Dear Editor,

We revised manuscript HESS-2016-168 once more. In doing so we also re-read the review reports from the first round to see if there was overlapping opinions among the five reviewers. In our reply we will focus on the second round reviews only, as we already responded to first three reviewers.

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One reviewer submitted his report entirely through the web text box, without supplements. We will address these comments first. We will adhere to the numbering provided by the reviewer.

1. This reviewer clearly does not like our paper, but also seems to have missed the point. We are unsure why there is an implicit suggestion that we should have tried various parameter estimation techniques when the one we used is well established. The other two points (no new data, and clarity of the best method) have been taken care of by including 21 more soils in the analysis and including our findings fror these soils in the discussion.

2 and 3. We reorganized section 2 by placing it in the supplement and only retaining those parameterizations that were selected for further testing.

4. Corrected as stated and rephrased as suggested.

5. Done.

6. The order in which the four test soils were introduced was partly caused by the fact that the clay samples were from another region in than the rest. We wrote different version of the results section and found that the line of thought was easiest to follow of we started with the intermediate texture and contrasted the coarser and finer textures with the intermediate. The tables are in the same order since they will probably be consulted each time a texture is discussed. The figures are there to compare different textures and were therefore arranged according to texture. There is something to be said for both viewpoints. The labeling is such that ambiguity is avoided, so we kept thigs as they are.

7. Vapor flow played a minor role. We added a statement to that effect at the start of the discussion.

8. Free drainage has always be defined as drainage under unit gradient at the matric potential of the lowest node. A seepage phase requires the matric potential to reach zero at the lowest node before water can leave the soil. Free drainage gives good results when the groundwater tables is so much lower than the lower boundary that it does not affect the matric potential at the lower boundary. We explained what it implies and how it affected early drainage in the Materials and Methods section.

9. One of the authors used pF in an earlier paper in HESS and found that non soil physicists were not always familiar with the term. Soil physicists are used to the pF scale. We resolved this by writing out in full what pF defines in order to conform to established soil physics practice without leaving the rest of the readership in the dark.

10. Done.

11. Before submitting we tried plots with the same axis but found that their readability deteriorated too much for the curves with low values, and therefore we selected the current graphs.

12. This is clear from section 3.2.2.

13. These parameter values for each conductivity model are given in the text. They are not fitted and should therefore not be placed in a table next to a table with fitted parameters to avoid confusion.

14. We rephrased this to clarify that this applies to the relative differences between the fluxes.

The other reviewer presented a report as a supplement. In his general comment he suggested to make the data points in two figures better visible. In one Fig. 4 (previously Fig. 3) there were so many data points that the overlapping could not be avoided while keeping the data points large enough to remain visible. We redid Fig. 3 (previously Fig. 2) and improved the readability.

The specific remarks by the reviewer are bulleted. We go by those in the order in which they are presented.

1. We rewrote section 2 and created a supplement. This should improve clarity.

2. We added 21 more soils to the analysis, covering a wide range of textures.

3. We considerably expanded the literature review, adding several references kindly provided this reviewer and a reviewer from the first round, and adding some more that we found along the way. We also added a discussion about multimodality in the Introduction and discuss the potential of the parameterizations that we tested for being expanded to multimodal forms.

4. The reviewer discusses the water potential profile during evaporation, while we addressed this under hydrostatic equilibrium. Lui and Dane (1995) found a profound effect. In particular they demonstrated that it could make brooks-Corey type soil with a sharp air-entry value and a power-law retention curve look like a van Genuchten-type soil with a smooth transition without clearly defined air-entry value and a sigmoid shape.

5. We added a discussion about the values of the variance of the data points to the Materials and Methods section. We fully agree with the reviewer that these values depend on the measurement method, and therefore vary with the matric potential.

6. Regarding the scaling of the error standard deviations (ESD), we found this to be necessary because the algorithm was struggling navigating the objective function when ESD values were small compared to the noise in the data. The four test soils had data points from several replicates in one data cloud, and the ESDs did not reflect that noise. More generally, the SCE algorithm works best when the ESDs are no more than an order of magnitude smaller than the values of the variable. We implemented the scaling to ensure that this is the case.

We disagree with the reviewer that this compromises the maximum likelihood attribute of the estimates. The ESD values are used to determine the weighting factor of each data point. The scaling preserved the relative magnitude of each ESD value with respect to all other ESDs, thereby ensuring that the weighting factors are not affected.

7. The reviewer is correct: the initial and boundary conditions are not consistent. The unit gradient imposed at the lower boundary leads to rapid drainage at the start of the simulation because the bottom of the profile is relatively wet. The deep soil dries quickly and consistency is achieved. The spin-up period takes care of this. In the second part of the comment the reviewer claims that conditions can be made more realistic, but we disagree. The free drainage lower boundary condition creates unit gradient flow at the bottom, which is routinely in soils above a groundwater level that is so deep that it does not affect the upper meters of the soil. Implementing this boundary condition eliminates the need to simulate the entire soil profile until the groundwater without much loss of accuracy in the top soil. Particularly in dry climates, hydrostatic equilibrium with the groundwater table is never achieved, the soil is always drier than that.

8. Hydrus reports mass balance errors and the number of iterations required. The user can observe computation times. Only for the Alexander-Skaggs conductivity model did these indicators signal that something was wrong, and we reported that dutifully in the paper.

We thank the reviewer for the list of references. We read these papers and included those that we considered to fit well into the paper.