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***Interactive comment on “Soil atmosphere
exchange of Carbonyl Sulfide (COS) regulated by
diffusivity depending on water-filled pore space”
by H. Van Diest and J. Kesselmeier***

Anonymous Referee #2

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GENERAL COMMENTS

This study aims to characterize the influence of local environmental parameters (specifically soil water content, water filled pore space, and temperature) on soil-atmosphere exchange of carbonyl sulphide. The results are based on laboratory analyses using four different soil types. Uptake by soils may constitute a significant global sink of COS, and analyses characterizing this flux are very sparse. This present study is, therefore, a valuable contribution towards understanding the processes governing the atmospheric budget of COS. I recommend publication once the authors have addressed the comments below.

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The paper is clearly written, for the most part. Certain sections (see 'Specific Comments' below) would benefit from further explanation and clarification. Much of the methodology and analysis follow that of a previous study (Kesselmeier et al. 1999) from the same group, and the reader is often referred to that study. This manuscript would, however, gain in readability and clarity with a more detailed discussion of the assumptions and rationale for the empirical flux model used to interpret the measurements (discussed in section 2 of paper).

SPECIFIC COMMENTS

1) A weakness is the insufficient discussion of the significant difference (factor of four) in the magnitude of the optimum flux for the German soil measured in this study (section 3) in comparison to Kesselmeier et al. 1999. This is ascribed to 'unknown' factors. Are these biological factors related to levels of bacterial/enzymatic activity? What could be the potential range of variation for sampling soils from one site at different times? While explicit quantification of these factors may be beyond the scope of this study, the causes and implications for soil uptake should be discussed, and would strengthen the contribution of this study towards improved quantification of soil COS uptake.

2) Another result which requires further discussion are the significant differences in optimum temperature and optimum WFPS between the German soil and the other soils (see Table 2). This is briefly touched on in section 3, but without any detailed discussion of possible causes.

More detailed comments follow, listed by section.

1. SECTION 1 : INTRODUCTION

1) Introduction, pg 2, 1st paragraph: COS also has a chemical sink in the troposphere through reaction with OH [Chin and Davis, 1995; Kjellstrom, 1998].

2) Introduction, pg.2, 3rd paragraph: 'These sinks and sources "obviously" drive the seasonality in NH and SH'. I suggest that the authors drop the wording 'obviously' and

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rephrase this sentence. The causes of observed seasonal variation have not been conclusively established. Montzka et al. [2007] suggest possible linkages based on comparison of observed variations in atmospheric concentrations with seasonal variations in the parameterized fluxes of Kettle et al. [2002]. Much remains uncertain in the COS budget, including seasonal variation of some of the components (e.g., the ocean fluxes).

3) Introduction, paragraph 4, pg 2-3: Lines beginning with 'One of the main controlling parameters for soil atmosphere exchange.. ..including the role of compensation points'. : This sentence should be clarified and expanded upon for non-specialists, particularly as the phenomena it refers to constitute the main subject of the study. A short explanation of the model underlying the characterization of soil-atmosphere gas exchange would be helpful (e.g., as in Kesselmeier et al. 1999, section 3).

2) SECTION 2 : MATERIAL AND METHODS

1) Section 2.1, pg 4, Equations 1 and 2 : Is the source reference for these equations Hillel (1980) ? This could be made clearer.

2) Section 2.2, Top of pg 5: Discussion of gas exchange rate : Need reference for Equation 3.

3) Section 2.2, pg 5 : Sentence starting 'Effects of fluctuations of COS mixing ratios..'. This needs more explanation, in particular, with regard to how use of deposition velocities eliminates COS fluctuations. Also needs a reference for method used.

4) Section 2.5 : This section as currently written is somewhat confusing. It could be improved by the following: a) Outlining the rationale for using the Meixner and Yang (2006) equation; b) Explicitly stating the equation or functional relationship used; c) A better explanation of the last two sentences of section : 'The mathematical fit.. Nevertheless the data point..'. As currently written, it is not clear what the authors mean here.

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3) SECTION 3 : RESULTS

1) pg 7, 1st paragraph : Comparison of current results with Kesselmeier et al. 1999, and factor of 4 difference in maximum uptake. As noted previously, the significant difference between measured maximum uptake rates needs further discussion to identify possible underlying factors (e.g., variations in microorganism population, other biological or physical controls ?). An appropriate caveat should be added to the claims of 'reproducibility' of measurements in this section. They have been shown to be reproducible with regard to the optimum temperature and WFPS, but not with regard to the flux magnitude.

2) pg 7, 3rd paragraph : Line beginning 'It is well known that a variable parameter.' needs to be supported by a reference.

3) Bottom of pg 7: Line beginning 'Surprisingly...'. Drop 'surprisingly'.

4) Section 3.1 : pg 8, The paragraph discussing the Finnish soil in comparison to the others is unclear. In particular, the sentence beginning 'In this temperature range, soil WCs between 7 and 20%...' should be rewritten. I think the authors mean to make the point that deposition velocities for the Finnish soil are higher than those for other soils for a range of WFPS values, however this is not clear from text. The switching between use of 'WC' and 'WFPS' in this section is also confusing. Do the authors mean to use WFPS throughout ? The figures discussed in this section are presented in terms of WFPS.

4) SECTION 4 : DISCUSSION

1) pg 10, 2nd paragraph : Discussion of optimal temperatures for temperate vs. boreal soils. The significant difference in optimum temperatures between the German soil and the other soils requires further discussion on possible causes. The sentence beginning 'These results may be discussed as an adaptation of boreal soils.' is not clear and needs further explanation.

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