The authors would like to thank the Editor/referee for the valuable comments on our submitted manuscript. The revised manuscript (if invited to submit one) will incorporate the comments raised.

We, however, wish to respond immediately to the main comments made by the referee while, at the same time, working on the final manuscript which incorporates all the comments raised during the discussion period. The following are direct comments raised by the referee/editor:

1. Neither the model structure, nor the parameterization nor the way the authors include the effects of surface preparation is reported in a way that is it is reproducible/transparent.

Response

It is noted that the experimental setup has not been presented in detail, although it was presented in previous publications by the authors in related publications. This will be included in the revised paper for the benefit of readers who may not familiar with previous publications.

How did you simulate soil moisture at the 4 different tubes with a lumped zero dimensional model? With multiple set ups?

Response
The four different tubes have not been used for simulation but, rather, have been used to check the simulated values. The four tubes present soil moisture values at the different locations along a downslope transect (See Fig 2). Moisture values at 30cm depth are used in the simulations as they correspond to the shallow rooting depth in the study area. These moisture values are then compared against the simulated values based on the rainfall and runoff inputs.

2. How to account for surface redistribution of water downslope (when looking at your figure 2), did you neglect that.

Response
Indeed the model neglects lateral water flow as described in Section 2. It is therefore a 1-D model.

The model has not incorporated variations of soil characteristics with slope nor the influence of the different treatments that were tested. This is mainly because detailed instrumentation for soil moisture, inflow and outflow could only be concentrated on one treatment (in this case, Treatment 1). Also, detailed investigations on this aspect were beyond the scope of this research/paper. In addition, the soil characteristics are not expected to change significantly within the cultivated strip of a width of only 10m.

The model compared the observed soil moisture against simulated values under two

scenarios when (a) no diversions were active, and (b) when water was diverted. The difference in moisture values under these scenarios is the benefit of the diversions.

I miss essential information about crucial model parameters governing evaporation, transpiration, interception, water retention in soil. Such information is indispensable for a scientific paper.

<u>Response</u>
Potential evaporation is part of the model inputs. Pan evaporation data is obtained from direct observations and further details will be provided in the revised paper.

Transpiration is calculated based on the FAO dual crop coefficient procedures (Allen et al., 2005) for calculating the actual soil evaporation and transpiration as illustrated in Fig 3.

Interception has been taken as a threshold value below which infiltration and runoff cannot occur (De Groen and Savenije, 2006). In our response to Referee 1 we explained the assumptions made for interception and provided a sensitivity analysis of the assumed values. This will be incorporated in the revised paper including an additional figure.

Water retention in the soil is calculated as the balance of water remaining after the infiltrated water has been partitioned to transpiration and deep percolation.

The authors would like to acknowledge that the paper, in its present form, may not be clearly explaining the process of how the input parameters are incorporated in the model. This will be significantly improved in the revised paper.

- Any kind of conceptual model has to be validated. Especially when dealing with input data that have a strong seasonality as in the present case. I cannot see how this was done. Without this the value of the model study remains unclear to me.

The performance of the model has been tested against soil moisture observations during wet and dry seasons over a period of two years, which is a reasonable record certainly for the local conditions (semi-arid and often very remote). The efficiency of the model was assessed by the Root Mean Square Error. Unfortunately, more data e.g. percolation rates, could not be measured in this project and, thus, we do not have another way of validating the modelling approach.

It should be noted, however, that the HYDRUS2D model (which the reviewer has referred to earlier) has been applied to, partly, confirm the model that is reported in this paper. The authors are preparing a separate paper on this as they have felt that separate papers would articulate the different approaches better without overloading the single papers.

Technical points
The comments raised have been noted and will be attended to in the revised manuscript.