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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.

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The authors wish to thank the anonymous referee #1 for thoughtful comments, many of which will be incorporated into the revised manuscript. Detailed responses to all the points are reported below.

Major specific comments

(1) I invite the authors to include/discuss two papers in the literature review.

The papers indicated are interesting and useful to improve the discussion in our

manuscript. We will include some comments about them in the revised version of the manuscript. In particular:

M. Soet and J.N.M. Stricker (2003)

The study achieved conclusions similar to those proposed in our manuscript: soil hydraulic properties obtained by the application of different PTFs (Rosetta, HYPRES and AP99) show substantial differences for identical soils. The adoption of different PTFs can introduce considerable errors in large-scale land surface modelling. Moreover, the functional analysis shows high percolation values when the PTF Rosetta is adopted. The same behaviour was highlighted in the results of our research. Also in the Soet and Stricker case study the reason of that was found in the relatively flat shape of the k(theta) curve near the saturation point, rather than to high value of Ksat (which, as a matter of fact, are low compared with the other PTF methods).

Nemes et al. (2006)

In this article, particularly for the reported case study 2, the authors underline the importance of the choice of the PTF to be adopted, since it explains the main part of the variability in simulated water deficits. An important result is that the impact of the use of different PTFs on the cumulative amount of water leached below 1 m at the end of the simulation period is unsignificant, while the variation in the distribution of the same amount over the considered period is relevant. Similar results were obtained in our case study: cumulative values of percolation at the end of the simulation period were found to be about 50 mm respectively for the S_LAB, S_RB, S_Ro, A_LAB and A_H simulation, while the distribution of the bottom fluxes during the period is much more variable. We are thinking about reporting in the revised paper a new figure with the cumulative values of bottom flux simulated by the models.

(2) The discussion section contains much largely repeated text from the results section.

OK, we will reduce the repetitions expanding the discussion section based also on the suggestions provided by the reviewers.

(3) I wonder if choosing (and reasoning about) the point to split the growing season is valid and justified.

Good point; we will improve its discussion in the revised version of the paper. The simulation results of both SWAP and ALHyMUS show that the ratio of transpiration to evaporation increases rapidly starting from about DoY 175 till DoY 186; as soon as this happens the agreement between measured and simulated ET starts improving as well. This is true also in a few days before the irrigation input occurs (between DoY 182 and 195), when the soil is relatively dry. We decided, therefore, to split the simulation period into a first part, where E plays a major role, and a second one where T is predominant. The day of transition between the two parts is when the ratio T/E becomes higher than 3, which is also the time when T becomes higher than its average value over the whole simulation period. This criterion is quite arbitrary, but seemed reasonable to us. We think that the data do not give enough support to the idea that model performances are better under wet soil conditions than under dry soil conditions. We have rather the impression that the T process is better simulated than the E one.

The observation of the Referee #1 gave us the possibility to find out that there was an error in Fig. 4: the maximum value of 8 mm/d on the ET axis was too small to show all the SWAP results, with a few points showing ET slightly larger than 8 mm/d. The revised version of the manuscript will include the correct version of the figure.

(4) To add to the point in (3) I would like to cite what is widely termed as a ‘warm up’ period for simulations.

The simulations start in DoY 156, with initial condition defined by the measured soil moisture contents. We could not use an antecedent period for which we do not have measurements: the soil moisture probes were installed in the soil profile after sowing to avoid a non-uniformity of the mechanical operations conducted in the field close to the instruments area. We agree with the Referee #1 that a ‘warm up’ period can eliminate the impact of the initial condition; however, the initial condition we used is reasonably well defined by the measured soil moisture contents and, in such situations, normally no warming up period is required. Nevertheless, we discussed whether we should have focused our analysis only on the period starting about DoY 190; we considered, however, that the initial head profile is itself influenced by the set of hydraulic parameters which is used, since the head values are derived from the observed soil moisture values through the retention curve. Therefore the unique initial soil moisture profile gives different initial head profiles, depending on shape of the retention curve, i.e. on the parameter set. The larger or smaller soil moisture redistribution simulated by SWAP in the first period is itself an effect of soil hydraulic parameterization and this is one reason why we decided to keep it in the functional analysis. Another reason is that, even considering only the period after DoY 190 the results of our analysis do not change substantially and our main conclusions remain the same. Anyway, we agree that this is a relevant point and we will expand its discussion in the revised version of the manuscript.

(5) I would have expected more discussion on the potential causes of significant differences between outputs of the two simulation models. [. . .] One wonders if (and what) a fundamental difference between the two models may be the cause of the major differences in behaviour during the period DoY $\sim$ 156-168 and $\sim$ 193-203. The latter story may as well be a part of the solution to the concerns listed under (3) and (4). Investigating such differences may could potentially be important and significant messages of the paper!

