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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Measuring and modelling water related soil–vegetation feedbacks in a fallow plot" *by* N. Ursino et al.

Anonymous Referee #2

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General comments:

The authors investigate the role of vegetation cover on infiltration and soil moisture distribution at a fallow plot in Southern Sardinia. The study was performed by a combination of experiments and modelling. Information about soil moisture distribution, gained from TDR measurements and indirectly from electrical resistivity tomography during an infiltration experiment, was used to analyze feedbacks between the sparse vegetation cover and the soil hydraulic conductivity. In addition, a simple bucket model was set up. The model was used to analyze differences between the fallow plot and a neighbored cultivated plot in the local water balances during the infiltration experiment. The model purpose was further to simulate changes in soil moisture of the upper soil and the deeper soil at the fallow plot during one year depending on soil coverage by

weeds. The experimental part of this paper is sound and also the interpretation and the conclusions, which are drawn from the experimental data, are coherent. I can also follow the reply of the authors to the questions of the Referee#1 to the discussion-paper, who argued that the experimental part of the study was already presented in a previous paper. In my opinion, the reinterpretation of the data, partially presented in Cassiani et al. (2012) but now based on a different inversion technique of the ERT signals, delivers substantially new insights in moisture dynamics at the fallow field and is therefore of high interest for the readers of HESS. Besides of some points already pointed out by Referee#1, I have only some minor comments on this part of the paper (see below). However, I have major concerns with the modelling part of the study, in particular with the long-term simulation over one year and how conclusions are drawn from the modelling results. The section "Model outcome" remains more or less speculative because the simulation results are based on questionable model assumptions and are extremely dependent on initial conditions, which seem to be arbitrarily chosen without an experimental basis. Moreover, some findings which are represented as "model outcomes" follow directly from the settings and assumptions in the model. Consequently this part of the modelling exercise provides no additional benefit to the entire study. I recommend, therefore, either to omit the one-year simulation and to focus on experimental results including the short-term infiltration model or to reformulate the model for the one year study. In the latter case, the model-based analysis should also include an analysis of the impact of different initial conditions on simulation results.

Specific comments:

My major concerns with the modeling part of the study are two model assumptions, which may be valid for the 7-day period of the infiltration experiment, before which probably a solid soil crust has established on the fallow plot, but are certainly not valid for a whole season with longer rain events during autumn and winter. The critical assumptions are i) no transpiration takes place from the USL and ii) Lu = 0 in case of bare soil, irrespective of saturation in the USL.

The first assumption does not longer hold as soon as soil moisture in the USL is significantly higher than permanent wilting point for more than two or three days, because plants react very flexible to favorable environmental conditions. For example, in figure 5 (right) in Cassiani et al. (2012) it is shown that the weed from the bare soil plot has clearly active roots in the upper 10 cm of the soil.

The explanation on page 11163, line 23 – page 11164, line 8, to which I totally agree, is contradictory to the second assumption, that Lu=0 in case of bare soil. Figure 5 clearly shows that a decrease in resistivity occurs even in those parts of the deeper soil, where the soil is not covered by plants ("piston flow").

Page 11166, lines 13-14: is there any evidence that there was no transpiration before DOY 80 and after DOY 274? Which species were grown on this plot? Are there observations of the growth stages of the weeds?

Page 11166, lines 14-16: how was this accomplished? Is it the result of a spin-up run of several years? How was Sd(1) fixed in case of CC=0? In this case equation (4) degenerates to d(Sd)/ dt = 0 and Sd(1) determines the saturation degree for the whole year.

Page 11166, lines 23-24 “the red and the green lines coincide in between DOY 170 and DOY 280”: This is not a model outcome but directly follows from the boundary conditions of the model (initial value of Sd(CC=0) = lower transpiration limit of CC=0.4).

Page 11166, lines 24-25 “During the summer season, Su is higher for CC = 0.4 (black line) than for CC = 0 (blue line) due to the vegetation shadowing”: This is not surprisingly, because it is the direct consequence of the assumption that in case of CC=0.4 a part of ET is taken from the DSL (Lu = 0, because Su<1 during summer), whereas in case of CC=0 all ET is taken from USL.

Page 11166, lines 25-27 “during the wet season Su is higher when CC = 0 (blue line) due to the fact that when CC = 0.4 the USL transfers water to the DSL that acts as a

reservoir and the vegetation facilitates the infiltration”: This is also no ‘model outcome’ but the underlying ‘model assumption’. Thus, from my point of view, the one-year modeling part of this study provides no further explanation of or insights into soil-vegetation feedbacks in this experiment and can be omitted.

Page 11167, lines 4 – 24: this is clearly no “model outcome” and should be therefore moved to a separate section “Discussion”. It would be much more interesting to see the differences between CC = 0.4 and CC = 0.0, when the model is run with identical initial values and assumptions. Are there differences in soil moisture distribution after one, two or more seasons due to the presence of vegetation?

Minor comments:

On the whole, the manuscript is well organized. However, in the section “Results”, it is sometimes difficult to distinguish between real results and speculation. I recommend, therefore, to introduce a separate section "Discussion". Likewise, some paragraphs presented in “Introduction” belong rather to the section “Material&Methods” as already mentioned by Referree#1.

Some information is given repeatedly, e.g. page 11157, lines 16-17 “occurred overnight . . . 42 mm”: this has been already mentioned on page 11155, lines 25-26 and does not need to be repeated here.

Page 11157, lines 21-23: avoid two times “before irrigation”.

Page 11159, line 13: probably “evaporation” instead of “transpiration”

Page 11159, equation 2: how is RO calculated? Or is it measured? If RO is not considered in the model, it should not be introduced in the equation.

Page 11161, line 8: insert “)”

Page 11162, line 4: “2010” instead of “2012”

Page 11163, lines 18-27: this explanation is further supported by results of Moijd and

Cho (2008), Vadose Zone J. 7:972-980.

Page 11166, line 20: again “evaporation” instead of “transpiration” (?)

Page 11177, Fig. 3: vegetation “greenness” instead of “fitness”.

Page 11179, Fig. 5 and : Page 11181, Fig. 7: the increase in resistivity cannot be seen from this legend.

Page 11182, Fig. 8: “Vegetation cover is evaluated by VIA” should not be repeated here.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11151, 2013.