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***Interactive comment on* “Exchange of carbonyl sulfide (COS) between the atmosphere and various soils in China” by J. Liu et al.**

Anonymous Referee #2

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This manuscript reports exchange rates of COS by different soil types, measured both in the lab and in the field, and respective dependencies of the exchange on the COS ambient concentrations, temperature, and soil water content. Data presentation and discussion is pretty much straight forward. The work is a valuable add to the small pool of available soil COS exchange data. The generalization of dependencies which were yet measured only for a few soil types represents the largest source of uncertainty in global COS budget exercises (Kettle et al. 2002).

Specific comments:

In general, I would suggest to omit the term “flux” in conjunction with enclosure measurements, as it is rather used in terms of micrometeorological measurements (eddy covariance, aerodynamic gradient etc.). Hence replace “flux” and “exchange flux” by

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“exchange” and “exchange rates” in the whole manuscript (likewise, omit “flux enclosure”).

I would suggest showing the data of COS exchange as a function of COS concentration in an individual diagram, like the authors did for the dependencies on soil water content and temperature. And/or show in Table 3 the intercept of the y-axis (extrapolation of the linear regression to zero COS concentration, with appropriate units), as an estimate for the COS emission/production potential (compare e.g.: Kesselmeier et al. 1999). Add the correlation coefficients of the linear correlation analysis. Also in this context: the flush rate of the enclosure was relatively small; hence the expected concentration differences at the inlet versus the outlet of the enclosure were probably high: please indicate whether the inlet or the outlet concentration was used to calculate deposition velocities (I do assume the latter).

The lack of data on Water-Filled Pore Space as an important parameter when examining different soil types (Van Diest and Kesselmeier 2008) was already addressed by Referee #1 (with respective author’s reply).

One main open question: what made the difference between the setup and/or soils sample investigated by Yi et al. (2008; reporting uptake of COS by dry paddy soils in the field and emission of COS only by waterlogged, non-planted paddy soils) and the field results presented in this manuscript. With a compensation point concentration of 765 ppt found in the laboratory for the paddy soil from Jiaxing, a deposition of COS would have to be assumed when the soil faces the extremely high ambient concentrations observed at the field site, which would be in accordance with the findings of Yi et al. (2008). This was not the case for the field data presented in here, and even at concentrations above 1500 ppt emission was observed. Obviously at the field site the conditions for COS ambient concentration, temperature or soil moisture all greatly varied in concert for the different patches shown in Table 4, hence it is hard to what are the determining variables. Devai and DeLaune (1995) found that the (potential) COS production rate generally depends on the redox potential of the soil. At negative values

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(under reducing conditions) the emission increases, whereas at positive redox potentials, it decreases. Waterlogged soils are anoxic and usually have low redox potential. COS production is assumed to be considerably enhanced in such soils, leading to an increased compensation point, as was found for paddy soils by the authors. Do the authors have an idea on the redox status of the paddy soils that emitted COS?

I would assume that the difference in soil water content, as investigated in the laboratory (decreased emission with increasing water content), might rather be a short-term effect of the higher gas diffusion resistance within the soil column due to the higher water content (for P1 the steady state exchange at high water contents was zero) . Only within a longer timescale the higher resistances will have an effect on the composition of the microbial community (hence COS production), as might have been the case at the field site (also relevant for the zero exchange at waterlogged conditions).

Minor comments, corrections:

The diagrams in the revised version (supplement) are now much better. I would suggest to add an x-axis at $y=0$ (like in the original version), to be able to better differentiate between emission and deposition.

Check the whole manuscript (several instances) for the typo “concentration”

Page 10558, line 10: rephrase “For the . . .COS emission increased exponentially with increasing soil temperature . . .and COS emission decreased with increased soil water content.”

Page 10561, line 7: As the humidification of air by the water bubbler was done after addition of high-concentration COS gas: did that setup actually have an effect on the COS concentration (by solution of COS)?

Page 10566, line 12: replace “hifger” by “higher”

Page 10567, line 12: Why should the negative pressure within the enclosure favour COS emission? A mass transfer of air from the soil to the atmosphere (induced by un-

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derpressure in the enclosure) would enhance the apparent (measured) emission rates in case when there is an production of COS by the soil (i.e., sucking COS enriched air into the enclosure); but would enhance the apparent (measured) deposition rates in case when there is an uptake of COS by the soil (i.e., sucking COS-depleted air into the enclosure). So, I do agree that an artificial underpressure enhances the measured exchange rates, but this can hardly be assigned for a change of the sign of exchange (inhere from deposition expected by the lab measurements, to emission observed in the field). For an alternative reasoning: do you have an idea of the redox potential of the individual samples paddy soils in the field, and their (waterlogged) history, respectively?

Page 10567, line 17: As some of your paddy soil patches in the field strongly emitted COS event at ambient concentrations above 1500 ppt, I would expect that the high ambient concentrations might stem from COS emissions by the soil.

Page 10567, line 25: "... might be useful in quantitatively predicting..."

Additionally to the global budget of Watts (2000), please check the work done by Kettle et al.: Global Budget of Atmospheric Carbonyl Sulfide: Temporal and Spatial Variation of the Dominant Sources and Sinks. *Journal of Geophysical Research*, 107 (D22), 2002.

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