

Interactive comment on “Carbonyl sulfide (OCS) exchange between soils and the atmosphere affected by soil moisture and compensation points” by Rüdiger Bunk et al.

Rüdiger Bunk et al.

r.bunk@mpic.de

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AUTHORS: We thank R1 for the critical and detailed review. Based on the input of R1, R2 and R3, the discussion section will undergo major changes and a restructuring.

General comments

REFeree: The manuscript presents a laboratory study on two important drivers of soil OCS exchange—soil moisture and ambient OCS concentration. The authors collected four soil samples from the field, incubated them in the laboratory, and determined their OCS exchange patterns under varying conditions of soil moisture and OCS

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concentrations. The experimental design covered the full range of relative soil moisture, from 0% to 100% with a fine step. This allowed the construction of well-defined soil moisture response curves of OCS exchange. By measuring OCS exchange under different ambient OCS concentrations, this study was also able to separate OCS gross uptake and production components from the net exchange and investigate their respective responses to soil moisture. However, the experimental design to derive compensation points was seriously flawed and unable to produce robust estimates of the compensation points. As the authors noted, they used measurements at 1,000 ppt and 50 ppt to derive the compensation points. This practice is essentially to fit a regression line with only two data points, and then extrapolate to somewhere up to 5,500 ppt! The uncertainty would likely be huge. Yet they present no uncertainty measures of this derived quantity in the figures or the text. That is not to say that the data shouldn't be published, but the authors should quantify the uncertainties and properly acknowledge the limitations of their method.

AUTHORS: We agree that the method used to derive compensation points in this study may be improved by investing much more measuring time and sample material. However we can rely on older reports about the linearity of OCS exchange between 50-1000 ppt in soils and plants. More data points to fit a regression would certainly be helpful. But it will further our understanding to present the current data set, also in view of the different soil types and chemical properties, and use it for discussion and development of new approaches. The limitations will definitely be addressed in a revised manuscript. However, our large data set provides a consistency over the whole range of the investigated soil water content, which covers a much wider range of soil moisture than in previous reports.

To address the uncertainties, we will add error estimations based on Gaussian error propagation for the net release (EOCS), uptake coefficient (k_{OCS}), gross uptake (UOCS), gross production (POCS) and compensation points (CP). Of further importance is the question about linearity of the relationship between OCS concentration

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and exchange rates at higher OCS mixing ratios. Earlier reports demonstrate good linearity up to approximately 1500 ppt OCS ambient concentration in measurements with soil (Kesselmeier et al., 1999). Investigations performed with lichens (Kuhn and Kesselmeier, 2000), which are lacking any outer protective cell layers and seem to behave similarly as soils, exhibit a comparable linearity relating exchange rates and OCS concentrations. In both cases the authors report a saturation effect at higher OCS concentrations, which leads to an increasing uncertainty. The reasons are not investigated but can be understood as metabolic saturation (enzyme saturation) or different/additional biotic processes influencing the rate of OCS exchange. These uncertainties limit the reliability of the compensation points calculated, especially if those are >1500ppt or at soil moistures above 60 % WFPSlab. However, despite this uncertainty, the CPs show a clear and consistent trend that cannot be disregarded. It can clearly be seen that the uptake coefficient (slope of the regression) first increases and then decreases again with decreasing soil moisture. Setting the reliability of the absolute values aside, such coherent trends contain information that can be explored. Additionally, the calculated CPs have reasonable overlap with older data. An important aspect of the paper is the observation of CPs over soil moisture which has not been reported before. Even with relatively high uncertainties this data is worth to pursue and to plan future experiments accordingly. For the current revision we propose to test the significance of all trends in EOCS, kOCS and POCS by using the first data point of each measurement as control and perform a t-test to see if the next value is statistically different from the previous one. It would highlight the idea of monitoring a significant trend in OCS over drying out.

