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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Closing the Dimethyl Sulfide Budget in the Tropical Marine Boundary Layer during the Pacific Atmospheric Sulfur Experiment" *by* S. A. Conley et al.

Anonymous Referee #3

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Conley et al present a very well thought through and analysed investigation of the DMS budget in the tropical Pacific. They estimate, based on airborne measurements during 14 research flights, the terms of the DMS budget identifying photochemistry as the main loss term. Nevertheless, the heterogeneity of the oceanic DMS source makes advection important as well as loss to the overlying buffer layer. This is a very well written paper and I have only minor comments. The variability in the presented data is very large, up to several ten percent, and the correlations (e.g. figs 9 and 11) are not always very strong. How reliable are flux determinations under these circumstances? Would it maybe be better to give ranges of fluxes rather than average values? Also, I missed details on the errors. Few figures show error bars, but I assume they are rather

substantial, please add and explain.

Specific comments

Throughout the paper the readability would improve if there was a clear distinction between scalar and vector properties, for example by using arrows or bold type setting for vectors.

p. 17267, eq. 1: no "." in brackets

p. 17270/17271: I didn't quite understand what this "model" is really used for. Firstly I would prefer the term "fit" as this is what seems to be done. Also I wouldn't share the optimism of the authors about the quality of the fit. Are the concentrations derived from this fit what is later used in the budget calculations? If so, it would be helpful to state this explicitly and explain in more detail. If not: what do we learn from the use of this fit ("model")? Further I don't understand why a reduction in variability at night should diminish the quality of the fit, could you explain that in more detail please?

p. 17274, l. 17: is there a word missing around "Fig. 5"?

p. 17276: One conclusion for the chemistry of DMS in this paper is that OH is the only required oxidant. Yet this is solely based on data from "personal communication" which are not presented in this manuscript. What is the day to day variability? Can the measured OH concentrations explain the chemical oxidation of DMS on all days?

p. 17276, l. 23 - 26: Could you please clarify the direction of the entrainment fluxes? I assume that DMS is being entrained all the time INTO the buffer layer but the text could be understood such that air is actually being transport downwards, i.e. entrained into the MBL. Please clarify.

p. 17277, l. 20: I assume that you refer to the vertical flux divergence term are the interface between MBL and BuL - maybe clarify. Also: what about small scale convection as source of DMS for the BuL? Were the conditions during the flights such that this could always be excluded?

p. 17281, l. 3: Please add degree symbols at beginning of line.

References: Please check the capitalisation. Earlier versions of the ACP bibtex style had a problem with this which seems to have been solved in the meantime.

Tables

Table 2:

On two flights (numbers 6 and 14) the flux of DMS into the buffer layer seems to be larger than the surface flux of DMS - how is this possible (it doesn't seem to add up when using advection and chemical loss)? Or is this simply due to the large errors associated with the fluxes? I think the dteailed errors should be given in this table as well and not just the net error.

Caption: please change "umol" to "μmol"

Figures

Figure 1: The vertical profiles of Θ and q are much more smooth than that of DMS. Is that caused by experimental issues or is the variability of DMS really that large in a well-mixed MBL (telling from the Θ profile)?

Figure 4: What is the unit of the y-axis?

Figure 6: Please check the 2nd line of the caption: what does LBL1.1 mean and what is the meaning of the 36 to 25 ratio?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17265, 2009.