Thank you for your detailed review of our article. Please find below our responses to your remarks and suggestions.

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, Pi, the depth of canopy drained water, Pd, the depth of throughfall, Pt, the depth of stored water in the canopy, Ws, and the depth of daily evaporated water from the canopy, Ec. 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?

The comment of the expert can be comprehended. The details on the Braden's model were checked and corrected.

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 expert is completely right. The Rutter-model is a well-accepted model. However, this also applies to the Braden model employed by us. This model is used in the well-known AMBETI model (http://www.dwd.de/bvbw/generator/DWDWWW/Content/Oeffentlichkeit/KU/KU5/Braunschweig/ Aufgabenspektrum/Agrar__Modelle/Bestands__Bodenklima,templateId=raw,property=publicationFil e.pdf/Bestands_Bodenklima.pdf,http://www.schweizerbart.de/resources/downloads/paper_preview s/76428.pdf) and is used for the advice by the DWD. The differences between different interception models are not subject of our paper.

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?

We agree that we did not introduce the maximum potential evapotranspiration rate in a sufficient way. We will deepen that in the second version of the manuscript.

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).

We agree that the numerical solution of the Richards equation can be shortened. We will restructure this section following your suggestions.

The model approach we choose for the simulation of hydraulic conductivity is foreseen to be implemented to a more complex global model approach (Biosphere Energy transfer Hydrology - BETHY/DLR; Knorr, 1997). The model approach was formulated as compartment model and the stability of the model was checked and proved correspondingly. Therefore, the adoption of the Kirchhoff transformation can be a starting point for further improvement of our model.

The main aim of the authors was to demonstrate that a simple and remote sensing based model can considerably improve the water balance scheme as used in global vegetation models such as BETHY/DLR. We agree with the expert, that the model approach by e.g. Szymkiewicz and Helmig (2011) can provide better results, but since it consumes enormous computational time a model approach as you proposed is not feasible n this context.

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.

We do not understand why the widely used terms of the permanent wilting point and the field capacity are imprecise. However we agree that the mean plant available soil water content is not defined precise enough and in addition was of false use here anyways.

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

There were no additional methods adopted than described in the manuscript.

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

Thank you for this suggestion. We will compute the Nash-Sutcliffe index in addition to the RMSE and introduce the results in a paragraph. However we believe since the ECMWF product is also not measured, but a model result, the idea to visualize the differences with the RMSE is the more appropriate way. Since the ECMWF model only has one global soil, our aim was to look at local differences which are due to the additional information we got from our possibility to distinguish 128 soils. A dimensionless index such as your proposed Nash-Sutcliffe index would also represent general agreement, but without absolute values, which we wanted to present.

There are several imprecise statements requiring a revision:

• The definition of the infiltration process in page 2342 line 20.

We reworded this sentence.

• What does 'highly empirically derived' (page 242 line5) mean?

It means "empirical". We will reword this sentence.

• What is a 'soil dependent saturated condition' (page 3244 lines 13-14)?

It is the saturation level which is dependent on the characteristics of a soil.

• Why do the authors fix the soil top layer depth in 3 cm (page 3244 lines 13-15)?

This is done because we assume that evaporation from soil can only take place in a very limited range of the top-soil layer. In literature many definitions and top soil layer depth can be found. We choose 3cm since we believe that this depth is representative for most of the global soils.

• Why do the authors fix the lower boundary condition to -15000 (units?) (page 3244 lines 18-20)?

The values are in psi. This dry deep-soil layer comes from the idea to make it possible that water can leave the system. It can be discussed if a partially dry layer is more realistic than a totally dry one. However in nature soil loses water in the meaning of unavailable for roots (beneath root-zone). The other option would be to ignore this process and to assume a layer with stagnant moisture (as done for e.g. Gleysols). This option does not seem to be realistic.

• The equation (18) is not needed since the readers must be familiar with the Newton-Raphson iteration scheme.

We agree and will delete this equation.

• How the authors refine the selection of physical parameter values for the soil horizons (paragraph in page 3246 lines 8-15)?

We can agree that this paragraph is too short for the decision process for deriving the soil parameters for each soil.

• The statements in page 3248 lines 20-24 are very confuse.

We will reword this section.

• The authors should explain with more detail the observed hysteresis effects in their data (page 3250 line 10).

We will reword and deepen this section.

• Why the bottom layer needs to be dry (page 3250 lines 17-19)?

Please see you question about the lower boundary condition.

• 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).

We are not sure which sentence you meant in particular. However we reworded some parts of the last paragraphs.

Misspellings:

• pedotransfer (page 3239 line 24)

• this (page 3240 line 1)

• lose (page 3250 line 28)

Fixed!

Trivial sentences that could be either removed or rewritten:

• 'The capacity of vegetation. . . ' (page 3240 lines 2-4)

Deleted

• 'Thus rainfall interception. . . ' (page 3240 lines 4-7)

Deleted

• 'In vegetation models. . . ' (pages 3240-3241 lines 30-1)

Deleted

• 'The parameter a and n. . . ' (page 3244 lines 4-5)

Deleted

• 'Therefore the ECMWF. . . ' (page 3249 lines 19-21)

Deleted

• 'The first layer of both models. . . ' (page 3250 lines 7-8)

We will reword this sentence, since we believe that it is worth to mention that the first layer of each model is highly influenced by precipitation.