REFeree: The manuscript identified the compensation point as a driver of OCS exchange, as the title and some section titles implied. But a critical examination of the mechanisms governing OCS uptake and production processes suggests that this is a misinterpretation. Compensation point manifests the dynamic balance between uptake and production. It is not an intrinsic property of the soil, but a variable that depends on soil moisture, as the authors have demonstrated in Fig. 3. Therefore, it is inaccurate to

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say that OCS exchange is 'affected' by the compensation point. The manuscript suffers from other flaws. The equations (Eqs 1 and 2) used to calculate the OCS exchange apply only to the steady state condition, which can be shown mathematically by solving a mass balance equation for chamber gas exchange. Yet, they did not assure the readers of the validity of the steady state assumption used in their measurements.

AUTHORS: See specific comments

REFEREE: In addition, the interpretations of data are insufficient, and sometimes superficial. I would suggest the authors to exploit their dataset for new understanding rather than confirmatory interpretations. The discussion is poorly organized and lacks novel insights. This part has to be rewritten for clarity and coherence. As a suggestion, please figure out the main point of each section and organize the paragraphs in a streamlined way that helps convey the main point and impart understanding to the readers. Detailed comments on specific issues are listed below. AUTHORS: The revised manuscript will be rewritten.

Specific comments

REFEREE: Abstract; P1L24–26: “The OCS compensation points (CPs) were highly dependent on soil water content and extended over a wide range of 130 ppt to 1600 ppt for the forest soils and 450 ppt to 5500 ppt for the agricultural soil.” If OCS exchange was measured only at “50, 500, and 1000 ppt” (P1L18), how did you obtain a compensation point of 5500 ppt for a certain sample? This has not been explained in the manuscript. Presumably, the only way would be to extrapolate a linear relationship between the net exchange and the concentration. The compensation points derived this way will inevitably have large uncertainties, since there were only three (and sometimes just two!) data points to fit a line. Please provide the uncertainty measures and address the limitations of this method.

AUTHORS: As already given above, we agree that the uncertainty and limitations of our method for determining the CPs should be addressed. We propose to mention that

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the method is limited and naming the magnitude of uncertainty here. Section 2.5 or a new section 2.6 seem to be a better place for a detailed discussion of this matter. We propose not go into too much detail in the abstract but handle it in the corresponding sections instead.

REFEREE: Introduction, P2L6: “In view of the potential role of OCS, . . .” What kind of ‘potential role’? Please clarify.

AUTHORS: In this context the potential role in radiative forcing/cooling.

REFEREE: P2L2–8: “Carbonyl sulfide (OCS) is . . . in the troposphere (Brühl et al., 2012).” I think the first paragraph of the Introduction can be shortened significantly without losing the sense.

AUTHORS: We see no need to further shorten this already compressed overview.

REFEREE: P2L16: “in both the carbon and sulfur cycles”. The ‘sulfur cycle’ is not a major concern of the studies cited here. I suggest removing it.

AUTHORS: We disagree. The carbon and sulfur cycle are overlapping in all these discussions. A new GPP estimation based on the sulfur exchange and the role and relations of sources and sinks for the carbon and sulfur cycle (here OCS) is a hot discussion and this approach can be exploited from both sides.

REFEREE: P2L15–26: “The relationship between concentrations of OCS and gross primary production (GPP) . . .” This paragraph is an elaboration on the use of OCS as a tracer for GPP. While broadly speaking this is a motive for many recent studies on soil OCS exchange, as plant uptake of OCS is not the topic of this study, having such a level of details in the Introduction seems excessive. I suggest cutting this paragraph down to three sentences or so.

AUTHORS: As the referee points out, the use as a GPP tracer is one of the main motivations of OCS research. As such, dedicating 11 lines in the introduction to pointing out the background does not seem excessive to us, as one of the points of an intro-

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duction is providing the context to the field of research. Readers that are more familiar with the topic and not interested in rereading can easily skip the paragraph. The role of forest soils is of significant interest in using the net OCS exchange as a proxy for forest GPP.

REFeree: P3L3–4: “Although the understanding of soils as a major sink helps to explain the ‘missing sinks’ for OCS, soil uptake still shows a wide scatter among different environments. ”Where does the “missing sink” come from, all of a sudden? Please explain.

AUTHORS: This was a translation mistake. We apologize. This part will be rewritten to: “Although the understanding of soils as a major sink helps to explain the general role, soil uptake still shows a wide scatter among different environments.”

