Hydrol. Earth Syst. Sci. Discuss., 8, C3485–C3487, 2011
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Infiltration-soil moisture redistribution under natural conditions: experimental evidence as a guideline for realizing simulation models" *by* R. Morbidelli et al.

Anonymous Referee #2

Received and published: 15 August 2011

Authors present a very interesting experimental and numerical analysis about infiltration process under natural conditions. The hydrological importance of the subject is evident due the absolute relevance of the evaluation of hydrological losses in many practical applications (e.g.: rainfall-runoff models, water resources availability, etc.). In my opinion, in the paper, there are both originality and novelty in some items as the HESS journal requires. Originality is, in particular, in developing some models (for homogenous and sealed soils) with a thin upper crust, formed under a sequence of rainfall events, that breaks up with freeze-thaw cycles. Authors, supported by many field measurements of soil water content (continuously monitored at different depths using TDR system), and of some weather related (air temperature and relative humidity, wind speed, solar radiation, evaporation and rain), highlight the important role of crust formation in infiltration processes in natural soils and how the model simulations under homogenous conditions fail to explain the observed data. For these reasons, my overall opinion of the paper is high positive.

I have the following questions and suggestions in order to give to authors the possibility to further improve their paper:

1. In my opinion the sub paragraph 2.1, despite their title, is too long and for improve the readability of the paper should be shortened; furthermore a table with the list of parameter values needed to start simulation, as an example for events analyzed, could be interesting for reader.

2. About the experimental set up (soil characteristics, TDR measurement, etc) some words about the uncertainty of measures should be spent in order to clarify their role in the analysis performed and the relative impact in results.

3. I have no understood which was the criterion used in events selection from data set (april – December 2010) considering there are (see fig. 2) many other events not considered. Authors must clarify this point.

4. Some information about "calibration events occurred in the period October 2009 – march 2010" are needed: rainfall depth and duration, maximum rainfall rate, etc. . .

5. An ulterior effort should be made in order to try to explain the variability of saturated hydraulic conductivity of seal layer within events. In my opinion a considerable improvement of the model could be an empirical relationship of the saturated hydraulic conductivity of seal layer with, as an example, with measured initial soil water content: (see the supplement file in pdf).

in this way, could the model offer a predictive ability? Which is the author opinion? Is it possible to test the model predictability for other events not still considered in the analysis?

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/8/C3485/2011/hessd-8-C3485-2011-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6199, 2011.

HESSD

8, C3485–C3487, 2011

Interactive Comment