

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2020-1246

Anonymous Referee #1

Referee comment on "Southern Ocean latitudinal gradients of cloud condensation nuclei" by Ruhi S. Humphries et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1246-RC1>, 2021

This paper by Humphries et al. "Southern Ocean latitudinal gradients of Cloud Condensation Nuclei" presents important new observations of CCN for the sparsely measured region and describes valuable datasets. The authors have prepared a clear, well written manuscript. Please consider expanding the discussion in a few places, where it can add value. I suggest publication with minor revisions.

Line 18: Long-term measurements at land-based research stations surrounding the Southern Ocean were found to be good representations at their respective latitudes. The long term stations should be regarded as the point for comparison with the much smaller ship-based dataset, e.g. "The ship-based measurements were in good agreement with the long-term measurements at their respective latitudes. The voyage data are only snapshots in time. This is acknowledged towards the end of the discussion (line 435).

Line 38 and following: "in-situ observations in this region rare, and until recently, only a handful of aerosol measurements" Agreed, and for completeness, it would be good to include reference to earlier work on latitudinal gradients of CCN, in particular Bigg, 1990.

Line 195 and following:

The authors have a thorough approach to treatment of engine exhaust contamination. Has the possibility of other lesser aerosol contamination been evaluated e.g. vent emissions from the cooking in the galley or from indoor air?

Line 315: also supported by the analyses of Vallina et al., 2006 In addition Fossum et al 2020 suggest that lower sea salt may also mean that a suppression mechanism for sulfate aerosol activation is removed at high latitude

Line 395 and following: "despite being very difficult to identify in variables by which the front is defined (i.e. meteorological)". This wording unclear, please be more specific to improve and clarify. Is this saying that there is not a sharp boundary in winds at the polar front (around 60°S between the polar easterlies in the Polar Cell to the south and westerlies to the north compared to the contrast observed in the Atmospheric Compositional Front term that you introduce? I question if this term adds significantly to the understanding or whether it is an additional way of defining the Polar atmospheric Front. I can see that the the Antarctic Sea Ice Atmospheric Compositional Zone (ASIACZ) will have unique properties and it is good to highlight this where low aerosol surface area air descending off the polar plateau in the Polar Cell aided with katabatic flow will meet the coastal marine air where there is significant production of biogenic gases and potential for new particle formation. I note the phenomenon was commented on in earlier work by Wylie et al., 1993 You also raise an important point that this area will not necessarily be well sampled by Antarctic coastal stations especially if they are sitting in areas dominated by surface outflows. Further, Bigg 1990 found a peak in CCN around 60°S which coincided with the northern limit of sea-ice with the suggestion that the a peak in surface aerosol precursors could coincide with the edge of the seasonal sea-ice.

Technical/Minor revisions:

Line 212: More precise to change "standard marine grade fuel oil" to "residual (heavy) fuel oil used by many vessels". <https://www.sciencedirect.com/topics/earth-and-planetary-sciences/bunker-fuel>

Line 251: Can a bit more explanation be given on significance of the trajectory kde bandwidth smoothing using Scott's factor?

Line 282: Change "southern ocean" to "Southern Ocean"

Line 299 and 389: Re "step change - a trend in concentrations is apparent" but Fig 2, A2, A3 do not seem to suggest a "step change" With Ref to Fig 2, it appears that the largest difference is in CCN05. compared to other variables. Can comment be made on this? Further, the step change is most apparent in CCNx/CN ratio in Fig 3. It is useful to discuss this finding against the large scale "averaged CCN04/CN ratio of ~0.4 reported by Andreae 2009 across a range of environments, indicating an extreme in ratio with polar air.

Line300 "in the CCN/CN ratios (Figures 2, A3, A4, A5, A6)," These seem to be presented in Figures 3, A6 and A7)

Line 400: Change "is not captured" to "are not captured"

Line 412: change "motivations" to "motivation"

Figure A7: Rightmost label on abscissa is missing. This figure is both MARCUS and CAPRICORN2. Why is the 65-70 category as shown in Figure 2 not included?

Refs:

Andreae, M.O. (2009) Correlation between cloud condensation nuclei concentration and aerosol optical thickness in remote and polluted regions. *Atmos. Chem. Phys.*, 9(2): 543-556. [10.5194/acp-9-543-2009](https://doi.org/10.5194/acp-9-543-2009)

Bigg, E.K. (1990) Aerosol over the Southern Ocean. *Atmospheric Research*, 25(6): 583-600. [https://doi.org/10.1016/0169-8095\(90\)90039-F](https://doi.org/10.1016/0169-8095(90)90039-F)

Fossum, K.N., Ovadnevaite, J., Ceburnis, D., Preißler, J., Snider, J.R., Huang, R.-J., Zuend, A., O'Dowd, C. (2020) Sea-spray regulates sulfate cloud droplet activation over oceans. *npj Climate and Atmospheric Science*, 3(1): 14. [10.1038/s41612-020-0116-2](https://doi.org/10.1038/s41612-020-0116-2)

Vallina, S.M., Simó, R., Gassó, S. (2006) What controls CCN seasonality in the Southern Ocean? A statistical analysis based on satellite-derived chlorophyll and CCN and model-estimated OH radical and rainfall. *Global Biogeochemical Cycles*, 20(1): GB1014. <http://dx.doi.org/10.1029/2005GB002597>

Wylie, D.J., Harvey, M.J., de Mora, S.J., Boyd, I.S., Liley, J.B. (1993) Dimethylsulfide and aerosol measurements at Ross Island, Antarctica. In: G. Restelli & G. Angeletti (Eds). *Dimethylsulphide, Oceans, Atmosphere and Climate*. Kluwer Academic, Dordrecht: 85-94.