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Comment

***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.

The manuscript describes a modeling study, which is an extension of a previous paper from the same group, concerning the impact of organic aerosols (both primary and secondary) from marine sources on air quality of coastal areas. The paper is interesting but has some issues that need to be addressed prior to publication to ACP. It presents potentially interesting results, but more in depth analysis is necessary, as mentioned below. General comments: 1. The modeling domain used is not appropriate for a study of the marine organic source. Less than 10% of it is dominated by the ocean, with the rest of it laying over continental US. Since this study aims to analyze the organic aerosol sources, the organic aerosol transport and their influence on coastal areas, a

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larger oceanic domain is necessary. Having the boundary conditions so close to the area of interest, and assuming that the boundary conditions have zero marine organic aerosols, makes the results fragile. Large range transport of the (mostly water insoluble, thus not so effectively removed) aerosols might augment their amount reaching coastal cities. In addition, about two thirds of the model domain (the east part) is never mentioned.

We disagree with the reviewer. The model domain is well suited to study the contribution of marine organics to air quality of the western United States. It includes large portions of ocean upwelling (high productivity) regions, open ocean areas, and land masses, allowing the simulations of the impact of marine organic emissions in the western United States. Prior to simulations, we have also carefully examined the effect of boundary conditions. Over the coastal US, the effect of boundary conditions was found to be small and generally evident above boundary layer (1.5 to 2 km). Boundary conditions had minor effect on surface concentration of aerosols and ozone. We agree with the reviewer that about two thirds of the model domain (the east part) is not discussed in the manuscript. However, without extending the eastern part of the model domain far enough, we would not have been able to examine the coastal transport of marine OC aerosols. Nevertheless, in the updated manuscript, the boundary conditions are discussed and the figures are augmented to reflect the negligible influence of marine emissions.

2. No discussion is ever made on the meteorological conditions prevailing in the area of interest. From figure 1 it is evident that there is a sharp decrease of marine organics when moving from areas above sea to areas above land. However, this figure is a three month average. When studying air quality, the average is of little importance; episodic events, where winds coming from the ocean, transporting organics above the coastal cities, are more interesting. The frequency of such events is also something that has to be mentioned. In addition, when marine winds dominate, they tend to transport local pollution inland, which can be beneficial in air quality, given the fact that

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the marine organics calculated are low. All these important issues are absent from the manuscript.

Although we disagree with the reviewer that average concentrations are of little importance (i.e. the PM_{2.5} standard in the US is both an annual average and/or 24-hour average), we appreciate the reviewer's comment regarding the episodic events. The updated manuscript has moved Fig. S2 to the body of the article and added additional information including observations and relative contribution from primary and secondary marine sources. The updated manuscript also includes discussion of this figure and general discussion about episodic events.

3. It would be very useful to present an open ocean comparison with measurements, if data are available.

While we are unaware of open ocean aerosol measurements within this model domain and time period with which to compare to the model, we do include some comparison of summertime OC concentrations from other locations.

Specific comments:

4. Sentence 1 of the introduction requires some references (more than one).

The requested references have been added to in the updated manuscript.

5. Page 6261: "Marine monoterpenes were not included in bottom-up simulations due to extremely low emission rates from diatoms (100 times lower compared to isoprene)" and "Marine monoterpenes are included in the top-down approach due to reported ambient concentrations comparable to phytoplankton-produced isoprene" are controversial. If both claims are correct, then the major source of marine monoterpenes is missing from the bottom-up approach. One simple guess could be that phytoplankton species other than diatoms produce much more monoterpenes.

As of today, Yassaa et al. (2008) is the only study reporting marine productions of monoterpenes. Monoterpene emissions were studied not only from diatoms, but

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from nine marine phytoplankton species (Coccolithophorid: *Emiliana huxleyi*; Diatoms: *Chaetoceros neogracilis*, *Ch. debilis*, *Phaeodactylum tricornutum*, *Skeletonema costatum* and *Fragilariopsis kerguelensis*; Chlorophyte: *Dunaliella tertiolecta*; cyanobacteria: *Synechococcus* and *Trichodesmium*). Ambient measurements were made in the atmosphere over the Southern Atlantic Ocean conducted in January–February 2007 (35°49'S, 20°22'E to 52°17'S, 67°73'W) and 1–25 March 2007 (from 53°10'S, 70°54'W to 20°9'S, 57°29'E) (Yassaa et al., 2008). So controversy the reviewer noticed is real, and requires future studies.

Although this is not the focus of the paper, the updated manuscript includes clarification of these conflicting statements.

6. *MSA is not included in the calculations, although it is one of the most important marine organic aerosol components. A discussion on how this is expected to influence the results should be added.*

The updated manuscript includes the requested discussion (page 12, lines 1-6).

7. *Page 6268, line 2 “air quality of coastal areas”: The study was performed at coastal California. Although the results are expected to be qualitatively similar with other coastal areas, they might differ considerably quantitatively. The authors cannot use their results to support such a general conclusion.*

This sentence has been adjusted in the updated manuscript to reflect the more localized conclusions concerning the contribution of marine organic emissions to coastal air quality. We state: “This is the first study to quantify the combined contribution of marine primary organic aerosol, isoprene, and monoterpene emissions to the air quality at the west coast of the U.S.”

8. *Figure S2 does not deserve to be in the supplementary material. In an air quality study (as the title of the paper implies) this should be the most important figure of the paper. The measurements should also appear on that same figure for comparison with*

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the model.

We agree with the reviewer that this figure is important, and the updated manuscript includes this figure (with requested updates) in the body of the paper.

Technical corrections: 9. *Colomb et al (page 6261, line 12) is misspelled. A map showing the locations discussed throughout the text should be added. Sciare et al (page 6267, line 3) is misspelled.*

These have been corrected/addressed in the updated manuscript.

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

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