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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Soil-atmosphere exchange of carbonyl sulfide in Mediterranean citrus orchard" *by* F. Yang et al.

Anonymous Referee #1

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In this study, the authors present soil fluxes of COS from a Mediterranean soil and examined the spatial and temporal variation of those fluxes. The authors find substantial differences in space and time, from emissions to uptake of COS, of which the variability is predominantly driven by soil moisture. Although the English grammar can be improved, this is a well-structured paper with a good discussion of the results. This study is a nice contribution to understanding the COS exchange at different ecosystems, of which the Mediterranean soil was still missing. Below are general and specific comments:

General comments

In the discussion on the different COS soil fluxes at the different sites it would be very helpful to visualise in Fig. 2, 3 and 6 to which site each data point belongs (e.g. use

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Discussion paper

a color per site). This would make it easier to understand how soil moisture varied between the different sites, instead of referring to the numbers in Table 1. Also, the result sections 3.1 and 3.2 could be better readable if the data are discussed while referring to the figures, instead of referring to Table 1. If the data points from the different sites are visualised in the figures I think that Table 1 would be unnecessary (could be an appendix?), as all the information is then presented in the figures.

The discussion focusses a lot on the variability of SRU, and I miss some statements on the importance and application of SRU earlier in the text. Moreover, the difference in size between SRU and LRU is used to discuss the implications of the soil COS fluxes for the use of COS as tracer for GPP, which I do not find very intuitive. Instead, I would compare the size of soil COS fluxes directly with that of ecosystem scale COS exchange, not that of the ratios of COS to CO2 fluxes. Comparing the size of soil COS exchange with ecosystem COS exchange would also make a very nice link with the results from the parallel study (Yang et al., 2018, Glob. Change Biol.), which has now only been little discussed.

It would also strengthen the message of the paper if the variability of the soil COS uptake in space and time is discussed in light of using COS as tracer for GPP. The soil COS exchange is relatively small compared to the ecosystem COS exchange, but the variability in space and time seems significant and may still complicate the COS tracer method? It would be good to discuss the implications of the variability of the soil COS exchange on the COS tracer method.

Specific comments

Abstract

P2L19: what do you mean with “exposed”? Exposed to the sun?

P2L20: “weighted mean uptake values”? Do you mean simply the average? Or what is it weighted by?

Introduction:

P3L60: “In spite of these efforts, more field measurements of soil COS exchange are clearly needed”. It would be good to mention here that also contrasting ecosystems need to be studied. Previous studies have focused on agricultural soils (Maseyk et al., 2014), wetlands (Whelan et al., 2013), boreal forest soils (Sun et al., 2018), grasslands (Kitz et al., 2017), but several ecosystems are understudied, and the Mediterranean soil is a highly needed addition to this.

I like that the soil profiles of COS have now also been measured (for the first time?). It can be mentioned that the soil profile measurements will also be useful for validation of soil models of COS exchange (Sun et al., 2015.).

Materials and methods:

One of the first questions I got while reading the manuscript was what the role is of photoproduction on the soil COS emissions, and only later I read that dark soil chambers were used. It would be good to mention already in the methods section that the chambers were dark and that photoproduction is not expected to play a role in the results.

It was shown by Kooijmans et al. (2016, AMT), that measurements of COS made with the QCL can be biased at high H2O. Were the measurements corrected for the effect of water vapour?

If available, it would be good to add more soil characteristics for this site, like pH and soil porosity.

Results:

P6L146: What do you mean with “interactions between microsite and season”?

P7L 178-179: “The response of soil COS fluxes to soil temperature varied among the three measurement sites (Fig. 3)”. This can only be judged from Fig. 3 when the data points in Fig. 3 get marked by site.

Discussions:

P11L289: “Temperatures did change over the daily cycle. . .”. Soil temperature?

P13L334-335: “We use SRU values also to assess the relative importance of the soil COS flux compared with the canopy.”. I don’t find this approach very intuitive. Why not simply compare the size of the soil COS flux with that of the ecosystem COS flux?

P13L336: “. . . the absolute value of SRU. . .”. The sign of SRU shifts from negative to positive due to change in sign of the COS flux. So I would say that referring to the absolute value is not appropriate here.

P13L337: Reference to the pine forest soil (Sun et al., 2018) is missing.

P13L342: A reference to Whelan et al. 2018 is given with LRU $\sim$ 1.7, which is the average over a large range of plant species. Why not refer to Yang et al. 2018 with LRU = 1.6 (at high light) that is specific for this site?

Conclusions:

P15L393: “we provide constraint, and validation of the closed chamber measurements . . . by the additional gradient approach”. What do you mean with “constraint”? I would just call it validation.

P15L397: This hypothesis on root distribution is introduced only very late in the manuscript, and it would be good to describe the different root distribution for the different sites already in the methods section.

Tables and figures:

Information in Table 1 is presented in Figures 2, 3 and 6, and the results section 3.1 and 3.2 could be better readable if it refers to the figures rather than the table.

Fig. 2, 3 and 6 would benefit from having the data points marked by site.

Figure 4 does not provide more information than Fig. 5, and I would consider removing it.

Technical corrections

-Be consistent throughout the text in the sign convention of fluxes. E.g. uptake of +2.5 pmol m-2 s-1, or a flux of -2.5 pmol m-2 s-1.

-Be consistent with the number of digits of fluxes. E.g. P9L222: -1.0 +/- 0.26 pmol m-2 s-1, which is -1.02 +/- 0.26 pmol m-2 s-1 in the abstract.

-CO2 flux units are missing the micro sign.

-mositure = moisture (both in text and figure axis labels).

Some textual corrections:

P3L42: "non-leaf contributions to the net ecosystem COS flux".

P3L50: Event = Even

P4L83: "described by Asaf et al. (2013).

P5L109: "...from the normalized ratio of CO2 respiration to COS uptake (negative values) or emission (positive values) fluxes".

P6L125: "soil COS and CO2 fluxes were estimated based on Fick's first law: ..."

P6 Eq4: Ts is not defined in the text, but later T is defined. If Ts and T are the same then make it consistent throughout the text.

P6L146: "both the spatial (microsites) and temporal (seasonal) scale".

P7L157: "In the UT site..."

P7L176: "The fit to the data..."

P8L206: Reference to Fig. 4 should be Fig. 5?

P11L279: induced = induce.

P11L288: remove “on”.

P12L315: maybe = may be

P12L319: Lab incubation results also indicated thermal production of COS in soil. . .”

P15L391: “Our detailed analysis...”

P21L569-571: caption of Figure 3, this sentence doesn’t flow logically.

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