

We thank S. Belviso for his comment to our manuscript, and will address the points raised in the following, with *his comments in italics* and **our replies in bold** font. We are pleased to discuss any further emerging questions.

The bottom-up approach of COS concentrations in the oceanic mixed layer used in this study relies on the relationship shown in Fig. 3 where the photoproduction rate constant (or quantum yield) at 350 nm (p_{350}) is linearly correlated to the CDOM absorption coefficient at 350 nm. A similar study was carried out by von Hobe et al. (2003) but, in that study, the authors investigated p_{313} and a_{313} . A wavelength of 313 nm was used because "it falls into the center of the wavelength band for UV and coincides well with the wavelength where COS surface production rate spectra show a maximum in tropical waters (Weiss et al., 1995)." I wonder why the authors calculated a new relationship at 350 nm which is the wavelength where COS surface production rate spectra show values about twice lower than at 313 nm (see Fig. 2C of Weiss et al. (1995)). Can you clarify this point?

It is true that the quantum yield for OCS is largest at smaller wavelengths, as found e.g. in incubation studies by Weiss et al. (1995) with seawater samples from the Pacific Ocean. We chose to use the a_{350} here, as the parameterization for dark production is also based on a_{350} and thus can rely on the same input parameter. That this assumption is valid is shown in our Fig. 3, as the linear correlation for p related to a_{350} yields a similar shape as the p - a_{313} relationship in von Hobe (2003) Fig. 5. The crucial point however is that p is fitted in a consistent way, as p is wavelength integrated and is thus only valid for the same light wavelengths in the forward and in the inverse mode. As we use the same model as von Hobe (2003) with respect to the UV light field, we can use his fitted p and relate them to a_{350} together with our new values.

The average action spectrum of COS used by von Hobe et al. (2003) is the one established by Weiss et al. (1995) from incubation experiments carried out in tropical and Antarctic regions. Weiss et al. compared the average action spectrum from their work to that of Zepp and Andreae (1990) who investigated coastal North Sea water samples.

Weiss et al. noted that differences in DOM origin between the two works may explain the pronounced discrepancy in relative action spectra. My interpretation of data shown in Fig. 3 is that the quantum yield of COS is about twice higher in the surface waters of the Indian and Pacific oceans than in the Atlantic. Because the COS quantum yield exhibits quite large variations even far from coastal areas, I wonder how the Lennartz et al.'s relationship can be representative for the global ocean.

Your interpretation to attribute the scatter in Fig. 3 to variations in the apparent quantum yield (or the concentration of precursors in relation to CDOM) is correct and certainly exhibits variability globally. A box model like the one used here is always a simplification, and the photoproduction rate constant is certainly the part where the model can be better constrained when more data is available. However, it has to be noted that this linear relationship as presented in Fig. 3 contains information from 3 very different oceanic regions with respect to the biogeochemistry, which is the reason for the scatter, as it takes into account the differences in spectral shape for light and CDOM absorbance. Given the fact that this one linear relationship does not only reproduce the data from the two cruises that cover a low CDOM (OASIS) and a high CDOM (ASTRA-OMZ) area, but also simulated global concentrations that agree well with observations (see below, new Tab. 5 in revised manuscript), it can be used as a first approximation for a global relationship. A more exact identification of the precursor instead of approximating it by CDOM would reduce the scatter. However, this relationship will not be the crucial point when addressing the missing source question, as assuming a higher quantum yield as in the Atlantic will lead to an overestimation of concentration and thus represents a conservative estimate in our case.

The latitude-time plots in Fig. S5 (this work) and in Fig. 8 of Launois et al. (2015) look pretty much the same in terms of hydrolysis rates in tropical regions (1 to 2 pmol L⁻¹ h⁻¹). How is this possible knowing that your model predicts considerably lower COS levels in tropical waters?

This difference is indeed present, and we will describe our way of calculation here using a concrete example: For the example, we assume a mean SST of 25°C, a salinity of 35 psu and a pH of 8.1. According to the implementation of the rate constant by Elliott et al. (1989) taken from von Hobe (2003), this results in a rate constant of $3.7 \times 10^{-5} \text{ s}^{-1}$, a range which seems reasonable given Tab. 1 in Elliott et al. (1989). The hydrolysis rate is then obtained by multiplying this rate constant with the actual concentration assuming first order kinetics. This would lead to the following hydrolysis rates when assuming a concentration of 10 pmol L^{-1} as found in the simulation discussed in the Lennartz et al. manuscript, or concentrations taken from S-Fig. 1 in Launois et al. (2015) (by eye) of 100 pmol L^{-1} :

Concentration	Hydrolysis rate
pmol L^{-1}	$\text{pmol L}^{-1} \text{ hr}^{-1}$
10	1.33
100	13.3

The $1.33 \text{ pmol L}^{-1} \text{ hr}^{-1}$ in relation to 10 pmol L^{-1} of OCS agree well with the values calculated for the tropics in the Fig. 5 in the supplement of the Lennartz et al. manuscript.

My last concern, to conclude, relates to the lack of validation of your extrapolations from published inventories. I refer to that of Mihalopoulos et al. (1992) who gathered cruise observations of the supersaturation ratio (SR) of COS in coastal areas and in the open ocean (their Tables 1 & 2) and of the latitudinal variations of seawater COS concentrations (their Fig. 2). The calculation of oceanic fluxes requires 6 variables (k_w, T , wind speed, C_w , C_a and H). That of SR only requires 4 variables (C_w , C_a , T and H). The direction of the flux can be assessed from SR values ($\text{SR} < 1 = \text{sink}$; $\text{SR} > 1 = \text{source}$). I think you should try to evaluate simulated monthly maps of SR against the inventories of Mihalopoulos et al. (1992). Scanned data plots can be digitized using online facilities.

This is certainly a point we will address in the revised version of the manuscript. We do not only include comparisons to observations from the study you suggest, but add 8 more studies to compare our model output against, including measurements from several Atlantic transects, the Indian and the Southern Ocean (>4000 measurements). We find an overall good agreement, even when comparing to our model output driven by climatological means (absolute mean error 6.9 pmol L^{-1} , mean error 3.7 pmol L^{-1}). As a direct comparison, we use mean concentration values presented in the papers, and compare them to corresponding months and regions in our model output, still acknowledging that the climatological input parameters of the model might not fully represent the conditions encountered during the OCEAT II and III cruises. Using the mean values provided in section 3.2 (b, open ocean), we obtain the mean value 19.9 pmol L^{-1} during the two cruises opposite to 11.2 resp. 17.7 pmol L^{-1} in our climatological means. The minor discrepancy may be explained by the fact that 20 resp. 14 measurements of unknown time of the day are compared to climatological means.

References

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