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Hydrology and Earth System Sciences Discussions

Interactive comment on "Uncertainty in the determination of soil hydraulic parameters and its influence on the performance of two hydrological models of different complexity" *by* G. Baroni et al.

Anonymous Referee #1

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The study is a functional evaluation of soil hydraulic pedotransfer functions (PTFs) and an evaluation of the performance of a simple and a more complex model to simulate processes in the unsaturated zone. The topic of the manuscript is important and interesting; since (1) an abundance of PTFs have been developed and more studies are needed that give a well designed evaluation of the practical performance of those PTFs; and (2) model abstraction opportunities are of interest as being able to use a simple simulation model may save the eventual user cost and effort in the collection of data that drive the model.

Data collection for the study appears complete and well designed and the manuscript is well structured and written in general. I do have some concerns, however, about the modeling aspect and the discussion and interpretation of the results. I invite the authors to consider suggestions below and add to the paper mainly in terms of discussion (as in the major comments). This can be an excellent paper, given that the discussion is expanded/improved.

General/major comments:

(1) I invite the authors to include/discuss two papers in the literature review, both of which test various PTFs in a simulation model and analyze the (dis)similarities in their performance from multiple aspects. Moreover, they use the HYPRES PTFs as well as Rosetta (the PTF or its data), and at least one of them also uses SWAP as the simulation model. As such, these appear to be closest in tools/methods to the current manuscript:

M. Soet and J.N.M. Stricker (2003). Functional behaviour of pedotransfer functions in soil water flow simulation. Hydr. Proc. 17, 1659–1670.

A. Nemes, J.H.M. Wosten, J. Bouma and G. Varallyay (2006). Soil water balance scenario studies using predicted soil hydraulic parameters. Hydr. Proc. 20, 1075–1094.

(2) The discussion section contains much largely repeated text from the results section. That should be avoided and real discussion/interpretation should be offered instead - which of course should refer to the earlier text. Please see additional comments that suggest additional topics for discussion.

(3) I wonder if choosing (and reasoning about) the point to split the growing season is valid and justified. The authors cite LAI and the evaporation/transpiration ratio as a factor in splitting the season. However, crops grow very gradually, and I think it is hard to argue convincingly that there is a clearly identifiable point in the season when that

ratio reaches a particular critical level that will cause abrupt differences such as that shown and cited in Figure 4. However, progression in the season also changes the (upper) boundary conditions – and in this case, the irrigation conditions. Your season split is at ∼DoY=184. Apparently, the sum of precipitation and irrigation during the preceding month and up to that day is about 30mm. However, almost immediately after DoY=184 there is a day with ∼40mm precipitation, followed by 140mm irrigation on DoY=195 and a generally wetter period of 40 days. Of course I do acknowledge that there is also generally more atmospheric demand in July and August in Italy than in June. However, the generally different soil moisture and flux conditions as well as upper boundary conditions after DoY∼184 may be the real cause of the differences – which are mimicked by (blamed on) the change in LAI. I believe this would need some investigation and further discussion.

(4) To add to the point in (3) I would like to cite what is widely termed as a 'warm-up' period for simulations. Users who evaluate model performance usually opt to run the model for a longer period and allow a certain period (weeks, but up to years(!), depending on the time scale and the purpose of modeling) at the beginning of the run to allow the model to 'warm-up'. This basically means that the model is allowed to run through cycles of different conditions so that the initial conditions do not have a great impact on the outcome at later time steps. I don't see such a period being allowed in this study. The potential problem with that is the lack of significant amount of moisture in the soil in the first 30 days or so, which eventually will not allow the model to run through any significantly wet cycles. I wonder whether that has an impact on the findings summarized in Figure 4? In any case, I invite the authors to relate to literature on this and discuss why it is unnecessary to consider a 'warm-up' period in this study.

(5) I would have expected more discussion on the potential causes of significant differences between outputs of the two simulation models – under some circumstances – when otherwise the hydraulic parameterization is the same. This is especially since one of the authors is one of SWAP's main developers, who has an enormous insight

and know-how towards the operation of simulation models, and especially SWAP.

