

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

Anonymous Referee #2

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GENERAL COMMENT

The manuscript deals with a topic of great relevance to scope of HESS particularly for the special issue on CSV interactions. In particular, the paper presents and tests optimality-based model relating root water uptake to carbon costs that is simple enough to be implemented into a coupled ecohydrological model allowing for simultaneous optimisation of above- and belowground vegetation.

DETAILED COMMENT

As such, this model represent a novel and intriguing approach to the new branch of ecohydrology since it is able to solve the intrinsic complexity of natural environmental system trying to overcome the scale gap between the point and the catchment

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scale. Though who writes is not an expert in the field of plant physiology, it seems that the scientific background is quite complete and up-to-date, and the starting point on the need for a better description of root profiles and functioning across biomes for the is the new frontier for ultimately improving the predictions of GCMs. In this direction optimality principles will represent a turning point in coupled ecohydrological modelling. The model equation are well posed creating a really realistic picture of the interaction between plant functioning and hydrologic response at catchment scale. Therefore the 3D solution of water flux component is a good example of compromise between physically-based conceptualization and mathematical simplicity. This allowed to demonstrate the existence of self-adapting root distribution profiles that according to the references reported by the authors is specific of some species adapted to very dynamic soil moisture regimes. Not similarly clear is the description of the adjustment of leaf area to the reduction of soil moisture in the soil profile that is often reported as the adaptation strategy of aboveground plant structures to reduce evapotranspirative demand thus meeting the reduced water availability in the dry season. Such a valuable modelling effort is reported limitedly to the climate, landscape and biome in the proposed study site. In addition to this, the complexity of some of the processes invoked couldn't but result in arbitrary assumptions with regard to a few parameters. An other point is the apparent discrepancy between the detailed solution of water movement and mass conservation equations that are not clearly considered in the validation where only observed transpiration rates and soil moisture measurements (not enough describes). Stream flow measurements, water table recording and soil moisture profiles could have added much more value to the validation issue (and may be also to the assumption of realistic values for model parameters). Despite the comments above, the results are sufficient to support the interpretations and conclusions. Nevertheless some readers might be expecting some discussion about the hydrologic response simulated with such an interesting model.

TECHNICAL NOTES

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On page 56, lines 25-27, there is a misprinting: saturated zone thickness z_s is wrongly named.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 51, 2008.

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5, S69–S71, 2008

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