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Discussions

Interactive comment on "The influence of dissolved organic matter on the marine production of carbonyl sulfide (OCS) and carbon disulfide (CS_2) in the Eastern Tropical South Pacific" *by* Sinikka T. Lennartz et al.

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Given interest in the global radiation balance in a changing world, this manuscript examines the production of carbonyl sulfide and related gas carbon disulfide in waters off Chile using a combined field and modeling approach. It is a relevant and needed compilation for ocean and atmospheric scientists. One of the issues driving any recent examination of sulfur gas fluxes from the ocean is the apparent imbalance between known/established source fluxes, the atmospheric inventories, and residences times – the known fluxes cannot account for the measured inventories. In this respect, this

paper doesn't solve the problem, but in fact they also missed the papers by Cutter and Radford-Knoery (Mar. Chem., 43: 225-233, 1993) that clearly demonstrate the importance of coastal OCS fluxes, and another by Zhang and Cutter (Mar. Chem., 61:127-142, 1998) that shows coastal sediments are a large source to the water column of OCS via sulfate reduction and enhance the sea-air flux. Another interesting feature in this 1998 paper is that low depth-resolution bottle sampling that have acquired OCS depth profiles to date, and that used here with a pump, may be missing large subsurface maxima in OCS that would then radically change the calculated fluxes. These same features could be present for CS2 as well.

In these authors' computations of photochemical and dark production, it would have been beneficial to examine the carefully measured and calculated rates in the Sargasso Sea (Cutter et al., 2004). In this respect, the Sargasso Sea dark production not only depends on the abundance of particulate organic matter, but also is ca. 3x higher than those reported here. Since they have endeavored to compare their results with others, this bears mentioning. Interestingly, their photochemical model using Weiss et al.'s 1995 Apparent Quantum Yields is 3-6 times lower than required based on field data (p. 12, line 22), but the Cutter et al. (2004) AQY parameters would nicely fit their field results. Thus, expanding their search for parameterizations would have solved some of their problems.

Finally, their speculation on p. 15 that dissolved H2S in the surface ocean could maintain OCS abundances is interesting, but in fact the major pathway for oxic H2S production is phytoplankton emissions coupled to assimilatory sulfate reduction, not OCS hydrolysis, and most of the resulting H2S is complexed with trace metals such as zinc (Walsh et al., Limnol. Oceanogr.,39: 941-948, 1994; Radford-Knoery [NOTE CORRECT SPELLING] and Cutter, 1994; Cutter et al., Deep-Sea Res. II, 46: 991-1010, 1999).

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