the winter mean threshold'. Threshold of what? Winter mean air temperature?

Pg 7, line 8: pressure observations are not mentioned here but they are in line 27 onwards.

Pg 7, line 19: what is meant by 'expected'? This is also used in terms of wind speed

later in the paper, and I don't understand its use.

Pg 7, line 27: What is meant by 'estimating' pressure from the AWS? Is pressure observed by the AWS or not? Pressure is also not mentioned elsewhere in the paper, so if it is not used, remove it.

Pg 7, line 28: remove 'a' from 'also estimated at a approximately…'

Pg 7, line 29: your lowest model level is 2m but only 23 sigma levels are used- this is very coarse. See major comments above. Are 2m diagnostics output from MAR? As you could use these instead of taking it from the lowest model level, if this should change when you run the sensitivity study for varying the number of vertical levels.

Results:

Pg 8, line 1: 'assess the extent to which each station is representative of larger scale climate variability'. Even though AWS14/Larsen and AWS15 are so close together? Do they have a different extent?

Pg 8, line 13: keep consistent with abbreviations.

Pg 9, line 3/4: you state coordinates/latitudes in the text but there are no coordinates on your Figure 2 plots. Include coordinates on the plots.

Pg 9, line 19: 'data sources ad secondarily' should read 'data sources and secondarily'.

Pg 9, line 19: What are the spatial resolutions of the data sources? You mention this, but then don't go into it any further. However, you mention the depths presumed for melt water content and then discuss it for the next paragraph. Perhaps more information on the spatial resolutions is needed.

Pg 9, line 28: Give some examples of these 'low' melt occurrence regions. From elevation information in supplement table 1, they aren't on the ice shelf, are they on the main spine of the AP?

Pg 9, line 28: heterogeneous in what way? Elevation? Surface type?

Pg 9, line 37: what is ‘N column’?

Pg 10, line 17: what is PMWAll-coincident?

Pg 10, line 31: ‘early pulse around Dec 15th’, do you mean Nov 15th? As there are small pulses of melt here, and December 15th melt looks much larger.

Pg 11, line 25: ‘during that period’. Which period? Be more specific.

Pg 12, line 4: remove ‘station’ after AWS.

Pg 12, line 8: remind readers of MAR-R/MAR differences here.

Pg 12, line 15/16: ‘demonstrate the consistency of wind biases’ and ‘how wind biases vary by latitude’ are slightly contradictory. Are they consistent or variable?

Pg 12, line 16: remove ‘whereas’, as you aren’t comparing AWS and MAR, as one is for low wind and the other for high wind speeds.

Pg 12, line 16: ‘MAR is dominated by northerly winds’.

Pg 12, line 27/28: Might be useful to highlight which rows of the table you mean here. When comparing all times and melt times. It isn’t immediately clear that ‘increased N and W flows’ means compared to when there all days are included.

Pg 12, line 34: citation style.

Pg 12, line 36: the abbreviation Ts is used in the table for when temperature is >0degC. However, in the text you say that when 2m-temperatures exceed 0degC. Stick to the T2m abbreviation.

Pg 13, line 5: remove extra space before -3.04.

Pg 13, line 8-12: include reference to figures here.

Pg 13, section ‘observed NE flow and observed SW flow’: It needs to be clearer that

when MAR has different wind directions to the observations, MAR is wrong. Especially in the case where there are large differences (NE vs NW for instance). And explain what the possible reasons are for this. Is MAR not getting the synoptic scale wind direction right? Or is there not enough blocking on the west of the AP to prevent flow over the AP when there shouldn’t be? This section is a good idea to see what impact the wind direction is having in MAR, but it should also be stated that if MAR is getting something like large scale flow wrong, it might be getting other processes wrong due to this.

Pg 14, line 1-6: In this section, it might be good to remind the reader, that in case 2, MAR is getting the wind direction wrong when compared to AWS. So that the reader can put these results into context.

Pg 14, line 7: Using Ts abbreviation but you have only talked about air temperature and used T2m previously.

Pg 14, line 11-13: confusing sentence. What is meant by expected?

Pg 14, line 13: I don’t think figure 6 e-h are necessary. They are not discussed as much in the text, and the information is given by the 6a-d. Similarly, figure 7 could be included into figure 6 in place of 6e-6h.

Discussion:

Pg 14, line 30: remove ‘in the aggregate’.

Pg 15, line 4: where should be when.

Pg 15, line 6/7: Any suggestions for why there are less westerly winds in MAR?

Pg 15, line 17-21: considering the impact of föhn winds is prominent in the abstract and introduction, this seems like a short discussion of them. See major comments.

Pg 15, line 19-21: wind speed may not be the biggest issue here if MAR is unable to get wind direction right.

Pg 15, line 23-25: include references to and discussion of non-hydrostatic models that have captured föhn flow- e.g Elvidge et al, 2015 (Met UM model), Turton et al 2017 (WRF model).

Pg 15, general: The abstract suggests that increasing the spatial resolution of MAR or the topography in the model may improve output, but this isn't discussed in your discussion. See major comment.

Pg 16, line 7: Figure 8 should come earlier in the text. This is a good summary figure and could be included in page 11 where interannual variability is mentioned.

Pg 16, line 19/20: 'melt in the NL region is particularly sensitive to föhn induced melt'. You need to support this with other studies (e.g Elvidge, et al 2015, Cape et al 2015), as your study only mentions föhn jets on the SW of the ice shelf in earlier discussion.

Pg 16, line 22/23: is this future work? As this study doesn't talk about large-scale atmospheric drivers at all. Or you need to support this with studies which look at large-scale atmospheric patterns and their related wind patterns in this region (such as Cape et al 2015).

Figures:

Figure 1: include in the caption that Larsen IS and AWS14 have the same MAR grid cell, which is why they are on the same marker.

Figure 1: Where is the topography data from?

Figure 1: include coordinates.

Figure 2: include coordinates.

Figure 3/4: make insert bigger, or include it in figure 1.

Figure 6: make a heading over a/b 'Case 1' and over c/d 'Case 2'. I don't think anything else is gained from e-h, as they are mentioned only briefly in the text.

Figure 6: g and h are not described in the caption.

Figure 6/7: 'yellow as only shown for g,h'. Not sure what this means, as yellow markers are used in every subplot, not just g and h.

Figure 7: could be combined with Figure 6.

Figure 8: if this goes earlier in the text, then the size of the insert is sufficient for the other figures which require it.

Supplementary Figure 6: lettering is not right. There is no a-c as in the figure, and g-m are not in the caption. There are only 6 subplots, so I assume a-f is correct.

Table:

Table 1: Ts should be T2m, unless actual surface temperature data is being used, but is not mentioned elsewhere in the paper.

Typos:

Pg 1, Line 29: satellites should be satellite

Pg 2, line 21: comma after citation

Pg 2, line 27: comma after citation

Pg 4, line 20: umlaut missing over o in föhn

Pg 5, line 31: comma after citation

Pg 6, line 6: remove full stop after algorithm, there is one after the citation.

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