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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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We wish to thank the anonymous referee #2 for the interesting suggestions and useful comments. We will use them for improving the manuscript. In particular:

Major specific comments

(1) I suggest to add a paragraph to the introduction which explains why it is worth while to compare these two models.

We agree that the motivations of the model comparison can be expressed in an im-

proved form in the manuscript. Therefore we will introduce a paragraph, with a short assessment of the advantages/disadvantages in using the two different modelling approaches as well as the added value of their comparison.

(2) how the root zone reservoir in ALHyMUS functions in this modelling exercise and (3) averaging the hydraulic parameters of various layers may lead to quite a different hydraulic behaviour within the soil profile compared to the layered soil profile as applied in SWAP. This should be pointed out clearly.

The Referee #2 points out an important issue when considering reservoir models: how the equivalent parameters for the reservoirs can be calculated from the soil parameters of the different soil profile horizons. Usually a weighted average of the soil parameters for the different horizons falling in the reservoir is carried out; the arithmetic average is commonly adopted for parameters which refer to a water content in the porous media (such as water soil content at the saturation, at the field capacity, at the wilting point as well as the residual soil water content), while the harmonic mean is usually adopted for the hydraulic conductivity when the flow is perpendicular to the layering (see, e.g., Freeze and Cherry, 1979). We must thank the Referee #2, because we realized that in the manuscript we erroneously wrote "geometric" instead of "harmonic" for the average of this parameter, therefore we will have the opportunity to correct this term in the revised version. In the particular case of the applicative study, we adopted a simplified approach for the parameterization of the second reservoir, fixing the parameters for the whole simulation period at the value calculated considering the maximum extension of the root zone (i.e. 70 cm, thus the parameters of 4 horizons are used in the calculation of the equivalent parameters for the second reservoir). As suggested by the Referee #2, the approach used should be considered in the interpretation of the results. In the revised manuscript we will try to take into account this point and to use it for the discussion. The effect of the simplification should be more evident for the bottom flux and for the first period of the simulation, when the roots are growing. In this period the reservoir has average soil hydraulic parameters typical of soils finer than the real one (i.e. upper horizons are coarser). The effect anyway doesn't emerge with evidence by the comparison of the simulation results with the measured data, because the initial soil condition is very dry and the percolation fluxes at the beginning of the simulation period are consequently scarce. When the percolation flux becomes predominant over the capillary rise, the roots are deeper and the simplified approach adopted in the calculation of the average soil parameters has a lower importance.

(4) Initial condition and warm-up phase.

We agree that a warm up period could solve the problem pointed out. Jadoon et al. (2008) show that, when the initial conditions are poorly known or not known at all, the simulation of the soil moisture in the first period can be affected for a long time by this uncertainty, depending on the soil type and the bottom boundary conditions. After this lag-time, the soil water content value starts to be independent from the initial condition. In this sense it is suggested to analyse only the data after this "time of convergence". Our simulations start in DoY 156, when the initial condition 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. Concerning the way in which the initial soil water content condition was set up for the two models, for

both of them it was derived from the measured data. The soil water content measures were available at the following depths: 5, 20, 35, 50, 70 cm. Before implementing the initial condition in SWAP the corresponding hydraulic heads were calculated by inverting the van Genucthen equation for the different sets of soil hydraulic parameters. The obtained values were then introduced in SWAP at the fixed depths and automatically SWAP operates the linear interpolation between the points. In ALHyMUS the measures at different depths were considered representative of the soil water content for a layer surrounding the probe (i.e. extending each measurement above and below the probe for half the distance between it and the next probe). A weighting average was then carried out to calculate the initial water content for the two reservoirs.

(5) From my point of view the performance evaluation for soil water content and bottom flux should be elaborated more clearly

a. How were the 'measured average soil water content' and 'observed bottom flux' determined exactly?

