Manuscript evaluation criteria:

Scientific significance:

Does the manuscript represent a substantial contribution to scientific progress within the scope of Hydrology and Earth System Sciences (substantial new concepts, ideas, methods, or data)? Between fair(3) and poor (4) since the contribution is not new, except for the application to Germany data possibly.

Scientific quality:

Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Between fair(3) and poor (4) since There are some difficult aspects commented below.

Presentation quality:

Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)? Poor(4) due to the problems in the English language use, the presentation of equations and the lack of any Table to express the goodness of the fit of model to data.

Specific questions:

1. *Does the paper address relevant scientific questions within the scope of HESS?* Yes.
2. *Does the paper present novel concepts, ideas, tools, or data?* Not many.
3. *Are substantial conclusions reached?* Not.
4. *Are the scientific methods and assumptions valid and clearly outlined?* Not at all.
5. *Are the results sufficient to support the interpretations and conclusions?* Not.
6. *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?* Not at all.
7. *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?* Yes.
8. *Does the title clearly reflect the contents of the paper?* Yes.
9. *Does the abstract provide a concise and complete summary?* More or less.
10. *Is the overall presentation well structured and clear?* Not, see comments below.
11. *Is the language fluent and precise?* Not.
12. *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?* Not, see the comments below.
13. *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?* Yes, see the comments.
14. *Are the number and quality of references appropriate?* There are relevant references missing.
15. *Is the amount and quality of supplementary material appropriate?*

General comments on the manuscript

The manuscript explores the evolution of soil moisture profiles in Germany during the 2002-07 period with a simple soil water balance model based on the integration of Richards equation, with a more detailed consideration of the interception process on the vegetation canopy.

The authors presents equations to describe the depth of canopy intercepted water, P_i, the depth of canopy drained water, P_d, the depth of throughfall, P_t, the depth of stored water in the canopy, W_s, and the depth of daily evaporated water from the canopy, E_c. They introduce an additional term, what they call canopy closure, with a acts as a limit to the canopy storage.

Being correct the selection of the interception variables, there are some aspects that should be revised by the authors: (i) one could assume that the dimension of these variables is length, L, for daily computation, or LT^{-1}, to be more precise, but this point should be stated in the manuscript; (ii) is equation (3) correct? one could think on

$$f_c = \frac{\Lambda}{\Lambda_{max}} f_{cmax} \tag{1}$$

better; (iii) considering on the widely accepted Rutter et al (1971) model, the first term on the right hand side of equation (7) could have a minus sign, since the gross rainfall rate, P, consists of throughfall, evaporation and drainage loss, and the canopy storage temporal change. Therefore the authors should discuss the deviations from the mentioned model to help to the readers to better understand their proposal.

The water evaporation from a canopy could be greater than the reference evaporation rate estimated with the Penman-Monteith equation or measured in standard weather stations, could the authors precisely define the maximum potential evaporation rate of page 3242 lines 9-10?

The numerical solution of the one-dimensional water infiltration process into soil is briefly described, except for the introduction to the Richards equation (pages 3242 and 3243, from lines 20 to 14), which could be obviated since it is very familiar to the potential readers of this manuscript. Nevertheless after considering the possible presence of saturated horizons in the profile, I wonder how the authors not prefer the use of Kirchhoff transformation to gain precision and to avoid stability problems (e.g. Berninger et al. 2011). The simple interpolation scheme adopted for the hydraulic conductivity, equation (14), may be less accurate than other schemes (e.g. Szymkiewicz and Helmig 2011).

The use of rather imprecise terms like field capacity or permanent wilting point in the model may difficult the interpretation of the results. In addition to, how do the authors define 'mean plant available soil water content' (page 3248 lines 11-12)? The manuscript should be carefully revised.

The manuscript should include a section dedicated to explain all the methods adopted in the research.

For comparative purposes a non-dimensional index such as the Nash-Sutcliffe index could be more useful than the RMSE (page 3249 line 2).

There are several imprecise statements requiring a revision:

- The definition of the infiltration process in page 2342 line 20.
- What does 'highly empirically derived' (page 242 line5) mean?
- What is a 'soil dependent saturated condition' (page 3244 lines 13-14)?
- Why do the authors fix the soil top layer depth in 3 cm (page 3244 lines 13-15)?
- Why do the authors fix the lower boundary condition to -15000 (units?) (page 3244 lines 18-20)?
- The equation (18) is not needed since the readers must be familiar with the Newton-Raphson iteration scheme.

- How the authors refine the selection of physical parameter values for the soil horizons (paragraph in page 3246 lines 8-15)?
- The statements in page 3248 lines 20-24 are very confuse.
- The authors should explain with more detail the observed hysteresis effects in their data (page 3250 line 10).
- Why the bottom layer needs to be dry (page 3250 lines 17-19)?
- The last two sentences in the Conclusions section do not seem to be justified from the results of the authors (pages 3251 lines 21-5).

Misspellings:

- *pedotransfer* (page 3239 line 24)
- *this* (page 3240 line 1)
- *lose* (page 3250 line 28)

Trivial sentences that could be either removed or rewritten:

- 'The capacity of vegetation...' (page 3240 lines 2-4)
- 'Thus rainfall interception...' (page 3240 lines 4-7)
- 'In vegetation models...' (pages 3240-3241 lines 30-1)
- 'The parameter a and n...' (page 3244 lines 4-5)
- 'Therefore the ECMWF...' (page 3249 lines 19-21)
- 'The first layer of both models...' (page 3250 lines 7-8)

References

Berninger, H., Kornhuber, R., Sander, O. 2011. Fast and robust numerical solution of the richards equation in homogeneous soil. *SIAM J. Numer. Anal.* 49(6):2576-97.

Szymkiewicz, A., Helmig, R. 2011. Comparison of conductivity averaging methods for one-dimensional unsaturated flow in layered soils. *Adv. Water Resour.* 34:1012-25.