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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An Overview of the Surface Ocean Aerosol Production (SOAP) campaign" *by* Cliff S. Law et al.

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The SOAP experiment is one of the largest, most comprehensive and most interesting efforts conducted so far to study biogeochemical surface ocean – lower atmosphere interactions. Some articles on topical studies within the global study have been or are being published, but there is the critical need for an overview paper like this that provides the context and describes the experimental approach. The present manuscript is definitely worth publishing to serve this purpose, even though it falls a bit short in enunciating the main findings and advances of knowledge. I particularly like the introduction, which does a very good job with summarizing the state of the art, the gaps of knowledge and the need for such an experiment. The oceanographic and environmental regional context is very much appreciated too. The other aspect I like best is

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the listing of the instruments and how they complement one another. This is something typically missing in many papers for a lack of space, and that the nature of this manuscript allows.

Thank you for these comments. We acknowledge that the paper does not include all the main findings, as some are still being evaluated and/or are not published. However, it does incorporate all published results to date, and also preliminary insights into some unpublished data, as well a revision of the regional mean DMSsw. However, to reflect that the paper is not a comprehensive report on all the SOAP results we have adjusted the title to: “An Overview and Preliminary Results of the Surface Ocean Aerosol Production (SOAP) campaign”

I miss comparison with previous similar cruises, such as the ACSOE or the SAGE, and statement of what is different and how SOAP goes a step forward.

We have included a Supplementary Table that briefly summarises previous campaigns, and added this text to the Introduction: “ Previous related research campaigns have examined the biogeochemical and physical factors influencing oceanic DMS and CO2Âň fluxes, as summarised in Suppl. Table 1, but few have linked this to the physical controls of air-sea exchange, and variation in aerosol and trace gas composition of the MBL. Similarly, other campaigns with an atmospheric focus, such as MAP (Decesari et al., 2011), have carried out detailed studies of aerosol chemistry, but not interpreted this with regard to surface ocean biogeochemistry. To address this. . . . “ and also: “Building upon the approaches used in previous studies, the SOAP campaign targeted three phytoplankton blooms of differing plankton community composition, to determine their respective influences on biogeochemistry, gas exchange and MBL composition”

In terms of a bloom-related study, SOAP is a bit disappointing. I mean, the links between each of the blooms, its biogeochemical processes, and the results of the air-sea exchange, are weak. Effort is made in the present manuscript to argument that each of the situations or blooms is not a static environment but dynamic, with changes as-

sociated with meteo forcing and so forth. This is sharp and honest – the drawback is that the blooms were not very clearly delineated so that process-based associations with aerosol precursors of more general applicability could be built. Do you the authors agree with this analysis? Along these same lines, the recent paper by Royer et al. (2016) in Scientific Reports shows dramatic changes in DMS concentration associated with the passage of a storm.

Although the SOAP Overview paper does not include direct evidence of links between bloom biogeochemistry & air-sea exchange in the figures, it summarises the results of SOAP publications that do address this. For example, Bell et al (2015) directly link surface DMS distribution to DMS flux, by calculating a flux footprint and highlighting the importance of considering the dynamics of the marine source. In addition, Walker et al (2017) relate near-surface DMS distribution and biogeochemistry to EC-derived DMS fluxes and kDMS. In addition, there will be forthcoming papers that relate bloom biogeochemistry to DMS and CO2 flux, and aerosol composition.

Specifics -Line 207: Mahajan et al. 2006 should read 2015

Corrected in text and references

-Line 314: remove parenthesis after 9nmol L-1

Done

-Page 17: when discussing about the underestimation of the current climatology for the region, and call for a revision into much higher concentration, to what extent do you think your numbers are biased high because you deliberately visited blooms? What can you say about average regional concentrations?

We have addressed this by adding the following to the Discussion: “Although the Pre-SOAP and SOAP sampling strategy of focussing on phytoplankton blooms may introduce bias towards higher DMSsw, the BOX voyage, which had broad spatial coverage of subtropical and subantarctic waters between 39.5-47oS, gave a similar mean

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DMSsw to the weighted mean for all voyages."

And also changed the wording in the Abstract to: "Inclusion of SOAP data in a regional DMSsw compilation indicates that the current climatological mean is an underestimate"

-Page 19: To me, it is pretty obvious that instantaneous correlations between chla and the aqueous concentration of DMS or any other biogenic volatile can be expected (yet not always found),. . .. . .

We have added additional information here: "There was a weak, but significant correlation (r = 0.12, p< 0.005) between Chl-a and DMSsw in the underway surface data during SOAP, but also significant variability in the slope and the sign of this relationship between the different blooms."

. . ...but not necessarily with the flux. The flux depends primarily on the aqueous concentration but also on e.g. the wind speed. Therefore, correlations between biological markers and the emission flux are to be expected, if anything, over longer time scales.

We agree, but have retained this observation with a caveat added: "ÂňCorrelations were apparent during SOAP between Chl-a and DMSP (Lizotte et al., submitted), and Chl-a with DMSsw and DMSa, but there was no relationship between Chl-a and DMS flux, as expected, due to the short timescales and flux footprint identified by Bell et al., 2015."

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-535/acp-2017-535-AC1-supplement.pdf

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