

## ***Interactive comment on “A new top boundary condition for modeling surface diffusive exchange of a generic volatile tracer: theoretical analysis and application to soil evaporation” by J. Y. Tang and W. J. Riley***

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Received and published: 7 February 2013

Below we put the reviewer's comments as italic. Our reply starts with "response".

*This paper presents a new simplified, theoretically derived expression of soil evaporation and compares the new expression with other existing simplified models. The modeling approach is original. However the current form of the paper suffers from a lack of observational data based assessment.*

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*The manuscript is thorough and may provide useful insight on how to parameterize soil evaporation by taking into account the various physical processes involved in evaporation.*

**Response:** We thank the reviewer for his/her positive comments on our study, and address the issue of comparison with observations below.

*However, modeled estimates are not compared with observations. As a result, I was not able to evaluate the proposed modeling approach. The new TBC is compared with existing empirical parameterizations of soil evaporation, but how to evaluate the improvement without comparing model results with observations?*

**Response:** We agree that we currently have difficulty finding a comprehensive observational dataset to evaluate our theoretical developments. We clearly made this statement in the paper (P11963 in the discussion paper) and discussed in some detail (section 4.1.1 and section 4.2) pitfalls of existing experimental studies that we are aware of. Since our conceptual model is rather different from that used in existing studies, one potentially beneficial outcome of the current manuscript is that experimentalists would find it worthwhile to make the relevant measurements so that our theoretical work can be more thoroughly evaluated under broader conditions (see below). However, in writing the paper, we have tried our best to evaluate various aspects of the model, including comparisons with measurement-based empirical soil resistance models (sections 2 and 3; P11959 – P11961). In our revision, we more clearly define what a minimal dataset would be to evaluate our model (P35, L21-23; P36, L1-8).

*The authors consider their approach as “physical” but a number of physical quantities ( $K_{sat}$ ,  $D_g$ ,  $D_w$ ,  $D_0$ ,  $K_1$ ,  $B$ ,  $D_1$ ,  $\Delta z_1$ ,  $\epsilon_1$ ) are difficult to estimate over extended areas. In my opinion, developing a model from physical considerations does not mean that the “physically-derived” model is more correct or robust than empirical ones. It is rather an assumption that should be tested using observational data.*

**Response:** We agree that a physically based model does not necessarily predict sys-

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tem responses more correctly or robustly than empirical approaches. However, as we stated in the paper, one benefit of our physically based model is that it provides an improved characterization of the caveats of existing empirical approaches. In addition, our approach is developed in a more general context than only of predicting soil evaporation, and it should enable a more consistent formulation of bi-directional exchanges of volatile chemicals between the atmosphere and soils. We note that these exchanges often cannot be accurately predicted by existing empirical formulations, which manifests as large uncertainty in existing numerical modeling studies (see e.g., Reichman et al. (2013)). The latter point is of critical importance in our development of atmospheric chemistry and isotope transport models and we hope for collaborative evaluation in future studies.

*Figure 3, 4, 7 and 8: the evaporative efficiency simulated by the new TBC is apparently not a monotonic function of soil moisture: it decreases slightly and then increases with soil moisture. Has this unexpected behavior a physical meaning? Or is this an artifact of the modeling approach?*

**Response:** This feature of the model prediction does have a physical meaning, and we thank the reviewer for highlighting this behavior. In the revision (P24, L20-23; P25, L1-7) we emphasize that this non-monotonic behavior in the evaporative efficiency indicates the transition from water vapor transport dominated evaporation to direct liquid water evaporation. However, under most natural conditions: (1) dew adsorption might dominate the evaporation for very dry soils and (2) the transition is of such a small magnitude that field experiments will find it difficult to observe such non-monotonic behavior.

*Table 1: van de Griend and Owe (1994) and Sellers et al. (1992): the powers of 10 should be replaced by the powers of e (exp. Function).*

**Response:** We corrected these typos.

**Reference** Reichman,R., Yates, S.R., Skaggs, T.H., and Rolston,D.E.: Ef-  
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fects of soil moisture on the diurnal pattern of pesticide emission: Numerical simulation and sensitivity analysis, *Atmospheric Environment*, 66, 41-51, doi: 10.1016/j.atmosenv.2012.10.002, 2013.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 11941, 2012.