Interactive comment on "Summer aerosol measurements over the East Antarctic seasonal ice zone" by Jack B. Simmons et al.

Anonymous Referee #2
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The authors thank referee 2 for their time and attention in reading this manuscript. The feedback provided by referee has resulted in significant changes to parts of the manuscript, changes which the author believe make this a stronger and more balanced piece of work. Specifically, the significance of the work has been made clearer and the relevance widened. Responses to specific comments are provided below.

Specific comments:
Abstract: I recommend rewriting the abstract. It should contain information on what was measured and when, on which platform etc. In the current form, it builds too much on the previous study by Humphries et al. and readers who are not familiar with that study will not get a good insight into what was done in this study. The abstract also heavily relies on knowledge of the Polar and Ferrel cells are, but those are not introduced. I recommend explaining what they are. It is also not clear whether the absolute humidity was measured or obtained from numerical weather prediction models. Furthermore abbreviations are not introduced.
Thank you for this suggestion. The abstract has been re-written as suggested, with a greater focus describing the measurements and main results of the work.

Introduction
l. 49: It is unclear which model parameters the authors refer to. – Thank you- this sentence has been edited..

l. 55: The argument why understanding the aerosol population over the East Antarctic seasonal ice zone is not very well fleshed out. Only a study of Shindell et al. (2013) is cited which highlights discrepancies between modelling and satellite observations. From this it is however not clear why this should researched further. This argument needs to be elaborated.
Thank you for this suggestion. Detail has been added to this section, and the introduction has been restructured to highlight the motivation more clearly including adding relevant recent references. Thank you specifically for the suggestion of McCoy et al., it is a very relevant publication!

l. 60-64: The discussion of results obtained from previous measurements over the Southern Ocean is extremely short and it is unclear what the main point is that the authors would like to make. Only two studies are cited and only one short result of one of the two studies is mentioned. Here, clearly, a more thorough review needs to be done and the main findings of several previous studies relevant to this particular work need to be synthesized.
Thank you for this feedback. We have significantly edited the introduction to include a more thorough review of the relevant literature.

l. 75 ff: Say where the cells roughly meet in terms of latitude.
At the end of the introduction, I expect a brief description of the campaign, i.e. when did it happen, what was measured...

Thank you for this suggestion. The authors agree this helps the flow of the manuscript, and as such have added a brief campaign description as suggested.

Methods
Fig. 1: I recommend plotting the sea ice extent for SIPEX-II an PCAN since this is an important environmental factor for this study.
Figure 1 has been updated with this change implemented. Thank you.

L. 110 ff: Meteorological measurements on ships can be biased because of the ship's superstructure (see e.g., Landwehr et al. (2020), 10.5194/amt-13-3487-2020). Did the authors do anything to check for potential biases and were the data corrected? A more detailed description is needed.
No bias correction was applied to the meteorological measurements. However, it is expected periods of meteorological measurements influenced by the ship's superstructure are coincident with periods of exhaust contamination in other parameters, which have been removed. This is due to the relative position of the anemometer positioned at the bow of the ship, forward of the superstructure and exhaust stack of the *Investigator.* This has been clarified in the text.

l. 115: Particles as small as 3 nm can easily get lost through diffusion. There is no reason not to characterize the inlet losses at least theoretically, if experimentally this is not possible. The authors can use for example the particle loss calculator by Von der Weiden et al. (2009), www.atmos-meas-tech.net/2/479/2009/. Also the length of the inlet line is not given and neither are the bends described. This needs to added to the paper. The concentrations reported in this work might be used for satellite and model validation, hence providing uncertainties with the numbers, and those include inlet losses, is essential.
Thank you for bringing this up- this is a very relevant issue. Experimental characterisation of inlet losses on the *Investigator* was planned for 2020 but could not occur. We have added a description of the inlet system along with theoretical inlet transmissions in Section 2.2. SIPEXII was aboard the icebreaker *Aurora Australis,* whereas PCAN occurred on the *RV Investigator.* Therefore, inlet systems used were different and have been characterised separately. This has been clarified in the text.

l. 120: How were size distributions measured? One expects this information here, where they are mentioned not further below. – This section has been restructured to improve the clarity. Thank you.

l. 140: There is no mention of what Rn measurements were used for and why.- An explanation of the inclusion of Rn measurements has been added- thank you.

l. 145 ff: How were CO2 and black carbon measured? This information needs to be included in the paper. – The instrumental techniques have been added to the manuscript, thank you.

l. 160: Say which variables were used along the trajectories and which uncertainties are referred to? Do the authors mean the 3 D coordinates or the uncertainties in meteorological parameters? More detail and a reference has been added describing trajectory uncertainty.

l. 169: Explain what the McGill et al. (1978) style is. Most readers will not be familiar with it and will have to look up the reference. – Thank you. A description has been added.

