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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Carbonyl sulfide exchange in soils for better estimates of ecosystem carbon uptake" *by* M. E. Whelan et al.

Anonymous Referee #3

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The topic of this manuscript is on the use of biogenic (preferentially vegetation) COS uptake as a proxy for GPP. The main assumptions behind this approach are that vegetation uptake is the dominant COS flux in land ecosystems and that soil or non-photosynthetic fluxes are either minor or well characterized (Maseyka et al. 2014). The latter is challenged by this discussion paper, and the potential bias introduced by soil COS exchange is shown. Two GPP estimates were compared: one accounting for COS leaf uptake fluxes alone, the other additionally including soil COS exchange. While soil was generally assumed to predominantly act as sink for COS in earlier studies on a variety of soil types, few recent publications revealed the capacity of agricultural soil being a strong source of COS. Dynamic soil (agricultural, forest, desert, and savannah) incubation chambers were used to assess the COS exchange in a controlled

setting in the laboratory. In general the authors confirmed the uptake of COS for most of the soils, at least for the relevant environmental conditions of their habitat. For an agricultural soy field soil type they found strong emissions, which confirm recent field flux studies, but are in contrast to earlier laboratory studies where no emission from agricultural soil was found. The reason for these discrepancies could not be unraveled.

The authors used "anticipated" vegetation COS uptake data to estimate the expected bias introduced by including/excluding literature soil COS exchange data, hence showed the potential impact of soil COS exchange on GPP estimates ranging from -220 to +119 % (Table 3). Furthermore, they used functional dependencies of soil COS emissions on temperature and soil water content, derived specifically from soy field soil, to account for both uptake and emission of COS. Applying their soil COS model to a specific agricultural soy field site (reported CO2 flux measurements; no in situ COS data for vegetation or soil) the bias was comparably low (-5 to +25 %), due to the fact that the net soil COS exchange was relatively low.

The subject fits well into the scope of ACP and publication is justified. The bunch of lab data are not conclusive, but rather disclose the dire need for more information about soil COS exchange. Future studies must disclose the extent of generalization of the model on COS emission from agricultural soils. There are some concerns, mainly on the reported variability of the assimilated database, and the traceability of data used for the different approaches. While solely using soy field soil results for the model is described in the Methods section, it should additionally be mentioned in the main text (beginning of section 3.2, and also mention again that the Bondville site is a soy field).

The laboratory results are quite diverse with sometimes "no discernible pattern" or contradicting results comparing different soil types. The data on the environmental functional dependencies of soil types other than soy field soil are somewhat distracting from the main idea of the manuscript. More than one environmental parameter is changing at a time, in some cases observed to be accompanied by a change in the sign of the exchange direction. In general soils showed COS uptake at low temperature and being wet, while emission was observed at high temperatures and being dry. The different soils did not show consistent exchange values with respect to, e.g., soil moisture manipulation. However, this work demonstrates the diversity of soil COS exchange behavior. The authors do address this issue, like “the link between soil moisture and COS fluxes for soils collected at other sites is not as clear” or “the pattern of COS fluxes over time after a change in soil water content was not consistent for given changes in soil moisture.” Hence the data base is somewhat vague, and to retrieve a sound model for soil COS exchange characteristics is challenging. For the latter reason (and to avoid misuse of the model) it should specifically be pointed out that the derived model parameters are, if at all, only applicable to agricultural soil types, as the main conclusions do not necessarily hold for (or can be transferred to) other soil types. However, the authors already emphasized: “we present this as a theoretical exercise investigating the possible magnitudes of soil COS exchange on broader scales”. I agree, at least as long as not all ingredients for a GPP estimate are measured simultaneously in situ.

Fig. 10: please indicate within the figure (horizontal bars or shaded areas) when wheat was present, when it was senescent, and when it was harvested to better emphasize the involvement of plants. The applied models are not meant to account for the influence of vegetation, although the wheat is assumed to have critical impact on the COS budget. It seems that not even the role of vegetation could adequately be modelled based on current knowledge (as assumed to be a sink) at this specific site, unless the soil source would be able to over-compensate the assumed COS uptake by wheat. May be this critical detail should also be discussed more clearly in the text.

Minor corrections/comments:

As the COS exchange is dependent on COS mixing ratios: which inlet COS mixing ratios were used for the laboratory dynamic chamber experiments (see respective comment of Referee #1) and could it have had biased the results?

When discussing the “important dimension of soil depth”, the authors might consider

a most recent paper by Sun et al. 2015 (A soil diffusion-reaction model for surface COS flux: COSSM v1; Geosci. Model Dev. Discuss., 8, 5139–5182, 2015, doi:10.5194/gmdd-8-5139-2015)

Page 21104, line 23: to prevent misunderstanding, please rephrase “Regardless of sign, COS fluxes increased with temperature”. To my understanding this would mean that increase of temperature on a negative flux (deposition) results in a more negative flux (stronger deposition); this is not what the authors showed in Fig. 4 and 5.

Page 21107, line 16: please remind the reader that the Bondville FLUXNET site, US-Bo1 is a soy field site.

In Table 3, the authors compare the impact of earlier results of (anticipated plant and observed soil) COS exchange from different biomes on GPP. At first view, I would assume that accounting for exclusively soil COS uptake (as in the case of Kuhn et al. 1999) would result in an uncertainty only in one direction, not in both as indicated in the table (+119 to -34%), as “F COS ecosystem” is always higher after including soil uptake. Please correct me if I am wrong. Else: Most of the field measurement references of Table 3 do not appear in the reference list. Please check.

Fig. 6: The information on soil moisture is redundant (x-axis title and color code); however, this color code of VWC would help in Fig. 7.

Fig. 8, legend: to be better comprehensive, I suggest “Estimated fluxes from abiotic (a) and biotic (b) processes of soil COS exchange from soy field soil.”

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 21095, 2015.