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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the impacts of phytoplankton-derived organic matter on the properties of the primary marine aerosol – Part 1: Source fluxes" *by* E. Fuentes et al.

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One cannot reliably predict the production and properties of sea-salt particles without understanding the role of surface films. Surfactants change the dynamics of rising bubbles and the way they shatter to produce film and jet drops, but the effect has been difficult to quantify. There are too many unknown/unconstrained parameters in open-ocean studies to isolate the effect; and laboratory studies suffer from the need to accurately represent open-ocean conditions on a much smaller scale. Even the technique for generating bubbles in the laboratory differs widely from group to group.

This group uses a plunging-waterjet bubble generator to mimic generation by breaking

waves. An SMPS was then used to quantify the size-dependent production of submicron salt particles. To artificial seawater they then added exudates from algal strains isolated from productive waters during a cruise. They find that diatomaceous exudate can have a significant effect, if its concentration exceeds 175 μM. The number peak moved to smaller sizes and more particles <100 nm were formed. Since these OC (exudate) concentrations are only achieved in the ocean during high levels of biological activity, the effect is unlikely to been important in most places and times.

The experimental results are reproducible and convincing. Suitably pure water shows no reduction is the aerosol size peak until the 175-300 μM OC threshold is exceeded. The use of plunging water for bubble production seems like a better analog to open ocean processes (originating at the surface, being pushed down by the plunging flow, then rising) than subsurface bubble-generation methods. The isolation of natural algae and farming them for exudates is also an improvement over the use of individual manufactured surfactants. The authors found similar changes in ambient aerosols within (more and smaller) and outside (fewer and larger) of a bloom region at sea, reinforcing the lab results.

The authors then try to relate the OC from exudates to satellite estimates of Chla. Frankly, I am rarely comfortable with the results of such derivations, because of the (often unquantifiable) uncertainties in the many relationships used. Just one example:

"Analysis of water-leaving radiances measured by ocean color sensors . . . provides the global distribution of diatom biomass in the oceans."

Does it? How accurately can one measure integrated ML biomass from heavily surface-weighted measurements? How well are atmospheric aerosol impacts removed from the signal received by the spacecraft? And on and on . . . There is an unsettling tendency to describe and use satellite products as if they were reality, while often they are closer to an educated guess. Note the sense of certainty in the quote above. That has an impact on readers, even when it is tempered somewhere below by admissions

of the qualitative nature of the production model they derive.

There is also an “arbitrary lifetime” thrown in there. The use of four significant figures in Eqn 8 (Using even one sig fig is questionable!) also encourages readers to feel more confident that they should about the nature of the result. To their credit, the authors follow Eqn 8 with a sensitivity analysis, but you can see the confused message this presents to readers.

I think the overall result, however, is well-supported: Submicron sea salt aerosol production is likely to increase in blooms, with the peak moving to smaller sizes relative to sea salt aerosol over the remainder of the world’s oceans. This is very useful and more quantitative than most formulations to date.

Specific comments:

Page 14097, line 5: particle number flux? Please specify.

Page 14103, line 22: Typo? Don’t you mean stained?

Page 14105, line 17: What is an f/2 nutrient solution? This needs to be readable for aerosol folks.

Page 14121, lines 14-17: This is confusing. The ocean is full of electrolytes. Are they negating their entire MS by saying that Asher 97 was right?

Page 14122, lines 17-19. Aren’t these two sentences in conflict? If not, please explain.

Figures: Virtually all of the axis labels and legends are unreadably small. It is not helpful to put a lot of information into the figure itself (as opposed to the caption), if readers cannot decipher any of it.

Overall this is a valuable paper. I recommend publication with only minor revisions.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14095, 2010.