

Interactive comment on “Sea spray aerosol organic enrichment, water uptake and surface tension effects” by Luke T. Cravigan et al.

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This document intends to provide information about sea salt aerosol sourced from the southern hemisphere. Justification for the study is given as a lack of southern hemisphere measurements and underestimation of low-level cloud cover. The measurements come from a 23-day ship voyage off the coast of New Zealand. Several chemical speciation measurements were taken along with VH-TDMA (water) and UFO-TDMA (ethanol) measurements. The author uses statistical analysis of the many variables to survey for correlations. Some of those correlations do not have legitimate causation. The document ends by trying to resolve the issues using OCEANFILMS (vs ZSR). The amount of work is significant and clearly represents measurements from the southern hemisphere. Some changes should be made prior to full publication.

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Page 3: Line 16: Does the sea salt samples (from manufactured sources) not create salt hydrates?

Page 3: Line 18: OVF not defined.

Page 4: Line 14: HGF not defined

Page 5: Line 8: beta 660 backscatter, pCO₂, and DMS_{sw} not previously defined.

Page 5: Line 10: DMS not previously defined

Page 7: Line 4: “Subsequent to heating the SSA was exposed to 90% RH and the hygroscopic growth factor was measured.” Please insert a comma or adjust to better display the subordinate clause.

Page 7 Line 4: RH not previously defined.

Page8: DOC not previously defined

Page10: Were the inverted volatility scans used as inputs to the Gysel inversion routine to calculate growth factor as insinuated by equation 4? After performing volatility, the particles shrink some. How is this shrink, prior to hygroscopic growth, represented using the Gysel inversion?

Page 10: Line 14 and 15: recompose sentence to read that sulfate mass was calculated from S, not all inorganics from S measurements.

Page 11: Why would a salt hydrate have a growth factor of 1?

Page 13: I am assuming the number of size distribution modes correlates with the four sintered glass filters. Is this true? If not, please dispel the misconception.

Page 14: Although this may be a little over critical, the natural sea water normalized concentration is missing 1% in Table 1.

Page 14: Line 17 and 18 and Figure 5: how do we know that the non-volatiles (OVFNV) are organic? If you have a proxy for total organic mass and a proxy for semi-volatile

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mass, wouldn't the involatile be the difference between the two using assumptions for density?

Page 15: The hygroscopic growth measurements are based on number population (as described in Section 3.4). The volume fraction (used in volatility) is based on both number and diameter. (unless everything is singly charged, the two numbers do not correlate). FYI, 80% of the population is singly charged for this situation. The averaged sampled population from mode 3 is 17% by number and 27% by volume. See table below. These calculations are based on three items: the non-diffusing DMA transfer function (Stolzenburg and McMurry 2008) and your reported DMA 1 settings, the charging fraction as defined by (Wiedensohler 1988), and the reported size distributions in Table 1. In the numbers below, I have multiplied the normalized population numbers in Table 1 by 100,000 for clarity.

In the tables below, columns represent the size distribution modes defined by Table 1. The rows represent the size distributions selected by DMA1 (e.g. +1 is the singly charged size distribution). The percentages are of the total. The first table is by number and the second table is by volume.

Unfortunately, I am unable to place the tables here in this document. Please see supplement for theoretical tables.

Page 16: Feel free to use the numbers from the tables to try to resolve any issues in error in volume fraction. I should note that the numbers above are based on your published average settings and will not be representative of an individual scan.

Page 18-Figure 7 caption: "Stars in bottom right plot represent the mean EF from TEM-EDS measurements of SSA generated from laboratory seawater, dotted error bars show standard deviation in the mean." – I do not see any stars in the panel.

Page 19: OM not previously defined.

Page 19 line 5: tot should be to

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Page 19 bottom paragraph: I noticed that the number fractions in the growth factor distribution roughly correlate with charges: the first charge constitutes 80% of the population. How do you know that the lower growth mode isn't the singly charged particles?

Page 20 line 1 through 5: This could be true (using the above tables), but it is likely more complicated. The first size distribution mode could also create the higher growth mode, by theory. I understand that there was statistical correlation, but I find no causal relationship for size distribution 3 being the only size distribution mode related to the second growth factor mode.

Page 20 Figure 8: Is it possible to keep the ordinate of panels (a) and (b) the same to show the increase in HGF due to heating?

Page 28 Line 22 and 23: I do not see any evidence in this work that shows a discrepancy between modeled CCN in the atmosphere and actual CCN measurements during the study. Use of the word "improve" seems inappropriate given the lack of evidence. A verb similar to "change" or "alter" seems more appropriate.

References

Stolzenburg, M. R. and McMurry, P. H. (2008). Equations governing single and tandem DMA configurations and a new lognormal approximation to the transfer function. *Aerosol Science and Technology* 42:421-432.

Wiedensohler, A. (1988). An approximation of the bipolar charge distribution for particles in the submicron size range. *Journal of Aerosol Science* 19:387-389.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2019-797/acp-2019-797-RC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-797>, 2019.

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