

Dear Reviewer,

The major revisions requested are categorized here, whereby each major Revision# is described in the file “MajorRevisions\_TC\_AP\_RDatta”, also attached. Our response to minor comments are listed after, followed by your specific original comments motivating the major revisions here.

- (1) **Model choice**, which is addressed in Revision #1. We have included a comparison between different resolutions as well as greater detail on the model options (Page 5).
- (2) **The hydrostatic assumption**, which is addressed both Revision #2 (non-hydrostatic models) and Revision #5 (the hydrostatic model RACMO2.3p2). We have included greater discussion of this topic in addition to an inter-model comparison.
- (3) **The overemphasis of föhn winds**, which is addressed in Revision #3. We have de-emphasized a discussion of föhn winds but included a section specifically focusing on northwesterly flow.
- (4) **Length of the model run:**  
The length of the study has been emphasized in several places.  
Abstract: P1, L18-20  
Introduction: P4, 1-2  
Discussion and Conclusions: P19,L1-5

Many thanks for your consideration.

Sincerely,

R. Tri Datta

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## Response to Minor 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. **See Response to Major Concerns above, #1**

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. **See Response to Major Concerns above, #1**  
**Topography shown in P5, L21-22**

### Introduction:

Pg 2 Line 7: remove ‘finally’ **corrected, P2, L2**  
Pg 2 Line 23: ‘suggested’ should be ‘suggest’ **corrected, P2, L18**  
Pg 3 Line 17: Remove ‘during recent warming’ at the end of the sentence.  
**Paragraph has been removed.**

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? **Paragraph has been removed.**

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.

**Added, P3, L1-12, L29-33**

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.

**Paragraph has been removed.**

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

**Corrected, P4, L5**

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

**Corrected, P4, L11-12**

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

**Corrected, “AP” used throughout**

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

**Corrected; P4, L 20**

Data and Methods:

Pg 4 Line 27/28: MAR and AWS have been defined earlier. – **Corrected, P4, L29-30**

Pg 5 Line 5: Which part of Antarctica?

**Corrected, Reference to Terra Nova Bay, Antarctica is added P5, L5**

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?

**Corrected: Substantially more detail provided about**

- **Initial snow density**
- **Reference to previous model setup (P5, L12)**
- **Resolution and domain, nesting, topography**
- **P5, L10-37**

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

**Corrected, P5 L28**

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.

**Addressed in Page 6, Line 1-11(rewritten)**

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

**The meltwater threshold is discussed in Response to Major Concerns #5**

Pg 5 Line 30: change ‘microwave sensors are weakly affected. . .’ to ‘microwave sensors are only weakly affected’.

**Corrected, P6, L26**

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

**Corrected, P6, L28**

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.

**– additional citations added, P7, L 4-5**

Pg 6, equation 1: what is  $T_c$ ? **The new paragraph is rewritten for clarity (P 7, L 32-36)**  
Pg 6: active and passive microwave: what are the spatial resolutions of the satellites?  
To allow some comparison with the 10km resolution of MAR.

**P6, L 34; P7 L 21**

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

**The new paragraph is rewritten for clarity (P 7, L 32-36)**

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

**The new paragraph is rewritten for clarity (P 7, L 32-36)**

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

**P8 L6,7 explicitly explains that the surface pressure comparison is provided in the supplemental material**

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.

**I have added an explanation for this in text, I am using the term "expected value" (derived from the shape and scale parameters in the Weibull fit) interchangeably with the term "predicted mean" or "1<sup>st</sup> moment" to differentiate it from an arithmetic mean**

**P7 L17,18**

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. **P8 L6,7 explicitly explains that the surface pressure comparison is provided in the supplemental material. Avoiding the word "estimated" P8, L 25**

Pg 7, line 28: remove 'a' from 'also estimated at a approximately. . .' – **corrected, P8, L 26**

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.

**2m values for P were not available in this model version  
(but will be in future model runs)**

## **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?

**It is not very different but there's a slightly greater correlation in AWS15 to southerly regions as well as to the other side of the AP which is enough to be more affected by southwesterly flow**

Pg 8, line 13: keep consistent with abbreviations.

