Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1213-RC2, 2020

Atmospheric Chemistry and Physics
Discussions

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

Anonymous Referee #2

Received and published: 28 December 2020

Review of "Summer aerosol measurements over the East Antarctic seasonal ice zone" by Simmonds et al.

This study presents aerosol measurements from the PCAN field study performed on board the RV Investigator during summer 2017 in the seasonal ice zone of Antarctica south of Australia. Particle number concentrations with a diameter larger than 3 nm, as well as particle size distributions in the range from 8 to 507 nm and cloud condensation nuclei number concentrations at a supersaturation of 0.55 % were observed. The main objective of the work is to compare the summertime data against springtime data from a previous campaing, SIPEX-II, published by Humphries et al. (2016), where a marked step-change in concentration was found between the Ferrel and Polar cells. Here, the transition between cells is much smoother and data were hence separated

by wind direction and absolute humidity to infer differences. The authors find still larger concentrations in the Polar cell and speculate on reasons why that is and why there might be a difference towards springtime.

Overall, the measurements are well described and the analysis is straight forward. However, there are a number of points to be improved: The particle losses in the inlet are not described. It is also not clear whether the same inlet system was used in Humphries et al., which is important to know because this work is based on an intercomparison. In addition, the scientific motivation for this investigation is not well presented. It seems that the mere purpose of this study is to compare against Humphries et al. (2016), which is not much scientific merit per se. Better arguments for the relevance of this study need to be provided to make it publishable. Much more in depth discussion on the significance of the findings is needed. After reading the manuscript the question "so what?" is not answered. In addition, the data were analyzed selectively following a too narrow scientific scope, namely those data that fit the scheme of Ferrel and Polar cell. All data should be presented and discussed in the main text to provide a more comprehensive idea of aerosol in the so scarcely measured Southern Ocean. Below follow some comments, which might help the authors to strengthen their manuscript. I recommend a major revision before publication is considered.

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.

Introduction

l. 49: It is unclear which model parameters the authors refer to.

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 modeling and satellite observations. From this it is however not clear why this should researched further. This argument needs to be elaborated. I also recommend referring to recent publications on the topic, e.g. by Mc Coy et al. (2020) 10.1073/pnas.1922502117

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.

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...

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.

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.

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.

l. 120: How were size distributions measured? One expects this information here, where they are mentioned not further below.

l. 140: There is no mention of what Rn measurements were used for and why.

l. 145 ff: How were CO2 and black carbon measured? This information needs to be included in the paper.

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?

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.

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.

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.

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.

l. 241: Please provide the formula in the manuscript.

Fig. 3: The writing is too small.

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.

Fig. 5: Diameter axes are typically logarithmic.

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.

l. 333: Was "SO" introduced?

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.

l. 390: What are the vertical lines in figures S2b and S3b?

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.

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.

Technical remarks:

l. 128: Cloud condensation nuclei number...

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