REFeree: P3L4–5: “Also, the drivers of soil OCS fluxes and their dependences to environmental parameters (such as soil moisture, soil temperature, and OCS mixing ratio) are still largely unknown.” This is not an accurate statement. Actually, you have cited quite a few studies in the next few paragraphs to show that the drivers are not ‘largely unknown.’

AUTHORS: That is true. We propose to remove this sentence. Instead, after the list of known drivers with their citation we propose to add: “Unfortunately, the way by which these drivers influence the soil-atmosphere exchange and the quantity of their impact is not fully understood and quantified yet.” A reference to the different temperature relationships (Q-10 values) for Pocs and kOCS that have recently been identified by Kaisermann et al., 2018 (ACP, Discussion Paper) will also be included.

REFeree: P3L5–6: “Uncovering the mechanisms for soil OCS fluxes would allow soil atmospheric OCS exchange to be estimated on broader spatial scales.” I think that Berry et al. (2013) and Launois et al. (2015) have already estimated soil–atmosphere OCS exchange ‘on broader spatial scales’. Please clarify what you mean by this statement.

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AUTHORS: The current global estimates are a hot matter of discussion with large questions for oceans and soils regarding sink/source strengths. The current publications are by no way the final answer! We propose to rewrite to: “Uncovering the mechanisms for soil OCS fluxes would allow soil atmospheric OCS exchange to be estimated on a reliable basis to increase the reliability of OCS exchange estimations from local and regional to broader spatial scales.”

REFEREE: P3L12–17: “Furthermore, there is a strong evidence that the OCS exchange between soil and the atmosphere is dependent on the ambient OCS mixing ratio . . .”. What’s missing from the introduction of the compensation point is the fundamental cause of it: OCS uptake is a pseudo first-order reaction and thus depends linearly on the ambient OCS concentration, whereas OCS production is independent of OCS concentration (Conrad, 1994).

AUTHORS: We propose to move the paragraph about compensation point calculation from section 2.3 to here (slightly modified), as you suggested later on.

REFEREE: P3L24: “To deepen our understanding of source and sink characteristics ...”. At the end of the Introduction it is still not clear to the readers what specific research questions this study aims to address. The authors should consider formulating research questions or hypotheses to better orient the readers.

AUTHORS: We agree that adding research questions to the introductions will be an improvement. We propose to add the following questions:

(1)What is the relationship of net OCS release (EOCS), gross OCS consumption (UOCS), gross OCS Production (POCS) and the OCS compensation points to soil moisture and what are possible mechanisms behind this relationship?

(2)How do chemical properties, physical properties and origin (topsoil samples such as Mainz soil and Finland soil vs. organic layer soils from Waldstein sites) influence the net OCS release (EOCS), gross OCS consumption (UOCS), gross OCS Production

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(POCS) response to different soil moistures.

(3) Are there differences in the OCS net release and other properties when a sample has been stored “field fresh” or dried before measurement?

(4) Are our laboratory measurements comparable to reported field measurements at the sampling site?

REFEREE: Material and methods, P4L3: “Soil samples were collected from four sites . . .”, P4L9–10: “We tried to use the same method to collect all soil samples, but we cannot exclude the variability over time.” Can you provide information on when each sample was collected?

AUTHORS: The sampling was 2012, 2012 and 2014 for the Finland soil, the Waldstein soils and the Mainz soil, respectively. We propose adding (sample collected 201X) with the proper year to each soil as they are described in section 2.1

REFEREE: P4L10–11: “Fresh subsamples of agricultural soil in Mainz were oven dried (at 40°C) for comparison.” Please specify how long they had been dried for in the oven. Typically, using the gravimetric method to determine soil moisture would require samples to be oven dried at 105° C for 48 to 72 hours, but of course that temperature would not be desirable because microbial communities could be destroyed. I assume that the choice of 40° C had something to do with this concern, but at this temperature, how did you ensure that the samples were dried completely and the maximum soil moisture used in Eqn (6) was not biased by any remaining water?

AUTHORS: The drying at 40°C described here has nothing to do with the determination of maximum soil moisture. It is just to prepare storage of active soils. We wanted to compare to discern between storage techniques. For the determination of the maximum soil moisture and soil dry weight used in each measurement, all samples were dried at the end of a measurement at 105° C for 48h. To avoid confusion, we propose to include this information both here and in section 2.4.

