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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 #2

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1 General comments

The paper describes a newly developed model coupling designed to accurately simulate soil moisture and energy balance to aid in the prediction of timings for agricultural management while begin computationally efficient. The material is relevant to current research specifically in agricultural research but also to other studies which consider soil atmosphere exchanges. However, I have significant concerns over the stated aims and the validation / evaluation. My concerns broadly cover two areas: 1) the lack of comparison with empirical data 2) an apparent disconnect between the stated objec-

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tives and parts of the results and discussion sections, and 3) the text gives the impression that the authors are not clear which objectives are the most important to the study. The paper should only accepted for publication after the below concerns have been addressed.

2 Specific comments

Overall the manuscript needs to be more concise, in particular there are elements of repetitious which could be removed. Moreover, a more concise text will have additional space to address the comments below.

The FHAVeT model is repeatedly (including the title) referred to as a "...couple model for soil atmosphere..." or "..coupled soil atmosphere model..." which implies there are feedbacks between the atmosphere and soil processes. This is misleading as the atmosphere acts only to provide forcing to the model. While I accept that there is coupling between different model components of soil hydrology and energy balance, the fact that the atmosphere is not really coupled to the model should be made clear.

The abstract does a good job of justifying the need for robust means of simulating soil moisture however I remain unclear exactly what the model described in the paper offers over existing systems Moveover, there are no empirical results within the abstract to justify the claims that the model is useful in achieving the stated objectives. The abstract would be greatly improved if some reference to the results are made, such as the compute time, errors between soil states or predicted management timings and observational data etc are made.

The description of the TEC model is important and relevant given the TEC model is being used for evaluation and that there is no data currently presented. Given that the TEC is itself a model and is not perfect it would be good to include comparison with data from the simulation agricultural sites (i.e. soil temperature, moisture content, evaporative fluxes?), to justify the results shown here.

Moreover, given the lack of any observational data the description of the TEC reference model needs to be extended (page 8579, lines 5-17) There needs to be some justification of why the TEC model is appropriate to use as a substitute for actual data. While there is a reference pointing to a comparison between observational data and the TEC model, page 8579 line 22-24, there is no indication of how well the TEC model performs. This is important given differences between the FHAVeT and TEC models are being attributed to 'errors' in the FHAVeT e.g. page 8583 line 5-7.

Page 8574, lines 16 to page 8575, line 20 deal with the Ross (2003) proposed method for solving the Richards equations and subsequent developments of the approach. However, given that the ultimate decision of the authors is to use an approach based on the original Ross model (page 8587, line 21-22), the level of detail given seems excessive. Please attempt to be more concise.

Page 8576, lines 6-11, explicitly state that the different model functions will be evaluated by considering soil moisture accuracy and timing in decision making based on soil moisture. Figure 9 shows results of differences between the day of a threshold event being simulated between the two models, however I am unable to find any detail of what management events are actually being simulated, i.e. ploughing, sowing, harvest? Based on the simulation dates, I assume that sowing date is the intended target. However further detail is needed, including some information on the uncertainty associated with the target values. Given the explicit statement that the paper aims to

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develop a model which effectively predicts timings for management practices additional detail is on your evaluation criterion is required. Particularly given that I find it difficult to believe that soil moisture would be correctly simulated given the lack of vegetation in the model.

The end of the discussion introduces the need for subsequent inclusion of vegetation to deal with "...water transfer due to vegetation." Given that the presence of vegetation should also impact significantly on surface energy balance through impacts on albedo as well as turbulent exchange, have you simulated any periods where vegetation is present on the ground? If so, do these periods coincide with periods of increased error?

The opening to the results section details technical improvements in the mass balance of the FHAVeT model over the TEC which is important given this is a new model. In fact this section could also include comparison of additional performance metrics, such as improved simulation time. A comparison of the simulation time would be appropriate given the the authors link to the need for computational efficiency for data assimilation (page 8585, lines 15-19). Therefore the technical objects should also be first raised in the introduction or model evaluation sections and made an explicit component of the paper's aims. On the point of the stated aims I am unsure how the new model is meant to be an improvement over the existing reference model. As far as I currently understand the paper aims to demonstrate a model with improved computational efficiency compared to the reference model without degradation of predictive skill, for use with other methodologies (e.g. data assimilation). However if this is incorrect and there is meant to be scientific / theoretical improvement then the paper needs to be revised to make its aims explicit and demonstrate the differences between the model clearly.

3 Technical errors

Page 8572, line 14: “... 6 times...” should be “six”

Page 8580, line 9: “...ranging...” not “rangeing”

Page 8581, line 23: the choice of “...unsatisfying” as a description for the model performance seems rather inappropriate please remove.

Page 8583, line 5: “As it may be observed...” seems to imply observations (i.e. data) which is not the case. Please rephrase.

Page 8584, line 23: ...introducing a coupling with the atmosphere...” the model is not coupled to the atmosphere, it is forced / driven by it.

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