

Interactive comment on “An optimality-based model of the coupled soil moisture and root dynamics” by S. J. Schymanski et al.

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I have found some difficulties at providing an assessment of this manuscript: on the one hand I have found the paper well detailed in the description of the adopted methods and of the obtained results; very relevant in terms of considered topic; well structured; and up to date. On the other hand there are some problems with the modelling approach (see point 1 below), and with the lack of empirical basis to support some of the obtained results (point 2 below), problems which may affect the interpretation of the main results of the paper in a profound manner. In short, I believe the paper deserves publication, because there are many ideas and hints for discussion which may stimulate an interesting debate in the eco-hydrological community; however, further clarifications and explanations of some of the modelling assumptions are required, and

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the obtained results should be reconsidered in terms of their realism (or lack of it). In the following I provide a list of the major problems that in my opinion affect the quality of the paper. Even if the tone of the comments may sound negative in some cases, I hope my original intent is preserved to provide a constructive review, for trying to improve a paper which is already very interesting and has the potential to become an excellent one.

1) Modelling approach There is in my opinion an unbalance in the accuracy of the modelling of the soil water balance and of the vegetation water balance, since the former one is treated following a very simplified approach, while the latter one using a very detailed modelling approach.

Some examples of overly simplified assumptions in the soil water balance model:

- the water table is considered to be horizontal (see Figure 1) in most of the calculations, except than in the definition of the seepage flow, notwithstanding the presence of slopes in the considered terrain. Also, the infiltration processes are not considered to be affected by the ground slope. In reality, the water table is not horizontal below an hillslope (e.g., Jackson, 1992; Salvucci and Entechabi, 1995) and hillslope infiltration processes are much more complicated (e.g., Philip, 1991; Jackson, 1992).

- the specific yield (which is the ratio between the volume of water released from storage per unit cross-sectional area of the aquifer, and the correspondent decline in the elevation of the water table) is implicitly accounted for in equation (13). However, it is here modelled as the product of soil porosity and average soil moisture content in the unsaturated zone, which is a rather unrealistic assumption (e.g., Duke, 1972; Nachabe, 2002; Ridolfi et al., 2008) that does not allow one to account for the vertical soil moisture profile in the unsaturated zone, whose shape has a strong influence on the magnitude of the variations of the water table position.

- the soil moisture content in the unsaturated zone is sometimes treated in the paper as an average value along the vertical direction, while of course it is variable from layer

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to layer, and this variability is very important in determining infiltration and redistribution processes. I was not able to understand where this vertical variability is accounted for, because of some confusion with notation (page 56, line 27; page 59, line 1; page 63, line 2 and eq. (12)-(15): are these all average values?).

- water-stress induced stomatal closure and its effects on transpiration does not seem to be explicitly considered.

In contrast, some examples of the extremely detailed treatment of the water fluxes in the plant system are the following:

- Equations (20) and (21) where root water uptake is modeled using an electrical circuit analogy;

- Equation (23), where the tissue balance pressure is expressed as a function of the tissue water content;

- Equation (24), where the hydrostatic head between roots and trunk is accounted for.

Of course I am well aware that simplifying assumptions are necessary when modelling physical phenomena (and, for example, the Representative Elementary Watershed approach may be a very useful one for a simplified representation of the soil water balance), and on the other hand I think the Authors should be commended for their attempt to representing in full detail the vegetation water balance. However, I believe a necessary condition for having an useful model is that the level of accuracy is the same for all the components of the model, because the combination of simplified and detailed modelling assumptions can provide results of difficult interpretation. For example, I do not see the point of considering the water storage in the plant when, on the other hand, the specific yield or hillslope infiltration processes are neglected. A possible alternative to avoid these problems could be to restrict the field of analysis to more specific conditions, so that the same level of detail can be attributed to all model components: for example, if think a representation of the processes on a flat terrain would allow a more

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detailed treatment of infiltration and exfiltration dynamics and would be more appropriate in this case, also considering that the model is actually applied in a flat study site (page 70, line 21).

2) Interpretation of results and comparison to other models One of the main results of the paper is that a dynamically adjusted root profile works better than an empirically based exponential profile in terms of capability to reproduce the transpiration fluxes and soil moisture dynamics. I have some doubts with this result, and I think some more comparison to empirical data are necessary in this case:

- the root distribution turns out to be very dynamic at the site under analysis, with root profiles that completely change in the course of a season (see Figure 3-4-5). This result is not an intuitive one, and it is now supported only by a generic reference to a couple of papers (Chen et al, 2002; 2004) where this dynamic behaviour was previously reported. However, I think the "strangeness" of this result demands more quantitative demonstrations, supported by some empirical data (i.e., some root biomass measurements at different times in the dry and wet season). Otherwise, the impression remains that these variations may be the result of a misuse of the model. For example, the maximum daily root growth rate in equation (30) is arbitrarily (Authors'word, page 69, line 21) set to 0.1 m² per m³, which is a very large value (the initial profile is uniform with surface area density 0.1, which implies that the density may double in a single day!). I believe that decreasing this root growth rate the result could dramatically change.

- the Authors claim that the model with dynamically adjusted root densities is better than another one based upon an fixed exponential root profile. However, I do not think this result is fully convincing: in fact, the assessment is simply based on the better capability of the dynamic model to reproduce measured surface soil moisture time series, while I think some measurements of root biomass, or at least of soil moisture along the vertical soil profile, would be necessary for a comparison between the two models. Also a comparison of the transpiration time series could be useful in this sense, but only the annual cumulated transpiration values are reported in the paper. Another problem with

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the comparison is that the adopted model has more parameters than the fixed root profile model: I agree with the Authors (page 54, line 26-27) that optimality-based models "in theory, do not rely on parameter tuning", but, when moving from theory to practice, the presence of parameters whose value is arbitrarily set (like the root growth rate mentioned above) makes me think that some non-systematic calibration should have been carried out, at least to understand the range of variations of these parameters. This could in turn imply that the better fit is an effect of the increased number of model parameters, rather than of a better capability of the model to represent the physical processes.

Minor points

- I was not able to understand the rationale behind Equation (2). From Figure 1 I would guess that z_u could be obtained from the expression $Z = z_s + z_s - (z_r - z_s) + z_u/2$, which provides a different result than equation (2). Equation (2), and some other results in the paper, are reported in the PhD thesis by Schymanski (2007): it could be useful if the Author could provide a web link to this thesis, maybe through his personal web page.

- I know self-citation is not orthodox, but I would like to signal to the authors that also in the paper by Laio et al. (2007) a model is implemented to represent plant root profiles as a function of basic climate and soil parameters. Optimisation is there implicitly accounted for by imposing root profiles which produce vertically uniform average soil moisture profiles.

- since the model is a rather complicated one, I believe a table with all of the models parameters (and the parameter values used in the application) could help the readability of the paper.

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