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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Soil fluxes of carbonyl sulfide (COS), carbon monoxide, and carbon dioxide in a boreal forest in southern Finland" *by* Wu Sun et al.

Anonymous Referee #2

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This study provides much-needed in situ measurements of the soil fluxes of COS and CO. Both are important trace gases for chemistry & climate impacts and as tracers for photosynthesis and anthropogenic activity, respectively. The data set is relatively rich, despite some significant gaps, and interesting trends in diurnal and seasonal patterns and relationships with environmental drivers are observed. The study has the potential to make a good contribution to the scant literature on the soil-atmosphere exchange of these trace gases. The paper suffers from a lack of specific language that is well supported by references and data. Specific instances of this are mentioned here and highlighted in the specific comments below. The paper would benefit from a greater analysis of the data at hand and less conjecture at mechanistic drivers that are not well

tested by the analysis. The paper requires revisions before acceptance.

General comment:

The paper should be more careful about the discussion and conclusions regarding the role of microbial activity in the COS and CO soil sink. While this is supported by the extant literature, this study is not capable of resolving respiration due to microbial activity from plant-derived respiration. Furthermore, both COS and CO can have significant production terms. Therefore, ratios of COS and CO to CO2 are not necessarily a simple measure of the ratio of the COS or CO-consuming microbiota to the total microbial activity. I would suggest the text is revised to acknowledge the limitations early on.

Specific comments:

P2L5: what does actively engaged mean for a gas?

P2L7: appropriate references for soil microbes should be included here such as: 1. Saito, M., Honna, T., Kanagawa, T. & Katayama, Y. Microbial Degradation of Carbonyl Sulfide in Soils. Microbes Environ. 17, 32–38 (2002). 2. Ogawa, T. et al. Carbonyl sulfide hydrolase from thiobacillus thioparus strain thi115 is one of the β-carbonic anhydrase family enzymes. J. Am. Chem. Soc. 135, 3818–3825 (2013). 3. Kato, H., Saito, M., Nagahata, Y. & Katayama, Y. Degradation of ambient carbonyl sulfide by Mycobacterium spp. in soil. Microbiology 154, 249–255 (2008).

P2L19: I don't find "plant + soil" to be written clearly enough.

P2L22: essential with regards to what?

P2L33: would be useful to give a clearer idea of the importance of soil uptake with respect to photosythesis (explored in Whelan et al., 2016)

P3L3: How is this statement justified: "The microbial types, enzymes, and metabolic pathways involved are, however, much more diverse than those of COS uptake "? The references only relate to CO.

P3L6: I wouldn't suggest that CO and COS uptake are similar here because of their broad optima. That is quite common. Would be more useful to state what optima are and how different this site is. For example, temperatures are likely well below optima for both gases.

P3L8: Is there a reference to say most soils are above their optimal moisture levels in the average? Otherwise this statement seems very subjective and unsupported. Seems entirely unnecessary to make this statement in any case.

P3L11: Should indicate whether biotic or abiotic or unknown.

P3L15-24: The expectations for COS and CO consumption and their link to CO2 respiration could be clarified and supported by citations.

P4L1: is it really necessary to use "ibid" and not just spell out what you mean?

P4L27: add citation for CO photochemical production

P4L29: COS emission from blank chamber. Was the temperature dependence of this tested? Can you be confident that accounting for it in the blank experiments (should be described in more detail) is sufficient? Was this only evaluated for one chamber, or both?

P4L30: define what you mean by "effect"

P5L33: Comment on whether the issue could have been due to condensation cycles driven by A/C

P7L5: Diurnal trend in CO, could it be from CO production during day? I do see a diurnal trend in COS, at least SC1, that is weaker than CO, but not insignificant. This is brought up on P9L1, but should be brought up before any discussion of processes (eg microbial activity) driving net fluxes is undertaken (eg page 8). The possibility of the diurnal CO source conflicts with the statements made in P8L6 about steady if any production rates.

P7L18: One cannot necessarily conclude that microbes control CO flux through correlation with CO2.

P7L28: What does "late growing season" refer to here? What statistical test was used to assess this and over which periods? It appears that there is a trend in increasing net COS uptake flux/deposition velocity over the time period.

P7L29: Ecosystem fluxes of CO2 on seasonal timescales have non-negligible contributions of photosynthetically derived contributions of plants through the rhizosphere. How does this study account for soil respiration derived from forest photosynthates (microbial, but often different communities than "bulk" soils) and root respiration? There is an extensive literature on hysteresis in diurnal patterns in soil respiration that should be cited as one possible explanation for the observations here.

P7L29: Statements of significance should be accompanied by relevant statistics throughout the text.

P7L18: Possibility that daytime production caused reduction in apparent daytime deposition velocity could be brought up here and addressed instead of waiting until discussion.

P8L3: I'm not sure what 'coexist' means here. Soils are sources and sinks of both, but this makes it sound like there is a connection, when that is not what these papers necessarily show.

P8L27: Only references to data, not models should be given for citations of temperature optima.

P8L31: Is this a given? "and that temperature and moisture co-vary in natural conditions"

P9L13 & Figure 5/6: The steep decline in Fcos/R and Fco/R are driven by Oct/Nov declines in R that occur at that low temperature (Fig 4 shows that only R is temperature dependent and is thus driving Fig 6 trends) . The possibility that plant-derived inputs are significantly reduced at that time, causing R to fall, must be addressed before suggesting any shifts in CO- and COS-consuming microbial groups. Even in a system without plants, it would be hard to argue that modest increases in CO and COS uptake and large decreases in CO2 emissions were the result of a less active microbial community but at the same time a significant increase in activity of strong CO and COS consumers. These points are alluded to in the P10 Pumpanen reference, but should play a more central role in the data interpretation as "autotrophic" contributions to soil respiration may be the most important factor in the gas ratios (instead of soil microbial activity).

P10L23: R may not be a mechanistic predictor for COS soil uptake, but can state that in this study does a well enough without resolving the drivers

P11L20: I would hesitate to draw conclusions about microbial drivers from the data generated in this study.

Table 2: Units are needed, especially because it is unclear if columns COS and CO are mole fractions or fluxes. What does n/a mean here?

Figure 2: soil temperature plots should all be on same y axis

Figure 2-3: trends would be easier to see if averaging in 1-2 hour bins. The current averaging shows large variability on the 30-min scale that does not look physically meaningful, especially in Oct/Nov

Figure 5: What is the take-away message here?

Table S1: which rows to columns 1-6 correspond to?

Figure S1: not clear how figures to right and left relate to each other. Caption should be more descriptive. What is a blank chamber test? Explain. The explanation of units is not sufficient.

Figure S2: use statistics to describe extent of correlation instead of statements like

"relatively well correlated"

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