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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inverse modelling of carbonyl sulfide: implementation, evaluation and implications for the global budget" *by* Jin Ma et al.

Anonymous Referee #2

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Dear Jin Ma and colleagues,

This study tackles the issue of closing the global OCS budget. In the past, a missing source was often assumed to be in the ocean, though actual oceanic observations are not extensive enough to answer the question well. Another possibility is that the sink is overestimated. Using TM5-4DVAR is a good method of trying to get at this question.

Comments

The uptake of OCS is tied to the OCS concentration within the canopy. There are large variations in OCS uptake as OCS depleted air flows through vegetation, e.g. Berkelhammer et al. 2020 (https://doi.org/10.1029/2019GL085652). Most canopies do not see free troposphere concentrations of OCS. This will affect the plant uptake flux

component substantially and should be addressed.

DMS should not be considered a major source of OCS. Many researchers still refer to the 7% yield figure from the Barnes 1996 paper, but note that chamber studies proceeded without NOx and at high DMS concentrations. Subsequent studies demonstrated that an alternative chemical pathway is typically taken, and that changes in NOx affect OCS formation greatly (e.g. https://doi.org/10.1016/S1352-2310(98)00120-4). In other words, if you would like to include DMS, it is important to also model NOx. This is most likely such a small contribution that in the Whelan et al., 2018 synthesis, it was concluded that DMS should only be included as a source of uncertainty in the ocean flux rather than a source itself.

If the lifetime of CS2 was 12 days, it might make sense to model CS2 separately, since the associated OCS will not show up in the air parcel until it has traveled nearly around the globe. However, more recent evidence suggested that the lifetime is much shorter than that. For example, see the 3D atmospheric transport study performed by Anwar Khan which focusses only on CS2 and estimates a lifetime of less than 4 days at maximum: 10.3934/environsci.2017.3.484.

The SiB3 model had a known phenology problem, where the growing season starts too soon (by a couple of weeks, perhaps) and ends too early. Does SiB4 have this issue? It would be good to check the seasonal timing by comparing to SiB3 or even another slightly complicating proxy, e.g. SIF.

The abstract should be revisited to better reflect the conclusions of the study and to tidy up the language, e.g. sources of OCS are obviously included in current budgets.

In short, revising the inclusion of DMS and addressing the first order plant uptake issue will certainly create a different budget overall, and may affect the conclusions.

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