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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Latitudinal distributions of organic nitrogen and organic carbon in marine aerosols over the western North Pacific" *by* Y. Miyazaki et al.

R. Duce (Referee)

rduce@ocean.tamu.edu

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This is a very fine paper that should be published in AC&P. It is one of the very few papers that looks at both organic carbon and organic nitrogen in marine aerosols and attempts to determine the relationships between them. It makes good use of additional chemical tracers to elucidate some of the processes related to sources and formation of these materials. I do have some questions and comments about the paper, but it should definitely be published.

Page 4, line 8 on - While the way the aerosol samples are collected should protect against local shipboard contamination in most cases, there are very few locations

where contamination is greater than aboard ship. Were the authors able to use any specific organic tracers that would have indicated more clearly whether or not they were sampling any pollution from the ship? The results themselves do indicate that contamination was likely not a serious problem.

Page 7, section 2.4 -I understand that the SeaWiFS data were used for chlorophyll a measurements, but were chlorophyll a measurements also made on board ship as the ship moved along its track? How similar were the SeaWiFS data compared with the measurements actually made on the cruise, if they were made? I note that from the mention of in situ measurements at the bottom of page 7 (which were apparently from a different cruise?) compared with the SeaWiFS data shown in Figures 2, the differences appear large.

Page 7, Section 3.1 - The relationship between MSA and latitude as well as SeaWiFS chlorophyll a shown in Figure 2 is very nice.

Page 8, paragraph beginning on line 5 - The explanation for the high nss-sulfate at low latitudes is given as volcanic activity, which may be correct. However, I note that the MSA concentrations are also higher near the equator, although not as much higher as the nss-sulfate. Could the higher MSA (and perhaps some of the nss-sulfate) be due to higher productivity in the equatorial regions?

Page 9, lines 28 and 29 - This sentence is a bit misleading. If one looks at the plot of DEA vs latitude in Figure 4 it appears that if the 2 highest numbers for DEA at about 45N are removed, the concentrations are about the same at most latitudes. Note that 3 of the 5 highest values for DEA are at latitudes below 30N. Admittedly there are no really low values for DEA above about 35N, but their overall argument here is not particularly strong.

Page 10, line 24 and following - As with DEA, the authors claim that the OC is higher at 40-44N compared with lower latitudes. Again, while this is clearly true for WSOC, it is not at all as clear for OC. Two of the 3 highest values for OC are at 30N or below and

5 of the 9 highest values for OC are below 30N.

Page 11, line 16 and following - The statement that WIOC on marine aerosols is produced from organic matter that accumulates in the microlayer of the ocean surface is only partly correct. That material also accumulates on the surface of the bubble as it rises through the water column, and that material "merges" with material already in the microlayer when the bubble breaks and ejects the particles. However, both soluble and insoluble surface-active material is concentrated in the bubble surface and the ocean surface microlayer. The higher WSOC at higher latitudes could partially at least be the result of more soluble OC in the bubble film/microlayer than at lower latitudes. What was the correlation between Na and WSOC? Also, the WSOC and the WIOC may be formed on different size sea salt particles due to different production mechanisms of droplet formation (i.e., film drops vs jet drops), which in turn may have different amounts of WSIC and WSOC on them. There are many possibilities here.

Page 12, line 1 - Were the higher ON/OC ratios found on the larger or the smaller particles in the Miyazaki paper?

Page 12, lines 4-7 - I am somewhat confused here. WSON/WSOC was 0.23 at a forest site, and WION/WIOC was 0.18 in this paper, but the authors indicate that there is more ON enrichment in marine aerosol compared with previous studies. I do not understand that conclusion.

Page 12, lines 8-15 - The data on nitrogen isotopes does not appear to be very helpful here.

Page 12, lines 16 to 22 - In the Cachier et al. paper referenced those authors found that the small sub-micrometer particle had C isotope ratios indicating a continental sources whereas the larger particles has a carbon isotope signature of a seawater source, if I remember that paper correctly. If correct, this is very important and should be mentioned here. Did the present authors make any C isotope measurements as a function of particle size? Where any samples collected at all as a function of particle

size?

Page 12, lines 26-32 - I like the conclusions here. However, I note that in line 29 it says that Fig. 6 shows data where MSA is greater than 20 ng/m3, whereas on Figure 6 itself it says that MSA is greater than 30 ng/m3. Which is correct?

Page 13, line 7 - Couldn't one also add "and WSOC" after "(i.e., DMS)"?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 28721, 2010.