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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tracer-based investigation of organic aerosols in marine atmospheres from marginal seas of China to the northwest Pacific Ocean" *by* Tianfeng Guo et al.

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Anonymous Referee #2 The manuscript reports the spatiotemporal distributions of organic tracers in TSP collected in two marginal seas of China and the NWPO in the spring season and how the East Asian monsoon carries biogenic and anthropogenic aerosols over these oceanic zones. In addition, the authors discussed the origins of SOAI over the SCS and NWPO. Overall, it is an interesting and inspiring work. However, the follow comments need to be addressed before it can be accepted for publication on ACP.

Response: We thank the reviewer's comments and revise our manuscript accordingly.

Major comments: 1) Line 152-154: it's better to see if the levo/TSP ratio had been increased. Otherwise it's inconclusive to say the contribution of BB aerosols to particle loading over the NWPO may have increased...

Response: The origin sentence is misleading and has been revised "Using these previous observations as a reference (Table 1), our observations suggested that the BB aerosols from the long-range transport over the NWPO in 2014 largely increased. Thus, an important question is raised, i.e., does the increase occur continuously and largely over the last decades in marine atmospheres over the NWPO?". The LEVO/TSP was 0.02% $\pm$ 0.03% (average $\pm$ standard deviation) and 0.02% $\pm$ 0.01% over the NWPO and over the YBS. It is meaningless to say the contribution of BB aerosols to particle loading over the NWPO.

2) Line 227: the relative contribution of SOA tracers to TSP in category 2 is much larger than that in category 1. Based on the authors' reasoning, is it realistic to infer that marine sources can contribute around 10% of TSP?

Response: We carefully check through the whole manuscript. We are sorry for the misleading, but we cannot find where cause this. In revision, we added "The average contribution of SOA tracers to TSP over the SYS was higher in category 1 (0.4% $\pm$ 0.6%) than in category 2 (0.06% $\pm$ 0.07%)." And "The average contribution of SOA tracers to TSP over the NWPO was higher in category 1 (0.008% $\pm$ 0.005%) than that in category 2 (0.005% $\pm$ 0.005%)."

3) Line 294: what are the possible major precursors for DHOPA other than BB emission? Response: The sentence has been revised as "leaving emissions other than BB emissions, e.g., solvent use, oil exploration, marine traffic, etc., as the major precursors for DHOPA in these marine atmospheres".

4) Line 362: it might be attributable to the different stability of 2-MGA and LEVO?

Response: Thanks for the suggestion. In revision, we added "The decomposition of

LEVO reported in literature (Hennigan et al., 2010; Hoffmann et al., 2010; Fraser and Lakshmanan, 2000) may lower the correlation between them. However, whether 2-MGA can decompose in ambient air remains poorly understood."

Reference: Fraser, M. P. and Lakshmanan, K.: Using levoglucosan as a molecular marker for the long-range transport of biomass combustion aerosols, Environ. Sci. Technol., 34, 4560-4564, https://doi.org/10.1021/es991229l, 2000.

Hennigan, C. J., Sullivan, A. P., Collett Jr., J. L. and Robinson, A. L.: Levoglucosan stability in biomass burning particles exposed to hydroxyl radicals, Geophys. Res. Lett., 37, L09806, https://doi.org/10.1029/2010GL043088, 2010.

Hoffmann, D., Tilgner, A., Iinuma, Y. and Herrmann, H.: Atmospheric stability of levoglucosan: A detailed laboratory and modeling study, Environ. Sci. Technol., 44, 694-699, https://doi.org/10.1021/es902476f, 2010.

Minor comments: 1)Line 28: change "discuss" to "discussed"

2) Line 237: there is a redundant "burning"

3) Line 181: change "surprised" to "surprising"

4) Line 350: better to change "regarding" to "given that"

Response: Done. Thanks for your advice.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-723/acp-2019-723-AC1-supplement.pdf

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