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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Eddy covariance measurements of sea spray particles over theAtlantic Ocean" *by* S. Norris et al.

Anonymous Referee #1

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Review of

Eddy Covariance Measurements of Sea Spray Particles over the Atlantic Ocean MS-NR: acpd-2007-0356

by S. Norris, I. Brooks, G. de Leeuw, M. H. Smith, M. Moeman, and J Lingard

This manuscript presents some of the first measurements of the flux of sea spray droplets from the ocean surface obtained by direct–i.e., by eddy correlation–measurements. As such, the manuscript is relevant to the scope of Atmospheric Chemistry and Physics and is a potentially valuable contribution to the literature. As the manuscript stands now, however, it still needs a lot of work. Important information is missing, unimportant information is included, and the editing and proofreading is careless.

1. Andreas et al. (2001) plead for care and precision in expressing the sizing convention in discussions of sea spray droplets that are always changing size. This paper does not mention what sizing convention it uses. The aerosol sampler being used, the CLASP, of course sizes the droplets it actually observes. To make their spray generation function comparable to other published forms, the authors must convert the CLASP's size bins to some standard sizing convention. In their Figure 6, they seem to have chosen r80, a symbol which they never define. (It's, presumably, the radius a droplet would have if it were brought to equilibrium at a relative humidity of 80%.) But the several functions represented in Figure 6 are based on several different sizing conventions in the original papers. How have the authors converted these source functions to a common sizing convention?

I do not believe the CLASP was generally seeing droplets with radius r80 because, according to Figure 4, the relative humidity for most of the observing period was well above 80%. The authors therefore need to explain what droplet sizing convention they are using and how they converted the size bins they observed to this sizing convention.

2. The authors are also imprecise in defining their spray generation function. In equation (1), they use F to refer to the flux of spray droplets. In Figure 5, they use dF/dr to denote the "flux." In Figure 6, they use dF/dr80 for the "sea spray source function." For these latter two, they present dF/dr without definition. Finally, on the vertical axis in Figure 2, the authors simply plot the "Flux," but it has the unconventional units of per cubic centimeter per millisecond.

3. Let me see if I can get this right. The CLASP draws sample air from near the sonic down a 0.5-m-long tube at a rate of 50 cmˆ3/s (volume of air per unit time) into the sampling chamber. Here, the droplets are categorized into eight sample bins on the basis of the intensity of scattered light (droplet counts per bin per unit time). These counts must be correlated with the turbulent fluctuations in vertical velocity measured

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by the sonic. Finally, the authors must convert the measured radius to a standardized radius. Therefore, to get the spray generation function–let's use the standard notation dF/dr–the authors must make the following conceptual calculations:

dF/dr (for one size bin)

= (droplet counts/bin size/time)(time/volume of air)

x(vertical velocity fluctuation)(measured radius/standardized radius)

= (1/micrometer/s)(s/cubic meter)(m/s)(micrometer/micrometer)

= 1/square meter/s/micrometer

Four of the terms in the second line are problematic as the paper now stands. As I mentioned, it does not tell us its radius convention; so the last term in the second line above is unknown to us readers. It does no mention the bin sizes, although Table 1 lists six of the eight size bins. But the table doesn't tell whether these are central sizes in the bins or upper or lower limits. In term one, we need the bin width. Also, what happened to the other two bins, up to 3.5 micrometers, mentioned on page 13248? Clearly, any uncertainty in the bin size affects the uncertainty in the computed spray flux. Finally, the volume flux of air through the sampler–the second term in the second line–is given to only one significant figure, 50 cmˆ3 sˆ-1, on page 13248. (Erroneously written on page 13248 as 50 cmˆ-3 sˆ-1.) That is, this term seems to be uncertain to +/-20%; the spray flux must then also be uncertain to at least +/-20%.

4. The size range covered by these measurements is relevant to cloud physics and the ultimate affect of clouds on climate, as the authors explain in their introduction. I found much of the review in the second and third paragraphs of the Introduction, however, to be unnecessary. The discussion of stage A and stage B whitecaps, for example, is unnecessary because the manuscript does not say any more about whitecaps. The discussion of spume at the top of page 13246 is also unnecessary and could be misleading because the measurements presented include no spume droplets.

