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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Gradient flux measurements of sea-air DMS transfer during the Surface Ocean Aerosol Production (SOAP) experiment" *by* Murray J. Smith et al.

Anonymous Referee #2

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It is a pleasure to read such a clear and well-written manuscript. The authors present a detailed comparison between eddy covariance (EC) and gradient flux (GF) measurements of DMS flux from the southern ocean during the SOAP cruise. The introduction is very thorough and can bring most readers up to speed even if they are not familiar with some or all of the experimental and theoretical details. The experiments, analysis, and discussion are all very clearly presented, and the conclusions appear to be sound. With a paper like this, the reviewer's task becomes not "is this paper any good?" but rather "What (if anything) can I suggest to improve the manuscript?"

The one thing I expected to see in this paper is some discussion of the role that surfac-

tants might play in air-sea gas exchange. There were one or two places where surface buildup, microlayer enrichment (P. 15, l. 9) or near-surface gradients (P. 3, l. 13; P. 4, l. 32) were mentioned, but these seemed to refer more to the buildup of DMS itself rather than surfactants. Certainly in phytoplankton blooms in the nutrient-rich southern ocean there exists the possibility of large multi-functional organic molecules accumulating at the surface and influencing exchange across the surface microlayer. See Pereira et al., Biogeosciences, 13, 3981, 2016 for a recent reference; many earlier publications (quite a few of them referenced in that one) have also suggested that surface layer composition can have a significant impact on air-sea flux of trace gases. This should affect the EC and GF results roughly equally, and not significantly change the conclusions of the intercomparison between methods. But it will not be accounted for in the COAREG parameterizations, unless the surface layer were thick enough to affect the measured turbulent energy dissipation rate. (Which seems unlikely, and epsilon only appears in Eq. 4 to the $\frac{1}{4}$ power anyway.) In contrast, the discussion of the role of air bubbles is informative and seems complete.

During periods of atmospheric stability, the agreement between the two experimental methods but disagreement with the COAREG parameterization, is intriguing. Is it worth extending (maybe not in this paper) this analysis to include previous EC or GF gas flux measurements during periods of atmospheric stability? (Is there something fundamentally different about stable conditions, or is it just particular to these results?) Figures 5d and 7 show the discrepancy very clearly for this cruise.

Some more detailed and technical comments:

Abstract, l. 14 Is east of New Zealand really the southwest Pacific? I had always considered 180 E or W to be the central Pacific, but that is in the tropics. Not important at all; if the study location is generally considered to be the southwest Pacific then don't change it.

l. 16 might want to define "API" = atmospheric pressure ionization

P. 3, l. 34 hyphenate Monin-Obukhov

P. 4, l. 9, Is “closeby” a word? (Two countries divided by a common language?)

l. 14, Might want to mention that Liss and Merlivat is one of the earliest parameterizations of K, or something like that. Or maybe everyone knows that already.

l. 14 I really don’t think “trialled” is a word. How about “performed trials” or “tried”? (See also P. 16, l. 31)

l. 21 “gas transfer parameterizations”?

P. 5, l. 17 “crowsnest” or “crow’s nest”?

l. 21 “ship’s”

l. 22 “Ecotriplet b660 backscatter” is a bit of jargon. Simplify (“backscattered light”), expand (to say what it’s for), or delete?

P. 6, l. 7 “LES” is defined twice.

l. 22 and 25, Since the integrated stability function depends on z, don’t you need to find the slope of the concentration profile vs. (ln(z) - Psi(z/L)) in order to calculate C*/kappa? (And Figure 4 shows that you did that too.)

P. 7, l. 23-26, Was the same aqueous DMS concentration used for both EC and GF analyses of K? (Which is perfectly reasonable, but means that intercomparisons of K between the two methods depend only on measured fluxes. The discussion of fetch effects in 4.3 is fine, no need to add much or anything here.)

P. 9, l. 14. I’m not sure how much validity there is in Rˆ2 for only four data points. (Just an opinion; no changes needed.)

P. 10, l. 10-12. Would a plot of u* vs. u* for the two methods be useful? Probably just the summary here (along with Figure 6) is sufficient; I suppose the authors have already done this.

P. 11, l. 10, stratification leads to high fluxes? Walker et al., 2016 mention surfactants as possibly contributing to suppressed ventilation until winds pick up. But fluxes would be low until this occurs.

P. 12, l. 19 Could real-time motion correction be made for REA? (This doesn’t need to go in the paper; just curious.)

P. 13, l. 3, “data are sparse”

P. 14, l. 29 “suppresses”

P. 15, l. 4, “near-surface”

l. 9, Could this be a place where surfactants are playing a role? (K from EC and GF lower than from using COAREG.)

l. 14, need to define K660

l. 18 “so they could have [had] a”

l. 19-21 True, there could be a bias. But if there were enhanced DMS at the ocean surface, it would lead to higher flux, and higher calculated K if using the bulk [DMS] at 5-6 m depth.

P. 16, l. 27, “The GF experiment/sampling equipment/whatever was deployed. . .”

P. 17, l. 18 First grant # looks like it is missing some digits.

Figure 1c. What is height above surface (or water level) on the spar buoy after it is deployed?

Figure 4 As mentioned earlier, the x-axis label text has the stability adjusted height. Might want to put something about the error bars in the caption (it is already in the text), because they are different between top and bottom even though they are both for DMS concentration or mixing ratio. Also would be good to make the tick labels, axis labels, and subplot titles a little larger. They are at the limit of readability now.

Figure 6 “Gradient Flux”? (y-axis label)

Figure 7 – put the arrow and DOY53 inside the axes or somewhere else. Right now they interfere with the x-axis label.

Figure 9 – also plot K-EC vs. K-COARE? And/or K-EC vs. Ke? This may or may not be informative. Figure 9 is pretty clear as is. And there is no need to make this paper any longer.

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