

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.

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General comments Sanchez et al. (Cloud Processes and the Transport of Biological Emissions Regulate Southern Ocean Particle and Cloud Condensation Nuclei Concentrations) provides a very good summary of airborne and marine boundary layer aerosol and CCN measurements over the Southern Ocean. These are extremely valuable measurements and provide excellent information on aerosol sources, atmospheric processing and the resulting CCN/CDNC. I have outlined some minor changes to the manuscript. A little more detail on the calculation and interpretation of the hygroscop-

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icity, and on the uncertainties would improve the clarity of the manuscript. Specific comments are outlined below.

Specific comments

L 41: What are the conclusions regarding the latitudinal gradient in particle composition and hygroscopicity based on? Are there measurements for this?

L 41: Isn't there biogenic/coastal emissions in the lower latitude Southern Ocean, and therefore lower sea salt fraction?

L 54: Which particle sources?

L 84: Alroe et al. doesn't show such dramatic increase in number concentrations, and was a summertime voyage.

L 92: This paragraph could be simplified. The discussion of PMA (e.g. from "In a recent study.." onward) could be separated out from the discussion of seasonality.

L 93: Biological particles is an unclear term, perhaps something like secondary particles from biological sources. Biogenic particles is used on L95.

L123: The sentence beginning "In contrast, the inclusion..." would be better placed in a paragraph about PMA.

L125: The sentence beginning "SO satellite-derived....", begins the discussion of cloud properties and could be the start of a new paragraph.

L120: Which sensitivity? McCoy et al. doesn't discuss aerosol growth via cloud processing.

L149: Do we have any idea to what RH the sample was dried?

L159: Some detail on how kappa was calculated is required. I assume the supersaturation at which the $CCN_c = UHSAS \# \text{ conc } (>0.07\mu\text{m})$ was taken as the Scrit. Then the Scrit/Dcrit were used to compute the kappa. Although I'm not sure how it was com-

puted, I don't it was measured at 0.07 μm (i.e. pre-selected). Therefore, perhaps the expression "the 0.07 μm diameter hygroscopicity parameter (κ_{70})" is a bit misleading.

L195: How do the authors know that the fitted PMA distribution isn't a subset of the total PMA? Particularly the deliquesced mode at $\sim 0.6 \mu\text{m}$, since the GSD ~ 1.44 is on the lower end of that observed by Saliba et al. 2019. Does the sensitivity testing that was done in Saliba et al. 2019 necessarily transfer across to these data?

L199: What are the mode (and sd) for the fitted PMA distribution? and how do they vary? Only N is reported here.

L208/Fig 1b: What is the uncertainty/variability in these averaged CN/CCN? Error bars would be helpful.

L228: "Aitken-mode particles (CN)" is unclear and suggests CN excludes the accumulation mode.

L249: "(RPF + Aged and RPF + Scavenged)"

L256: What was the rationale for taking the 90th percentile? and how was this implemented? the 90th percentile for each vertical profile?

L256/Fig 3c: It would be good to represent the variability CDNC, and how this variability influences the relationship between CCN and CDNC.

Fig 3: Error bars are required in Figure 3b and c.

L266: The sentence starting "The two regimes.." should be revised. The figure shows that the CDNC for aged is greater than CDNC for scavenged, there is too little data to draw conclusions for the RPF cases.

L310: Does removing these transported outliers result in statistical significance in the trend? to what p-value?

Fig 5: Could error bars be added to the CCN, UHSAS and kappa?

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L315: It is not clear how the CCN and CN (>0.07 μm) concentrations by themselves imply anything about composition/hygroscopicity.

L319: The computed kappa values are lower than that expected for sulfates, particularly at high SO latitudes. At lower latitudes the kappa values are consistent with sulfates. This should be noted/discussed.

L332: Seawater biogenics (e.g. chl-a) increase in the low latitude SO, north of the sub-antarctic front. This isn't necessarily associated with the Aus continent. McCoy et al. 2015 pointed out that secondary aerosols played a more important role in CCN/CDNC in the lower latitude SO, with the contribution from PMA increasing with latitude up to 60S.

L346: In general the variability using this method is quite high. How does the variability compare with that in Saliba et al.

L414/L41: The hygroscopicity values are low and aren't necessarily consistent with sea salt in the north and sulfates (+organics) in the south.

Technical questions

L29: "coarse-mode" not "course-mode"

L101: "though" is not necessary

L235: RPF regimes exhibit

L370: I think it should be "< 0.7" not "> 0.7"

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