Ocean Sci. Discuss., 9, C1497–C1504, 2013
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Ocean Science Discussions

Interactive comment on "Field measurements of aerosol production from whitecaps in the open ocean" *by* S. J. Norris et al.

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1. The size range of the spray droplets observed (i.e., R80 < 10 μm) is more relevant to the direct and indirect aerosol effects on radiation and climate than to spray-mediated heat, moisture, and momentum transfer (which results, mainly, from droplets with R80 > 10 μm). The authors should mention these applications in their Abstract and Conclusions.

The first paragraph of the introduction explains the importance and relevance of the size range of aerosol discussed in this study. Nowhere is mention made of sea-spray impacts on heat, moisture, or momentum fluxes; explicitly stating that these measurements are not relevant to these processes seems a bit redundant. We don't believe it is critical to make this statement in the abstract, but the size range is now explicitly

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stated there.

2. Page 3362, line 7. Only saline droplets shrink until they reach equilibrium. Fresh water droplets can evaporate entirely. It's surprising how many people do not realize this difference. It might be good to point it out here.

I have included a statement at this location that clarifies this difference.

3. Page 3362, line 13. The < and > symbols are messed up.

Corrected to (0.1 < R80 < 5 μm)

4. Page 3366. The sentence that begins in line 5 with "During analysis" is pretty contorted and thus doesn't read well.

Text changed to: "During analysis a portion of each image was selected; this was chosen to exclude the region where the ship's bow wave might be visible, and cropped out the sky and the region of increased brightness close to the horizon."

5. With the discussion at the end of the first paragraph on page 3367, it might be good context to mention that, besides Wu (1993), the current study is the only one to establish the simultaneity of increases in aerosol concentration and wave crests.

Added.

6. At the bottom of page 3367 and elsewhere (e.g., page 3374), I am unsure why bubble spectra from Mårtensson et al. (2003) are at all relevant. These spectra came from a glass beaker with a volume of about 1 litre and were created by forcing air through a glass filter in the beaker. That is, these bubble spectra are totally artificial. Comparing them with your open ocean observations attributes much more meaning to them than they deserve. I'd recommend removing most of the discussion of the results from Mårtensson et al.

We agree with the reviewer about the artificial nature of the Mårtensson bubble spectra and that they are not relevant to open ocean conditions. However a number of mod-

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elling studies use the Mårtensson aerosol parameterisation derived from that bubble spectra – from discussions with modellers we are painfully aware that they are often completely unaware of the nature and shortcomings of the measurements upon which the parameterizations are based. We believe it is important to show just how different the artificial bubble spectra is to the open ocean data in order to emphasise this fact. We have modified the text slightly throughout to highlight the discrepancy between the lab and open ocean spectra and included a paragraph discussing the artificial nature of the Mårtensson bubble spectra in the section 3.2 Bubble Concentrations.

7. The discussion in the first paragraph in Section 3.3, which regards Figure 6, is hard to follow because of the faulty legend in Figure 6. Page 3369 mentions the SEASAW whitecap parameterization, but neither the legend in Figure 6 nor its caption helps us identify which line is the SEASAW parameterization.

The legend in the figure has been corrected.

8. In the line above Eq. (3), W must be the actual whitecap coverage (whitecap area per unit surface area), not the percent coverage.

Changed.

9. The approach described on page 3370 in the paragraph that starts in line 14 - multiplying the aerosol number concentration with the whitecap coverage and dividing by a time scale - is the same as used by Monahan et al. (1982, 1986). That history is worth mentioning here.

I've included that history in that sentence and added the reference.

10. The discussion in the first paragraph on page 3371 refers to Figure 7. This figure has two unique features. For droplets with R80 less than about 1 μm, the SEASAW data are above even surf-zone measurements. For droplets with radii above 1 μm, the SEASAW data have no wind speed dependence. In the second and third paragraphs on page 3371, the authors seem to be explaining the high values of the SEASAW data,

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but the logic seems backwards. If bubble concentrations were higher in the surf zone, wouldn't the spray generation be higher? I did not see that the authors even attempted to explain why they saw no wind speed dependence in the larger droplets in Figure 7.

We agree that with higher bubble concentrations you would expect the production flux to be higher in the surf zone. Note however that the open ocean and surf zone bubble spectra are not directly comparable. The open ocean spectra are all time averages, and not representative of the bubble plumes in an active breaker. The surf zone spectra include both active breaker only spectra (Deane & Stokes, 1999) and time averaged spectra (Phelps et al 1997), but even the latter is likely to be more representative of active breaker spectra due to the high frequency of wave breaking in the surf zone. None of the surf zone aerosol production flux estimates have coincident bubble measurements, so we can infer nothing about links between bubble spectra and aerosol production from them. A wind-speed dependence of aerosol production flux for a particular particle size relies upon a wind-speed dependence of the bubbles from which those particles are generated. At higher winds wave size increases so we can expect larger breakers entraining more air. Assuming a simple increase in bubble numbers, but a similar spectral shape, this will result in an increase in large bubbles rising rapidly to the surface and hence an increase in film droplet production. The small bubbles from which jet drops are generated rise only slowly and tend to remain in the water column. It is not obvious that a larger wave will necessarily result in more jet drops being produced within the active bubble plume...although we might expect more production outside the active plume as these bubble slowly rise to the surface over time. These would not be captured by our measurement of the production from an active whitecap.

We have completely revised, and extended the discussion of these issues.

