Manuscript: Climate controlled root zone parameters show potential to improve water flux simulations by land surface models

Major remarks

The authors analysed the effect of using a climate dependent root zone storage capacity S_r instead of a vegetation type dependent S_r on simulated runoff and evaporation fluxes in Australia. They estimated this 'climate controlled' S_r with the "memory method" (MM) in which S_r is derived from the vegetation's memory of past root zone water storage deficits and introduced this into the HTESSEL land surface scheme. By using forcing from the GSWP-3 dataset, the new S_r led to improved seasonal climatologies (1975–2010) and inter-annual anomalies of river discharge over 15 selected small catchments while only a negligible impact on evaporation fluxes and long-term mean model biases was found. As the climate control on root development is not regarded by most of the existing land surface models (LSMs), this study is a valuable contribution on climate – hydrology interactions within the topic of Earth System Modelling.

My only major remark is that I miss a more thorough analysis on why the HTESSEL evaporation is rather insensitive to the changes in S_r. Opposite to the present study, the evaporation of other LSMs reacts usually more sensitive to water holding capacity changes. However, many climate models tend/tended to have LSMs with more shallow soils, and, hence lower S_r, so that in related studies, S_r was often increased. In the present study, CTR seem to have a rather large S_r, and there is a general reduction of S_r using the MM method. Does this has something to do with this insensitivity?

In addition, the authors state that E is rather insensitive to changes in SR because E depends on the relative soil moisture. It is well known that E is sensitive to soil moisture when soil moisture in the transitional regime between the wilting point soil moisture (dry regime if moisture is below) and a critical soil moisture above which evapotranspiration is occurring at its potential rate E_{pot} (wet regime). In order to investigate this further I suggest considering in which catchments, the soil moisture is in the transitional regime, and whether the relative soil moisture changes due to the introduction of the new S_r. If, for example, a catchment is in the wet or dry regime for most of months, then E will not react to changes in S_r.

The paper is generally written well so that I suggest accepting the paper for publication after minor revisions have been conducted.

Minor remark

In the following suggestions for editorial corrections are marked in *Italic*.

p. 1 - line 17
… long-term annual mean *river discharge* are …

p. 7 - line 146
It is written:
… long-term mean transpiration derived from the water balance ($E_t = P_e - Q$) …

The evapotranspiration at the land surface (without the canopy, such as in your balance equation) comprises also evaporation of snow and evaporation over bare soil. While the first may not play a role over Australia, the latter certainly does as I do not expect that all catchments are completely covered by vegetation so that bare soil fraction equals Zero. Please elaborate on this issue in more detail.

p. 10 - line 198
It is written:
Total discharge (Q) is the sum of Q_s and Q_{sb} (Eq. 13).

Do you consider lateral flow within the catchment and the respective delay due to lateral transport? Or are the catchments small enough so that this delay is negligible. Please comment!

p. 10 - line 217
… would *cause* the model …

p. 11 - line 221
It is written:
It should be noted that the layer depths for thermal diffusion calculations are not modified in the MD model.

Do you assume bedrock (i.e. zero moisture) below the root zone for the thermal calculation if z4 is reduced for water? What do you do? How does this affect your simulation?

p. 13 - line 281
… affected as *shown* in …

p. 17 - line 324-325
… related to *the applied methodology* which will be further discussed in Section *4.2.*

As Section 4.2 is about 'Methodological uncertainty', I assume you point to Sect. 4.2, and not 4.3 as written in the manuscript?!

p. 19 - line 379-380
…is *only a* function …

p. 19 - line 385
It is written:
$S_{r,CTR}$ was found to be considerably larger than the climate controlled estimate S_r …

How do these values compare to those of other LSMs? This comment is also related to my major remark.

p. 19 - line 403-404
It is written:
On the other hand, surface and subsurface runoff depend on the cumulative moisture content of the soil at any given time.

What do you mean with “cumulative” content? Looking at Figure 4, I assume that both depend on the moisture content above a certain threshold. Please clarify!