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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ship impacts on the marine atmosphere: insights into the contribution of shipping emissions to the properties of marine aerosol and clouds" *by* M. M. Coggon et al.

Anonymous Referee #2

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General comments: This paper focusses on the processing of ship emissions by marine clouds using a number of measurements on two platforms. It is well organized and clearly written with an interesting and potentially important message. I have one major comment and some minor comments.

Major comment: P14401, line 24 to P14402, line 4 - This is an interesting and important observation, but does Figure 4 only mean what you imply? The mass concentrations of organics are comparable to those of sulphate (in and out of cloud) as shown in Figure 2. Therefore, if I interpret this correctly, the org:so4 ratio of 1.5-2.0 associated with LWC implies much more organic produced in cloud than sulphate - if that is the only

process leading to the result in Figure 4. That would be an extraordinary conclusion. This needs a more comprehensive discussion. Are there other processes that may contribute to your result in Figure 4? E.g., can deposition from clouds perhaps skew the ratio of organics to sulphate?

Minor comments:

P14395, lines 14-15 – do 42 and 99 really dominate the organic mass or just stand out? It is difficult to tell the exact m/z from Figure 10.

P14398, lines 17-19 – Perhaps say something like "However, there are relative differences between periods of low and high mass cns".

P14398, line 30 – This is the first and only mention of the "sub-isokinetic inlet". Some further discussion is needed.

P14399, lines 9-11 – wind speed can also affect DMS ventilation.

P14399, lines 26-27 – many components of the marine aerosol can be categorized using the AMS, but not sea salt.

P14401, lines 8-9 - Yes, it may be indicative of cloud processing, but does it have to be? Can that sort of distribution not also form from other reasons: e.g. take the ship distribution, dilute it and mix it with an aged marine aerosol or an aged anthropogenic aerosol?

P14401, line 24-25 - Do the products of aq-phase oxidation have to remain in the particles? E.g. could not oxalic acid volatilize?

P14402, lines 8-12 – You say that the Ca, Mg and K are representative of the total metal content, but then say you assume they originate from soluble species. I think this paragraph could be made a little clearer.

P14403, line 11 (section header) – "background" or clean? I like your earlier discussion of the issues with defining a background aerosol, but the use of 'background' here

seems like a violation.

P14403, lines 20-21 and Figure 6 – Is the difference between 0.23 and 0.27% significant, and is there any further interpretation of the comparison of critical supersaturations mean when you have 90% of the total activated in one case and only 60% in another?

P14404, lines 15-16 – do you mean "we show that increased sulfate. . ."?

P14404, lines 17-18 (and line 22) – Certainly increased sulfate will lower the activation threshold, but what about the carbonaceous component? For example, Langley et al (2010) showed that the increased CCN number concentrations were better correlated with ship emissions of organics.

P14405, line 13 – concentrationS

P14405, line 21 (and lines 28-29) – SO2 oxidation in the gas-phase, aq-phase or both?

P14408, lines 18 and 22-24 – Phinney et al. (2006) showed AMS spectra of ship emissions.

P14410, lines 7-10 – You may be overstating the "cloud-processed mode". What evidence do have that the 77 nm mode was cloud processed, since the skies were clear?

P14410, lines 26-28 and P14412, line 24 – It is curious that only these two fragments stand out, the difference of which is 57. Do you see a significant oxygenated fragment in your Hi-Res m/z 57?

P14411, line 12 – this may be another suggestion of aq-phase processing.

P14411, lines 24-28 – but only for cloudy conditions?

P14413, line 19 – wording – it is not the evaporation that produces the m/z 42 and 99.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14393, 2012.