Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-847-RC2, 2019

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 #2

Received and published: 6 November 2019

The authors analyze particle size distribution data from coastal Antarctica using statistical methods to draw conclusions about aerosol sources and atmospheric processes. The results presented are both valuable and novel and are definitely within the scope of ACP. The context of the analysis and some of the actual discussion, especially as it relates to the existing literature, needs to be expanded but the necessary additions are minor with regards to the overall manuscript. Therefore, I recommend final publication with (generally) minor revisions.

Major comments:

Clustering analysis (especially S.2.2.1 and S.3.1.2) – The discussion on the clustering analysis needs to be greatly expanded as this is a fairly novel technique in atmospheric science. Many people fall into the trap of thinking that this machine learning method

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is actually machine intelligence and simply gives a “correct” answer as opposed to a mathematically valid solution. First, the values given for the Dunn Index and Silhouette Width need to be given context. Primarily, plots of both versus cluster number should be offered as many readers have no experience using or analyzing cluster analysis results. Secondarily, the values themselves need to be discussed in much greater detail. The 4x increase of the Dunn Index is good but 10ˆ-3 is still an extremely small value and implies that the clusters are extremely sparse (not compact), are not very far apart, or both. A graph of the cluster points to visually inspect both compactness and distance between clusters may be useful but may also be misleading as ambient data sets are often quite messy. Second, some additional validation of the cluster choice must also be presented. One way to do this may be to perform the analysis on heavily curated data to see if the results broadly match the overall analysis. I would highly (and very strongly) recommend that the authors run the analysis on a time frame where there are a minimum number of clusters expected (e.g. June). If there is broad agreement between the results there and the overall results, this would lend a great amount of strength to the overall conclusions. This could, and possibly should, be done in the context of air mass back trajectories as well where air masses could be broadly classified relative to their time spent over the continent, sea ice, or open ocean (this also comes with major caveats though, also see minor comment about back trajectory analysis).

S.2.3 – is missing?

S.4.1 – Much of the length of this section could be moved into the introduction and the remaining text expanded to give a more complete view of how these results fit into the existing literature. Overall, the authors do a fine job of finding relevant papers but do not necessarily discuss the conclusions presented completely. In particular, more discussion regarding measured composition and size distributions and the results presented here may be useful. The results of Rankin and Wolff (2003), Preunkert at al. (2007, 2008), Saiz-Lopez et al. (2007), Schmale et al. (2013), Giordano et al. (2017),

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and many others should likely be discussed in greater detail. Additionally, the presence and lack of photochemistry should be given some context as this is a fairly dominating factor in the polar regions' winter vs. summer months.

Diurnal profiles (Fig. SI 3 especially) – The basis of this analysis, especially considering the weight the figure is given in the text itself, needs to be better justified. Diurnal profiles are generally helpful in visualizing the impacts of either photochemistry or timed anthropogenic activities (or both). Neither of these cases apply to the Antarctic continent. Either the analysis should be rerun in a more nuanced approach (e.g. diurnals for periods of 24-hours of sunlight and lack thereof, only run in the short timeframes of clearly demarcated sunrise/sunset) and discussed in that context or should be removed completely. These results could be analyzed to give important insights into the potential role that the Polar sunrise/sunset plays in aerosol size distributions but this analysis may be beyond the scope of this manuscript.

Minor comments:

P.7, S2.2 – A few sentences about the transmission efficiency of the aerosol stack for relevant sizes of aerosols should be added. The authors could consider applying a correction to the size distributions to account for inlet losses but I imagine they are fairly small for the relevant sizes.

S.2.4 and 3.3 – A few sentences regarding the accuracy of HYSPLIT being used in regions of sparse meteorological measurements should be added. A more detailed description of the initialization conditions for the model should also be added.

S.3.3 – The conclusions discussed in this section should be moved to S.4 and discussed in the context of the existing literature.

S.4.2 – The conclusions should be separated from the discussion. The work presented here is worthwhile and the main points should not be hidden.

Overall – consistency in figure references, especially for SI figures, should be double

checked. E.g. Fig. SI 3 @ P.11 L11 vs Fig. SI3e @ P.11 L33).

Overall – references need to be double checked in both the main text and the references section.

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