Geosci. Model Dev. Discuss., 8, C2075–C2078, 2015
www.geosci-model-dev-discuss.net/8/C2075/2015/

Geoscientific Model Development Discussions

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

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Received and published: 17 September 2015

Response to Referee #2

> *The code used to construct this model and data used to validate it should be publicly available. Please provide a doi link to these.*

We prefer to provide the code on request, because active development is still going on at this time.

> *Pg 5146 line 10: I agree with Reviewer #1. You need to either include aqueous diffusion or state that you are only dealing with gas phase diffusion*

> *(and justify either choice).*

Please see the response to Referee #1 and changes in the manuscript.

> *Pg 5146 line 14: You assume that the litter layer diffusivity function is assumed to be the same as that of the soil. A sentence or two justifying this assumption would be helpful here.*

In the absence of experimental data of litter layer diffusivity, we consider that a similar relation for the effective diffusivity in the litter serves as a good approximation because litter is a porous medium just like the soil. The parameter b in Eq. (13) could be different for litter layers, given that this parameter depends on soil textures. For the litter here which have very high porosity (0.94 m^3 m^{-3}), we have $\theta_w \ll \theta_{sat}$, and $\theta_a = \theta_{sat} - \theta_w \approx \theta_{sat}$. Therefore, practically the term $(\theta_a/\theta_{lit})^{3/b}$ for the litter will be very close to unity no matter what the value b is. We have added this point for justification.

> *Pg 5146 line 16: Can you briefly (sentence or two) say how Seibt determined the CO2/COS ratio of 1.2? I think that would help to clarify why your Dm is reasonable.*

It was calculated from Chapman-Enskog theory, which links the gas-phase diffusivity to collisions of ideal gas molecules. Gas molecules are assumed to be rigid spheres that undergo elastic collisions, from which the molecular mass flux and hence diffusivity are derived. This has been clarified in the manuscript.

> *Pg 5147 line 11: Can you add a reference for this statement? I think it's true in sea water but it would be good to refer to some field observations here.*

We have added two references for field observations of COS concentration in seawater there. The molar fraction of COS in seawater is usually $\sim 10^{-12}$.

Pg 5153 line 14: This paragraph needs to be clarified. It kind of jumps out of nowhere. Maybe I missed it but I didn't see a discussion of setting Teq and Topt nearby so a sentence or two justifying the numbers chosen will better help the reader to follow this.

We have rewritten both this paragraph and the description of the two variables below Eq. (22) to clarify this. Only T_{eq} is the parameter to set, and T_{opt} is obtained from Eq. (22) based on T_{eq}.

Pg 5153 line 19: A reference that would justify the approach of interpolating between the sparse soil moisture measurement depths is needed here.

Unfortunately, at the SGP site there is no measurement of soil moisture below 30 cm. In choosing this interpolation, we followed the examples of simulated 1D soil moisture profiles with gravity drainage boundary condition in Walker (1999), in which the changes of soil moisture in deeper layers are much smaller than in the top 20 cm. See Walker, J. P. (1999), Ph.D. Thesis, University of Newcastle. <http://users.monash.edu.au/∼jpwalker/thesis.html>. We have included the reference here. Moreover, the conditions below 10 cm do not significantly influence surface flux according to Figure 11.

Pg 5153 line 5: You haven't mentioned WFPS since page 3. please explain it again here to save the reader looking back.

We now define the acronym "WFPS" again in the beginning of section 4 Discussion, so the readers won't need to scroll back to the introduction section.

Figure 8: The figure caption should read Observed and simulated fluxes I presume...

We have amended the caption of Fig. 8. It now reads: "Simulated COS fluxes at the Stunt Ranch site for three scenarios, *compared with the observations*. . . ".