In the revised paper we will try to explain better these points. Indeed, the Referee is right in the sense that both variables were derived from measurements, but not directly measured. Therefore we agree to replace 'observed' with 'estimated from measurements' for both the bottom flux and water content in the root zone. In particular the measured average soil moisture is calculated with the same procedure adopted in the calculation of the initial soil water content condition for ALHyMUS. For the flux at the bottom of the root zone, the values are obtained as residuals of the water balance with a daily time step. All terms of the water balance come from field measurements (i.e. as a consequence the bottom flux can be estimated only for days in which all the measurements are available). During the root growth phase the bottom depth increases during a time step; the bottom flux during time step t is computed with reference to the bottom depth at the beginning of the time step. The water balance equation has the following form:

$Q_{b,t} = U_t - U_{t-1} - P_t - I_t + ET_{a,t}$

Where:

Q_{bt} = bottom flux in day t (negative for percolation and positive for capillary rise) [mm];

U_t = water content in the root zone at the beginning of day t [mm];

P_t = precipitation in day t [mm]

I_t = irrigation in day t [mm]

b. I suggest to compare the simulated average root zone soil water content and bottom flux exclusively to the 'measured' values.

We cannot agree completely with the Referee #2 on this point, since in our opinion also the comparison between models given a certain data-set is interesting, because the differences can be read as a measure of the uncertainty in the results that can be attributed at the adoption of a different modelling approach. Anyway, with the aim of simplifying Tables 6 and Table 7 (see also points relative to these tables in the answer to Referee #1), we will show in the revised paper only some elaboration of the data reported in the two tables.

c. For me it is very difficult to extract the information from Figure 7. Probably one could clarify this figure by plotting cumulative bottom flux instead of the daily values. We are trying to improve the figure, as suggested also by Referee #1, 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).

Unfortunately we cannot use the cumulative flux because we don't have enough bottom flux values "estimated from measurements" (i.e. white bars in Figure 7), since we could calculate the bottom flux only in those days for which the measurements of all the other terms in the water balance were available.

(6) The major part of section 4 is more a summary than a discussion. Probably you

could rename section 3 into 'Results and discussion' and use 'Summary and conclusions' for section 4.

OK, we will do it, we agree with Referee #2.

Concerning the conclusions I would be interested in a more general assessment of the results, e.g., - What is the authors' overall assessment concerning the application of both models in combination with the parameters inferred from the different estimation techniques?

Our assessment 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-study 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.

- Which kind of model and which kind of parameterisation should we use in future in order to model root zone water fluxes or are there other approaches which should be explored?

R&B PTFs proved to give good performances in our case study; though extension to larger areas needs more observations, our results are in agreement with previous observations of Ungaro et al. (2001) for the soils of the Padana Plain. Regarding to models, as reported at point (1), SWAP is a widely applied and validated model: when adequate local data set are available it demonstrated in many studies to perform very well. When only standard data-set are available (for instance from regional data-bases) it is still hard to say which model performs better in estimating soil moisture and water fluxes.

- How can we improve the presented approach in order to reduce the uncertainty in the modelled fluxes and average water contents?

Our result show that there is not a single method for the determination of soil hydraulic parameters that is better than the others, the suitability of the method being also dependent on the type of model which is used. The large difference among PTF's shows that substantial improvement is possible: use more undisturbed samples, larger measurement ranges and more accurate measurement methods.

Minor specific comments

(1) provide a figure describing the various soil layers.

OK, we will add a figure to better explain the procedure followed in the assignment of the soil hydraulic parameters in the case of the two models.

(2) p. 4071, l. 5: CS616 measurement.

OK

(3) p. 4071, l. 9: Please add information about the meteo sensors used and at which height the measurements were taken since this is important for ET calculations.

The temperature and the air humidity were measured by a Valsala HMP35C sensor, the pluviometer is a ARG100, the wind cup anemometer is a WSD-1. The instruments are installed in a grass parcel located at 200 m distance from the experimental field. The wind speed is measured at 2 m height. The global radiation is measured by the 4 components radiometer (Kipp & Zonen CNR-1) installed at the micro-meteorological. In the paper we will add the most important information reported in this paragraph.

(4) p. 4071, l. 9: Please specify/explain PAR sensors.

PAR (Photosintetically Active Radiation) sensors are LI-COR LI190SB. We will add this information in the revised version of the manuscript.

(5) p. 4071, l. 16: How was the amount of infiltrated water calculated in detail? Please explain.

