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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CCN measurements at the Princess Elisabeth Antarctica Research Station during three austral summers" *by* Paul Herenz et al.

Anonymous Referee #1

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Review of "CCN measurements at the Princess Elisabeth Antarctica Research Station during three austral summers" by Paul Herenz et al.

This study describes the aerosol particle number concentration (CN) and cloud condensation nuclei number concentrations (N_CCN) at various supersaturations at the East Antarctic research station Princess Elisabeth. Also, limited particle number size distribution data, aethalometer and meteorological data are available. The authors focus on the austral summer measurements between 2013 and 2016. They analyze CN and CCN data additionally with two methods: (a) a particle dispersion model to quantify the fractional contribution of air masses from certain source regions that the authors

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define, and (b) with a Hysplit based potential source contribution function model to illustrate the potential source regions for the highest 25 % occurrences of CN and N_CCN. The study finds that N_CCN can vary by 2 orders of magnitude reaching up to 1300 cm-3 for supersaturations (SS) between 0.1 and 0.7%. The Aitken mode is the dominant particle mode and 94 % of the particles are < 90 nm. Most air masses reside relatively long over the ice sheet before reaching the station, while a minor fraction arrives more directly from the Southern Ocean. The latter types of air masses bring higher CN and N_CCN concentrations. The contributions to the hygroscopicity parameter kappa, which ranges widely, are discussed, but no conclusion is derived.

There is no doubt that this data set needs to be published, because it furthers our understanding of CN and N_CCN in eastern Antarctica, a region that is heavily undersampled. The manuscript is clearly structured and generally well written. Before the manuscript can be published, however, it will need to undergo major revisions which are pointed out in the following with subsequent more specific comments. Technical and some specific edits can be found in the attachment.

First, it is unclear why the authors use the NAME dispersion model to create the footprints with the regional classification and the PSCF based on Hysplit for the potential source contributions. These are two different models used for similar purposes. There is no discussion in how far the models produce similar or deviating results, especially in light of the presumably different meteorology (GDAS vs UM meteorological field data). Either, option a), there needs to be a very clear explanation why two different models are used, including a comparison between them to show that the results are comparable, or, option b), either the footprint analysis needs to be done with Hysplit or the PSCF needs to be done with NAME. This might require slight modifications of how the models are run.

Second, the regional classification does not make sense and is misleading. The authors imply that the “Reactive Zone” is the largest source of particles and that ammonium and organic nitrogen might be an important contribution to particle mass. Both

aspects are likely wrong:

a) The larger source of particles will be the open ocean through sea spray production (Quinn et al., 2017), because this area is much larger than the “Reactive Zone”. Generally the particles are acidic and not neutralized from ammonia emissions. Those have a limited regional effect.

b) The animal colonies are an important source of ammonia and lead to secondary particle mass enhancement. But (a) the source is not that important (Riddick et al., 2016; Riddick et al., 2012) compared to the Southern Ocean sea spray emissions, and (b) the authors do not show whether the ammonia emissions are responsible for the increase in CN and N_CCN at PE because they don’t have chemical composition data and the kappa value discussion is too vague. Also, ammonia emissions do not necessarily lead to an increase in particle number concentration. They typically lead to the increase of particle mass. This has been shown by e.g., Schmale et al. (2013). By the way, it is unclear why the authors use a references from an Arctic study (Croft et al. 2016) when there is a study on ammonia emissions from sea birds and seals in the Southern Ocean (Schmale et al., 2013).

c) Including the permanently and seasonally covered sea ice areas into the “Reactive Zone” for the reasons given, i.e. organic nitrogen and secondary particle formation, is highly misleading. The contribution of organic nitrogen as found by Dall’Osto et al. 2017 is miniscule. The finding is interesting but the relevance for N_CCN is far from being understood and not large. Also, the authors consider the sea ice zone as static, without taking into account the year to year variability. A proper analysis of the influence of the sea ice region requires using satellite images of the sea ice and marginal ice zone borders for each of the seasons considered in this work. Again, conclusions can be misleading.

d) The coastal areas around the continents and islands are another component of the “Reactive Zone”. The reason the authors give is the enhanced chlorophyll concentra-

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tions. Here again, it does not make sense to statically classify an area as chlorophyll-rich throughout the considered seasons. In addition to the fact that blooms will not be active to the same degree throughout the whole summer season and every season and year, there are plenty of other regions in the Southern Ocean where enhanced chlorophyll is typically observed (Valente et al., 2016). Those regions should be included as well. Again, satellite images of chlorophyll-a concentrations for the respective periods would have to be used to come to a trustworthy conclusion.

e) The term “Reactive Zone” is uninformative. The authors mainly refer to regions with enhanced microbial productivity, hence calling it “productive zones” is more appropriate. Furthermore, in the entire manuscript phytoplankton is not mentioned once. The reason for chl-a detection and VOC emissions that can be converted into particles (e.g. DMS to MSA) is the presence of phytoplankton. This link needs to be included explicitly in the manuscript to describe particle sources in the Southern Ocean and Antarctica.