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REFEREE: P4L16 and P4L26: “deionized water (R 18.2 M Ω)”. The unit of DI water resistivity is M Ω _cm, not Ω M.

AUTHORS: Yes. This will be corrected.

REFEREE: P5L21–23: “This is based on the linear relationship between OCS uptake and OCS mixing ratio shown by Kesselmeier et al. (1999) and the assumption that the ambient OCS mixing ratio does not influence OCS production(see 4.1).” This sentence explains the theoretical basis of the ‘compensation point’ and should better be placed in the Introduction.

AUTHORS: We agree that this should be part of the introduction.

REFEREE: P5L9, Eqn (1): $EOCS = \Delta OCS \times Q / m_{soil}$

This equation works only under a steady state condition. How did you ensure that OCS concentration in the chamber headspace reached a steady state? Please provide relevant details and reasoning.

AUTHORS: That is not true. Using this equation for dynamic equilibria is a long standing and accepted practice. See Breuninger et al. (2012) for detailed explanations. Breuninger et al. explain leaf chambers, but the same principle applies to soil chambers. See R. Oswald et al. (2013) or similar publications for examples of eq1 being used for dynamic chamber measurements. In short: In a well-mixed chamber (ensured by the activity of a fan in the headspace of each cuvette in our setup) the concentration of a trace gas is determined by the concentration in the flushing gas, the rate of flushing and the release or uptake by the soil. The flushing rate is constant and monitored. Factors like soil moisture are very slow changing in relation to the measurement period averaged for calculation by eq1. Chamber effects are accounted for by using a soil free identical chamber for reference. While true steady state conditions can only be archived with static chambers, a dynamic equilibrium was archived in flushed chambers that fulfills all conditions required to employ eq1. This dynamic

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equilibrium was ensured as described below and can/will be involved in the revised paper: Switching the analyzer inlet from one chamber to the next of course causes some disturbances in the chamber and also the inlet tube needed flushing. We did extensive pre-measurements by observing measured chamber concentration with chambers that were empty or loaded with soil samples, waiting for steady concentrations to be measured. With the length of inlet tube and flushing rate used, stable concentrations were always observed after less than six minutes. Based on these empirical determinations, the time each chamber was connected to the analyzer was set to ten minutes. Of these ten minutes, the first 7 minutes were discarded to allow for proper equilibration within the chamber and at the same time allowing proper flushing time for the analyzer and analyzer inlet tube. The following 2.5 minutes were averaged for 30 seconds intervals and used for exchange calculations. The last 30 seconds of each chamber cycle was discarded again to eliminate any chance of accidental overlap.

REFEREE: Results, P7L6–7: “OCS uptake at medium soil moisture was 20% stronger for the Waldstein soil with its young spruce understory than for the one with a blueberry understory.” What range is the “medium soil moisture”? Be quantitative. Waldstein spruce soil showed a peak uptake of 23 pmol g⁻¹ h⁻¹ and Waldstein blueberry soil showed a peak uptake of 13 pmol g⁻¹ h⁻¹ (P7L11). This does not seem to be just “20% stronger”. Please reassess the figure and revise this statement.

AUTHORS: The number will be revised to the proper percentage. Medium soil moisture will be replaced by 35 to 50% WFPSlab.

REFEREE: P8L2: “The OCS compensation points were found to be variable in close dependence on the soil water content.” This is not the case of the Waldstein blueberry soil. This exception needs to be acknowledged in the text.

AUTHORS: That is not entirely true. The dependence is less pronounced but some minima exist at 25 % WFPSlab and 90+ % WFPSlab. This is caused by the overall weaker reaction of UOCS to soil moisture in this soil. We will revise to acknowledge

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the relationship being weaker than with the other soils.

REFEREE: P8L14–15: “The values of POCS were 2, 4, 7 and 30 pmol g⁻¹ h⁻¹ for Mainz soil, Waldstein soil with blueberry or young spruce understory, and Finland needle forest litter, respectively.” Which summary statistics are these numbers? Means or medians? Please specify.

AUTHORS: Medians, rounded to full pmol. The median for the Finland soil will be revised to 27 pmol g⁻¹ h⁻¹. The text will be revised to “median POCS”.

