Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-778-SC2, 2016

Atmospheric Chemistry and Physics Discussions

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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Received and published: 9 November 2016

Dear Sinnika,

I read your paper with great interest and I have a few comments that relate primarily to the photoproduction part of your work. Note I generally support the reviewers' comments on better acknowledging and including uncertainties associated with your estimates. This is especially important because the paper's conclusions contradict the main findings of other published work

In particular:

1. The photoproduction rate calculations deserve to be explained in more details, es-

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pecially when it comes to the global calculations. In particular, are the rates reported integrated over the mixed layer (as is suggested by equation 2) or are they an average for the mixed layer, or at a discrete depth (as would be suggested by the pmol L-1 h-1 units reported on figure S5)? The caption mentions production rates but that's it. f the rates are integrated over the mixed layer then the units of photoproduction rates should be in units of pmol per unit time and unit AREA (not per unit VOLUME). It would be good to clarify this because it would help facilitate comparisons with other approaches. Note I attached (see supplement) a number of figures of calculated rates using the Fichot and Miller (2010) model implemented with the single wavelength-resolved apparent quantum yield of Weiss et al. (1995) for comparison purposes and to illustrate the differences in photochemical rates calculated at different depths or for different depth ranges (mixed layer, and sunlit layer).

2. The attempt to constrain the variability in the photochemical rate constant p is welcomed, but it should also be recognized in the paper that using wavelength-independent p for the global calculations can also lead to significant and potentially large uncertainties in the calculated rate. In the van Hobe (2013) paper, it was suggested this had a little impact, but I believe the comparison using wavelength-independent and wavelength-resolved constants was made at the same location where there should be minor differences in the spectral characteristics of the UV downwelling irradiance spectrum. For global calculations, this simplification can be much more problematic because the spectral characteristics of the downwelling irradiance can vary quite dramatically with latitude or with atmospheric conditions. This is potentially a large source of uncertainty that would be good to discuss in the paper.

3. It should also be clearly acknowledged that the modeled downwelling (or downward) irradiance (as described in the paper) are crude approximations and do not even include the effects of clouds for example. This is another important source of uncertainty.

4. It should also be better mentioned that there are uncertainties in the a350 retrieved from satellite data and that uncertainty will be compounded when p is derived from

a350. There is also uncertainty due to the fact that the GSM model used to retrieved the absorption by detritus+CDOM at 443 nm and not just CDOM. Finally, another uncertainty is associated with the use of a single spectral slope for CDOM to derive a350 from a443.

All these combined uncertainties can amount to a large error in the estimates of OCS photoproduction alone, and I think there is a need to better acknowledge all sources of uncertainty and their implications on the conclusions, and there is a need for a more cautious language with regards to the conclusions of this work.

I also would like to take the opportunity to use the attached figures of COS photoproduction rates integrated over the mixed layer to highlight that the regions studied in your paper are generally not as photochemically active (generally lower photoproduction rates) as the region studied in Mihalopoulos et al. (1992). This is in relation to the comment posted by Dr. Belviso.

I hope this information is helpful.

Best,

Cedric

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-778/acp-2016-778-SC2-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-778, 2016.

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