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Discussions

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

Anonymous Referee #1

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The manuscript describes the model development of a coupled soil-atmosphere model based on Ross (2003) solution for the Richards equation and the De Vries (1963) soil energy balance equation. The model should be used for as predictive software, and therefore, computational time should be reduced. After model description, the newly developed model was compared to an existing model (TEC of Chanzy and Bruckler, 1993). In general, the topic and the research presented are appropriate for publication in HESS. Unfortunately, the manuscript does not always match all scientific criteria for publishing (see specific comments). Therefore, the I recommend major revisions.

General Comments:

The general, the introduction should be carefully revised (see specific comments). The major drawbacks I do see is the fact that the model mainly focus on faster computation of the Richards equation and the coupling to a heat transport equation while other processes necessary for precise prediction of the soil water balance and state are neglected such as plant growth and root water uptake. Additionally, I do not see the necessity to increase model complexity by including the heat balance equation while neglecting important processes such as root water uptake. Additionally, sources of uncertainty are also mentioned in the introduction such as uncertainties associated by the choice of PTF while other sources of uncertainty (and maybe more important ones) such as unknown rooting depths, root water uptake, atmospheric forcings are not discussed. Actually, I do not get the full message of the ms. There is a strong focus on the coupling between water and heat transfer but only the water flow will be evaluated later on. This problematic are also stated in P8579 L 19 to 25. Therefore, the data and the scope of the ms do not really fit together. Finally, I would suggest benchmarking the new model (or implemented Ross approach) versu analytical solutions for different boundary conditions instead of benchmarking versus a model which might be set up in a different way such as grid discretization etc. (see also special comments later). As far as I understand the major advantage of the new model approach is fast computation. Therefore, I wonder why only mass balance errors are shown as indicator for the goodness of the model instead of comparing CPU time of the models for same setups and problems.

In general, either use soil moisture or soil water content. Please be consistent within the manuscript.

Specific Comments:

P1 L17: what was the outcome of the day detection?

In general, the introduction needs revision at some parts. I also miss out a critical review about processes which should be accounted for to reduce uncertainty and pro-

HESSD

11, C3193–C3198, 2014

cesses which are not accounted for.

P8572 L21; add comma before whereas.

P8572 L24. Should be: actual water content.

P8572 L25: . . .soil moisture probe development. . . any references for this?

P8572 L26: . . .spatial soil variability. . .. any references for this?

P8573 L1: I agree that modelling SWC is essential but for predictive purposes spatial soil heterogeneity should be accounted for using either full 3D or distributed 1D models. Please discuss carefully.

P8573 L9: this might be only true if you do have a bare soil. If you would have crops a scientific sounding module for crop growth and root water uptake is also mandatory.

P8573 l18: especially upward flow is often neglected in capacity models leading to less water available in the root zone during dry conditions, especially if the water table is shallow.

P8573 L23: should be: . . .water retention and the hydraulic. . ..

P8573 l24: should be: . . .of these parameters and the hydraulic. . ..

P8573 L25: Rosetta (by Schaap et al) might be one of the mostly used PTF.

P8574 L1: . . .under discussion. . . especially for wet conditions where preferential flow occurs.

P8574 L22: reference for the SiSPAT model missing

P8574 L23: same for the Hydrus model

P8574 L24-26: This sentence is somehow out of line. Please rephrase in put it into the context. Does it play a role if BC will be used?

P8574 L27: Ross resolution. Do you mean solution?

HESSD

11, C3193–C3198, 2014

P8575 L1-2: It is clear that different hydraulic properties will influence the outcome. For me it would be more important to know why the different PTF behave so differently.

P8575 L6-7: here the question arises whether there is more uncertainty propagated through the model by using different PTFs or variability in the atmospheric forcings. It is widely known that atmospheric forcings (especially precipitation) are highly variable in space.

P8575 L7-10: This sentence is out of line.

P8575 L12-13: I do agree that a full representation of all physically based processes would lead to the most exact solution but does it make sense for predictive software? Maybe other processes such as crop growth and root water uptake are much more important compared to the head balance. Please discuss carefully.

P8576 L1-6-8: that's not a hypothesis. You stated the hypothesis later. Please rephrase.

P8577 L2: Shouldn't it be Eq.1?

P8577 L12: air or soil temperature? Please be precise.

P8577 L22: Shouldn't it be Eq. 3 to 5

P8578 L7: Should be Darcian flux

P8578 L18: Should be actual heat transport parameters.

P8578 L21: How do you treat the air phase?

P8579 L4: I am aware that most models assume rainfall having either a constant or air temperature (both assumptions are wrong). Can you later comment on this limitations?

End of paragraph: In general, it is not clear to me how the two models differ. It seems that FHAVeT is a 3D model (later used in a 1D mode) but how is it with TEC. Maybe some more words are necessary to introduce both models and to clarify differences.

HESSD

11, C3193–C3198, 2014

P8580 L8: should be: ranging

P8580 L19 and 21: should be normal n for the shape parameter (same also in Table 1).

P8581 end of upper paragraph: How did you treat the lower boundary? Did you test on grid convergence?

P8581 L16-17: I do not understand. Later you stated that the accepted error threshold should be 1%. So why does the TEC does not hold this threshold (I do see only mass balance errors <1% for the TEC model). Also the question arises whether the mass balance error in the TEC model is a consequence of solving the Richards equation numerically (so called solver problems) or if the mass balance errors are a consequence of the grid discretization (too large grid sizes close to the surface). Therefore, I would suggest not to benchmark the FHAVeT model versus any other model (here the TEC model and use the mass balance) rather than using analytical solutions which do exists not only for water but also for heat flow for various boundary conditions (e.g. benchmark over BC flow) in a first step. Additionally, the points shown in Fig. 3 are only selected mass balances for predefined time steps. As far as I understand mass balance was calculated as the absolute error. To my understanding large positive and negative errors can also compensate each other and might lead to an overall small error. If the time step for calculating the mass balance is large the overall balance might be still OK but the timing of the water flow might be wrong. Is this right?

P8581 L22: Should be Richards equation

P8581 L26: should be conductivity curve

P8582 L8: in Figure 4 the WC after 2 hr are shown. Why did you select this time? Why not show all data for all times?

P8583 L4: I agree that vapour transport might play an important role leading to the differences observed. But again can you exclude any other influencing factors leading

HESSD

11, C3193–C3198, 2014

to differences in flux or state such as differences in grid settings or time step control (actually affecting the mass balance)?

P8583 L5: should be: . . .,that the model. . .

P8583 L10: should be: . . .for each model under drying consitions.

P8583 L11: maybe better: . . .are comparable below 30. . ...

P8583 L15: I do not fully agree. For sure the profiles will correspond much better after infiltration but how is it concerning fluxes over the BC (upper and lower) for the entire simulation period. And how does these differences in fluxes over the BC affect root water availability?

P8583 L16-17: maybe better: . . .for agronomic management such as irrigation.

P8583 L18: maybe better: . . .status reaches. . ..

P8584 L2: maybe better: . . .threshold is reaches

Tables:

Table 4: is the wind velocity the mean velocity? Please indicate.

Figures:

Figure 3: why not express the mass balance error directly in %. Would be more intuitive.

Figure 4. Why use the WC after 2 hr?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 8571, 2014.