

***Interactive comment on* “Quantifying the impact of groundwater depth on evapotranspiration in a semi-arid grassland region” by M. E. Soylyu et al.**

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We would like to thank Gerrit de Rooij for his comments on our paper and his synthesis of the referees’ discussion. We have made significant revisions to the paper and have posted responses to each referee, addressing all of their major and minor concerns. We believe the paper has been significantly improved as a result of this discussion. Specific point-by-point responses to the editors’ comments are provided below.

1) Dear authors,

We have available three reviews which vary widely in their appraisal of this work. In view of the differing opinions I have read the paper afresh to reach a decision. Unfor-

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tunately I am inclined towards the view of the most critical reviewer.

Large portions of section 3 are confusing. The section detailing the models and the set-up of the simulations is incomplete and can be better structured. Clearly outline (perhaps in a table or a scheme) what domain is modeled (I saw two depths passing by), what the upper and lower boundary conditions are (I do not understand how one can simulate evaporation with Hydrus using a nodal distance of 30 cm for instance), and what values you assigned to model-specific parameters to guide the simulations: convergence criteria, initial time step and allowed time-stepping range (simulating transpiration with a daily time step is improbable). The most critical reviewer also gives useful comments on this.

Authors' Response (AR): A significant amount of new information has been added to sections 2 and 3 to clarify the models and methods. Moreover, we have added a table showing the details of the compared models.

2) One reviewer remarks that the bucket model is surprisingly accurate. I believe this is an artefact of the time-averaging over a 10-year period. You justify your work (correctly, in my opinion) by pointing to the importance of land surface processes and the interaction between the atmosphere and subsurface water (the reviewer also alludes to this). But these processes operate on time scales between seconds (infiltration) and hours (evapotranspiration), and it is at this time scale that the feedbacks to the atmosphere are key. The absence of any comparison of model performance on these time scale therefore seriously limits the impact of this study.

AR: The significance of groundwater contribution to land surface processes and atmospheric circulation has been getting more attention over the past decade. Therefore, we believe that it is critical to show how the selection of model parameters can significantly affect the land surface energy balance. As we have also addressed in our response to the reviewers, our primary intent in this study is not to provide "detailed model validation" across multiple timescales, climatic zones, and land cover types.

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Rather, we are simply trying to point out the sensitivity of simulated ET to various soil parameters and model formulations in a region with varying, shallow groundwater. We believe that the paper has met this objective even with our limitation to hourly and longer timescales. Future work can (and should) address some of the high-frequency land surface/atmosphere interactions, but this is beyond the scope of our present study.

3) The sensitivity of the simulated fluxes to the choice of the parameterization of the soil hydraulic properties (SHPs) is unfortunate but important and a valuable finding. However, as one of the reviewers noted you are not the first to find this. At the very least connect this result to similar results in the literature (the reviewer provides one reference for a warmer climate).

AR: There are relatively few previous studies that have shown the sensitivities of surface ET to soil hydraulic properties in areas where groundwater is a significant contributor. However, large uncertainties associated with using mean soil hydraulic properties were reported in modelling studies that evaluate groundwater recharge. An additional paragraph was added in section 4.1 that discusses previous research on the sensitivity to soil hydraulic properties.

4) Why do you describe the field study in such detail? The available data are underutilized in the work. Overall, the link with the experimental work is weak, it appears you only use the weather data as input and only compare soil moisture simulations with readings from sensors. There is no comparison between independently estimated and simulated evapotranspiration fluxes, which is more relevant than comparing soil moisture contents. Furthermore the paper suggests you had only three soil moisture sensors (without information on type, measurement volume, or measurement frequency), each at its own depth. One of the key points in land surface models is the need to handle the large spatial units for which atmospheric models require their input. In essence you performed a point-scale test of various models for subsurface flow, and you do not address issues related to heterogeneity of SHPs, land use, and weather conditions (e.g., convective rainfall). You can frame this study as a model evaluation loosely based

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on conditions at a particular field site (which is what it is now). Still you can carry out a much more thorough evaluation of the results by comparing with the data in Figs. 2 and 3 without averaging in time (see my remark above), provided you can use the available data to estimate the actual evapotranspiration. But to really evaluate the potential for the tested models for use in land surface modeling, more work is needed to address the problems associated with the much larger spatial scales for which fluxes across the land surface need to be quantified. The field site can be very useful for that but again you would really need the actual evapotranspiration (which is not given in Fig. 2).

