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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 #3

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The manuscript by Sanchez et al. "Measurement report: Cloud processes and the transport of biological emissions regulate Southern Ocean particle and cloud condensation nuclei concentration" reports on particle and cloud condensation nuclei measurements around the Southern Ocean during the SOCRATES and the CAPRICORN-2 campaigns. The measurements from SOCRATES were interpreted as a combination of high and low CN and CCN concentrations (four combinations) and linked to back trajectories and fitted with a course-mode fitting procedure to isolate the PMA contribution to CCN and CDNC. In parallel CCN measurements were conducted on-board a

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research vessel (during CAPRICORN-2). Overall the measurements are very interesting and novel and should be published as a measurement report. I do however find that the manuscript needs improvement in several aspects. In particular I miss information about measurement methods.

General comments

1. Title: I find that the title should be revised. I think that the use of the word *regulates* is too strong and not justified.

2. CCN measurements
(a) Is the miniature CCNc (mCCNc) used and described in more details in literature? Or is it a miniature custom-made version of the one described in Roberts and Nenes, 2005?
(b) Details on the supersaturation calibration procedures is missing (both the mCCNc and DMT CCNc). Could the authors elaborate on this (in the manuscript or in data repository) and give details on how they estimate the instruments supersaturation? (e.g. assumed water activity? type of calibration aerosol? drying conditions? uncertainty related to reported supersaturation?). I find it valuable and useful that the data is available online. Nice that you estimate the uncertainty in the CCN number concentration.
(c) What caused the scanning mode mCCNc instrument to have problems during RF01-05? How is this reflected in the uncertainty?
(d) How dry was the air before entering the CCN counters?

3. Hygroscopicity Another major concern is related to the κ parameter and hygroscopicity. Could the authors elaborate on how the κ_{70} was derived from scanning CCN spectra and UHSAS number size distributions? Did the authors estimate an κ uncertainty?

Specific comments

L63: Could iodine new particle formation be a source in the SO? e.g. see the recent work of Baccarini et al. (2020) and Sipila et al. (2016).

L101-103: "some studies suggest biologically productive waters enhance PMA production". Some studies show this relationship is not that simple. I suggest modifying the text to also reflect on this. See e.g. the work of Collins et al. (2016), Bates et al. (2020)

L108-110: "... can further reduce the hygroscopicity". A couple of studies also show that the hygroscopicity is not changing that much. I suggest the text to also reflect on this, e.g. see the recent work by Bates et al. (2020), Christiansen et al. (2020).

L170-172: Some details on aerosol inlet, sampling, calibration, data analysis or a reference to a paper/data repository giving these details is missing.

L193: "UHSAS particles in the SOCRATES campaign were not fully dried". Could the author explain how this would affect the derived hygroscopicity parameter κ?

L200-204: A bit confused here: "The TEM analysis shows almost all particles $>$ 0.2 μm in diameter consist of sea salt; and sea salt particles account for 25% to 100% of particle number concentrations at particle diameters $>$ 0.4 μm (Saliba et al. submitted)." How can almost all $>$ 0.2 μm be sea salt, but not particles $>$ 0.4 μm? How much is *almost all*? What kind of diameter is this (geometric, optical, mobility)? In addition, the title of the **Saliba et al. submitted** states "...in the North Atlantic". Was

SOCRATES field campaign also in the North Atlantic?

L212: Kernel density estimates. Interesting approach. Did you try k-means clustering analysis? If yes, how do they compare?

L232: The **Aged regime**: in addition, could aging be caused by atmospheric oxidants (OH ect.), e.g. changing chemical composition due to chemical reactions?

L265: Sanchez et al., 2017. Still in discussion for ACP? Wrong citation.

L280: FT not defined I think.

L317: Would prefer to use **lower** κ instead of smaller κ.

L319-320: "... relative to sea salt (κ = 1.3)". It has become clear that the hygroscopicity of NaCl and sea salt are not the same (Zieger et al., 2017; Christiansen et al., 2020) and that the κ value of sea salt $\sim$ 1 rather than 1.3. On a side note, Bates et al. (2020) writes about the CCN results in Quinn et al. (2014) that "It is critical to thoroughly dry the aerosol before it enters the CCN instrument. The SSA was not sufficiently dry during the WACSâĂŘ1 measurements".

L333: "...phytoplankton emissions...". To help the reader, please be more specific here on what type of emission.

L347-348: "The calculated PMA number concentrations moderately correlated to wind speed (r = 0.53, Figure 6a)". This is interesting. Why did you use a linear fit here?

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L447-448: I can not find the processed CCN data for CAPRICORN-2 / *RV Investigator* through the provided link.

References:

Baccarini, A., Karlsson, L., Dommen, J. et al. Frequent new particle formation over the high Arctic pack ice by enhanced iodine emissions. Nat Commun 11, 4924 (2020). https://doi.org/10.1038/s41467-020-18551-0

Sipilä, M., Sarnela, N., Jokinen, T. et al. Molecular-scale evidence of aerosol particle formation via sequential addition of HIO3. Nature 537, 532–534 (2016). https://doi.org/10.1038/nature19314

Bates, T. S., Quinn, P. K., Coffman, D. J., Johnson, J. E., Upchurch, L., Saliba, G., ... Behrenfeld, M. J. (2020). Variability in Marine Plankton Ecosystems Are Not Observed in Freshly Emitted Sea Spray Aerosol Over the North Atlantic Ocean. Geophysical Research Letters, 47(1). https://doi.org/10.1029/2019GL085938

Christiansen, S., Ickes, L., Bulatovic, I., Leck, C., Murray, B. J., Bertram, A. K., ... Bilde, M. (2020). Influence of Arctic microlayers and algal cultures on sea spray hygroscopicity and the possible implications for mixedâĂŘphase clouds. Journal of Geophysical Research: Atmospheres, 125(19). https://doi.org/10.1029/2020jd032808

Collins, D. B., Bertram, T. H., Sultana, C. M., Lee, C., Axson, J. L., Prather, K. A. (2016). Phytoplankton blooms weakly influence the cloud forming ability of sea spray aerosol. Geophysical Research Letters, 43(18), 9975–9983. https://doi.org/10.1002/2016GL069922

Zieger, P., Väisänen, O., Corbin, J. C., Partridge, D. G., Bastelberger, S., Mousavi-Fard, M., . . . Salter, M. E. (2017). Revising the hygroscopicity of inorganic sea salt particles. Nature Communications, 8, ncomms15883. https://doi.org/10.1038/ncomms15883

Technical corrections

Overall, there are quite many mistakes in the reference list. This gives the reader a bad impression. The authors should solve this issue.

L452: Remove uppercase “AEROSOLS, CLOUD MICROPHYSICS, AND FRACTIONAL CLOUDINESS”

L463: Replace + with page number 363. “359-+,”

L467: What journal? Only says “aerosols, , (November), 1–27”. No DOI.

L470: Remove uppercase “SEASONAL RELATIONSHIP BETWEEN CLOUD CONDENSATION NUCLEI AND AEROSOL METHANESULFONATE IN MARINE AIR”

L495: Fix “67-+,”

L517: Remove uppercase “NEW PARTICLE FORMATION IN THE MARINE BOUNDARY-LAYER,”

L634: Is the * supposed to be there? “...Low Cloud Feedback*”

L670: ?? “Available from: %3CGo,”

L674: Make lowercase

L681: Make lowercase

L701: Remove symbol

L735: Who is “O'”. Maybe cite the final paper.

L742: Make lowercase

L757: No title??? What journal?

L801: Make lowercase

L808: Make lowercase

Table 1: $\pm$ what?

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