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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new model for the global biogeochemical cycle of carbonyl sulfide – Part 1: Assessment of direct marine emissions with an oceanic general circulation and biogeochemistry model" *by* T. Launois et al.

Anonymous Referee #1

Received and published: 11 September 2014

General Comments: This study is a timely and valuable contribution to our understanding of the budget of OCS. I agree with all of the comments of Referee #2 and will not repeat them here but I would like to bring up a few more points:

Overall, there is a lot of discussion of what is higher/lower but not enough quantification of the disagreement nor a discussion of the causes.

I wonder if the overall range in OCS emission (which seems unreasonable large) could be better defined by constraining the model to the observation of OCS concentrations

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that have been mentioned. There are many unphysical emissions discussed in the text and the range seems larger than will be useful in future work.

Specific points (generally excluding Referee # 2 points): Pg 20679, line 25: How much have the soil and vegetation fluxes changed since the Watts or Kettle budgets? Montzka et al (2007) and Suntharalingham et al (2008) updated the budget before Berry et al 2013.

Pg 20679, line 26:"Berry et al suggests" Pg 20679, line 29:"levels" -> mixing ratio

Pg 20680, line 6:All of these references are modelling studies. You should reference the actual DMS-> OCS lab measurements instead (Barnes et al 1994, 1996, Patroescu et al 1999) and include the product yield (0.7% DMS -> OCS). These references suggest that the DMS -> OCS product yields are negligible due to titration by NO. Have the authors considered recalculating the emission flux without

Pg 20680, line 13: Are there any observations to verify the suggested high and mid latitude OCS emissions in Chin and Davis, 1993; Watts, 2000?

Pg 20680, line 29: Methods used to measure soil fluxes were found to be faulty (around 2000) and recent analyzer advances have shown that OCS can be emitted from rubber/neoprene and some plastics. Were the sampling volumes used in the von Hobe (2001,2003) studies free of these materials?

Pg 20680, line 1; reword: "dark-production rate is also linked..."

Pg 20683, line 5: What cloud cover is used as the surface irradiance input?

Pg 20683, line 17: important? In what way?

Pg 20684, line 1: I don't fully follow this paragraph. Can you better explain how you use the light absorption of CDOM at 350nm to represent the CDOM concentration. You don't know the concentration of CDOM so you are using the absorption coefficient at 350nm. Does this need to be a function of CDOM to be used? I think a little more

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information here would help with that. Also, why have you chosen 350nm, when the two parameterization use 440nm and MODIS uses 320 nm?

Pg 20684, line 24: "has been" should be was... And what remote sensing products were used? Needs more details. Pg 20686, line 12: Need some references for this range of AQY. What is the range of AQY? What causes the variability?

Pg 20684, line 16: What data are you using for the calculation of equation 7. Why was this data chosen? How different is the calculated value in different waters?

Pg 20687, line 1-14: The theory of how the dark production can be related to the light absorption of DOM really needs more explanation. (see Pg 20684, line 1 comment above)

Pg 20688, line 16: [COS] should be [OCS]

Pg 20689: It might be better to include to combine Section 2.3 and 2.2.3. That will help give sufficient detail about the AQY.

Pg 20691, line 5: Can you quantify the effect of the underestimate of the chlorophyll concentration in the oligotrophic subtropical zones?

Pg 20691, line 10: Are there no other OCS depth profiles from other areas that could be used for comparison?

Pg 20691, line 19: "twice as low".. replace with "half as much"

Pg 20692, line 2: "important" replace with "high" Pg 20691, line 5: needs to be clarified

Pg 20692, line 28: Can you quantify the agreement between the simulated and observed OCS profiles? Can the observed data be added to Figure 5?

Pg 20694, line 1: What is the overall southern hemisphere overestimate in chlorophyll and, therefore, OCS. Is there anything that can be done to correct for this overestimate? There are a number of places where the authors discuss an over/underestimate. Is

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there a way to quantify the magnitude of these over/underestimates?

Pg 20695, line 6: Full stop missing.

Pg 20695, line 13-15: These sentences should be clarified. They appear to contradict each other.

Pg 20696, line 1: Why is the Sargasso sea photoproduction so much higher than the total ocean simulation? I am not surprised at this result given how clear the water is in the sargasso but some suggestions as to the reason for this result would be useful.

Pg 20696, line 4: "appears"? or is? Have you done an analysis of the main drivers of OCS dark production in the model?

Pg 20696, line 20: Should the model be tuned to the few observations that are available? If more than Cutter et al is available, this should be used in the initial analysis.

Pg 20697, line 3: "levels"? Do you mean concentrations?

Discussion: Overall, the paper might be easier to follow if the Results and Discussion were rolled into one. Some of the discussion would be much more useful earlier on in the text! Discuss the implications of the results of the model parameters as they are brought up initially.

Pg 20701, Line 5-10. This could be more concise and direct. That would help to get the point across better.

Pg 20702, Line 5: Are these references correct? Berry and Suntharalingham studies suggest a terrestrial sink much larger than 300 Gg S yr-1 as far as I remember?

Pg 20703, Line 5: The Kamyshny et al paper states that their hydrolysis estimates are in line with the Eliott estimate. How does this tie in with this statement?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 20677, 2014.