

## ***Interactive comment on “Marine carbonyl sulfide (OCS) and carbon disulfide (CS<sub>2</sub>): a compilation of measurements in seawater and the marine boundary layer” by Sinikka T. Lennartz et al.***

### **Anonymous Referee #1**

Received and published: 9 November 2019

This data set will fulfill a need and make a valuable contribution. I listed some comments and suggestions below as they appear in the manuscript.

Page 2, line 10: There is good reason to think atmospheric OCS has not been stable for the past four decades. The available firm air measurements suggest an OCS decline in the atmosphere through the 1980's and 1990's (Montzka et al., 2004). Many of the data sets in this compilation are from the 80's and 90's. The impacts will likely be limited to the interpretation of the atmospheric data and the atmospheric change is on the order of 10%. This is not a science paper and the possible standardization and calibration issues between labs are probably on the same order, so this is not a major

C1

issue. However, a brief cautionary note to the potential end users of the data product is necessary.

Page 3, line5: Would be nice to add a column to Table 2 or 3 indicating the presence/or not of the met or other physical data.

Page 4, line 8: In the Excel and text files that I downloaded, I see only NaN notation. Are these for missing or N/A data? Please include explanation in the manuscript.

Page 4, line 21: It should be “e.g. with liquid N<sub>2</sub>” because the cryogen is not always liq. N<sub>2</sub>.

Page 6, line 23: From what I see in Fig. 3e, about half the data have longer than 4 hr resolution and therefore not appropriate for diurnal work based on the criteria used in this manuscript.

Page 6: Carbonyl sulfide in seawater It is interesting that concentrations are higher in the southern hemisphere (Fig. 3a). Is this expected from a mechanistic perspective? Can it be because a larger fraction of the SH measurements is from regions of high OCS production? I'm also puzzled by Fig. 3c, in terms of two NH mins and when they appear.

Page 7, line 4: The figure reference should be to Fig. 3c.

Page 7, line 9: Better to be more specific and say “seasonal variability” instead of “temporal variability” here, I think. There are a few more instances in the paper where the same change would work better.

Page 9: Carbon disulfide in the marine boundary layer There are data from the north Pacific too (Fig. 2). Anyway, a 40+ ppt average in the boundary layer sounds pretty extreme for CS<sub>2</sub>. The global coverage is pretty poor as pointed out in the manuscript. It would be useful to put this number in context with data from flask networks that see marine influence if possible.

C2

Page 10: Recommendations It would be nice if the authors also commented on priorities (location wise) going forward with the ocean going OCS and CS2 measurements. Where do we need the measurements most in terms of improving the ocean OCS source? For example, there are no data from the Arctic ocean, but there is also precious little from the vast southern Pacific.

Table 2: Cruise ID#27 should include CS2.

Fig. 1: Hard to match the lines representing the cruises to what is in the legend because a lot of the detail gets lost. The points are too small to be seen easily.

Fig. 3: Check b) and d) labels.

Fig. 6: When I look at the cruise ID#7 that has both ocean and air side CS2 measurements in Fig. 1, I see a cruise that starts in Europe and ends in South America. However, I do not see boundary layer data in Fig. 2d from this cruise, although the cruise is listed in Fig. 6b.

---

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2019-162>, 2019.