REFEREE: P8L20: “This might indicate involvement of multiple OCS uptake processes (see section 4.2).” The observed OCS gross uptake vs. soil moisture patterns can be reproduced in model simulations even if there is just one OCS uptake process. See Sun et al. (2015) and Ogée et al. (2016). This pattern can be explained as the result of two competing effects of soil moisture—̄inhibition on OCS diffusive transport and activation of microbial activity. In other words, it does not require the presence of multiple OCS uptake processes.

AUTHORS: Though we generally agree with the referee, that microbial activity and diffusion can model a significant part of the exchange, we disagree when looking closer. There is a pattern of maximum OCS uptake under moderate soil moisture for Mainz soil and Finland needles and this is highly different to the other soils and must be discussed. Furthermore, the chemical quality may be of further relevance with special respect to the ammonium content. Additionally, we should regard a mix of enzymatic reactions (CA, Rubisco, PEP-Co and others?) and a mix of organism.

REFEREE: Discussion, P8L24–25: “The OCS exchange from our laboratory measurements of the Finland litter layer soil is of the same magnitude as the field OCS exchange measurements performed by Sun et al. (2017) at the site where our samples were taken . . .”. The units are different (here, pmol g⁻¹ h⁻¹, and pmol m⁻² s⁻¹ in the cited study). How did you make the comparison?

AUTHORS: As we have both the soil mass and the chamber diameter we can easily calculate the OCS exchange of our samples either in $\text{pmol g}^{-1} \text{h}^{-1}$ and in $\text{pmol m}^{-2} \text{s}^{-1}$, as mentioned in section 2.3. It is only a question about to which reference value (surface area or sample weight) the concentration differential is referred. For consistence with our other publications we chose to show our data in $\text{pmol g}^{-1} \text{h}^{-1}$ but for the comparison with Sun et al. we referred to the exchange data calculated according to Eq2. We will make sure to clarify this again in this section.

REFeree: P8L29–30: “. . . the deposition velocities of our lab measurements were corrected based on the temperature optimum curve presented in Kesselmeier et al. (1999) by a factor of 0.852 (the ratio of OCS uptake at 15°C to OCS uptake at 20°C in Kesselmeier et al., 1999).” This correction implies that the temperature dependence of the Finnish soil follows the same relationship in Kesselmeier et al. (1999), which was originally derived from an agricultural soil, with a temperature optimum around 18°C . However, Sun et al. (2017) has shown that the temperature optimum of the Finnish soil exists at a much lower temperature. Therefore, the correction applied to the deposition velocity is not justified.

AUTHORS: Sun et al. explicitly state they could not find a temperature optimum (mainly because they could only measure a small range of temperatures, as per their own statement). See one representative quotation from Sun et al., 2017 at the end of this answer below. Of course one could take that as an argument for not using a temperature correction in itself. But we still think that using the Kesselmeier et al. 1999 temperature curve is the best approximation (though not optimal) we can get. Another approach would be to follow the Kaisermann et al., 2018. They found 1.23 for kOCS Q10 value for 10°C and a large set of different soils. So for 5°C it should be 0.615. Therefore, we can either make no correction at all and ascribing the higher uptake in our measurements to the higher temperature (pointing out the small t-range in the Sun et al. measurements) or use the Kesselmeier et al. 1999 temperature curve as a correction to the best possible approximation and discuss in the text that there

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probably is some shift because of different temperature optima. We prefer the latter procedure. Statement in Sun et al 2017, p8130: “However, a temperature optimum for COS uptake cannot be identified for our site. This is not surprising given that 90% of the data were measured at humus layer temperature in the range of 8.3–16.4°C (below 30 the optimum temperature of ca. 20° C, for example, observed by Kesselmeier et al., 1999), and that temperature and moisture co-vary in natural conditions”

REFEREE: P9–P10: “All three organic forest soil samples were almost exclusively OCS sinks, . . .”. The rest of this section seems purely speculative, theorizing with little support from the data, and strays further from the main point. The discussion is not a place for literature review. Even if there is an abundance of literature on autotrophic and heterotrophic microbes, OCS exchange data alone can neither verify nor falsify a particular microbial mechanism. Please reconsider what you intend to mean in this section.

