

Interactive comment on “Monthly resolved modelled oceanic emissions of carbonyl sulfide and carbon disulfide for the period 2000–2019” by Sinikka T. Lennartz et al.

Anonymous Referee #1

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In general, this is a good and useful paper that provides a clear product for the modelling community: a monthly time series of OCS and CS₂ fluxes from the ocean in the period 2000–2019. I have a number of suggestions that might improve the paper.

The basic formulas underpinning the model are presented in equations 1, 2, etc. I tried to grasp these formulas, but quite some details are missing. I am not suggesting to repeat the information from previous papers, but a full mention of units would be very helpful. For instance, equation 2: the non-trivial unit for the photochemical rate constant p (pmol per Joule, see Lennartz (2017)) has to be derived from the other units (which are given). I would be good to provide all units clear. Also, the link between the

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Discussion paper



main text and the figures and table could be improved (e.g. the assumed atmospheric mole fractions for OCS and CS₂ are given only in a table).

The abstract could better reflect the method used in the paper, and should also mention the data that drive the model. . . something like, we use a 1D model of the ocean' mixed layer driven by ERA5 data from ECMWF and CDOM from MODIS. . . For "temperature" the ocean skin temperature is used. I agree with this choice, because it given information about the sea surface temperature. I still wonder, however, how sensitive the results are for alternative choices, such as Sea Surface Temperature from ERA (see e.g. Luo, B.; Minnett, P.J. Evaluation of the ERA5 Sea Surface Skin Temperature with Remotely-Sensed Shipborne Marine-Atmospheric Emitted Radiance Interferometer Data. *Remote Sens.* 2020, 12, 1873.).

I find the analysis of the "drivers" of flux variability in table 3 not very well described. I am a bit surprised that this analysis is performed on "global" and "yearly" data (monthly time-series of the global variables appear in figure 5). Although I clearly see that years with high CDOM are also years with higher emissions, I wonder if the global/yearly scale analysis if the most appropriate here. At least is should be made clear how the data are averaged (area-weighted?). But it might be more revealing to present a regional analysis.

Besides these points, the paper reads well, and provide interesting points of discussion. In reading the paper, I have annotated the pdf, which I include for further and minor remarks.

Please also note the supplement to this comment:

<https://essd.copernicus.org/preprints/essd-2020-389/essd-2020-389-RC1-supplement.pdf>

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-389>, 2020.