

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

### **Anonymous Referee #2**

Received and published: 7 August 2015

The paper is well written and logically organized. The methods are clearly described. I recommend publication of this paper with a few minor corrections.

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

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).

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.

Pg 5146 line 16: Can you briefly (sentence or two) say how Seibt determined the

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CO<sub>2</sub>/COS ratio of 1.2? I think that would help to clarify why your Dm is reasonable.

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.

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.

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

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

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

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

**GMDD**

8, C1628–C1629, 2015

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