



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

Received and published: 11 September 2014

We thank you for your review on our paper. We addressed the following points in a general answer to all reviewers

- Confusing description of the aim of the paper
- Choice of the benchmarking
- Modeling the vegetation
- Performance of the model in regards to computation time

C3731

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Therefore, we will focus this answer on other points.

The grammatical and language remarks were all taken into account in the revised version of the paper, the terminology “water content” was chosen over “moisture content” as suggested for coherence in the manuscript. The numbering of equations and subscript description to discriminate temperatures are modified within the revised document.

Tables:

Modifications in table 4 was made accordingly Table 1 (and text): The term is the greek letter η and is different from parameter n. This parameter is occasionally referred as “tortuosity” and sometimes labelled L. It is often documented with a value of 0.5, but Schaap et al. 2001, for instance stated that generally fits point towards a value of -1.

Figures:

Figure 3: The mass balance cannot be expressed in %, indeed its unit is in m^3/m^2 due to the 1D description, modification was however made in the text for coherence. Moreover, the figure scale was extended to show the points with errors > 0.01 .

Figure 4: The results are used every two hours in order to allow significant description of the accuracy of the models with time variation. The legend of the figure was modified to point out the results were outputted every two hours.

References:

The following references were added to the text:

soil moisture probe development:

C3732

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Evelt, S.R. and Parkin, G. (2005) Advances in soil water content sensing: the continuing maturation of technology and theory. *Vadose Zone Journal*, 4, 986-991

spatial soil variability:

Evelt, S.R., Schwartz, R.C., Tolk, J.A. and Howell, T.A. (2009) Soil profile water content determination: Spatiotemporal variability and neutron probe sensors in access tubes. *Vadose Zone Journal*, 8(4), 926-941

SiSPAT:

Braud, I., Dantas-Antonino, A.C., Vauclin, M., Thony, J.L. and Ruelle, P. (1995) A Simple Soil Plant Atmosphere Transfer model (SiSPAT). *Journal of Hydrology*, 166, 213-250.

Hydrus:

Simunek, J., Sejna, M., Saito, H., Sakai, M. and van Genuchten, M. Th. (2008) The HYDRUS-1D software package for simulating the one-dimensional movement of water, heat, and multiple solutes in variably-saturated media. Department of Environmental Sciences. University of California Riverside. Riverside, California.

ROSETTA:

Schaap, M., Leij, F.J. and van Genuchten, M.T. (2001) ROSETTA: A computer program for estimating soil hydraulic parameters with hierarchical pedotransfer functions. *Journal of Hydrology*, 251, 163-176

Abstract

Metrics were added to the abstract in order to explicit the results such as the outcome of day detection.

The following points were added to the text as suggested:

C3733

HESSD

11, C3731–C3742, 2014

[Interactive
Comment](#)

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



“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”

“especially for wet conditions where preferential flow occurs.”

The discussion points are addressed below:

Discussion on 3D and 1D distributed models:

You noted that: “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.”

We agree that 3D / distributed 1D models are necessary for a more precise description of the problem. The question of the efficiency of the model is even more important since the computation time increase significantly with the amount of 1D models or with the dimensions. Both models may be used in 3D, however we limited ourselves to a 1D study. Indeed, the aim of the paper is to evaluate the accuracy of FHAVeT against TEC rather than establish the effect of parametrisation or modeller’s choice. Comment on extension to 3D / distributed 1D models and the advantages of FHAVeT for such uses is added to the introduction.

Pedotransfer functions:

You noted that: “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?” and “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.” and “P8575 L7-10: This sentence is out of line.”.

In the original Ross description (and this is noted as a drawback of their method in the
C3734

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



2003 paper), the only Brooks and Corey type hydraulic curves may be used. In the Ross solution, the Kirchhoff potential (which requires integration of the hydraulic conductivity against soil potential) is necessary. When using Brooks and Corey description for the hydraulic characteristics, this integration is straightforward and can be done analytical. This is not true when using Van Genuchten – Mualem curves. However, many pedotransfer functions are developed for Van Genuchten – Mualem description. Therefore, in order to have a generic model that may be used with several PTF (such as the ones of Wosten et al.(2001) or ROSETTA (Schaap et al. 2001), an extension of the Ross description, and mainly the numerical calculation of Kirchhoff potential is necessary. Crevoisier et al. (2009) already added the use of Van Genuchten – Mualem with the restriction of $\eta = 0.5$ and using a numerical integration method. For this work, we allowed an even more general use of PTF since we developed a numerical method for Van Genuchten – Mualem with $\eta \leq -1$ (Schaap et al. (2001) noted that fits generally lead to $\eta > -1$) and a more exact solution to Van Genuchten – Mualem functions for $\eta > -1$ using beta functions.