**– Corrected, P9, L10**

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.

**-Corrected, now Fig. 5**

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

**Rewritten, P11, L 29**

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.

Added explanation in text, P11, L26-29

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?

-Addressed, P 12, L2-3

Region also coincides with NL region, described: P12, L 34-36

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

P12, L1,2

Pg 9, line 37: what is 'N column'?

Corrected, P12, L11

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

Rewritten for clarity. P 12, L 27-30

Table 1 now has abbreviations

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.

Clarified. P10, L 8-12

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

Corrected, P14, L1-2

Pg 12, line 4: remove 'station' after AWS.

Corrected, P14, L16

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

References to MAR-R are removed for clarity because results are unchanged, but the definition for MAR-R is included in Table 1 and kept in the figure

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?

Rewritten for clarity, P14, L23-27

g 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. - Rewritten for clarity, P14, L23-27

Pg 12, line 16: 'MAR is dominated by northerly winds'. . Paragraph rewritten

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.

References to table rows added throughout

Pg 12, line 34: citation style.

Corrected, P15, L12

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.

Corrected

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

Paragraph rewritten

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

Paragraph rewritten

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.

Paragraph rewritten due to recomputation in wind fields

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.

Paragraph rewritten due to recomputation in wind fields

Pg 14, line 7: Using Ts abbreviation but you have only talked about air temperature and used T2m previously. Paragraph rewritten due to recomputation in wind fields

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

Paragraph rewritten due to recomputation in wind fields

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.

Fig. 9 now shows more general wind biases in response to the recomputation of wind fields and figures have been combined

### Discussion:

Pg 14, line 30: remove 'in the aggregate'.

Corrected, P.16, L22

Pg 15, line 4: where should be when.

Discussion has been rewritten for clarity

Discussion has been rewritten for clarity

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

See: Response to Major Concerns #1

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.

See: Response to Major Concerns #3

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

The corrected calculation for wind direction has altered these results considerably, and we have emphasized that wind biases account for a relatively small proportion of melt occurrence captured by satellites, but not by MAR

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).

See: Response to Major Concerns #3

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.

See: Response to Major Concerns #1

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.

This has now been moved to Fig. 2 and Sect. 3.1 (along with a comparison with RACMO in Fig. 3)

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.

See: Response to Major Concerns #3

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 largescale atmospheric patterns and their related wind patterns in this region (such as Cape et al 2015).

I've eliminated this section to discuss a paper more specifically which is currently in progress.  
P19, L1-5

### 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. – addressed
- Figure 1: Where is the topography data from? - addressed
- Figure 1: include coordinates. - addressed
- Figure 2: include coordinates. - addressed
- Figure 3/4: make insert bigger, or include it in figure 1. - Inserted in Fig. 2
- 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. – Addressed. Figure is combined into Fig. 8, although the recomputation of wind directions means that different (more generalized) biases are discussed
- Figure 6: g and h are not described in the caption. – Now removed
- 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. - addressed (with a legend included)
- Figure 7: could be combined with Figure 6. - addressed
- Figure 8: if this goes earlier in the text, then the size of the insert is sufficient for the other figures which require it. – This is now Fig. 2
- 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. - corrected

### Table:

- Table 1: Ts should be T2m, unless actual surface temperature data is being used, but is not mentioned elsewhere in the paper. - Corrected (now table 2)

### Typos:

- Pg 1, Line 29: satellites should be satellite Abstract has been changed considerably
- Pg 2, line 21: comma after citation Corrected, P2, L16
- Pg 2, line 27: comma after citation Corrected, P2, L23
- Pg 4, line 20: umlaut missing over o in föhn Final paragraph of Intro changed
- Pg 5, line 31: comma after citation Corrected, P6, L28
- Pg 6, line 6: remove full stop after algorithm, there is one after the citation. Corrected, P7, L3

## Reviewer Original Comments Addressed:

### 1) Model Choice.

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.

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.

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.

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## **2) The hydrostatic assumption**

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 nonhydrostatic, 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.

### **3) Overemphasis 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.

### **4) Length of the model run**

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.