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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "On the annual variability of Antarctic aerosol size distributions at Halley research station" *by* Thomas Lachlan-Cope et al.

Anonymous Referee #1

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The authors analyzed a unique data set on year-round particle size distribution (PSD) measured at the coastal Antarctic station Halley. They based their data evaluation on statistical cluster analysis, which has been applied as beneficial tool in several comparable investigations (References: Dall'Osto et al., 2019, 2018, and 2017). The manuscript at hand presents valuable, meaningful, and novel findings from a region where only very few studies on the variability of aerosol physical properties are available. Without doubt, the topic addresses the scientific scope of ACP, particularly considering the fact that aerosol-cloud interaction in the southern Ocean realm is still poorly understood leading to strong biases in climate modelling. Most notably in this context, PSD measurements from this region are qualified for assessing the potential of the aerosol to act as cloud condensation nuclei (CCN). Hence, I recommend a final

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Discussion paper

publication after some more or less basic revisions.

General issues:

(i) Presentation and discussion of the results are largely restricted to the “higher-level” output of the cluster calculations. Therefore, you should clearly substantiate the advantages and benefits of this method. The short section provided on page 6, lines 1 to 8 appears scarce. To be more specific (or even provocative): Two of the main conclusions drawn from this study and mentioned in the Abstract as point (1) and (2) (page 2, lines 22 to 28) can be easily derived without using any cluster analysis.

(ii) Moreover, from my point of view, it would be beneficial or even necessary to focus from case to case more on the original SMPS data, primarily when assigning air mass origins to NPF events. Here a more detailed discussion of air mass histories along with the original, individual PSD-spectra could be much more meaningful (the sketchily approach presented on page 14, lines 12 to 32 is hardly adequate). In case of “Nucleation” cluster: Do the individual PSD-spectra show particle growth in contrast to the spectra assigned to “Bursting”? Especially here, you may present some examples from the original data set to demonstrate the unique characteristic.

(iii) Air mass back trajectory analysis is a fundamental scaffolding of this study. The trajectory cluster analysis is interesting on its own but, however, somewhat detached from the PSD cluster analysis. I recommend presenting a figure analogous to Fig. SI 7 in the main text, but showing here trajectory ensembles sorted according to the PSD clusters as described on page 14, lines 12 to 32. Just another (minor) point concerning Fig. SI 7: The plot for cluster 1 (sea ice) shows terrain heights typically around 200 m or so, though the air masses travelled across the Weddell Sea (terrain height should be around zero!) – please check and clarify!

(iv) I recommend moving Figures SI 3 and SI 4 presented in the Supplementary Information (SI) to the main text, because they contain crucial information.

(v) Whenever possible, provide corresponding uncertainties or standard deviations of the results, especially for any values given in “%” (regarding text and figures).

(vi) The pivotal question you raise addresses the balance between secondary vs. primary aerosol in this region (see Abstract lines 8 to 11 and p. 16, lines 2 to 4). I suggest picking up this quest in your conclusions more explicitly. Finally: Do you have any suggestion for future research on this topic?

Some specific and minor points:

1. Abstract: Please concretely state here size range, temporal resolution and measuring period.

2. Page 4, line 29: . . .higher NPF instead of higher N.

3. Chapter 3.2: The association of PSD with meteorology, physical and chemical parameters appears rather descriptive. Do you have any ideas regarding the physical background of your findings?

4. Page 9, lines 17 to 19: Hijman (2019) and Becker (2018) are not listed in the references.

5. Page 14, line 19: Why did you relate to a total travel time of just 60 h and not 120 h (5 days back trajectories)?

6. Page 16, line 12: Please state “low particle number concentrations” more precisely.

7. Page 17, lines 5 to 16: I guess, during winter nss-sulphate aerosol should be negligible compared to sea salt. Maybe an additional closer look into the material presented in Rankin and Wolff (2003) or previous results on the chemical composition of the bulk aerosol from that site could be revealing, especially assessing the role of primary aerosol acting as CCN.

8. Page 18, line 16: Please state “baseline” more precisely.

9. Figure 4, caption, line 9: . . . during the year 2015 (not: during the year 365).

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