Title: Carbonyl sulfide exchange in soils for better estimates of ecosystem carbon uptake
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Post-discussion review of revised submission
Referee: Jürgen Kesselmeier

The paper has been improved significantly, but I still have strong reservations before publication.

1. Thanks for taking into account the Sandoval et al paper to cite all those reports, the GPP-OCS proxy is based on. The new global estimate by Sandoval et al. was only reasonable as it was done, by relating the relative uptake rates to GPP. The authors mention the 2004 AGU Abstract by Steve Montzka, suggesting that link. Well, if we want to take into account the conference presentations, there has been a presentation by Sandoval-Soto et al. on the AGU meeting one year earlier (2003) already (A32A-0122). On that poster we presented the first time the global OCS sink estimate based on relative uptake rates and GPP. But I guess it is not very helpful to base our discussion on such conference contributions. Furthermore, the idea to relate OCS uptake by vegetation to the gross uptake of CO2 was used by Sandoval-Soto et al to derive a new OCS budget corrected by the relative uptake ratios (Deposition velocities / atmospheric ratios) which are also used now in relation to a whole ecosystem. I remember a lot of discussions with colleagues acknowledging this as an important step to use it for GPP estimation. And I think it is fair to cite that paper within this historical development.

2. Fluctuations of CO2 and OCS in the chamber: This is still a very critical point. Large fluctuations of OCS and CO2 as well may affect the enzymatic consumption of OCS. Furthermore, they may directly affect the data when taking samples from the chamber (see also point 3, chamber technique). If fluctuations are significant, the differences between incoming and outgoing air may be meaningless. Fluctuations and the very close relationship between OCS concentration and OCS uptake is discussed in the supplement. Why not in the main paper? ACP has no limitations in page numbers. Furthermore, I do not understand, why the supplement is not even mentioned in the main paper.

3. In order to be able to get a feeling for the reliability of the data (also when reading the paper in the future and comparing with future measurements), I think it is essential to know more about the experimental technique. Were the sampling performed by switching between in- and outlet or were they performed simultaneously? I guess they were switched. This should be mentioned and discussed. There is even no information about the chamber(s) themselves. Was one chamber used or were there two, i.e. reference and sample chamber? I think such information would create confidence.

4. The authors included error bars. That is very helpful in order recognize significant developments. However, information about accuracy and precision of the gas analyzers is missing throughout the paper. The effect of water on the spectral measurements is given as less than 1%. I guess this is the information as given by the manufacturers. However, this may be valid only for a very narrow range of water vapor. At least our LGR instrument was found to have severe problems with water vapor. And we had to find special solutions to get reliable measurements, especially when water vapor concentration were fluctuating, which is often the case due to evaporation or transpiration. Are there any tests available?