

## ***Interactive comment on “The impact of marine organics on the air quality of the western United States” by B. Gantt et al.***

**B. Gantt et al.**

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Note: All reviewer comments are in *italics*. All author responses are in normal format.

Reviewer 1:

*This paper provides an interesting look at the influence of natural sources on air quality in urban locations. If marine organics influence the PM<sub>2.5</sub> concentrations in cities that are out of compliance, new standards may have to be enforced. Since the contribution is small, the regulations might not change. In addition, it may be hard to define coastal cities depending on how large they are and how much they are influenced by onshore winds and other factors.*

1. *Overall evaluation: This is a short study of the modeled changes to OC and O<sub>3</sub> concentrations compared to observations, primarily in California. The study uses state-of-the-art models and emissions and compares to available quantitative measurements and qualitatively to related literature. The conclusions that the marine sources are negligible in the polluted non-attainment regions of California seems predictable (based on observations), so the results are not surprising. In some ways, it seems the hypothesis that was tested (based on the title. . .“the impact of . . .”) had what is usually perceived to be a negative result (i.e. a negative impact). While I can provide intellectual support for the necessity of publishing results even if negative, as written these results might be of more interest to a more focused journal like JAWMA. If not, then I would request that the authors address the following issues in order to make the context and impact of their results clear to the more broad audience of ACP. In my opinion, the authors lose an opportunity by not identifying an outcome of the modeling that goes beyond the observed fact that levels of marine OC are much smaller than OC in populated coastal/urban areas; perhaps there is something that the interaction in coastal areas causes? A title focused on an interesting positive outcome (rather than the lack of “impact”) would then be advisable.*

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We appreciate the suggestion of the reviewer of a title change. We have changed the title of the updated manuscript to “The contribution of marine organics to the air quality of the western United States.” The paper deals with a broad range of topics of interest to atmospheric chemists, marine chemists and biologists, scientists studying health effects associated with air pollutants, biogeochemists focusing on the global C cycle, and researchers focusing on climate change. Therefore we believe that the paper is of general interest to the ACP readership.

2. *For example, interesting questions that come up while reading this paper are: how far inland do the marine aerosols contribute to the PM<sub>2.5</sub> and OC<sub>2.5</sub> levels? Is this level higher or lower than the upwind background in a continental region? Is this higher or lower than the contribution of a forested area? Does the different chemical composition of these particles have a different impact on the heterogeneous chemistry in urban*

areas? Is California a good example? Would there be a different answer in Oregon or Canada with less coastal population? And how will a coastal area be defined for future air quality standards and models?

We appreciate the insightful questions of the reviewer. Most of these questions have been addressed in the updated manuscript. We feel as though some of the reviewer's questions such as heterogeneous chemistry and site-specific effects/classifications may be beyond the scope of the article.

3. *Abstract: What is defined as a coastal city? Interesting conclusion that marine organics are impacting air quality in coastal cities and should be included in air quality models. L2: change "to" to "on"*

In the updated manuscript, we define San Francisco, CA as a coastal city that is affected by marine organics. As we have changed the term impact to contribution in the updated manuscript, we have kept the "to" because it is more appropriate.

4. *Introduction: Explain why isoprene and monoterpenes were chosen as the main ocean emitted VOCs. Do the coastal cities with larger wave breaking have a different contribution of organics from marine sources? Does the percent contribution to PM2.5 from marine aerosols change with distance from the coast? Explain how are the organic aerosols from bubble bursting transported to the coastal areas. Does topography affect concentrations?*

We agree with the reviewer that these questions are both interesting and pertinent to this study. Most of these questions have been addressed in the updated manuscript (page 4, line 10-17; page 11, lines 4-6; page 11 line 8-13, etc). For example on page 11, lines 1-2 we state: "The gradient of marine contribution to PM2.5 is especially sharp in areas with steep topography."

5. *Method: Explain the importance of choosing the summertime. Do you think the results would be different during a different time of year?*

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The updated manuscript includes the requested explanation (page 5, lines 29-30; page 12, lines 22-27). For example on page 12 we state: "Although summertime simulations with potentially the highest emissions of marine BVOCs and rapid photochemistry were chosen for this study, the addition of isoprene and monoterpenes from marine sources did not have considerable effect on O<sub>3</sub> or SOA surface concentration in coastal areas. Therefore, the effect ocean emitted trace gases on coastal air quality during a different time of year is expected to be even smaller."

6. *2.2: Explain if emissions from diatoms are representative of emissions from all marine species and the potential differences in emissions.*

The updated manuscript includes the requested explanation (page 6, line 13-15).

7. *Results: 3.1: How will the new air quality models incorporate the difference in the contribution from marine aerosols based on the amount of onshore wind? Why is there consistent disagreement between the model and observations? This supports the negative correlations shown in this paper, but what can be done to increase the correlations and better represent the measurements? 3.2: Why is the largest change off the coast of Northern California?*

Most of these questions have been addressed in the updated manuscript (page 10, line 1-8; page 10, line 24-26, page 11, line 16-20).

8. *Conclusion: How far inland does the onshore flow bring the marine aerosols?*

The inland extent of marine aerosols has been discussed further in the updated manuscript (page 12, line 15-16).

9. *Table 1: Further explain why all of the correlations at the Point Reyes site are not strong. In the text, explain why the correlation of the bottom up and top down are not that much higher than the baseline. Table 2: None of the correlations are great. Why are the Ventura simulations so different from Oceanside? What impact does location and coastal topography have? Figure 1: Do the marine organics really contribute to*

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*surface OC2.5 all the way across California and into Arizona and Nevada? Would this be different for a different season? It might help to show only the Pacific Coast and less of the rest of the country. That way, more of the change in percent between the coasts and inland areas can be seen. Figure 2: It might help to make 0 a different color so that it is more obvious what is impacted and what is not. Are the grey regions not involved in the study? What is off the coast of Northern California that is producing such high concentrations of marine organics? Figure 3: Might consider making the letters (a&#x2013;AR&#x2013;d) larger because they are not very apparent in the figure.*

It is not clear to us the cause of some of the differences between the stations besides the considerable uncertainty in the marine organic emission parameterizations. The inland extent of the marine emissions, the cause of the high concentrations, and the seasonal variations have been addressed in other responses. We agree with the reviewer that adjusting the figures to show more of the coast would be helpful, and have made that change in the updated manuscript (pages 25-28).

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6257, 2010.