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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Open ocean DMS air/sea fluxes over the eastern South Pacific Ocean" *by* C. A. Marandino et al.

C. A. Marandino et al.

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Reviewer comments are noted by RC-. Author responses follow the comments and are denoted by AR-.

Reviewer: B. Huebert

RC-In both atmospheric and water cases, they chose to add isotopically labeled standard after all the sampling apparatus and just before entry into the APIMS, so the cal gas could not have identified nor corrected for any inlet or equilibrator problems. An example is the biofouling they observed in productive waters.

AR-In the case of air sampling, the isotopically-labeled standard was added at the air intake on the bow. Therefore, the internal standard experiences the same line losses as the ambient air. The manuscript was not clear on this point, and has been modified (p12085, line 7). In the case of seawater sampling, the reviewer is correct that the internal standard was added after the equilibrator. In this case, the internal standard was used only for quantifying the response of the APIMS. Adding the internal standard prior to the standard could help account for variations in equilibrator temperature, seawater flow rate, and possible incomplete equilibration. In this system, temperature and flow rate were measured continuously. There are two possible effects of biofouling. One is spurious production of unlabeled DMS from biological activity in the equilibrator. Adding the internal standard prior to the equilibrator would not address this issue. A second issue is a reduced rate of gas transfer across the equilibrator membrane due to the presence of biofilms. Adding the internal standard before the equilibrator would help identify this potential problem.

RC-My only negative comment concerns significant figures, at the start of 3.2: It makes no sense to say a flux is 3.87917 when the error bounds are 2.5 and 5.5. One digit after the decimal would be plenty.

AR-The reviewer is clearly correct. The significant figures have been corrected.

Reviewer: A. Soloviev

RC-There is some difference in the wind speed dependence of the Knor-06 data with other DMS data sets (BIO, H04, Phase I) in Figure 7. According to Marandino et al. (2008), the difference in sea surface temperature among these cruises cannot explain the difference in the gas transfer coefficient. Other possible causes of the difference, which are not mentioned in Marandino et al. (2008), are as follows: effect of surfactants, diurnal cycling, wave age dependence, and different experimental setups.

AR-These effects are addressed on p 12091, line 14. The differences in wind speed-dependence of k among these studies may reflect real differences in gas exchange in the different environments sampled, due to variations in microlayers, boundary layer dynamics, wind wave interactions, etc. We have modified the sentence to explicitly include wage age dependence and diurnal variations in boundary layer stability. Possible

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differences due to methodological differences are addressed in the remainder of that paragraph.

RC-...it would be appealing to bin by wind speed the Knor-06 data separately for high (equatorial upwelling, subpolar water, and coastal waters off Chile) and low (the gyre region) bio-productivity zones.

AR-We agree that binning the data by water mass type could provide insight into the factors controlling variability in gas transfer coefficient. Unfortunately, the amount of data from this study is so limited that the statistical uncertainties would render the comparison unsatisfactory. We hope to generate sufficient data for such comparisons in the future. Binning by water mass types across all of the DMS flux studies would also be interesting, but outside the scope of this manuscript.

RC-The dependence of the gas transfer coefficient on wave age seems to be the only remaining explanation for the difference between the different data sets in Figure 7 (of course, provided that the observed differences among different data sets are not a result of somewhat different measurement methods). A dependence of the interfacial component of gas transfer on wave age follows from the Soloviev (2007) model. Verification of this hypothesis would, however, require collecting data on the stage of surface wave development.

AR-The reviewer notes that the difference between DMS gas exchange studies occurs at all wind speeds, and argues that variations in wave age are a more likely explanation than microlayer or diurnal effects which are mainly at low wind speeds. This is a good point and we have added some text to this effect (p12091 line 16).

RC-Finally, it should be noted that a realistic parameterization for the DMS exchange should also account for the concentration difference at the air-side of the interface (which is negligible for most of the other gases less soluble than DMS). To the best of my knowledge, such parameterization has not yet been developed.

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AR-Under the conditions of the field studies discussed in this paper, the ocean is highly supersaturated, and air side DMS concentration does not exert a significant effect on the air/sea concentration gradient. The DMS flux studies do take the air-side concentration into account in computing the gas transfer coefficient. As shown by McGillis et al., 2000, under high wind speed, low temperature conditions, air-side resistance of DMS can be significant, and the DMS gas transfer coefficient needs to be resolved into ka and kw components.

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