Hydrol. Earth Syst. Sci. Discuss., 7, C3103–C3106, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C3103/2010/

Hydrology and Earth System Sciences Discussions

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

G.H. de Rooij (Editor)

gerrit.derooij@ufz.de

Received and published: 22 October 2010

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. Unfortunately 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.

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.

The sensitivity of the simulated fluxes to the choice of the parameterisation 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).

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 ad-

dress 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 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).

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.

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)?

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.

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 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.

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

Sincerely yours,

Gerrit de Rooij Editor

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