The discussion on the different results provided by the two models will be extended also introducing the comments reported at point (3) and (4). Regarding the differences in the evaporation process (i.e. DoY $\sim$ 156-168) between the two models, ALHyMUS calculates this term on the basis of the dual crop coefficient proposed by Allen et al. (1998), while in SWAP the evaporation is derived from Penman-Monteith equation taking account the limitation due to the soil water content of the upper layer (see Kroes and Van Dam, 2003). With these set up in general the simulated evaporation pattern is similar with the two models but the former provides usually higher values compared with the latter. This behaviour was evident also in further comparisons carried out between the two models considering different soil types and a 13-years simulation period, and it has to be investigated in more detail. In the particular case study we don't have other measurements in bare soil conditions and thus, at the moment, we cannot give any practical advice. Anyway we will better underline also this point in the revised paper. In relation to the second period mentioned (DoY ∼ 193-203), we underlined in the paper that SWAP can account for a limitation to transpiration due to extremely wet soil conditions. Since ALHyMUS does not include this limitation we did not consider it in SWAP as well. We agree that this point is worth more attention and will run new SWAP simulations, considering the default values of the parameters accounting for limitation to transpiration due to high soil water content.

Minor comments

We thank the Referee #1 for all the technical corrections that we will introduce in the revised paper. We only wish to comment on the following points to give a contribution to the discussion:

P4070, L22 Eddy covariance?

Replaced "eddy-correlation" with "Eddy covariance".

P4071, L22 four horizons

Yes, four horizons. As discussed also with Referee #2, we are evaluating the possibility to introduce a figure for a better illustration of the soil profile and the discretisation operated by the to models (see Refere 2# point (1) of the "Minor specific comments" for the figure).

P4076, L22 What is the source of these limits used? and Table 3: What is the source for the -8000 value?

In the default set up of SWAP the wilting point is fixed at -8000 cm for maize. The same values is reported in Hupet et al., 2004.

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

NRMSE is unitless while ME is not. For the latter index the unit will be added.

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

An exhaustive explanation is given in Gerosa et al. (2003). We will add explanations in the paper, clarifying that the damages provoked by high ozone concentrations conduct to a general reduction of the productivity (average crop yield loss of 5% for experimental conducted in open-top chambers) as well as to an increase of the crop sensitivity to other biotic and abiotic stresses.

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

We will rephrase the sentence in the revised version of the paper. It means that a good performance is achieved with SWAP using PTFs RB and Ro and with ALHyMUS using PTFs RB , H and Lab.

P4081, L27: inter-model?

Replaced "intra-model" with "inter-model"

P4081, L19-28 "It is worth observing that the performances of parameters sets derived by PTFs are similar – or even better in the case of SWAP – to those of parameters sets obtained by direct methods.": These are very important messages, make sure you have a strong set of conclusions built out of these!

This fact is more evident for the physically-based SWAP model, for which values of NRMSE higher than the ones found for both direct methods, considering the simulation of soil water content. This suggests that when the physically based models cannot be calibrated with local measurements, the use of PTFs with site-specific textures could be a good choice. As a matter of fact, the conceptual models like ALHyMUS turn out to be less sensitive to the choice of the set of hydrological parameters (NRMSE always < 1). Our impression is that when the model parameters are calibrated on the basis of local observations of soil moisture and pressure head, then physically based models provide better performances than conceptual models. However, when the model parameters are derived from either direct or indirect methods, but no calibration is carried out, the performances of the two type of models are quite similar; in our case ALHyMUS proved to be less sensitive to the parameter set and to provide more homogeneous results compared to SWAP. This is an important message for large scale, spatially distributed, model applications.

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.

Guber et al. use an approach for the vadose zone models that is widely used in weather forecasting: the use of different models (e.g. PTFs) and the evaluation of different method of combining the simulation results. It is a good suggestion to comment on this, based on the results of our simulations, and we will add a discussion of the point in the revised version of the paper.

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. . .)

The ‘observed’ fluxes at the bottom of the root zone were calculated as residuals of the daily water balance in the root zone, as described in detail in the section “Major specific

comments” - Referee #2. In the revised paper we will try to give a better explanation of this point. The cause of the delay is the relative low value of K(theta) that achieved with the particular hydraulic parameters. In the case of ALHyMUS, the effect is due also to the unit gradient assumption.

P4083, L4-21 and P4084, 1-19: repetition of text from previous section.

We will improve the discussion as discussed for points (3) to (5) and we will try to eliminate the repetitions.

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

See points (3) to (5).

P4084, L12-13: Be clearer writing this in the discussion section, and be more interpretive. P4085, L1-3: This is the kind of interpretation that is needed elsewhere as well!

Ok

P4085, L14-18 “the errors in surface and bottom fluxes compensate each other and cannot be captured by looking just at soil moisture patterns”: I would further emphasize that this happens using both models!

We agree with Referee #1 that this is an important message. In the revised paper we will point out that this fact can happen using both models.

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. and 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.

Comment reported also by Referee #2. We are thinking about reporting in the revised paper only some elaboration of the data in Tables 6 and 7.

Figure 1: variables in heading need definitions.

OK. We will include in the revised paper the 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.

See discussion point (5). We will evaluate the suggested possibility to obtain a clearer figure.

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?

See discussion point (4) and above, under your comment: P4081, L19-28

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).

See discussion point (4) and above, under your comment: P4081, L19-28

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

This point is reported also by Referee #2. We are trying to improve the figure showing only two shorter windows in the simulation period (i.e. one at the beginning, when capillary rise is the main term, the second covering a percolation event).

Figure 8: Include units as applicable.

OK, we will do it

References

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