AR: Discussion of the field site is limited to section 3.1, with evaluation results presented in section 3.2, so we don't feel that the level of detail is excessive. On the other hand, some level of detail is necessary for future researchers who may wish to undertake similar work for comparison with results from our field site. We have, for example, added additional information about the soil moisture probes. Finally, as explained in our response to Referee #3, neither lysimeter nor eddy covariance data were available at our site for calculating actual evapotranspiration. The hourly meteorological data were used primarily as drivers for the IBIS model simulations. We also note (in the summary) that more in-depth validation (e.g., ET) would be something worth exploring in future work. We have tried to make it clear in numerous instances that our focus here is more on model sensitivity, and less on model evaluation. The field data have been included in the study in order to simply provide a level of credibility to the model results that would not have otherwise been available if no comparison had been made with observational data.

5) If you decide to include a more thorough comparison with the data in Figs. 2 and 3, a more thorough description of the collected data and the sensors used is required. In that case, please also indicate the distance between the rain gauge and the field location you modeled.

AR: We have added more information about sensor type and placement in section 3.1 and have also shown the locations of piezometers, soil probes, and the meteorological

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station at the wetland field site (in the map shown in Fig. 1). The rain gage is co-located with the meteorological station at the field site.

6) It appears to me that the strong effect of the parameterization of the SHPs negates the use of Hydrus as the benchmark model. Of course one could arbitrarily pick one parameterization and declare Hydrus runs with this parameterization the benchmark. But then any deviating caused by the use of other parameterization cannot be called erroneous, just different. Is there any possibility to verify against observations (see my earlier remarks)?

AR: We agree that the paper is really about comparing models. We chose Hydrus-1D as the “benchmark” model since it uses a more sophisticated theory and numerical scheme than the other soil models that we used. However, in the revised version of the paper we do not refer to Hydrus-1D as the “benchmark” model, but simply compare the other models to Hydrus-1D as a reference. As explained earlier, the primary observational data available to us for model validation is soil moisture content, and further model/data comparisons (e.g., with observed ET) were beyond the scope of this work.

7) The manuscript reflects little care for detail (as the reviewers also noted): different variables are identified by the same symbol, the diffusivity is called diffusion coefficient, and many grammatical errors appear (notably inconsistencies between singular and plural forms of the subject and the verb, and in the use of past and present tense). Furthermore, superscripts appear as subscripts, the vertical coordinate is defined positive upward and then downward, and minus signs are omitted in expressions for flux densities (particularly confusing in combination with the ambiguity of the direction of the vertical coordinate). None of this is acceptable in the final version.

AR: We have taken great care to correct all errors in notation, equations, and grammar. Thank you for pointing these out.

8) Given the essentially positive reviews of two reviewers, the interesting differences between the models, and the large effect of the parameterizations of the SHPs you

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found, I recommend a major revision. I request you to very carefully read the review reports, and pay particular attention to the comments of the most critical reviewer – they are substantial and valid. Before I can accept the paper for publication in HESS I need to see a more substantial contribution than is currently provided, a more structured presentation of the methodology, and an elimination of the distracting errors in grammar, definitions, and use of symbols.

AR: We have completed the requested major revisions and believe you will find the new manuscript to be greatly improved.

9) Please do not forget to reply to the reviewer reports on the HESSD website.

AR: We have replied individually to each reviewer's report.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6887, 2010.

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