Specific comments:

PSCF: In how far is the altitude of air trajectories included in the PSCF? An air mass that travels aloft, will likely not be influenced by surface emissions, hence the PSCF result can be misleading if any trajectory altitude is considered. This requires clarification and potentially recalculation. Also, on p. 8, l. 24 the authors state that the criteria are empirical without clarifying where the empirical evidences comes from.

NAME and PSCF: It seems that mostly surface sources of CN and N_CCN have been considered. Over the Southern Ocean and Antarctica it is assumed that a significant fraction of particles comes from particle formation in the free troposphere e.g., in the outflow of clouds (Quinn et al., 2017 and references therein). This is not considered at all, but needs to be included into a comprehensive discussion of particle sources.

Growth Rate (GR) discussion: On p. 11, the growth rate discussion seems incomplete. There is no connection to the observation discussed in the published literature (Aboa, Dome C) and the observations at PE (no GR was determined at PE). Furthermore,

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the reader is left puzzled by the statement that the GR was higher at Dome C than at coastal sites. An explanation or at least discussion of this counter-intuitive observation needs to be included. Kappa: There is no explicit discussion of the sources of uncertainty for the derivation of the hygroscopicity parameter kappa (p. 12, l. 32 f), just the reference to the Monte Carlo method. More details are necessary.

Origin of particles > 110 nm: The authors list 2 options (p. 13, l. 21ff): NPF and subsequent growth, and sea spray / sea salt particles emitted over the sea ice region. Based on the previously cited literature on GR (1 – 2.5 nm/h) a sustained growth over a period of 40 to > 100 hours would be needed which is unlikely given the lack of condensable gases. Why is there no discussion about cloud-processed particles and emissions from the open ocean? Those are two mechanisms likely to produce accumulation mode particles. Those mechanisms should also be considered in the discussion on p. 12, l. 1-2.

Size of particles as function of transport time: This argument brought forward on p. 12, l. 2ff is not convincing keeping in mind the comment above. Particle size can be a function of the emission source and the cloud-processing as well. I recommend plotting the particle size vs transport time to back up this statement.

Figure 3: The purpose of the figure is unclear. It does not contribute any relevant information.

References:

-Quinn, P. K., Coffman, D. J., Johnson, J. E., Upchurch, L. M., and Bates, T. S.: Small fraction of marine cloud condensation nuclei made up of sea spray aerosol, Nature Geosci, 10, 674-679, 2017.

-Riddick, S. N., Blackall, T. D., Dragosits, U., Daunt, F., Newell, M., Braban, C. F., Tang, Y. S., Schmale, J., Hill, P. W., Wanless, S., Trathan, P., and Sutton, M. A.: Measurement of ammonia emissions from temperate and sub-polar seabird colonies, Atmos.

Environ., 134, 40-50, 2016.

-Riddick, S. N., Dragosits, U., Blackall, T. D., Daunt, F., Wanless, S., and Sutton, M. A.: The global distribution of ammonia emissions from seabird colonies, Atmos. Environ., 55, 319-327, 2012.

-Schmale, J., Schneider, J., Nemitz, E., Tang, Y. S., Dragosits, U., Blackall, T. D., Trathan, P. N., Phillips, G. J., Sutton, M., and Braban, C. F.: Sub-Antarctic marine aerosol: dominant contributions from biogenic sources, Atmos. Chem. Phys., 13, 8669-8694, 2013.

-Valente, A., Sathyendranath, S., Brotas, V., Groom, S., Grant, M., Taberner, M., Antoine, D., Arnone, R., Balch, W. M., Barker, K., Barlow, R., Bélanger, S., Berthon, J. F., Beşiktepe, Ş., Brando, V., Canuti, E., Chavez, F., Claustre, H., Crout, R., Frouin, R., García-Soto, C., Gibb, S. W., Gould, R., Hooker, S., Kahru, M., Klein, H., Kratzer, S., Loisel, H., McKee, D., Mitchell, B. G., Moisan, T., Muller-Karger, F., O'Dowd, L., Ondrusek, M., Poulton, A. J., Repecaud, M., Smyth, T., Sosik, H. M., Twardowski, M., Voss, K., Werdell, J., Wernand, M., and Zibordi, G.: A compilation of global bio-optical in situ data for ocean-colour satellite applications, Earth Syst. Sci. Data, 8, 235-252, 2016.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-245/acp-2018-245-RC2-supplement.pdf

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