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 Lennarz (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
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 gives 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 is the most appropriate here. At least it 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

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