AUTHORS: based on reviewer input and reconsiderations we have decided to remove the discussion about heterotrophs and autotrophs as well as strongly reducing most discussion of microbial communities. Instead, other options as physical and chemical soil properties shall be included in the discussion of the revised version. Especially a topic of diffusion limitation by high soil moisture and a brief discussion of the possible role of Ammonium oxidizing organisms, as there are significant differences in Ammonium content in the examined soils that coincide with curve shapes of UOCS and kOCS, will be included.

REFEREE: P11L2: “Wet soils have shown to tend towards emission of OCS” ‘Wet soils’ is not accurate, I suppose you mean ‘water-saturated soils’.

AUTHORS: We agree that ‘water-saturated soils’ is much more accurate and will adapt this wording.

REFEREE: P11L8–9: “We suspect these processes to be connected to autotrophic organisms, as discussed in Section 4.4.” This statement is too speculative. First, there

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is no data supporting the presence of OCS consuming autotrophs in your samples. Second, heterotrophs, too, may consume OCS.

AUTHORS: We have decided to remove all discussion about autotrophs and heterotrophs, as stated above. Also, the section can be shortened as it is covered in our follow up study already under review. We propose a merger of the current sections 4.2 and 4.3 (as kOCS and EOCS are necessarily tightly related) by compressing the points discussed therein and adding additional points of view (chemical and physical soil properties, diffusion limitation). One aspect that should be highlighted is the role of Ammonium, as there are major differences in the Ammonium content of the soils in this study.

REFEREE: P11L12–13: “This is in clear accordance with the assumption of a linear dependence of UOCS on the ambient OCS mixing ratio . . .” This ‘assumption’ needs to be introduced in the Introduction first. P11L22–23: “Aside from that, only the experimental data obtained at 50 ppt and 1000 ppt were used to calculate the compensation points.” This goes to the Methods, not the Discussion.

AUTHORS: We agree this belongs into the methods section and should be added there. However, when discussing the results, it makes sense to mention the fact here, too.

REFEREE: The question why the ‘k-coefficient’ varies with soil moisture and shows an optimum remains unanswered.

AUTHORS: The k-coefficient is dependent on EOCS. Since this varies with soil moisture, so will kOCS. kOCS was used here because it is better suited to illustrate how soil moisture affects

REFEREE: P13L23: “Samples had been stored 5 to 9 months.” This information should be provided in the Methods.

AUTHORS: We will add the information to section 2.1

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REFEREE: P13L23–24: “For both, there was uptake of OCS at about 12% gravimetric soil water content, which was reduced when the soil contained a higher or lower amount of water, gradually switching to emission of OCS at wet and very dry states as demonstrated in Figure 7.” Why not just say that the optimal uptake of OCS occurs at 12% SWC?

AUTHORS: Because that would leave out a lot of relevant information. P14L1–3: “Some changes occur both over time and are induced by two different ways of sample treatment (storing ‘fresh’ or air drying the sample before storage), illustrating the importance of consistent treatment and storage of samples that are meant to be compared to each other.” Is there evidence that the differences in soil moisture response patterns arise from the “two different ways of sample treatment”?

AUTHORS: Both samples are from the same site and were collected together (actually one sample was divided after collection and homogenization into 2 subsamples). They underwent the same experimental procedure at the same time. They were stored under the same conditions (dark, 4 °C.) The only difference is their treatment before storage. So while it cannot be excluded that the differences were caused by random fluctuations, the pre-storage preparation is the only difference in the two samples which may explain our results. Prominently, this pre-storage treatment led to an increase of the ammonium content of the soil. According to Thion and Prosser (2014), Ammonium oxidizing bacteria (AOB) get reactivated after drying very fast and utilize the ammonium enriched during the drying.

REFEREE: This whole section should either be removed or reduced to two sentences. The point boils down to the first sentence that soil OCS exchange is a small fraction of the ecosystem OCS exchange; the rest is merely a lengthy digression (P14L12–30). Not only does the latter half of the paragraph lack relevance, but it philosophizes over leaf area index without any support from empirical data. The purpose of this section needs to be reconsidered.