One of the relevant sections would be e.g. P4080, L12-14. Another example is triggered by those shown in Figure 3. One wonders if (and what) a fundamental difference between the two models may be the cause of the major differences in behavior during the periods DoY ∼156-168 and ∼193-203. The latter story may as well be part of the solution to the concerns listed under (3) and (4). Investigating such differences could potentially be important and significant messages of the paper!

Minor comments, corrections:

P4068, L15: Minasny

P4069, L22: also be taken into account

P4069, L28: Feyen

P4070, L22: Eddy Covariance?

P4071, L8: table, 90-120

P4076, L12: four horizons?

P4076, L15: field measurements, no strict correspondence exists between

P4076, L16: (see Fig 2); therefore the retention and . . .

P4076, L22: What is the source of these limits used?

P4077, L16: Is NRMSE unitless? Also, it would be good to mention the units of ME throughout.

P4078, L11: variation is remarkably large in the case of

P4078, L24: with increasing suction

P4079, L6: with increasing suction

P4080, L8: parameter-sets

P4080, L9: parameters, and thus are not shown.

P4080, L21: and model performance is better

P4080, L25: which are the accuracy of crop parameter values and. . .

P4080, L26: conditions

P4080, L24-28: Add a sentence or two to this, describing briefly what way that inhibition takes place.

P4081, L11: very good for both SWAP and ALHyMUS:

P4081, L13-14: pls. rephrase one of these sentences, as RB is used twice for ALHyMUS

P4081, L27: inter-model?

P4081, L19-28: These are very important messages, make sure you have a strong set of conclusions built out of these!

P4081, L22-25: The approach of using multiple parameter sets (or even models) to give estimations is known in literature as multi-model estimation. A good recent paper about this in the field of soil hydrology is by Guber et al. (Vadose Zone J. 2009 8: 1-10.). The advantages of this approach could be commented on and discussed at a later point.

P4082, L15-19: Could it happen that the 'observed' fluxes were not calculated correctly? Also, what can be the potential causes of the delay? (governing Ks/K(theta) is too low? etc. . .)

L4083, L1: inter-model?

L4083, L4: Information. . . (delete 'the')

P4083, L4-21: repetition of text from previous section

P4083, L23-24: This is caused by the actual ET rate being close to the potential ET, as in. . . (try to avoid using the structure "by the fact that", it will make the writing style better!)

P4083, L26-27: Again: is it a possibility that your reasoning for the splitting date is masked by some other, more important factor?

P4084, 1-19: repetition of text from previous section

P4084, L12-13: Be clearer writing this in the discussion section, and be more interpretive.

P4084, L24: parameter set

P4085, L1-3: This is the kind of interpretation that is needed elsewhere as well!

P4085, L11: Fig. 8 shows

P4085, L13: A-RB; while the same figure also shows

P4085, L14-18: I would further emphasize that this happens using both models!

P4085, L23: results using both of the tested models

P4085, L24: performance

Table 1: Zea Mays

Table 2: Silt

Table 3: What is the source for the -8000 value?

Table 6: I wonder if this table is needed? It supports a section of 3 lines – thus is useful – but is a complex table with no good transparency at all.

Table 7: The same applies as for Table 6. Would there be an alternative way to show

the main message of these results? We may not need all the details.

Figure 1: variables in heading need definitions

Figure 3: One wonders if (and what) a fundamental difference between the two models may be the cause of the major differences for DoY ~156-168 and ~193-203. Investigating such could be important messages of the paper. Please see general comment No. 5 above. Also, there is a visibility concern of the 5 almost identical lines. Perhaps show one, and cite that the others ran parallel, plus or minus a maximum of X mmd-1.

Figure 5: Just as in the case of F3, one wonders what fundamental difference in the models would yield the differences that are seen in DoY ~160-195. SWAP is very sensitive to the parameterization, while ALHyMUS is not. What can be behind this?

Figure 6: Similarly to F5, what is the mechanism behind SWAP responding very sensitively to the choice of hydraulic parameter set, while ALHyMUS is largely insensitive? The period/conditions of interest is again DoY 160-195, as there is a large difference there, while for the rest of the modeled period model responses are comparable (mostly parallel).

Figure 7: There is a visibility concern of the 5 almost identical lines.

Figure 8: Include units as applicable.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 4065, 2009.