Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1081-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Marine productivity and synoptic meteorology drive summer-time variability in Southern Ocean aerosols" *by* Joel Alroe et al.

Anonymous Referee #2

Received and published: 24 January 2020

The paper by Alroe et al. reports the result of a Southern Ocean cruise and marine aerosol features from the underexplored region. The paper is fluently written and was a pleasure to read, but unfortunately contains little scientific advancement beyond the current state of knowledge. The results broadly agree very well with the already published papers in the topical area, but I was wondering why the authors did not come up with unique insights possessing a good dataset and all the relevant instruments. Was the study not from the Southern Ocean and without thorough trajectory and ocean colour analysis I would have difficulty recommending it for publication, but I would like to encourage the authors to take a second look at their results and try to enhance the value of the paper by highlighting certain novel aspects. I hope my comments will

motivate the authors and may help in improving the paper. For example, were there any oceanographic data available like in-situ chlorophyll measurements to infer phytoplankton blooms or more general tracer measurements by e.g. fluorometer? Last, but not least properly fitted size distributions may help to deepen the discussion of particle sources.

Key comments

The authors used a sea salt tracer to estimate sea salt mass, but they could also cross-check with SSA estimated mass and number from recent of earlier sea salt source functions at the observed wind speed. Something more can be done here if proper lab calibrations were not performed for ACSM which would have been very useful given instrumental differences between high resolution AMS used by Ovadnevaite et al. and ToF ACSM used in this study.

It is important to realise that the observed particles and their size distributions are a product of cloud cycling. Or in other words, the observed size distribution is a product, not the source for cloud processing. Indeed, all accumulation mode particles can undergo subsequent cloud cycles, but Aitken mode cannot given their modal diameter of 30nm. Bi-modal size distribution has exactly arisen after cloud cycling. If there were no clouds accumulation mode chemical composition would have been entirely made of primary sea salt. As was noted at the beginning properly fitted size distributions (Comments regarding Figure 3) and the above comments may enhance the discussion.

The authors must redo the fitting of log-normal distributions. The shape of "bi-modal" distribution is clearly suggesting more modes, e.g. nucleation mode of $\sim$10nm and second accumulation mode centred at $\sim$420nm. I do not see symmetrical log-normal modes, only one side of them which makes me wondering what the inventive fitting was applied. Specifically, significant departure of log-normal mode from the observed size distribution is suggesting additional mode(s) together with the potentially excessive geometric spread (sigma) above $\sim$1.5. Consequently, nucleation mode is suggesting

contribution from new particle formation at the coast or open ocean depending on the trajectories. But first, please do the proper fitting.

Minor comments

Page 4. Line 8. I could not find in this paper nor the referenced paper information about the sampling inlet. Was it community sampling duct of certain dimensions with individual instruments sub-sampling from it? What was the total flow and laminar conditions? Was it experiencing a significant drying during air passage through it?

Page 4, line 24. Was the drier used for ACSM to limit the excessive humidity?

Page 6, line 13. BC threshold was set conservatively in general, however, given pristine nature of the Southern Ocean may have been set even lower, especially as it was an hourly average. Also while number concentration criterion is appropriate, number concentration is not a great arbiter when new particle formation events may increase N10 concentration substantially. It would very useful to present BC data in Figure 2.

Page 6, line 28. I am confused with this statement as sea spray usually contributes significant number fraction to submicron aerosol based on several established sea spray source functions. Possibly the authors were meant to say that the supermicron sea spray mode contributed a small number fraction to the observed size distributions which would be true.

Page 7, line 28. Please provide ranges of temperature and wind speed.

Page 8, line 31. Notation is confusing with that of sulfur species. Perhaps is better using upper script notation or mSO-first, mSO-second and so on.

Page 9, line 5. Why mSO4 is preceding mSO3 period and mSO3 preceding mSO2?

Page 10, line 19. There is no mention nor reason why CCN not reported for mSO periods.

Page 11, line 29. The only Antarctic terrestrial sources of BC are the scientific bases.

Page 12, line 21. This conclusion must be supported by ammonium concentration and degree of neutralisation (DON). There is no mention nor reason of the absence of ammonium concentration. ACSM is perfectly capable of measuring ammonium ion.

Figure 2. BC concentration would be extremely useful on a separate scale of radon graph.

Table 1. Air mass notations should be spelt below the Table or in the caption. NH4 is an important species discriminating between different neutralisation degrees depending on continental impact. DON could be presented too. DL should be noted below Table or simply like <0.XX

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