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Interactive comment on "A soil diffusion-reaction model for surface COS flux: COSSM v1" *by* W. Sun et al.

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Response to Referee #1

> *I have found the paper "A soil diffusion-reaction model for surface COS flux: COSSM v1" by Sun et al very well organized and clearly written. The description of methods and their discussion of results are comprehensive and persuasive. I only have two minor issues with the paper (see below) and recommend the paper for publication after minor revision.*

We thank the referee for the positive evaluation of this manuscript.

> *My first issue is the on the representation of diffusivity. The authors indicated the model tries to resolve the dual-phase transport, however, the equation (13) only resolves the gaseous phase diffusivity and no equation for aqueous diffusion is provided. Ignoring aqueous diffusion implies no COS transport for a fully saturated soil or transport is only restricted to the first numerical node. I suggest the authors to clarify this, or consider adding aqueous diffusivity explicitly. As a matter of fact, adding aqueous diffusivity will make their model well posed under all moisture conditions, and even enables its application under freezing conditions.*

The current model is mainly for upland soils and not for wetlands, since COS processes in wetlands are not well understood yet. We did not explicitly resolve aqueous diffusion but rather linked it with the gaseous diffusion process by assuming chemical equilibrium between gaseous and aqueous concentrations. Nevertheless, the mass balance of the aqueous concentration is accounted for by the Henry's law constant in Eq. (1) on page 5142.

There are two major reasons for this implementation. First, for most soils that are COS sinks, the fact that aqueous diffusivity is 10^{-4} smaller than gas-phase diffusivity makes the error from neglecting aqueous diffusion much smaller than the detection limit of a dynamic chamber system. Second, dissolution equilibrium between the gaseous and aqueous phases also acts on a timescale much shorter than aqueous diffusion. Implementing an explicit solver of aqueous diffusion would significantly increase the computational demand, while the gain in accuracy would be marginal in most of the conditions. We have provided in the supplement a quantitative evaluation of the possible error from neglecting the aqueous diffusion.

The only condition in which neglecting the aqueous diffusion causes significant errors is when the soil is fully water-logged. This mostly happens in wetlands that are typically COS sources, for example, freshwater and salt marshes. However, field data of COS flux in such environments are extremely limited.

> *My second minor issue is on their discussion of the advective effect in Page 5143. I think they should mention that change of atmospheric pressure will also affect the COS emission, just like it would affect soil emission of CO_2 and CH_4, which were often observed in the fields and might be too import to ignore when their model is integrated in a large scale model for applications over a wide range of environmental conditions.*

We agree with the referee's point. We have changed "uptake" to "flux" to clarify this.

Interactive comment on Geosci. Model Dev. Discuss., 8, 5139, 2015.