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AUTHORS: We agree and propose scrapping section 4.6, moving the main message (OCS exchange from that soil being only a small fraction of the ecosystem exchange under normal conditions) in a much reduced form to another section.

REFEREE: The Conclusion is poorly structured and reads like a first draft. Please find out the meaning and rewrite it.

AUTHORS: The conclusions will be rewritten, following the research questions from the introduction.

REFEREE: Figure 1 & 2, Both figures show the patterns of OCS exchange versus soil moisture. What is the reason to split the data into two figures? Why not show the 500 ppt data and the “Mainz dry soil” data in Figure 1?

AUTHORS: We agree and will merge Figure 1 and Figure 2.

REFEREE: Figure 3, Please consider adding error bars on compensation points. The uncertainty in OCS exchange should propagate to the compensation point.

AUTHORS: We agree and will add error bars.

REFEREE: Figure 4, 1. The unit of kOCS is missing. 2. Net OCS flux is referred to as “net release” here, but “net exchange” in other parts. Please harmonize the terminology.

AUTHORS : 1. The unit is $\text{mol g}^{-1} \text{h}^{-1}$ and will be added in all relevant places.

AUTHORS : 2. Agreed. EOCS shall be “net release”, UOCS shall be gross uptake and POCS shall be gross production and in all instances only these terms will be used. We are using the term release because “flux” is defined as in $\text{pmol m}^{-2} \text{h}^{-1}$, while a release is in $\text{pmol g}^{-1} \text{h}^{-1}$.

Technical comments

REFEREE: P1L12: “Carbonyl sulfide (OCS) is a chemically quite stable gas in the

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troposphere (lifetime ~2-6 years) and consequently some of it is transported up to the stratosphere where it contributes to the stratospheric sulfate layer.” This sentence is verbose. In the second half you may just say “. . . and is a precursor of the stratospheric sulfate aerosols.”

AUTHORS: Both are acceptable.

REFEREE: There are updated estimates of OCS lifetime from Campbell et al. (2008): 1.5 to 3 years. (I assume that you got the 2–6 years estimate from Khalil et al., 1984.)

AUTHORS: Agreed, we will use the updated estimation

REFEREE: P1L13–14: “Due to the similarities in uptake mechanism between OCS and CO₂” ‘uptake’ → ‘leaf uptake’

AUTHORS: Yes, thank you. It is of course better to be specific.

REFEREE: P2L13: “Furthermore, changes of the sink strength of vegetation as a response to global change is a matter of discussion”: ‘is’ → ‘are’

AUTHORS: Yes, thank you. We will correct this.

REFEREE:P3L4–5: “their dependences to environmental parameters . . .” “their dependence on environmental parameters . . .”

AUTHORS: Yes, thank you. We will correct this.

REFEREE: P7L23: “1000 ppm OCS” I believe this should be “1000 ppt OCS”.

AUTHORS: Yes, thank you. This was a typo.

Referenced literature:

Breuninger, C., Oswald, R., Kesselmeier, J., and Meixner, F. X. (2012) The dynamic chamber method: trace gas exchange fluxes (NO, NO₂, O₃) between plants and the atmosphere in the laboratory and in the field, *Atmos. Meas. Tech.*, 5, 955-989, doi:10.5194/amt-5-955-2012.

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R. Oswald et al. (2013) HONO Emissions from Soil Bacteria as a Major Source of Atmospheric Reactive Nitrogen. *Science* 341, 1233, DOI: 10.1126/science.1242266.

Kaisermann, A., Ogée, J., Sauze, J., Wohl, S., Jones, S. P., Gutierrez, A., and Wingate, L.: Disentangling the rates of carbonyl sulphide (COS) production and consumption and their dependency with soil properties across biomes and land use types, *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2017-1229>, in review, 2018

Kuhn, U. and Kesselmeier, J. (2000) Environmental variables controlling the uptake of carbonyl sulfide by lichens. *J. Geophys. Res.* 105 (22), 26783-26792.

Thion, C., & Prosser, J. I. (2014). Differential response of nonadapted ammonia-oxidising archaea and bacteria to drying–rewetting stress. *FEMS microbiology ecology*, 90(2), 380-389.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-20>, 2018.

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