

## ***Interactive comment on “Oceanic emissions unlikely to account for the missing source of atmospheric carbonyl sulfide” by Sinikka T. Lennartz et al.***

### **Anonymous Referee #2**

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I find it striking that such broad generalizations are made from data that cover a fairly small portion of the global ocean. The authors have a great deal of new observations to address the important issue of quantifying OCS fluxes from the ocean. But to draw broad conclusions without considering the previous data more carefully is inappropriate. Their data add to the picture in useful and interesting ways. To "conclusively answer the question of whether the missing OCS source... can really be ascribed to the direct OCS emissions from tropical oceans" would seem to require another level of effort that isn't yet part of this manuscript.

I realize that the model helps the authors extrapolate their results to broader scales, but the results derived are only as good as the data considered by the model. If the

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authors really hope to be able to "constrain the variability of OCS emissions in the tropic[al ocean]" then I would think they would have to consider the details of previous ocean-going measurements (ocean basin, ocean regime, season, etc.) together with their new data to determine if, in fact, that most of the major global ocean regimes have been adequately sampled to allow such a conclusion. For example, it is informative and important that their results are consistent with an upper flux limit from Kettle et al., 2002, but no mention is made of how that consistency actually increases our understanding of total OCS flux from the ocean. If the sampled regions were similar, then this is a confirmation of that original estimate, but potentially not much progress in understanding the broader-scale contribution of the ocean to atmospheric OCS abundances. Suggestion: scale back the broad conclusions and focus on your results and how they compare with others, or consider the broader literature on OCS flux measurements (observation-based and model-derived) in a more detailed fashion to determine if the available data allow an accurate quantification of the global and, most importantly, tropical OCS flux from the ocean.

On uncertainties. Any comparison of derived oceanic fluxes with a shortfall needs to fully consider uncertainties. Many uncertainties are discussed (air-sea exchange, mixed layer depth, parameterization of production, etc.,) but aren't explicitly included in the derivation of the direct flux of 130 GgS/yr and in the discussion of the global budget discrepancy (no uncertainty is provided on this number). Similarly, the origin of stated uncertainties in the derivation of indirect fluxes from DMS are not discussed, but I would imagine are substantially larger than estimated. Without a fair treatment of these uncertainties, it isn't possible to gauge the true magnitude of the budget shortfall, which is a main point of the manuscript.

Details: P1, line 7-8. It only has implications for GPP derived from OCS observations on certain scales, not all.

p.2, line 25-26. It also makes much more chemical sense given our understanding of how COS interacts with carbonic anhydrase in leaf waters.

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Table 1 and p. 9 line 27-30. The paper from Suntharalingam does not state or suggest that the missing source is oceanic.

p.3 line 13. Underrepresentation of global flux is also true for the measurements made here, despite the use of a box model for extrapolation.

Figure 3, and line 10, p. 9. Is  $R = 0.7$  or  $R^2 = 0.7$  in this relationship? These different values seem given for one relationship. Also, given the non-normal distribution of these results this value is not as significant as one might presume.

The conclusion section is a bit unusual in that it includes speculations not supported by the work. It also reads like a research planning document. On what basis do the authors conclude (p. 11, line 22) that observations could be reproduced “without increasing the vegetation sink”? An extensive body of recent literature has suggested that the interaction between OCS and vegetation is best explained by a substantially larger sink than discussed in earlier papers; to discount those studies without substantial evidence is inappropriate, making this conclusion one that does not follow from the evidence presented in the paper.

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