This is definitely an important study. I agree that Land Surface Model could be improved in the definition of soil depth. The impact of rooting depth in determining the active soil moisture and the amount of water that can transpire ultimately influence the occurrence and amplification of heat waves, an increasingly pressing issue in an era of climatic changes. The model development proposed in this study could improve climate services that are primary forms of climate adaptation in many sectors.

The manuscript is generally well-written and contains high-quality research. However, I was not familiar with the method used in this manuscript. This is reflected in some questions and comments that the author could consider.

The Memory Method is definitely very interesting. I believe it is reasonable enough to assume that local vegetation adapts the rooting depth according to the drought frequency. However, it’s difficult to me to understand the way it is implemented in the model, i.e. varying the total soil depth in the model grid cells. I think it would have been more reasonable to change the rooting distribution Z from the model formulation instead of varying the bottom soil thickness. Afterall, in reality it is the vegetation adapting the roots, not the soil changing thickness. Why this approach was not followed?

The modelled approach assumes that the maximum holding capacity should be equal to Sr. If I understand well it should rather a minimum value corresponding to dry years right? For tall vegetation, the root depth is defined through the memory method as a function of the 40 years return period drought and 2 years for low vegetation. With the implementation considered in this study it seems like the soil cannot hold more moisture than the one available in dry years. Maybe this is the reason of the apparent systematic underestimation of Sr by the model.
Also, the definition of Sr for the observations is a linear superposition of different values that are obtained for high and low vegetation, suggesting the these values are different. Modelled Sr is defined as one value for all vegetation types. So not only the implementation is the model seem to assume that the soil cannot hold more water than in dry years, but also that does not depend on the vegetation type. Again, maybe a different approach is needed.

Finally, I think the modelled Sr could be computed exactly in the same manner as it is done for observations. I understand the author prefer to use a more physical definition, but probably computing it in the same way as in the observation would be a fairer comparison that could be used to better calibrate the modified model.

Minor comments

Equation 2 is formally incorrect. The time-dependency S(t) does not match the right-hand side where t is the integrating variable that should disappear after the integration is performed. The actual variable that survives should be related to t0 and t1, which are not defined either than in Appendix A. I think a subscript for the hydrological year should be preferred in that case. About the subscripts, here 'd' is used. 'r' in used elsewhere, why? I think 'd' stands for deficit and 'r' stand for roots. If so, is the right hand side of equation 7 there should be 'd', not 'r'.

I agree that considering constant ratio of actual vs. potential evapotranspiration is a crude approximation, especially in water limited regions. I also agree that the other factors mentioned by the authors (groundwater, irrigation) are important as well. These are difficult to improve in the model in short times. However, the author of implemented an iterative step in their approach to reduce strongly the uncertainty relate to the interannual variability of that ratio (Appendix A). Couldn't they use somehow the observed evaporation to further improve the estimation? I think this would improve the estimated Sr, especially regarding the intraseasonal variability, and eventually improve the modified model calibration.

To the uncertainties, I would add the model drainage rate that, in the current framework, it could as important as the rooting depth. The results obtained by the authors could be due to excessive retention (slow drainage) rather than too deep root sone. This is also supported by the fact that other models are rather augmenting the soil depth adding a groundwater layer instead of reducing it (e.g. CLM).

Equation 22: where is the equal sign?
Figure 6: Shouldn't $Q + E$ equal $P$ in the long period? If this should be true, the average anomalies of the modified model on the top panel should be equal the average anomalies of the bottom panels as precipitation does not change, but this is not true. $E$ anomalies are much smaller, Why? Am I missing something?