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Interactive comment on "Worldwide soil moisture changes driven by future hydro-climatic change scenarios" *by* L. Verrot and G. Destouni

Anonymous Referee #2

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Worldwide soil moisture changes driven by future hydro-climatic change scenarios HESS, Verrot and Destouni 2016

This paper analyzes the change in soil moisture features (dry/wet event frequency, change in water storage) for different projected climate regimes. I am not very convinced about the methodology, or about the relevance of this paper. Major revisions are needed before the paper could be considered for publication.

1) What is new in this paper compared to earlier studies that investigated the effect of climate projections on soil moisture? Please explicitly state the new findings or advancements. The introduction refers excessively to Destouni or Verrot and findings by major research institutes specialized in climate projections are barely discussed.

2) The paper refers to Verrot and Destouni (2016), which is in review and not available.

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Is fig. 2 copied from that paper? Why even spend time on GRACE in this paper in HESS, if it is already included in another paper? If GRACE is essential in this paper to justify the validity of the new approach, then please explain exactly how the climatology of GRACE is compared to the model. How are the scaling parameters used in the GRACE data processing, which spatial resolution is used? These GRACE scaling parameters are model-based, so the evaluation would have to be done very carefully to make any sense.

p.5, L.113 refers to Fig 2 without discussing it in the text. Why are only a handful of catchments shown, if both CMIP5 and GRACE have global data? How is GRACE soil moisture extracted from the total water storage (=snow+soil moisture+biomass+groundwater) changes? How is the comparison in snow-covered catchments? Please discuss the figures or leave them out if already included in a previous paper.

3) Honestly, I don't understand why this new modeling approach is introduced and I see nothing but problems with it. Please clearly justify the need for the new modeling approach in this study:

- depth-average soil moisture: up to which depth? The depth to bedrock varies in space. How can we make consistent conclusions about the soil moisture features if the depth is different everywhere? A shallow layer will respond very differently to a deep layer (different memory).

- Eq. 1 essentially says that soil moisture = scaled runoff. All other terms are constant parameters in time. I do not think that any of the subsequent analyses would differ if the constant parameters were simply omitted, so why even worry about them? This Eq also assumes that evapotranspiration is constant (not affecting soil moisture) and consequently, this assumes that the relative partitioning of runoff and ET varies in time. Why introduce all these assumptions, if we have land surface models to calculate soil moisture? The CMIP5 model output must have soil moisture estimates that are

ready for use and they must be superior because they come from a coupled simulation (with feedback between land and atmosphere), whereas the presented simulations presumably include no feedback.

- the method assumes an insignificant change in long-term subsurface storage. (L.130). This is an invalid assumption in many regions where the groundwater is depleted (cfr. studies using GRACE data over California, East Africa, India).

- around L.174: an upwards flux may perhaps be a replenishing of the surface layer by the groundwater or some other deeper layer soil moisture. There is nothing unphysical about it. Please check the model structure and explain this phenomenon, rather than treating the data as if they came out of a black box. It may affect the analysis results.

- around L. 191: why did the authors derive soil parameters based on the HWSD texture information? Why not simply use the parameters that were used in the model simulations to be consistent?

4) Results: please verify all figures and explain the findings:

- L. 262: Nam9 is not shown in fig 3

- explain the reason why catchments may react in “opposite” ways under RCP 2.6 and 8.5.

- how are all these results affected by the lack of feedback from the land surface to the atmosphere?

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