

Interactive comment on “Contribution of dissolved organic matter to submicron water-soluble organic aerosols in the marine boundary layer over the eastern equatorial Pacific” by Y. Miyazaki et al.

Anonymous Referee #1

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The authors present chemical analyses of marine aerosols sampled during the TORERO cruise and use isotopic fractionation of water-soluble and total organic carbon (WSOC and TC, respectively) to conclude that the aerosol organic species are of marine, rather than terrestrial, origin since the former exhibit substantially higher $\delta^{13}\text{C}$ ratios. Three regions of interest are identified: two that are distant from the Americas (R1 and R2) and one that is coastal (R3). The authors find strong marine organic contributions to the aerosol sampled in each region. Overall, the paper is well written and within the scope of ACP. The findings are important and complement past literature showing a strong link between marine sources and elevated organic aerosols in pristine regions. Thus, the manuscript should be publishable after the following comments

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are satisfactorily addressed:

1) The authors convincingly show that regions R1 and R2 are not affected by terrestrial airmasses; however, Figure 1 (right) shows that the 5-day back trajectories for R3 all recently had crossed the continent. How can it be convincingly stated that the increase in WSOC/Na⁺ and ozone can be attributed to aging and secondary production of aerosol from local marine organics rather than transport from other regions? Adding a vertical cross-section of the back trajectories in Figure 1 (right) might help to make this case.

2) Similar to Point #1, the percentage exposures given in Figure 7 and discussed on Pg. 7, Lines 8-13, seem of little value to me as even a short exposure of a given air mass to the strong aerosol emissions sources in the terrestrial mixed layer would be enough to likely overcome marine influences over subsequent days. A better treatment of the air mass back trajectories including the past horizontal and vertical transport would be more informative here. In addition, does the model provide any information about cloud processing or rainout over the transport period?

3) Please add error bars to compositional traces in Figures 2, 4, 6, and 8 that reflect the uncertainty associated with each measurement.

4) Non-normal observational distributions need to be treated more carefully than just a simple arithmetic mean and standard deviation. This is apparent from the large standard deviations reported for some species in Table 1 and the non-physical result of 90±25% reported in the abstract and conclusions – the latter of which is particularly glaring. The authors should reassess the distribution of the data that go into the summary statistics and evaluate the appropriateness of geometric means and geometric standard deviations (if logarithmically distributed) or another functional form for reporting the data or, if there is not a good functional form, then median and percentile values should be reported.

5) The WSOC field blank concentrations are discussed on Page 3, Lines 30-31, but

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similar values for the speciated organic species and inorganic ion concentrations are not included. Please add these values to this paragraph. Is there any contamination associated with storing these samples in glass containers, which can leach inorganic cations? Also, please report the uncertainty associated with the $\delta^{13}\text{C}$ and $\delta^{15}\text{N}$ values in the subsequent paragraph on Page 4.

6) I don't understand the value of Figure 6 and associated discussion on Pg. 6, Line 36 – Pg. 7, Line 7. Are the authors concluding that there is some sort of relationship between $\delta^{13}\text{C}$ and $\delta^{15}\text{N}$? The data do not seem to support this.

7) On Page 4, Line 34, and throughout the text, a “correlation coefficient (r^2)” is reported, which is confusing and needs to be fixed. Typically, a correlation coefficient is denoted as “ r ” and a coefficient of determination is reported as “ R^2 ”. Which type of coefficient is being calculated and reported here?

8) The final line of the manuscript states that “This study provided direct evidence that the contribution of DOC was the dominant control on the submicron WSOC mass regardless of the oceanic areas over the study region.” Similar statements are elsewhere in the manuscript (e.g., Pg. 1, Ln. 30; Pg. 10, Ln. 5). While I agree that there is indeed a compelling correlation between the concentrations of water-soluble sugars and the overall aerosol WSOC concentration and less compelling correlations with MSA and fatty acids, I do not think that this supports the strong assertion that DOC is the dominant control on submicron WSOC. This conclusion should be reworded to be more consistent with what is actually being demonstrated by this study – a “strong correlation”, not a “dominant control”.

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