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Discussions

Interactive comment on "Determination of empirical parameters for root water uptake models" *by* M. A. dos Santos et al.

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This paper presents the results of a comparative modelling exercise: the performances of several empirical root water uptake models are compared against a common benchmark, a physics-based model of water uptake and transpiration for the complete soil-plant-atmosphere system. I like the approach taken, because I think the best way to evaluate models is in a comparative test framework.

However, the results of this kind of exercise are sometimes tricky to interpret and I do have a few concerns in this respect. The conclusion that modified versions of the Li model are recommended seems quite shaky, since for some scenarios the derived value for the lambda parameter seems to take non-physical values (point 16). The model works well here because you are calibrating against a physical model, but how

much confidence can you have in blind predictions with this model, which only has a weak physical basis? I also miss a discussion and explanation of why this model performs best even though it lacks a sound physical basis (see points 7 and 10). I would also like to see a fuller discussion of why the empirical models JMm and PMm give simulations that better match the complete physical model of the soil-plant system, VLM, than the model JMII, which has a physics-based treatment of the soil that is, in principle, identical to that of VLM, but which excludes plant resistances (see points 13 and 19). Is it because introducing a threshold function of matric flux potential (with Mc as the critical value) for local uptake in the empirical models mimics the fact that a constant plant resistance dominates the overall resistance to water uptake in the early stages of soil drying? When M becomes less than Mc, then the soil resistances start to play a more important role. This may be the same reason why the original Jarvis (1989) model, which gives the local resistance as a threshold function of saturation, also seems to work quite well in practice. But there may be other reasons (one other likely candidate is mentioned in comment 13). I also wonder if it is fair to compare an uncalibrated model (JMII) with calibrated models in this way. Perhaps the number of calibrated parameters should be considered in a comparative assessment of model performance? For example, would it be better to assess model performance using the Akaike information criterion, which penalizes models with more parameters? All these aspects should be addressed.

Another less critical question I have is that you are treating the soil hydraulic parameters as fixed and known. This is OK for your particular modelling exercise, but in reality this will not be the case. Uncertainty in the hydraulic parameters is likely to affect the outcome of the comparison of different root water uptake models. Their performance may become indistinguishable even with only moderate uncertainty in these (and other) model parameters. This could be briefly discussed.

I am not so keen on the title of the paper, as it doesn't really reflect the contents so well: two alternative suggestions are: "Comparative test of empirical root water uptake models” and “Benchmarking test of empirical root water uptake models”.

The specific comments in the attached file should also be addressed.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-59/hess-2016-59-RC1-supplement.pdf

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