Results
l. 175: The question is why one should expect a step change? Just because this was observed previously, once only, it does not mean that this is the norm. The authors should explain why their implicit assumption is the step change, or they should drop

it and describe more neutrally that a step change has been observed previously, but one cannot expect it to be there permanently as it will depend on the meteorological situation. The Polar and Ferrel cells are descriptions of longer-term average circulation features. Those can deviate significantly on the shorter term. It is also unclear why it is so important to determine the transition between the two cells in the first place, and in the second place why is it important to use aerosol parameters? Those two points are not very well motivated. We have attempted to change the tone of writing here, and comment more objectively on the aerosol populations observed during the PCAN voyage. It is noted that the distinction between climatological and meteorological phenomena is important in this case, and as such this in mentioned in text. Thank you for your comments.

Fig. 2: All fonts are way too small. There are also substantial measurements near 180 _ in Fig. 2b. It is unclear why those are not considered in the analysis if the argument, as given in the caption, is the frequency of observations. Fig. 2c has no y-axis scaling.
Thank you for this comment. I have adjusted this and made the 'Southerly' measurements (near 180°) a more prominent part of the manuscript. I have also included the y-axis on 2C.

l. 220ff: Why did the authors exclude measurements from the South? Those are also important and interesting. Just because they do not fit the scheme of Polar and Ferrel cell does not provide a good argument. I recommend data are shown and discussed. Also statistics for "no category" should be shown for completeness.
Thank you for this suggestion. I have added the 'Southern' and 'No category' classifications to Figure 4 and include median size distributions for these categories in the supplementary. The results are also discussed in text at the beginning of the 'Results' section.

l. 241: Please provide the formula in the manuscript. Thank you for this suggestion. The formula has been included.

Fig. 3: The writing is too small. The figure font size has been increased. Thank you.

l. 290ff: It is an interesting result that the accumulation mode diameter in the Polar cell is smaller than that in the Ferrel cell. This is opposite of what Schmale et al. (2019) found for the Western site of Antarctica. The authors should discuss their findings in a broader context, going beyond what has been found South of Australia. The question of larger particle number concentrations closer to Antarctica is not limited to that sector of the Southern Ocean. The author very briefly hint at potential causes in l. 485, but do not make the connection as suggested here.
Thank you for suggesting the reference. Comparison to these results from west Antarctica has been added. I do not think the difference in median sizes large enough to be commented upon in this case (this is likely only 1-2 bins of the SMPS at this size). This is a difference, however, compared to Schmale et al., who note a larger difference in accumulation modes in their clusters.

Fig. 5: Diameter axes are typically logarithmic. The x axis in this case is logarithmic. The caption has been edited to state this. .

l. 334: the Types in Chambers et al. (2018) need to be explained here, otherwise it is not clear what the authors try to convey. – Thank you, this explanation has been added.

l. 333: Was "SO" introduced?- No, the acronym has been removed. Thank you for the pickup.

l.370ff: This is highly speculative. What are the observations that allow the authors to draw the conclusion that the lifetime of aerosols is longer in the Ferrel than the Polar cell? Particle lifetime is strongly determined by precipitation for the considered size range. What are the differences between the cells? How long would it take to

increase the size of an accumulation mode particle if the authors assumed it is sulfuric acid with a given accommodation coefficient? How long does coagulation take? Is this in line with the expected longer lifetime? Also, if the source is similar in both cells, why are there less particles in the Ferrel cell? The most likely explanation is precipitation scavenging, this would however counteract the lifetime argument. This statement clearly needs more elaborate discussion to back up the assumption. Thank you for this suggestion- in fact this thought was rather frivolous, without the required analysis. It has been removed.

l. 390: What are the vertical lines in figures S2b and S3b? – These represent the interquartile range of the trajectories. I have added this to the caption.

Conclusion:
l. 515: Why does it need strong low pressure systems to exchange air masses between the free troposphere and the marine boundary layer? Smaller convective processes during cloud formation and dissipation will do the same. Here more discussion is needed on how air masses might be exchanged. Thank you for reminding us of this fact. Low pressure systems are mentioned specifically as there is anti-correlation between particle concentration and surface pressure observed in the measurements. I have added a sentence immediately below regarding smaller scale exchanges.

l. 520: This work does not provide evidence for a seasonal cycle. It merely compares summer to spring measurements. There is no information on the fall and winter yet.- Agreed- this sentence was poorly phrased. It has been edited.

Technical remarks:
l. 128: Cloud condensation nuclei number... – This has been edited. Thank you.