Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1229-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Disentangling the rates of carbonyl sulphide (COS) production and consumption and their dependency with soil properties across biomes and land use types" *by* Aurore Kaisermann et al.

M.E. Whelan (Referee)

mary.whelan@gmail.com

Received and published: 27 March 2018

This is a well-conceived set of experiments to further our understanding of oxic soil OCS exchange. The approach to calculate the hydrolysis constant is commendable. It's also nice to see the 1996 Lehmann and Conrad study getting more use. Please note the follow up study in 2000 by Conrad and Meuser, "Soils contain more than one activity consuming carbonyl sulfide" Atmospehric Environment, 21, 3635-3639.

Major Comments

P3:L22-25 I'm not sure how theta going to zero results in the simplified equation presented here. Are there some assumptions that need to be spelled out?

P5:L5-20 Please add more detail. For either method, were the jars partially sealed, generating higher CO2 levels than ambient? Were any sensitivity studies performed – for example, did you find that incubating the soils for less than 2-3 days or two weeks led to different results? For method 2, were these soils kept in the dark as well? When soils were air dried, were they put into a jar or spread out in a pan for a more even drying? Sieving is an important choice here, too. Litter plays a role in surface OCS fluxes, sometimes contributing nearly all of the OCS uptake. Sieving removes most of the litter and soil structure. While we can't have everything in our experiment vary, it would be worth justifying the method approach a bit more. Regarding maintaining soil moisture by adding water – if the soil has dried out enough (probably crossing some threshold that has yet to be described) and water is added, the soil can experience a dramatic increase in OCS uptake that takes several hours to days to recover from. This is akin to the "Birch effect" for OCS. Please mention how much water was typically added to the soils. Did you see any decay curve in the soils that were maintained this way? Also, 18 hours is a long time to have dry air run over soils without substantial water loss. Were soils checked and re-watered during the incubations?

P7:L10 Is 5 degrees sufficient to calculate a meaningful Q10? Also, OCS uptake rates tend to exhibit a temperature optimum. The Q10 idea links the rate of reaction with a constant increase in rate with increase in temperature. Please justify the use of Q10. It would be good to know the natural variation in temperature of the sites as well.

P7:L15 I was not aware that soil redox potential could still yield a valid measurement after 2 weeks. How do you think this variable changed during the incubations themselves?

P8:L10 What is going on with the green points in Fig 2 that have a wide spread? Also, it appears that sometimes production is negative. Do greater uncertainties need to be

included?

P10:L29 I would expect that soils experiencing generally higher temperatures would also experience higher optimum temperatures for soil OCS uptake. Also, there's a seemingly abrupt shift in the discussion in this section, where referring back to "our finding" on L32 is a bit of a whiplash.

P11:L2-3. OCS production from autoclaved soils is assumed to be abiotic, with some sort of organic material as the substrate. In this way, OCS emissions from "dead" soils is directly related to past biological activity. Some enzymes can survive autoclaving. I am skeptical that these enzymes can then continue their OCS production for days in high temperatures and with very little water. Please do this experiment! Otherwise, this part makes it sound like only in tact enzymes can relate emissions to biological activity in dead soils. We do not need so creative a hypothesis for the argument.

Minor Comments

P2:L2 The global warming potential of OCS is roughly balanced by its "global cooling" potential, see Brühl, C., Lelieveld, J., Crutzen, P. J. and Tost, H.: The role of carbonyl sulphide as a source of stratospheric sulphate aerosol and its impact on climate, Atmos. Chem. Phys., 12(3), 1239–1253, 2012.

P3:L3 We did do a variable OCS concentration experiment in Whelan et al 2016 (the soil incubation study), without high OCS concentrations, see Fig 4 in that paper.

P4:L11 Reports of their values are scarce.

P4:L30-31 missing references. But do you really need a reference for linear regression?

P5:L22 Were there any sealants used to get the lid air-tight?

P6:L26 Is SFdry just Fdry?

P7:L22 This sentence is a lot to unpack. Please break it up.

P8:L2 The variability of the net fluxes?

P8:L22-P9:L6 This section needs a better paragraph structure. The first sentence is good. the ending is good. In between needs better vision of why each number is being reported.

P9:L16 Should be Whelan et al., 2016.

P9:L21 errant comma

P9:15 to P10:L16 The first part of this discussion has good content, but unnecessary parentheticals and some needlessly complicated sentences. Please rework.

P10:L25-26 Conrad did a follow up study that claims a second OCS soil uptake pathway at high concentrations, see citation above.

P11:L6 "Agricultural" is a better word than "arable" here. I know they're referred to as arable soils in the literature, but arable refers to soil that could support crops, where agricultural means that there are actual crops present. In all studies referred to here, there are crops present.

P12:L13-17 This overstates the case for the study. It's not clear why the relationship between N and S is now relevant where it wasn't before, or why the relationship between N inputs and S emissions constitutes a new modeling framework for atmospheric chemistry.

P12:L25 and elsewhere. Ogee 2016 model publication didn't have a production rate that wasn't redox dependent. Referring to the model via its citation might be misleading.

P13:L10 It is well known now (hopefully) that, although the Kettle 2002 study was an excellent first guess, it should not be used for global modeling studies.

P13:L12 Do you mean to have the second "modelling" there?

P13:L19-23 This sentence has a lot of information crammed into it. Please rephrase it,

perhaps breaking it up into two sentences.

P20, and elsewhere, you need a Whelan 2016a and 2016b.

P24 Figure 3 demonstrates the complexity of the analysis without adding further information. Please move this to the supplement.

P25 Figure 4, it looks like the color bar has discrete colors, but the numbers are on a continuous color spectrum? This is a little confusing, because it looks like different data might be shown on either side of the diagonal. Unless I'm misreading it, this figure only needs to present the rho's once (use either side).

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1229, 2018.