

Interactive comment on “Oceanic emissions unlikely to account for the missing source of atmospheric carbonyl sulfide” by Sinikka T. Lennartz et al.

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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

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spectra show values about twice lower than at 313 nm (see Fig. 2C of Weiss et al. (1995)). Can you clarify this point?

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.

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?

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 ($SR < 1$ = sink; $SR > 1$ = 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 on-line facilities.

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