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Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #1

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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.

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. However, modelled 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?

The authors consider their approach as "physical" but a number of physical quantities (Ksat, Dg, Dw, D0, K1, B, D1, deltaz1, epsilon1) 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.

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?

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).

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