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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Uptake of gaseous formaldehyde by soil surfaces: a combination of adsorption/desorption equilibrium and chemical reactions" *by* Guo Li et al.

Anonymous Referee #2

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This paper discusses a laboratory assessment of HCHO uptake on a soil sample using a novel soil chamber design. Recent work has begun to illustrate the potentially large atmospheric impact of soils as both a sink and source of many important trace gases. This manuscript describes laboratory experiments motivated by the need to improve our ability to parameterize the net impact of soils on global HCHO budgets. The work is very well organized and presented in a clear and concise manner. The experimental design is well thought out and for the soil type sampled a very thorough analysis of parameters such as humidity and concentration are presented. While the applicability of these measurements to real world quantification of the atmospheric impact of soil emissions would improve significantly by the addition of further experiments, I find the

paper of sufficient quality and content to warrant publication in the current form provided the comments discussed below are addressed. In particular, I believe the paper is in need of additional discussion to temper the conclusions of the paper in terms of the general applicability of the results to soil globally and even regionally, my opinions and suggestions will follow in the general comments section of this review.

General Comments:

I can imagine that the following comment will not be new to the authors but would like to see it discussed a bit further in the manuscript. The work is performed on a soil sample that has been 'sterilized' such that the effects of microbial communities can be eliminated from the experiments. In doing this only the physical and chemical properties of the soil are considered when developing a mechanistic interpretation of the results. The manner with which the authors present the results suggests that these uptake coefficients and mechanisms can be applied to a wide range, if not all soil types, and be used in as a parameterization in an atmospheric model. However, as many soil studies have shown before and the authors undoubtedly would admit by the treatment of the soil samples, addition of microbial communities have the potential to drastically alter the results of these experiments. This concept is even discussed by the authors stating that sterilization was performed to yield reproducible results. Furthermore, addition of biological litter mixed with the soils, as a soil would be expected to exist naturally, can radically alter the trace gas emission or uptake. These points make any parameterization of uptake coefficients derived from a sterile soil unrepresentative of global soils and their potential impact on atmospherically relevant gases. The authors need to put their results in context in a much more conservative manner explicitly identifying the limitation of utilizing this overly simplistic soil laboratory model. Furthermore, in using only a single soil claims about the applicability of these observations to other soil systems are a stretch and not backed by the laboratory data. This paper is a fine example of how these types of experiments should be performed and certainty presents a novel technique and a unique useful data set, however I feel the conclusions attempt to extrapolate beyond the capacity of the experiments presented. As presented, the results here have very little relevance to the atmospheric impact of soils on HCHO.

Specific Comments:

Calibrations for this instrument were done using liquid standards, were their any corrections applied to the data to account for removal efficiencies of the scrubber? Would there be differences in the calibration if it were done with a gas phase standard? How is the measurement potentially affected by the changing humidity, e.g. is there a humidity dependence in the HCHO measurement? I understand that these experiments were likely performed when flowing clean N2 over the coated reactor, but can the authors include a statement on how the instrument background changed with relative humidity? If there are background humidity effects these could potentially be amplified with the addition of HCHO.

The statement “the partial reversibility of HCHO uptake as shown in Fig. 7 can thus be expected as a general feature for various kinds of soils.” is a conclusion based on the authors support of the idea that most soils share a similar metal oxide composition. However, I am not entirely sure the claim of various soils showing similar reversibility can be made considering the mechanism the author discusses within this paper do not deal with the chemical composition of the soil, but seem more controlled by surface area effects which could be expected to vary largely between soil types. Surely other factors such as soil pH and surface morphology of a given soil would have a more dominant control on the reversibility of uptake than the metal oxide composition.

Page 7, line 19: “Under dry conditions, higher HCHO concentrations”, in this case what is the higher HCHO concentrations used? Page 8, line 15: It seems this sentence is in need of an edit as it reads awkwardly: “. . .dissociation and desorption equals to that increased in the gas flow per unit time.”

Page 8, line 16: delete the word ‘as’ after “coefficient k,”.

Page 9, line 6: delete the word ‘in’ after “the soil investigated”

Page 10, line 20: delete the word ‘the’ before “regime III”.

Page 11, line 26: edit to read “a similar effect” Page 11, line 29: edit “partial” to ‘partially’

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