

Interactive comment on “Oxygenated volatile organic carbon in the western Pacific convective centre: ocean cycling, air–sea gas exchange and atmospheric transport” by Cathleen Schlundt et al.

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This is a really interesting paper that describes a set of oxygenated volatile organic compound (OVOC) measurements in surface ocean/lower atmosphere of the western Pacific. The authors explored relationships between seawater OVOC concentrations and phytoplankton groups as well as types of dissolved organic matter. They also constrained 1) the contribution of air-to-sea flux on the oceanic OVOC budget, 2) the contribution of sea-to-air flux on the atmospheric OVOC budget, and finally 3) the contribution of boundary layer OVOCs to the upper troposphere (relevant for HO_x cycling). The number of data points is somewhat limiting (especially the atmospheric OVOC concentrations, which are necessary for the flux calculation), but the analysis is sound

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and the data interpretation seems reasonable. I think the paper is publishable after minor revisions.

Specific comments:

p.2 Line 6-31. To be more precise, the authors should specify the sources and sinks of individual OVOC compounds in these 2 paragraphs, rather than always using the term “OVOCs”.

p.3 line 4-10. The ‘either...or’ scenario doesn’t completely hold due to photochemical destructions of OVOCs, for example. This paragraph also feels disconnected from the neighboring paragraphs. Can probably just move it to Section 3.2.2.

p.4 line 5-19. Did the authors thoroughly test the effect of using potassium carbonate as the drying agent – that it doesn’t cause any loss/contamination in OVOCs?

Out of curiosity, in this setup how much of each OVOC compounds is purged out from the water phase after 20 minutes approximately?

The fact that the water standards were prepared in Milli Q water while the actual samples were seawater will likely create a small bias in the measurement (due to the effect of salinity on gas solubility, and thus purging efficiency)

Given the analytical errors and reproducibility stated, what are the limits of detections for these compounds in seawater?

Line 21-31. Was potassium carbonate used to dry the air samples too?

p. 6, line 11-19. As discussed by Yang et al PNAS 2013 (www.pnas.org/content/110/50/20034.abstract), the Duce et al. 1991 parameterization probably underestimates airside diffusional resistance and thus overestimates total airside transfer velocity.

p. 10 line 24. If atmospheric OVOC input is important for the ocean mixed layer OVOC budget, some of the spatial variability/correlations in seawater OVOC can preassem-

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bly be in part explained by variability/correlations in atmospheric deposition fluxes of OVOCs?

Have the authors looked for correlations of OVOCs in air? By eye some of the compounds appear to strongly correlate.

p. 10 line 39-40. This sentence contradicts with what's stated in your abstract, that most of the OVOC fluxes were into the ocean. I'd pick a tone and stay with it.

Also, do the OVOC seawater concentrations, atmospheric concentrations, and air-sea fluxes vary diurnally? If so, it might provide further hints to the possible sources.

p. 11 line 23 (typo here, should be Section 2.8, not 2.6)

p. 12, line 31-33. This is reasonable, though it's still possible that some unmeasured oceanic hot spots upwind of the eastern region were contributing to the atmospheric OVOCs.

p. 12, line 40. Are only positive sea-to-air fluxes used here (as implied by the omission of acetaldehyde), or do the average air-sea fluxes include both positive and negative fluxes? In the case of the former, this calculation would represent the maximum contribution to UT.

More generally, I think the authors can benefit from differentiating between net vs. gross fluxes here (i.e upwards and downwards; see Yang et al. ACP 2014). Even in a case where the OVOCs are in air-sea equilibrium, molecules of OVOCs from the sea will still emit into the atmosphere and molecules of OVOCs in the atmosphere will still dissolve into the sea (just the two fluxes cancel out mathematically). It could be that the molecules of OVOCs in air and in water come from rather different sources. Long story short, to look at the upper-limit impact of ocean emissions on the UT, I think it'd be useful to do another FLEXPART model run prescribed with the gross emission fluxes ($= K * C_w$). Of course, for the ketones that have reasonably long lifetimes, some molecules of ketones in the atmosphere will have come from the ocean and vice versa.

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Figure 9. to make it easier for the readers, can the authors please stick to one times-tamp format (either day of year or day/month)?

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