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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal characteristics of emission, distribution and radiative effect of marine organic aerosols over the western Pacific Ocean: an analysis combining observations with regional modeling" *by* Jiawei Li et al.

Anonymous Referee #2

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This study focuses on primary and secondary marine aerosols over the Western Pacific Ocean and their radiative effect. The findings are interesting and this study is well suited for ACP. In its present form, I found this study is too lengthy which makes it challenging to read. Important methodological information and analysis are missing regarding the radiative impact of MOA.

Abstract is very long. I recommend the authors try to highlight no more than 2-3 key points.

line 25. It reads like a new finding but as far as I understand this just describes the parameterization of Gantt et al. This should be clarified

line 70. The introduction focuses primarily on literature published prior to 2012. A lot has been done both in terms of observations (field and lab) and in terms of parameterization since that needs to be discussed by the authors.

Here are a couple of studies (by no mean an exhaustive list) that the authors may want to consider

Conte, L., Szopa, S., Aumont, O., Gros, V., & Bopp, L. (2020). Sources and sinks of isoprene in the global open ocean: Simulated patterns and emissions to the atmosphere. Journal of Geophysical Research: Oceans, 125, e2019JC015946. https://doi.org/10.1029/2019JC015946

Bates, T. S., Quinn, P. K., Coffman, D. J., Johnson, J. E., Upchurch, L., Saliba, G., et al. (2020). Variability in Marine Plankton Ecosystems Are Not Observed in Freshly Emitted Sea Spray Aerosol Over the North Atlantic Ocean. Geophysical Research Letters, 47, e2019GL085938. https://doi.org/10.1029/2019GL085938

Quinn, P. K., Bates, T. S., Coffman, D. J., Upchurch, L., Johnson, J. E., Moore, R., et al. (2019). Seasonal variations in western North Atlantic remote marine aerosol properties. Journal of Geophysical Research: Atmospheres, 2019; 124: 14240– 14261. https://doi.org/10.1029/2019JD031740

Brüggemann, M., Hayeck, N. & George, C. Interfacial photochemistry at the ocean surface is a global source of organic vapors and aerosols. Nat Commun 9, 2101 (2018). https://doi.org/10.1038/s41467-018-04528-7

Betram et al. Sea spray aerosol chemical composition: elemental and molecular mimics for laboratory studies of heterogeneous and multiphase reactions. Chemical Society Reviews, 31 Mar 2018, 47(7):2374-2400 DOI: 10.1039/c7cs00008a

Quinn, P., Coffman, D., Johnson, J. et al. Small fraction of marine cloud conden-

sation nuclei made up of sea spray aerosol. Nature Geosci 10, 674–679 (2017). https://doi.org/10.1038/ngeo3003

S. M. Burrows and O. Ogunro and A. A. Frossard and L. M. Russell and P. J. Rasch and S. M. Elliott A physically based framework for modeling the organic fractionation of sea spray aerosol from bubble film Langmuir equilibria Atmospheric Chemistry and PhysicsÂă 14Âă 13601–13629Âă (2014) https://doi.org/10.5194/acp-14-13601-2014

line 169 It would be worth mentioning that this range exceeds the valid range for Monahan (0.8 < r80 < 10mm)

line 202 GEIA is a portal for many different inventories. Are the authors using a climatology of MEGANv2? That would be surprising since very detailed year-specific inventories are used for anthropogenic and biomass burning emissions. Please clarify.

line 226 It seems there could be a lot of other reasons for this difference. MODIS vs VIIRS Chl-A, differences in wind speed and sea salt parameterizations.

line 229 This section completely ignores the abiotic source of isoprene (see references above), which may be as large if not larger than the biological source in marine environments.

line 265 I may be missing something but I am not sure how to reconcile the source function for sea salt (radius > 0.1um) which is used to derive MOA emissions, with the assumed diameter of MOA (0.1 um). Please clarify.

line 267 do the authors also use 5 size bins to represent MPOA or do they only consider sub-micron MPOA for this work?

Section 3. I suggest to have a section devoted solely to describing the different sources of observations, such that section 3 can focus solely on the model performance. The analysis of BC should focus more clearly on how these observations can help understand marine organic aerosols. The detailed BC analysis presented here could be moved to supporting materials, which would help shorten the manuscript.

It would be helpful to evaluate the simulated Na+ with the UMiami/Prospero dataset.

line 385-396. Suggests removing or moving to supporting materials

line 471 The model does not seem to capture the variability in OA from 4/7 to 4/13 (e.g., it shows high values in 4/11 for instance). Could the authors comment on this discrepancy? Could the model underestimate land SOA (which will not correlate with BC) over this time period?

linee 478 This needs some reference. What is the size range of fungi spores?

line 583. Many aspects of the overall OA budget remain challenging to represent (https://acp.copernicus.org/articles/20/2637/2020/). The contributions of MOA is fairly small at most sites. Could optimization of the land source of SOA or the removal of OA also reduce the model bias?

line 626. Does MOZART include MOA emissions?

line 711. I suggest to also compare with satellite AOD (MODIS, MISR, VIIRS) so that performances over the Western Pacific Ocean can be better assessed.

lines 771-775. You cannot mix your regional estimate with previous global estimate. Instead you would need to run your model globally to draw such conclusion. This also means that the abstract and conclusion need to be revised. Same issue on line 801.

line 885. Please clarify why this is impressive.

Sections 4.3 and 4.4

While there is an excess of details in previous sections, more analysis/method descriptions are needed here.

Please provide the equations to estimate DRE and IDRE. It seems that you would need more than 2 experiments to estimate the IDRE for the different types of aerosols. Are these estimates based on an ensemble of 1 yr simulations? Are the differences shown here significant (relative to natural variability)? In general the authors need to better quantify the uncertainties associated with their estimates? This is especially important for the IRE_MOA. The authors also need to discuss their findings in the context of recent work that suggests a small role of SSA for CCNs (e.g., Quinn et al. DOI: 10.1038/NGEO3003)

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