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5. I am pleased that the authors also formulated their spray generation function in terms of the friction velocity u* (top of page 13254 and in Table 1). As they imply, u* is more dynamically relevant than the wind speed because its use obviates the need to worry about a reference height for wind speed, stratification effects, and wave state. Here, though, the authors need to confirm that u* came directly from their eddy correlation measurements from the sonic and not indirectly from wind speed and a drag coefficient, as is typical practice.

6. I cannot understand why Section 3 opens with a discussion of Taylor's hypothesis. As far as I can tell, the authors had no reason to invoke Taylor's hypothesis. All of their measurements are of time series, and their spectral analysis represented in Figure 2 is of a frequency spectrum. They show no material in which they converted their time series to a spatial lag or to wavenumber space. Hence, they can omit the opening paragraph in Section 3.

7. The discussion of averaging period in the second paragraph in Section 3 is useful. Computing ogives is a clear way to evaluate how long the averaging must be (see Figure 2). I would tend to quibble, though, that the ogive traces in Figure 2 do not all seem to "level off" after 20 minutes of averaging, as the authors claim at the top of page 13251. In fact, since these traces stop at 20 minutes, it's hard to judge just what the behavior is around an averaging period of 20 minutes.

8. I've never heard of an instrument called an aethalometer (page 13249, line 16). Explain what it does.

9. The manuscript also contains some other undefined acronyms and abbreviations. For example, PMS, FSSP, OAP, and PCASP on page 13249, lines 15 and 16. DMS on page 13251, line 11. OMI on page 13251, line 22. ASASP-X on page 13254, line 27.

10. While I applaud the authors' attempts at an error analysis on page 13254 and in Table 2, that analysis is only marginally successful because the authors don't explain very well the terminology in Table 2. For example, in Table 2, the Instrument Error in the

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reported droplet flux is given explicitly as a percentage of the droplet flux; but the Statistical Error is given only as a percentage. A percentage of what? Why is one column explicit and the other three are not? Do we get the expected overall uncertainty in the reported droplet flux by summing the Instrument Error and the appropriate Statistical Error? Figure 2 includes droplet radii up to 2.25 micrometers, but Figures 5 and 6 and Tables 1 and 2 fail to include this size range. Why not?

11. A related point is that Figure 6 shows surprisingly good agreement of the current measurements with published spray generation functions. But as I have been hinting, the current analysis contains a lot of unspecified parameters: see item 3 above for some examples. We can therefore not be sure whether the authors properly specified the width of their radius bins and did their radius conversions correctly. Maybe when they saw the initial plots, they saw the good agreement and did not implement any of these other conversions for fear of changing the agreement. For example, they do admit in the last paragraph of Section 4 to not converting their measurements, made at 16.5 m, to a standard reference height such as the surface or 1 m or 10 m. Although this height conversion would seem to have a negligible effect for the droplet sizes involved, without explicit discussion of some of the other steps in the analysis, we cannot be sure whether the good agreement in Figurer 6 is real or the result of selective data handling.

12. In Figure 1, I do not see the black cube mentioned in the caption. I see a black rectangle, but not a cube.

13. In Figure 6, some of the legend designations do not have lines associated with them: namely, Vignati in panels a and b and de Leeuw in panel c. Why?

14. The grammar, the punctuation, the editing, and the proofreading in the manuscript are poor. None of the six coauthors must have read the final version; if they had, these errors couldn't have persisted. To me, this carelessness is a real concern and basically undermines the work. Publishing the results of a scientific endeavor is one of the most important activities of any scientific project. But if a scientist is careless in completing

this part of a project, it is logical to assume that he or she was equally careless in making the measurements and doing the analysis on which the report is based. Here are some of the errors in editorial quality that I found.

15. One of the authors is G. de Leeuw. But throughout the text and in the references, he is referred to equally as De Leeuw and de Leeuw.

16. The manuscript cites a paper by lead author Mårtensson. But the cited reference is given as Martensson; and, in the text, the paper is variously attributed to Martensson (e.g., page 13255, line 14), Mårtensson (e.g., page 13255, line 25), Martenssen (page 13244, line 26), Mãrtensson (page 13246, line 21), and Martesson (Figure 6).

