

Responses to additional referees' comments  
to paper HESSd-8-5083-2011

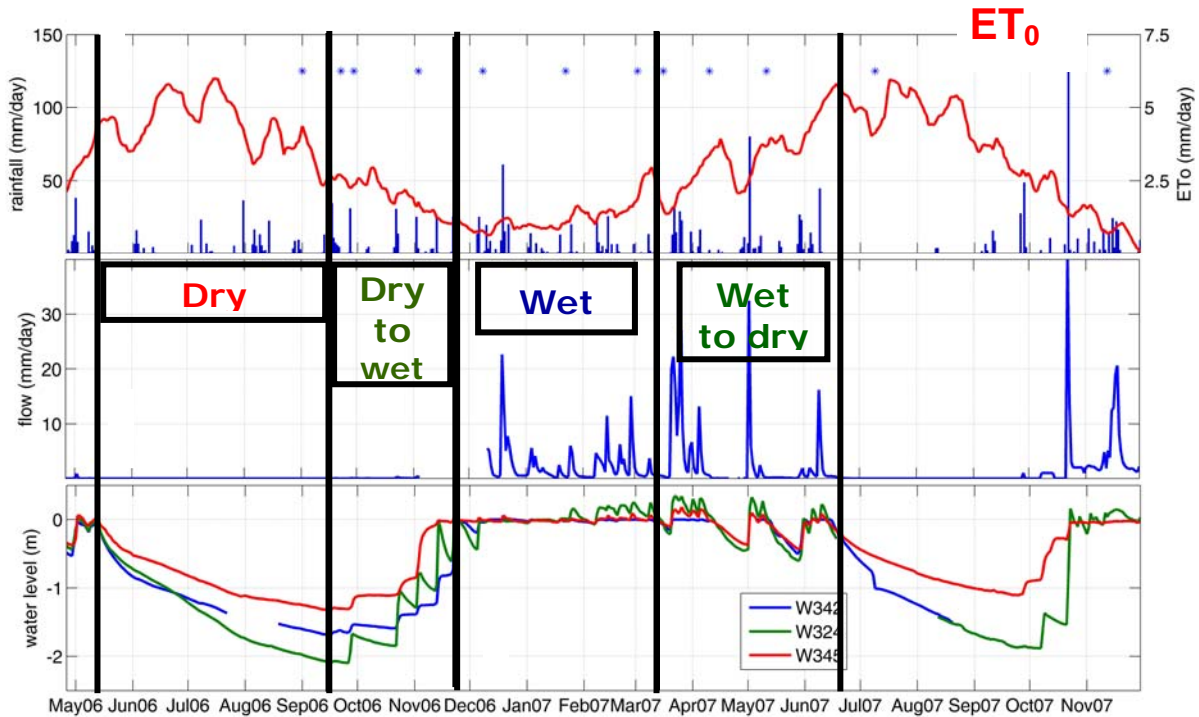
We would firstly thank all of the referees for their comments and constructive criticisms. Below are replies to some common points raised by referees and then we will address a few specific comments. Other questions will be treated more specifically in the revised version of our work.

- Reference of Romano and Santini (1999) is as follows (to be added in the revised version): Romano, N. and Santini, A.: Determining soil hydraulic functions from evaporation experiments by a parameter estimation approach: Experimental verifications and numerical studies. *Water Resour. Res.*, 35, 3343-3359, 1999.
- Eq.(2b) requires correction as parameter  $\tau_{VG}$  should be the exponent of variable  $S_e$ . This equation will be changed accordingly in the revised version. Actually, Table 1 correctly reports the value we used for this parameter [i.e.  $\tau_{VG}=-1.0$ , and not 0.5 as shown in Eq.(2b)].
- Admittedly, the text is not clear with respect to the type and number of simulations performed, particularly in the abstract where we refer to “100 time-series of stochastically-generated daily rainfall data” (lines 8-9 page 5084), and in section 3.2 where we state that we used “100 time-series of synthetic daily rainfall records” (lines 7-8, page 5099). Actually, the study was carried out by one continuous simulation for a period of 100 years and NOT by 100 simulations, one year long each. Thus, the top boundary condition was defined by one time-series of stochastically-generated daily rainfall 100 years long, obtained by combining two different Poisson Rectangular Pulse models, for the regrowth and dormant vegetation phases, respectively. Therefore, the results are not influenced by the initial conditions.