The aim of our work is not so much to compare different PTF (such work has been done previously, for instance in Chanzy et al. (2008)) but to evaluate if FHAVeT allows most generic PTF to be used with little discrepancies compared to TEC.

In regards to variation due to the use of different PTF, we would like to refer to Chanzy et al. (2008) who provide a literature review on such issues. It should be noted that PTF were built against different databases from different regions in the world. Moreover, the PTF are not necessarily fitted in the whole range of saturation for both water retention and hydraulic conductivity which may lead to extrapolation, which can be very different depending on the formulation of the hydraulic characteristics.

The manuscript introduction was revised to clearly define the objective of the paper. The added capacity of the model in regards to PTF was developed in the model presentation (subsection 3.3).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Discussion on sources of uncertainty:

You noted that: “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.”

In any models there are a number of sources of uncertainties. Aside from the uncertainties due to the vegetation, uncertainties may be due to parameterization, time evolution of characteristics (porosity for example may evolve with rooting), and spatial variation of characteristics as well as spatial evolution of atmospheric forcing or surface water redistribution through runoff. Dealing with those uncertainties are a critical point. However, it would be too costly to fully consider all parameters and their evolution with space and time directly from experimentation and / or direct measurement. Therefore, such issues cannot be realistically dealt with a water transfer model. Data assimilation is likely a way to overcome such sources of uncertainties and efficient models allow a more practical implementation of data assimilation.

The added value of the development of an efficient coupled model such as FHAVeT for other techniques (like data assimilation) is described in the introduction.

Capacity in prediction of the software:

You noted that: “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.”

Non-physically based models are only site specific and are not predictive by nature. A discussion on semi-empirical models drawback is made in the document and es-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



pecially highlights that some parameters are not measurable. Non-physically based models may become predictive if fitted against data but will lose this capacity if there is a significant change (nature of crops, climatic change, site geometry change ...). Therefore using physically-based models is essential to allow predictability and versatility of the model. Representing all of the processes may not be always necessary. But it allows a more general description and once again more versatile software. Clearly, when working on agricultural management a crop development and root water uptake module are necessary to the model. Such modules exist; however, for the paper presented we chose to focus on mass and heat balance and therefore limited ourselves to bare soil. Discussion on the choice of the model and processes is made in the general comments.

We revised the introduction altogether to point out more clearly the objective of the paper and the working hypotheses.

Air phase:

For the soil mass balance air phase is considered at equilibrium as it is always considered when using Richards equation (it is actually an hypothesis used when developing the equation). For the soil energy balance, the air phase is neglected for both the capacity (there is about 3 order of magnitude difference in heat capacity weighed by density of air compared to other phase due to contrast in density) and convection terms. This assumption is very classic when modeling heat transfer in porous media. Moreover, as stated in the paper, heat conductivity is dependent of soil water content and therefore the evaluation of the conductivity term takes into account the air phase. That said, we noted an approximation in our detailing of the equation, and therefore we added the more accurate description as follows:

$$(\rho C)_{eq} = \rho_h C_{eq} = \rho_w C_w \theta + \rho_s C_s (1 - \theta_s)$$

Where ρ_s and C_s the solid density and heat capacity respectively and h is the soil bulk density.

Rainfall temperature:

You noted that: “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?”

Rather than a limitation, we consider a constant rain temperature to be a working hypothesis. It should be noted that the model could handle an evolutive rain temperature but we did not have any data to support such a characterization. An inexact value for rain temperature may lead to inaccurate description of the surface heat fluxes especially during heavy rain periods or if a water layer is formed over the soil. However, we would like to point out that, in his thesis, Mumen (2006) showed that thermal boundary conditions had relatively little effect on water content.

Ref: Mumen M. (2006), Caractérisation du fonctionnement hydrique des sols à l'aide d'un modèle mécaniste de transferts d'eau et de chaleur mis en œuvre en fonction des informations disponibles sur le sol. PhD Thesis, 169 pp., Université d'Avignon et des Pays du Vaucluse, Avignon, France.

The constant temperature was clearly identified as a working hypothesis within the document.

Differences between FHAVeT and TEC:

You noted that: “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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



differences.”

