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

1 Introduction:
The whole introduction needs to be re-structured to better describe the state-of-the-art of 1-D soil water transport models, with special emphasis to the highlights of your model (e.g. interception, detailed description of the soil profile and use of remotely sensed LAI).

We will deepen our explanation of the state-of-the-art 1D soil water modelling approaches and restructure the entire introduction, following your suggestions. However we will not focus on the description of diverse previous developed models, since we present at least two review articles, which already did this part.

2 Theoretical background:
The inclusion of evapotranspiration in your model (also parameterized using remotely sensed LAI) would certainly increase the interest on it.

We agree that the inclusion of evapotranspiration could increase interest to the model scheme. However our main focus was to introduce a remote sensing driven infiltration model and a globally applicable parameterization of our model. Since we did not focus on the interaction of vegetation with soil we believe that evapotranspiration does not need to be considered for this study.

Some of the equations used for the calculation of the interception and infiltration are rather classical and could be omitted if adequate references are provided.

We agree on your point.

3 Input data:
Some spatial databases' spatial resolutions are given in lat-long ◦, but it could be interesting for the readers to show also their spatial resolutions in km2.

We will add for all given lat-long ° and km² information both values.

Why did you chose the FAO 74 soil classification instead of the FAO 90 or FAO 85?

We choose the FAO 74 classification, because we believe that a retranslation of the two newer FAO classifications to the older one is more consistent than vice versa. This was needed to be performed, because the HWSD has no area-wide information for all three classifications but is subdivided area wise to the '74s, '85s or '90s classification. Since one of our main aims in this study was to derivate global applicable and consistent van Genuchten parameters for the FAO soils we choose the '74s classification scheme.

Last sentence of page 3246 (The LAI…): This affirmation is not strictly correct, the LAI does not necessarily reflect changes in phenology and neither does phenology control interception (at least in all vegetation types).

Indeed the LAI does not reflect all aspects of phenology. However it is widely accepted as indicator for vegetation development. We will rephrase this sentence to stress that LAI on the one hand gives information about vegetation activity and on the other hand vegetation activity (i.e. development of leaves) controls interception. In addition LAI is a physical value, which can be measured by using remote sensing. The combination of different remote sensing data in our model is a major aspect of our investigations.

Please give more information on the CYCLOPES product of the POSTEL database.

We will deepen this paragraph, to describe the CYCLOPES product in further detail.

4 Results and discussion:
The comparison of plant available water content between your model and the ECMWF's is not clear. First, the plant available water content is not defined, and in any case it should be restricted to the root-zone, and not to the whole soil profile depth. The differences in soil depths in both models are responsible for the results, so it is not possible to judge whether the description of processes in your model is adequate.

We will add a paragraph, to define in detail the plant available water content, which is based on the definition of the ECMWF model. Only by using this definition, a comparison is possible.

Page 3249, lines 8-10: This affirmation does not necessarily imply that your model simulations are correct.

Since the ECMWF only represents one single and consistent global available soil, we believe that our model can indeed represent local characteristics better than the ECMWF model.

In Figs 5-7 the soil moisture outputs of both models are represented for three differing soil types. The ECMWF modeled soil moisture is calculated taking into account evapotranspiration or not? In case it is, both models results cannot be compared. In any case, the comparison is very difficult due to the differing soil layer structure.

Indeed the ECMWF soil takes into account evapotranspiration. However we believe that general characteristics, as e.g. the reaction to precipitation can be compared. You suggested doing a further comparison with e.g. ASCAT, AMSR-E or SMOS data would add more value to a comparison. We will perform such a comparison and discuss the results in a further section.

The assumption of a dry deep soil layer beneath leads to unrealistic soil moisture values in the deeper soil layers. Why did you consider this dry deep layer?

The 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, too.

Table 1: Theta_s stands for saturation soil moisture or field capacity?

Theta_s is the saturation soil moisture. We will fix that.

Figs 5-7: Both model simulations are difficult to compare in these figures. I suggest to add precipitation in vertical bars.

Thank you for this idea. We will add the precipitation rates.