



## ***Interactive comment on “Development of an efficient coupled model for soil–atmosphere modelling (FHAVeT): model evaluation and comparison” by A.-J. Tinet et al.***

**A.-J. Tinet et al.**

anne-julie.tinet@univ-lorraine.fr

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We would like to thank the reviewers for their comments. Considering that some of the points addressed are common to all the reviewers and especially some key comments, we chose to answer those points in a general reply. Other specific points are addressed in replies to each reviewer. The general points that came to the attention of the reviewers may be divided in four parts:

- Confusing description of the aim of the paper
- Choice of the benchmarking

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- Modeling the vegetation
- Performance of the model in regards to computation time

Aim of the paper:

It seems quite evident in all the reviews that we failed to express clearly the objectives of the paper. In order to remediate this issue, the introduction was completely rewritten to:

- Express clearly and directly the objective of the paper which is to evaluate if switching the regular Richards equation to a generic (therefore extended) Ross solution within a coupled model did not lead to significant discrepancies in results.
- Limit the literature reviews on side subjects which may create confusion, and therefore be more coherent altogether. Specifically, discussion on PTF was partly moved to the model description as well as working hypotheses.

Choice of the benchmarking:

Two reviewers pointed out the choice of TEC as a benchmark. Reviewer 1 suggested benchmarking the model against analytical solution. The Ross solution has already been benchmarked against analytical solution (Varado et al., 2006a) as well as the soil energy balance module. However, to the authors' knowledge there is no analytical solution for a coupled model such as the one studied here. Reviewer 2 regrets the absence of comparison to experimental results. As pointed out by the reviewer, the model TEC has itself been validated against experimental results (in Chanzy et al. 2008), which is the reason why we chose TEC as benchmark. Initial evaluation of the model included both validation against analytical results (in non-coupled conditions)

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and against experimental data (similarly to the work done with TEC). However, the objective of the study is to see if the degradation of the resolution method for Richards' equation using Ross solution and looser coupling allowed acceptable results. To this end, we need a benchmark considering heat and mass transfers and a way to consider different soil hydraulic characteristics. This is not possible with analytical solution and comparison with experimental data leads to questioning the parameterization (of modeler's choices) rather than the model itself. Therefore, the flexibility and the physics of TEC are appropriate to do such a comparison. We tried to be more explicit on the choice of the benchmarking within the introduction of the TEC model.

### Modeling vegetation:

We understand the importance of vegetation related processes (root water uptake, transpiration, crop development...) when dealing with agronomical modeling. Such consideration of the vegetation leads to strong uncertainties related to the plants "geometry" and the plant behavior in regards to water content and atmospheric forcing. Moreover, and unlike the case of soil mass balance or soil energy balance, there is no consensus on how root distribution, root water uptake and transpiration should be modelled. Therefore, we believe that the study of such processes requires a focus on their own. This is the reason why we chose to first validate our model on bare soil, studying specifically other processes. In particular, we studied the coupling with heat transfer, which is necessary to allow a coupling with surface energy balance and thus have the capacity of driving simulation through atmospheric forcing. That said the developed model, as its name suggests, is further developed to account for vegetation. Details on the choice of bare soil were added to the introduction and mention of the development of vegetation related models in present in the perspectives.

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## Computation time:

More information on the efficiency of the Ross solution in the coupled model was added. However, for detailed work on the efficiency of the Ross solution, we refer to the works of Ross (2003) and Crevoisier et al. (2009) who demonstrate the efficiency of the Ross solution and its ability to deal with coarse grids. Moreover, since we used already published results on TEC, the computation was not done on the same hardware (neither with the same configuration of the hardware). Both computers were 'regular' desktop computers but all these points influence the results on computation times. Therefore, due to all this uncertainties, we choose not to present detailed results on computation time but orders of magnitudes. Finally, I would like to point out that proving the efficiency of Ross solution was not the objective of the paper, but rather that using an efficient (as demonstrated in literature) model such as Ross in a coupled and more generic model would not lead to significant errors in numerical results.

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