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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement report: Cloud Processes and the Transport of Biological Emissions Regulate Southern Ocean Particle and Cloud Condensation Nuclei Concentrations" *by* Kevin J. Sanchez et al.

Anonymous Referee #1

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The manuscript analyzes measurements of particle number concentrations (CN) and cloud condensation nuclei (CCN) obtained by airborne measurements during SOCRATES and by ship-borne observations during CAPRICORN-2 in the Australian sector of the Southern Ocean. The study comprehensively shows the effects of cloud processing, precipitation and air mass origin on particle size and number, and on CCN. To this end the authors combine direct observations and re-analyses data. They also show nicely that in most cases new particles are formed in the free troposphere and not in the marine boundary layer.. These measurements make an important contribution

to our understanding of CN and CCN processes in the Southern Ocean. I recommend that the paper be published, however, only after considering below points, which I believe will help improve the study. Also, if possible, I recommend that this manuscript be published as a normal research article rather than a measurement report. The depth of analysis is not untypical of that in research articles.

General comments:

A map with the cruise and flights tracks is needed.

The discussion of NPF in the Southern Ocean (SO) boundary layer is partly incorrect, because it is said that the condensation sink (CS) is low. See specific comments in the attachment and also further below.

Some statements about the Southern Ocean are too general, e.g., the claim that microbial activity is low compared to the Antarctic coastal region. There are hotspots, like South Georgia, and if measurements had been taken in that region of the Southern Ocean the paper would report different observations. Hence acknowledging the regional variability of the SO is very important. Otherwise incorrect messages about this large region are published. This comment is also true for the conclusions. See comments in the attached.

The introduction jumps between topics, particularly ll. 92 – 110. The main message is not clear. I suggest to structure this part of the introduction as follows: observations of NPF, CN and CCN near the Antarctic coast, observations of NPF, CN and CCN over the open southern ocean (and not only between Australia and Antarctica), then discuss how the coastal and open ocean regions are connected, then go deeper into cloud processing.

Some more details in the methodology section are needed, in particular regarding the mini CCNCs and the calculation of kappa. Also the inlet system and position of the CCNC on R/V Investigator is not described. In addition, it is unclear why the CCN data were not compared at the same supersaturation. It is possible to interpolate from the spectrum. See attachment for more specific comments.

The calculation of back trajectories is not well described. If it is really the case that only one location per leg was used, the results will be highly uncertain. Some more clarification is needed, particularly a better description of flight legs.

Specific comments:

l. 193: A quantification or at least better approximation of the RH in the sample flow is needed to make this study comparable to previous and future studies.

l. 202: the description of the fraction of PMA to particles > 0.2 μm is inconsistent (see attached comments).

l. 229: An explanation of how the Aitken mode was derived is missing. The UHSAS was only used for particles greater 70 nm, so cannot have been used for that purpose. Was the CPC data used? If yes, how were CPC and UHSAS intercompared?

Section 3.3 on cloud processing relies strongly on ERA 5 data. Some discussion on the representation of clouds and particularly precipitation in the reanalysis product is needed. Over the SO there are not many observations that would constrain the reanalysis.

Section 3.4 Latitudinal Gradient: Recent observations by Schmale et al. (2019) also highlight the higher concentrations of CCN near the Antarctic coast. See also their discussion of kappa for MSA and the role of particle size to activate as CCN. I recommend referring to their work in section 3.4, since they already came to similar conclusions presented in section 3.4.

l. 346: Do the authors means the low variability +/- 0.04? Why would a wet diameter lead to a lower variability?

In section 3.5 PMA Marine Aerosol, again it would be useful to put the results into

context with recent publication from other sectors of the SO. Schmale et al. (2019) show in their table 3 the contribution of their similarly identified sea spray mode to CCN and find between 20 and 30 % for SS = 0.15 %. l. 363 The low condensation sink (CS) is not really true because the presence of sea spray leads to such a high condensation sink that new particle formation in the marine boundary layer is rather an exception (also due to other factors). Compared to the Arctic Ocean (Baccarini et al. (2020), https://doi.org/10.1038/s41467-020-18551-0), the CS in the SO will be a factor four, or even more, higher. The authors have the necessary data to actually calculate the CS. Compared to other oceans (except polar oceans) the CS might be lower, but given the low new particle formation occurrence, saying low CS is not completely correct.

l. 366, which trend?

Section 3.6, please provide the number of data points per vertical profile. It is difficult to understand how representative the six profiles from figure 7 are and why particularly those were chosen. How many were there? How was the histogram in Fig. 9 calculated, is there one ratio per profile? L. 400: The explanation of long-range transported CCN from the Antarctic coast is in contradiction to the minimum near 60°S. If the higher concentrations near the coast of Australia are due to specific long-range transport events, this should be said explicitly.

l. 392 f: The information on the four regimes is repeated in l. 404ff. Consider removing some redundancy from the conclusions.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-731/acp-2020-731-RC3-supplement.pdf

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