17. At the bottom of page 13248 is a footnote citing a manuscript in review. The lead author is Hill, but the other five authors seem to be the same as five of the six authors of this manuscript. But maybe not, because the Brooks in the footnote is B. Brooks, while our author is I. Brooks. A typo, or two Brooks coincidentally working in the same field?

18. The abstract says that the size range of droplets being reported is 0.15 to 0.35 micrometers, but the top paragraph on page 13248 says the radius range is 0.15 to 3.5 micrometers.

19. Here are some cases when subject and verb do not agree.

Page 13245, lines 11-12: . . . whitecap; this has a small surface area and are rich in bubbles . . . .

Page 13254, line 28: . . . the CLASP spectra is not consistently higher . . . .

20. Rule 7 in Stunk and White (1972) is "a participial phrase at the beginning of a sentence must refer to the grammatical subject." Opening Section 3, we find

When taking spot measurements as a function of time in a turbulent field, Taylor's hypothesis is often used.

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Obviously, Taylor's hypothesis is taking measurements. Illogical!

21. Several cited works are not listed in the references. Here are some.

Page 13250, lines 19 and 23: Massman (2002)

Page 13252, line 7: Slinn and Slinn (1982)

Page 13255, line 22: Lafon

Page 13256, line 9: Lewis and Schwartz, year missing

22. Moreover, here are some problems with the references.

In the title of the A. D. Clarke reference, the word before the colon is waves, not wave.

The lead author of the Frederickson reference is P. A. Frederickson, not A. Frederickson.

23. The manuscript has lots of problems with punctuation and sentence structure. Here are some examples. I show in square brackets punctuation that I recommend be added (i.e., [,]) and in parentheses punctuation that should be removed [i.e., (,)].

Page 13244, line 18ff.

Sea salt aerosol are the single most important factor controlling the scattering of radiation(,) and[,] hence[,] the radiation budget near the surface(,) over the oceans (Haywood et al., 1999)(;)[.] [T](t)hey dominate the particulate mass concentration in unpolluted air(,) and contribute approximately 44% . . . .

Page 13247, line 16ff. One sentence, sixty-four words, three commas. Rewrite!

Such direct measurements of sea spray fluxes have been possible very recently, as instruments with sufficiently high sample rates have become available, but remain sparse due to the bulky and expensive nature of most of the aerosol instrumentation, which makes it awkward to use close to the sea surface without either causing flow distortion or risking damage to the instrumentation in a hostile environment.

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Page 13248, line 1ff. Something's missing.

The objective of this paper is to demonstrate the feasibility of size-resolved sea spray flux measurements using the eddy covariance method with a novel instrument developed at the University of Leeds couples a high sample volume to fast time response.

Page 13251, lines 15ff.

DMS production can occur in October on the east US coast(,)[;] however[,] there were no plankton blooms in the area during the field campaign, and chlorophyll concentrations mapped by the NASA Terra satellite were below 1 mg m^-3(,). (w)[W]e thus conclude there was limited biological activity to sustain local DMS production.

Page 13256, lines 11ff. Something's missing.

The results were not corrected to another height because the log wind profile to be very steep due to the very low roughness length over the ocean[;] (and,) thus, any correction of wind speeds for height difference will be very small.

Page 13257, Acknowledgements.

The work presented in this article is part of the PhD thesis work of Sarah Norris(,) [and] was supported by TNO, The Netherlands, through the TNO OIO funds.

And many, many other examples.

24. Another common language problem is the misuse of adverbs. Here are examples.

Page 13245, line 29: . . . in situations 'where' the wind stress . . . .

Page 13250, line 19: They can be identified as periods 'where' the time series . . . .

Page 13253, line 11: Only periods 'where' the instruments . . . .

Situations and periods are temporal concepts; the where in each case should be when.

REFERENCES

Andreas, E. L, M. J. Pattison, and S. E. Belcher, 2001: "Production rates of sea-spray droplets" by M. J. Pattison and S. E. Belcher: Clarification and elaboration. J. Geophys. Res., 106, 7157-7161.

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