11. As I mentioned earlier, I do not think the Mårtensson et al. results provide a meaningful comparison in Figure 7. Moreover, the only other comparisons in Figure 7 are to data collected in the surf zone (namely, Clarke et al. and de Leeuw et al.); and the authors themselves describe how whitecap, bubble, and spray conditions are different

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between the open ocean and the surf zone. Hence, why not compare apples with apples. The spray generation functions of Monahan et al. (1986, bubbles-only term) and Andreas (1998, 2002) are open-ocean expressions that are more comparable to the SEASAW observations. In my casual comparison, these and the SEASAW data are in the same range for R80 between about 1 and 10 μm, the lower limits of Andreas's functions.

There is some confusion here between production fluxes and source fluxes. We are comparing "apples with apples" in figure 7 (now 8), to the extent that all the functions shown are aerosol production fluxes per unit area whitecap; as far as we know there are no other studies that estimate the production flux in the open ocean. As suggested we have added the Monahan et al (1986) bubble only function to the figure. Note that this function was already included in Figure 8 (new figure 9). However, we further note that this is not an open ocean expression since it uses whitecaps generated in the laboratory to estimate aerosol productivity from a single whitecap. We have included a Figure A, a modified version of Figure 9, at the end of this response to the reviewer which shows Andreas (1998) and Smith and Harrison (1997) from which Andreas 2002 is a modified. It is hard to compare these source functions with our own as they focus on much large particle radius. We don't included Andreas (1998) or (2002) in the new Figure 8 as they do not use the whitecap method and do not present a production flux comparable with the other functions in the figure. While we agree that the Mårtensson results are not relevant to the open ocean, they are widely used, so merit inclusion so as to draw attention to their shortcomings.

12. Page 3371, line 16, mentions the 3-μm droplets of Mårtensson et al. in Figure 7, but this size is not apparent in Figure 7. I think the de Leeuw et al. result covers it.

The figure has been changed to make sure that the de Leeuw result does not cover any of the other curves.

13. Figure 2, panel e, is identified as the "total" particle number concentration. This

description is misleading because CLASP is able to see only droplets with ambient radii between 0.12 and 9.25 μm (see page 3365, line 2). Clarify this statement by including in the caption the range of sizes that the CLASP measures.

Done

14. Figure 3, again, is the “total” number concentration; but as with Figure 2, these totals include only droplets with radii from 0.12 to 9.25 μm. Again, mention the CLASP range in the caption.

Done

15. In Figure 6, Callaghan et al. (2008) are mentioned in the caption but not identified in the figure. Moat et al. (2011) are mentioned in the legend but not identified in the caption or in the text. Moreover, Moat et al. is not listed in the references. What is the gray line in the figure? It isn’t identified either.

The legend has now been sorted out to make sure all the curves are clearly identified.

16. In Figure 7, the orange line is identified as 20 March; but in Figures 4 and 8, the orange line is labelled 30 March. Are these labels correct?

Yes it should be the 30th March not the 20th. It has been changed in the legend of the figure.

All the below have been applied or corrected. Here are some proofing and other editorial issues.

17. The manuscript contains some long paragraphs. I’d start new paragraphs here: Page 3368, line 6, with “For the smallest.” Page 3369, line 2, with “The variability.” Page 3372, line 22, with “For aerosol.” Page 3372, line 29, with “At large particles (sic) sizes.”

18. Page 3361, line 9, “bubbles rise”

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19. Page 3363, line 26, “measurement.”

20. Page 3372, lines 22–26. This is a long and convoluted sentence. Rewrite.

21. Page 3373, line 3. “Micron” is no longer an acceptable S.I. unitâĂŤuse micrometer.

22. Page 3373, line 5, “is matches”?

23. Page 3373, lines 4–8. This is another messy sentence that needs rewriting.

24. Page 3373, lines 20–24. Rewrite this sentence into shorter segments.

25. Page 3374, lines 17–21. Rewrite this long sentence.

26. Page 3375, line 19. By “the shelving beach,” do you mean the shoaling beach? I don’t know what a shelving beach is.

Interactive comment on Ocean Sci. Discuss., 9, 3359, 2012.

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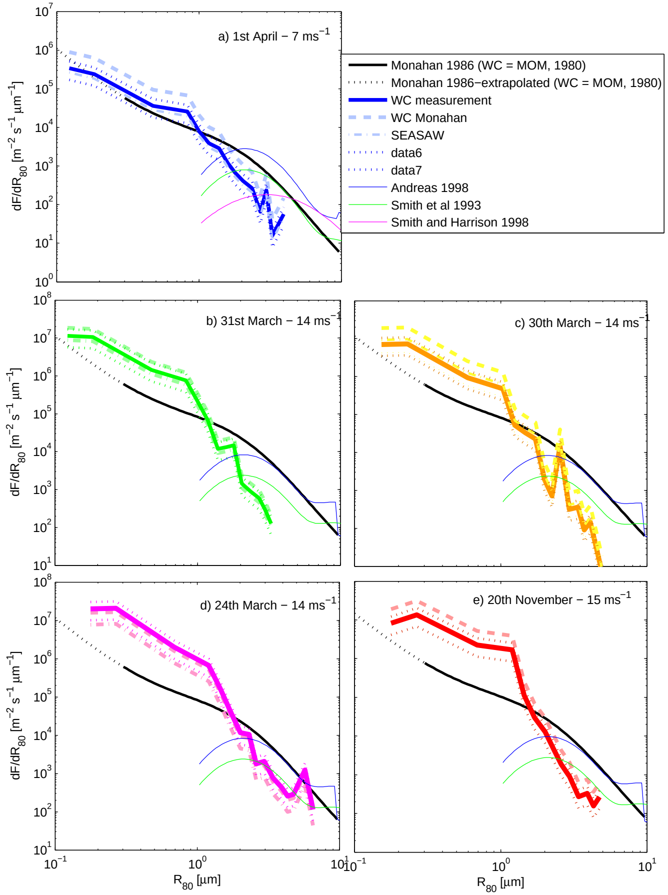

Fig. 1.

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