Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-174

Byron Blomquist (Referee)

Referee comment on "Dimethylated sulfur compounds in the Peruvian upwelling system" by Yanan Zhao et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-174-RC1, 2021

This is a very nice paper presenting detailed observations of primary surface ocean sulfur species for the Peruvian (Humboldt) coastal upwelling region. The paper is well written and the methods conform to expected standards in this field. I have a few general comments below. I find very few punctuation or other formatting issues.

General Comments / Suggestions:

line 17, 25-28 and elsewhere: The conclusion that seawater DMS was 'relatively low' seems too vague and imprecise. Better to just state the mean/range/variance of the observations and compare this to previous measurements. I wouldn't stress a comparison with Lana et al. 2011 too much (PMEL database is better). As you have mentioned, there isn't much data from the "Humboldt Current Coastal' province, as defined in Lana et al., and their seasonal extrapolation for this province from limited data is just a best guess. This is a region where we expect seawater DMS concentration to vary quite a bit, spatially and temporally (on seasonal, yearly and perhaps decadal timescales). Combined with previous measurements, these data provide a better picture of seawater DMS in this region than the Lana et al. gridded product. Since this study doesn't represent extremes in the ENSO cycle it would be interesting to know how well previous studies have sampled ENSO variability. Can the authors discuss their study and previous measurements from the ENSO perspective a bit more? Are we still far from a representative sample of DMS variability during ENSO extremes? Should this be a focus of future studies?

line 19 and elsewhere: the terminology 'flux density' is a bit odd and not typically used in the air-sea flux community. It's potentially confused with usage like 'spectral density' for power spectra, etc. Better to just say 'flux' or 'fluxes'

lines 129-131: Although it doesn't make much difference to the conclusions of your study, we should really discourage the use of transfer models like Nightingale 2000 for DMS flux estimates. Numerous direct studies of DMS air sea flux have been conducted over the past decade or more and the relationship with wind speed (or friction velocity) is closer to linear, especially over the wind speed range of your study. Transfer models based on highly insoluble tracers don't represent DMS transfer well (see Fig 3 in Bell et al. 2013). Better to use a model that's actually validated with direct DMS flux observations. The COARE gas transfer model has been used (e.g. Bell et. al. 2013, Blomquist et al. 2017) but for this paper a simple linear relationship is probably fine: e.g. Huebert et al. 2010, Fig 3 or Blomquist et al. 2017 Fig 5.

line 269: I'm confused by the reference to 'terrestrial DMS sources'. What are these potential sources? I'm not aware of any, especially in the arid coastal climate of Peru and Chile. A more likely source of high atmospheric DMS variability would be hotspots close to shore, which might be implicated if trajectories in the marine boundary layer follow the coastline for some distance.

Minor notes:
line 15: don't need a comma after 'present'

References:

Bell et al., 2013, https://doi.org/10.5194/acp-13-11073-2013
Blomquist et al. 2017, https://doi.org/10.1002/2017JC013181
Huebert et al. 2010, https://doi.org/10.1029/2009GL041203