Technically, both models could be used in 3D. The main differences between the two models may be summarized as follows:

- The soil energy and mass balances coupling is different. In TEC we used a de Vries approach which leads to the computation of water content (and soil potential), soil temperature and vapour pressure. FHAVeT uses a loose coupling neglecting the vapour transport. The consideration (or not) of the vapour flux is one of the major difference.
- The equation method of resolution is different. In TEC, a Galerkin Finite Element Method with an implicit scheme is used, whereas in FHAVeT the Ross method is used (for the mass balance).

In the introduction, we highlighted the added value of our model compared to previously developed model (including TEC) as well as a justification of the choice of TEC as a benchmark. Also in the description of TEC model we developed the differences between the two models.

Model setup (boundary conditions and grid convergence):

You noted that: “P8581 end of upper paragraph: How did you treat the lower boundary? Did you test on grid convergence?”

The setup (discretization, boundary conditions, initial conditions, ...) used for both models are developed in the “model intercomparison” section. This section was subdivided into subsections for easier reading. One of this subsection is labeled “model setup” and contains all necessary informations. The choice behind the boundary conditions was well developed and justified in the work of Chanzy et al. (2008). Considering

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the aim of the paper no further development was added to the text. A study of discretization effect for Ross solution was done in the paper of Crevoisier et al. (2009). Compared to this study, we chose to use a fine discretization in order to limit the numerical shortcomings of a poor discretization scheme (whether on soil mass or soil energy balance). However, it should be noted that the work of Crevoisier et al. (2009) demonstrate that Ross solution allows a coarse discretization scheme. In the case of the TEC model, the discretization scheme is also fine and moreover (as described in the paper) is refined close to the surface to limit numerical errors or divergence due to discretization.

Mass balance errors in TEC and calculation of mass balance:

You noted that: “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).” and “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.” and “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?”

In regards to the mass balances in TEC both sources of errors (solver problems and grid discretization) must be considered.

The mass balance is calculated as follows:

$$\varepsilon = \max_t \left| (V_{wat}^t - V_{wat}^0) - \int_0^t (Q_{prec}^\tau - Q_{evap}^\tau - Q_{drainage}^\tau) d\tau \right|$$

In other words, the mass balance error corresponds to the maximal value of the difference between variation in soil water content from the initial time and accumulated

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



boundary fluxes. Considering we chose the maximal value rather than, for instance, the final value we tend to consider the worst conditions. Clearly, the error may be compensated along time, but when the error occurs (for instance if the timing of infiltration during precipitation is off) it would show and the mass balance error would be affected. Moreover, other metrics later shown in the paper tend to demonstrate a rather accurate timing.

Discussion on the sources of errors:

You noted that: “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 to differences in flux or state such as differences in grid settings or time step control (actually affecting the mass balance)?”

We agree that the lack of vapour transport may not be the sole possible reason for the discrepancy. However, the fact that such an error does not appear systematically and only in drying conditions tend to point towards that direction. Other possible causes were added to the paper with a justification as to why we consider the lack of vapour transport as our main suspect.

Model resilience (comments to Figure 8):

You noted that: “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?”

We agree that our interpretation of the results might have been too optimistic. We modified the analysis to a more prudent and reserved approach. Specifically, we eliminated

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



the subjective comments such as “This may demonstrate that the neglected volume of evaporated water is not very important in regards to the total amount of water”. We however added some objective metrics. Namely, the maximal water content discrepancy is of $0.087 \text{ m}^3/\text{m}^3$ in the dry state, compared to $0.015 \text{ m}^3/\text{m}^3$ in the wet state (in other words the local maximal error is about six times higher in the dry state than in the wet state). The total water volume (on the whole domain) difference between the two models is of $0.0071 \text{ m}^3/\text{m}^2$ in the dry state and $0.0052 \text{ m}^3/\text{m}^2$ meaning that 27% of the missing water was recovered in a matter of 8 days. This tends to show that some of the water is recovered though not all, moreover the error in terms of water content is diluted along the domain.

The effect on evaporation fluxes may be observed in Figure 7. This figure shows that the evaporation fluxes are off during the drying period but do not demonstrate stronger discrepancy after the drying period (in comparison to before the drying period). Finally, further impacts on this subject are evaluated to the decision making. Specifically, the decision making evaluation demonstrated that while decision may be off during the drying period this is not the case after reinfiltration.

All these considerations were added to the text.

HESSD

11, C3731–C3742, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