1) Reply to A.J. Guswa

Andrew made interesting comments and raised some important questions to our discussion paper. We agree with the additional analysis suggested and basically the core of his suggestions will be presented in the revised version of this work. However, in the following we would like to give some short replies to a few points and clarify some other statements.

While points -1- and -2- will be specifically discussed in the revised version, we would point out here that the selected seasonality sequence of dry and wet periods can be considered as representative of a Mediterranean climate in southern Italy, particularly in sub-humid and semi-arid areas, where a relatively fast transition occur from dry to wet conditions and vice versa. Below, we present as an example a picture describing some experimental data in a sub-humid area in Campania, southern Italy. We parameterized the climatic forcing according to Pumo et al. (*Hydrol. Earth Syst. Sci.*, 12, 303–316, 2008), who refer to an area in Sicily.



As for point -3-, we definitely would stress that the “drain” method is a more physically-based and consistent method and therefore should be in any case preferred with respect to more empirical techniques such as those that suggest to estimate water content at field capacity from the hydraulic conductivity function or, in a worst way, from the water retention function. In this study, we decided to show comparisons between this latter (and more employed) technique and the “drain” method.

Note that at P.5095 we presented the method by Meyer and Gee (1999) who suggested to estimate field capacity from the  $K(\theta)$ -curve as the water content when  $K$  takes on values ranging from  $10^{-6}$  cm/s ( $=8.64 \times 10^{-2}$  cm/day) to  $10^{-8}$  cm/s ( $=8.64 \times 10^{-4}$  cm/day). Well, for our two soils we have:

- loamy sand: if  $K_{fc,SL} = 8.64 \times 10^{-2}$  cm/day, then  $s_{fc,SL} = 0.485$  ;
- clay: if  $K_{fc,Cl} = 8.64 \times 10^{-4}$  cm/day, then  $s_{fc,Cl} = 0.550$  .

These two “fixed” values for the water content at field capacity still differ somewhat from those obtained by the “drain” method, especially for the clay soil in this case. In the specific study cases, field capacity computed with the “drain” method corresponds to unsaturated hydraulic conductivity values equal to 0.188 cm/day for the loamy-sand soil and to 0.0663 cm/day for the clay soil. Both values are different from what suggested by Andrew (0.05 cm/day).

It is also worth noting that the use of a “fix” method based on the unsaturated hydraulic conductivity function (the same apply for the “fix” technique based on the water retention curve), would apply under the hypothesis of a uniform soil profile.

## 2) Reply to R.S. Crosbie

As for the first specific comment, we understand Crosbie’s view of the problem, but we would maintain, as far as possible, a physical meaning to the BM and RE-SWAP model parameters. The way proposed by this referee might give the impression to some readers, if not well written and explained,

that we may suggest looking at the field capacity value as a calibration parameter. Moreover, it could not be clear which variable should be employed in the optimization procedure. Namely, using for example transpiration fluxes may lead to a field capacity value that differs even markedly if one makes the calibration with respect to, let's say, the drainage fluxes.

Concerning the comment about the results presented in Table 5, during a doctoral period we made analyses for other soil textural classes retrieved from our database (e.g. Ceres, F., G.B. Chirico and N. Romano, 2010. Functional evaluation of the field capacity concept for water balance analysis under climatic seasonality condition. EGU 2010 General Assembly, Vienna 2-7- May, EGU2010-2901). We have presented here results for two somewhat contrasting soils. However, one should admit and recognize that the "drain" method is undoubtedly the preferred method to determine the field capacity value (and that will be even more valuable when dealing with layered soil profiles). Rather, we would show in a functional manner under what conditions and to what extent the well-known "fix" retention method can generate the larger discrepancies. This objective is different from the "parametric" evaluation performed by Twarakavi et al. (2009).

### 3) Reply to L. Peeters

The major questions raised by this reviewer have been address in the previous responses.