The gross amount of water applied by the first irrigation (25 mm) was controlled as carefully as possible during the operation. In order to confirm this amount we computed the increase in soil moisture measured by the probes installed at different depths in the profile. The sprinkler irrigation time was about 8 hours, while the time step at which soil moisture data are recorded is 30 minutes The difference between the soil moisture content measured after the end of the irrigation and just before its start by each probe was considered representative of the layer surrounding the probe (i.e. extending above and below the probe for half the distance between it and the next probe). Only the values measured by the three upper probes showed significant changes. To obtain the total volume provided by irrigation, the additional volume stocked in each layer was summed up and the ET of the irrigation period was added at this amount. The calculations were repeated for a few different time periods of increasing length, always starting from the beginning of the irrigation and extending after the end of the irrigation (with a time step increase of half an hour, which is the monitoring instruments time step) and gave almost constant values, very close to 20 mm; taking into account the water losses before reaching the soil surface this value is in good agreement with the gross irrigation amount and therefore we kept 20 mm as irrigation supply to the soil surface.

(6) p. 4076, l. 4–11: I suggest to use HLIM(1:::5) instead of HLIM. Please specify parameters a, k and p.

In the revised version of the paper we will use HLIM (1. . .5) instead of only HLIM. a is an empiric parameter used in the equation for the estimation of the intercepted rainfall (Braden, 1985) for both models. k is the extinction coefficient for global solar radiation which links the cover fraction (CF%) to the leaf area index (LAI). p is the fraction of the Total Available Water (TAW) that identify the Readily Available Water (RAW) for the crop.

(7) p. 4076, l. 12: Is five horizons for SWAP correct (not four)? If five horizons were used, please specify. What was the vertical profile discretisation used in the SWAP simulations?

OK, there is an error at p. 4076, l. 12. It's 4 and not 5. As underlined at point (1) of the "Minor specific comments", we will add a figure to better explain the assignment of soil hydraulic parameters both in the SWAP and ALHyMUS cases. The vertical discretization for SWAP is reported below:

LAYER SUBLAYER HSUBLAY HCOMPARTMENTS NCOMP

1 1 10.0 1.0 10

2 2 30.0 1.0 30

3 3 15.0 1.0 15

4 4 35.0 1.0 35

where LAYER is the number of the soil horizons with different soil properties;HSUBLAYER is, in this case (i.e. LAYER and SUBLAYER are corresponding for the case study), the thickness of each LAYER, HCOMPARTMENTS is the distance between nodes for which the Richard's equations is solved and finally NCOMP is the numbers of nodes for each LAYER.

(8) p. 4076, l. 14–18: . . .the textures of horizons 3 and 4 are quite different. Here, one has to consider an influence of the combination of both layers on the modelling results when comparing the outputs to those derived from the 'complete' parameter sets. I suggest to add a comment on this in the paper.

The "field" parameter set considers a unique layer from 40 to 90 cm depth, while the characteristics of the soil profile show that there is a change to a more clayey soil at 55 cm depth. The Referee is right that the combination of the two bottom layers has an effect on the simulation results and we will comment on this in the revised version of

the paper.

(9) p. 4078, l. 19–23: What was the range (max/min values) in water contents and pressure heads measured at the different depths? Please also indicate this range in Fig. 2.

OK, we are adding gray strips to Fig. 2 to highlight this important information and it will be used for improve the discussion.

(10) Figure 3: Please indicate dates were measurements were taken.

The dates can be seen in Figures 3, 5 and 7, where the values of ET, soil water content and bottom flux that were measured (or estimated by measurements) are shown as black dots. We hope this answers the Referee's question.

(11) Figure captions and legends: I would like to have the figure captions and legends more self explaining.

OK, we will add information in captions and legends.

Technical corrections

We accepted all the corrections. Thanks.

References

Freeze, R.A. and Cherry, J.A., Groundwater. Prentice Hall, 1979, 604 pp.

Ungaro, F. and Calzolari, C.. Using existing soil databases for estimating retention properties for soils of the Pianura Padano-Veneta region of North Italy, Geoderma, 99, 99–121, 2001.

Braden, H.: Ein Energiehaushalts- und Verdunstungsmodell für Wasser- und Stoffhaushaltsuntersuchungen landwirtschaftlich genutzer Einzugsgebiete, Mitteilungen der Deutschen Bodenkundlichen Gesellschaft, 42, 294–299, 1985.

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