

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

Cliff S. Law et al.

cliff.law@niwa.co.nz

Received and published: 18 September 2017

This manuscript provides an overview of the multi-disciplinary SOAP cruise off the coast of New Zealand in 2012. I believe such an overview is important and that the manuscript should be published with the following modifications:

Thanks for these comments

1. Line 37 You don't show a correlation between chlorophyll-a and DMSsw.

We do not show the correlation in a figure, but have added the following to the Conclusions: “Overall there was an 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

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blooms”

3. Line 80 You are mixing aerosol mass and number here.

Rewritten to improve clarity: “Breaking waves and associated bubble formation are a major source of Primary Marine Aerosol (PMA), supplying most the aerosol mass in the marine boundary layer (MBL) over the remote ocean (Andreae and Rosenfeld, 2008), and particularly in regions that experience high winds and breaking waves (de Leeuw et al., 2014). This is reflected in PMA contributing only ~10–20% of CCN number concentrations over the remote Pacific Ocean (Blot et al. 2013; Clarke et al. 2013), but up to 55% over the Southern Ocean (McCoy et al. 2015).”

4. Line 173 What is secondary production?

Biomass production by consumers (as opposed to primary production by phytoplankton)

5. Line 199 Should read “aerosols and their precursors”.

Changed to “aerosols and precursors”

6. Line 263 Could you please give more details on the bubble chamber.

Now added: “The composition of primary marine aerosols was also examined using a 0.45m³ bubble chamber, in which sea spray was formed via the bursting of bubbles produced by passing clean compressed air through sintered glass (Mallet et al., 2016).

7. Line 263. The Supplementary table should be in the main manuscript. It would be helpful to have a reference for each measurement.

This table is now in the main manuscript as Table 1, but we have not added references for each measurement, as this would require too many additional references.

7. Line 276. What do you mean by “biogeochemical signals”?

Modified to “elevated chl-a and DMS_{sw}, and pCO₂ drawdown”

8. Figure 5 needs to be larger to make it more readable.

Fig. 5 is now revised, so that text and labels are clearly visible

9. What is the light blue line in Figure 6b?

The cyan line indicates wind direction. This was noted in the Figure legends in the text, but omitted from the legend below Figure 6

10. Line 340. Aerosol number concentration:

Corrected to “Aerosol number concentration”

11. Line 386. CCN data should include the % supersaturation. Were all measurements made at the same supersaturation?

Now added “at 0.5% supersaturation”

12. Line 389. My guess is that the CCN activation ratio was higher because the particles were larger. I doubt if it has anything to do with the 3 conditions you mention.

We have added “the median particle diameters during clean marine periods were consistent between the three blooms” which contradicts the referees’ suggestion, and so retained the comment that “that particle composition, secondary organics or coagulation may have impacted CCN activation at B1”. This is also further supported by: “preliminary results from an application of the ACCESS-UKCA model (Woodhouse, pers comm.), which simulated the additional impact of emissions of marine secondary organic carbon under the conditions determined during SOAP.

13. Line 390. This could be the explanation or it could be coagulation.

Coagulation is now included as an alternative reason for particle growth (see above)

14. Line 454. Can’t you say how the three DMS instruments compared?

Now added: “Intercomparison of the PTR-MS and SCD during SOAP, involved analysis of two air samples and two diluted DMS gas standards with a concentration range of

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158 – 354 ppt. The instruments showed very good agreement, with a mean difference of 5% and maximum 10%.”

15. Line 490. Can the comparison be quantified here?

The comparison of the micrometeorological techniques is in Smith et al. (to be submitted).

16. Line 510. What was the result?

Now expanded to: “In addition, SOAP data was used to parameterise whitecap coverage against wind-speed, and identify that maturing waves may obscure and lead to underestimate of the variability of breaking waves (Scanlon and Ward, 2016).

17. Line 576. Influence of SSM on air-sea exchange?

Now expanded to: “on DMS emissions”

18. Line 579. Entrainment. Can you say more about this in the manuscript?

The sentence mentioning entrainment has been removed

19. Line 584. Chl-a is an indicator of plankton biomass, not productivity.

Changed to “phytoplankton biomass”

20. Line 602. Where are the rest of the data available?

Added: “The remaining data is available by request email to cliff.law@niwa.co.nz”

21. Figure 2. What is the line?

The line has been removed from Figure 2

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-535>, 2017.

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