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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Formation of gas-phase carbonyls from heterogeneous oxidation of polyunsaturated fatty acids at the air–water interface and of the sea surface microlayer" *by* S. Zhou et al.

Anonymous Referee #3

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The manuscript of Zhou et al. describes interesting and timely experiments aimed at studying the heterogeneous chemistry occurring at the sea surface, specifically interactions between organics in the sea surface microlayer and ozone. This chemistry could lead to the production of a variety of oxygenated products via ozonolysis, and these could be released to the overlying atmosphere. This type of chemistry, however, remains poorly understood and this work provides important information regarding the nature of this chemistry and a potential mechanistic-level understanding of the processes involved. As such, this work is certainly relevant to the readership of ACP. The

experiments have been carefully designed and carried out, appropriate analytical techniques have been applied to obtain high-quality data, and the conclusions drawn from the data are reasonable and valid.

Other than a few minor typos, I did not find any significant shortcomings of this manuscript. It was concise and well written and the figures were nicely designed, clear, and easy to read/interpret.

Minor suggestions/questions:

-Label the product names in Figure 6 (as you did in Figure 8)

-In Figure 4 you caption it "Example of ozone and product profiles..." while in Figure 5 you simply say "Ozone and product profiles...". I would make these consistent.

-Figure 4: right axis, glyoxal is misspelled.

-Regarding the type 1 vs type 2 experiments, it isn't clear to me how the addition of the smaller amount of LA via "dilution" by DCM might be impacted by the presence of the DCM itself. The text indicates that in both cases a monolayer was formed, but I'm just curious if one can consider the nature of this monolayer "equal" (just more dilute in one case) in both experiment types? The presence of DCM would change the surface tension and also the intermolecular interactions. It's unlikely that impacts the ozone chemistry, but perhaps something worth mentioning in the text.

-A curiosity more than anything: did you see any evidence of halogen chemistry occurring during the experiments? Some intriguing papers have come out discussing ozone/halogen chemistry (particularly iodine) and the influence of the surface microlayer. The detection technique may not lend itself to this (e.g. CIMS would be ideal if you were looking for halogen production), but the halogens could potentially influence the products identified. To me, this is beyond the scope of this particular manuscript and I'm in no way implying this NEEDS to be part of the discussion, but a mention of the possible impact of simultaneous halogen chemistry induced by ozone reactions at the surface could be a nice tie in to some of the other literature available.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 17545, 2013.