Hydrol. Earth Syst. Sci. Discuss., 12, C6839–C6840, 2016
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Hydrology and Earth System Sciences
Discussions



# *Interactive comment on* "Comparison of measured brightness temperatures from SMOS with modelled ones from ORCHIDEE and H-TESSEL over the Iberian Peninsula" *by* A. Barella-Ortiz et al.

**Anonymous Referee #2**

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The paper tries to identify the sources of SMOS TB errors by comparing the satellite observation with the CMEM forward simulation (driven by two LSMs simulations of soil moisture and soil temperature). Such studies are highly welcomed by the community, as the errors in TB can propagate to the final retrieval of soil moisture products.

The analysis approach deployed by the authors is also conceptually well chosen: temporal, spatial correlation and corresponding EOF analysis. However, in the current shape of the manuscript, the authors failed to present the whole study in an consistent and convincing way. And yet, the English is a barrier for me to get insights into their



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studies. Therefore, i dont recommend a publication of their works for HESS. Nevertheless, i do provide some comments, which hopefully can be referred by the authors for their further updates on this work.

Major concerns: 1. The description of the methods and the results is fragmented. For example, a mixed introduction of ORCHIDEE and HTESEEL confused me on what configuration exactly used for each LSM? And the discussion of temporal and spatial correlation are not clearly separated and most of time mixed, which hinders the understanding of their studies.

2. The discussion on the precipitation and LST errors is wired to me. First of all, the E-OBS precipitation and LAND-SAF LST were not used in CMEM to derive TBs. However, they were used as the reference to derive the errors of precipitation and LST from ECMWF reanalysis data (e.g. forcing data for LSMs). And, then, the authors try to link such analyzed errors to the TB errors? It seems to me a major flaw in the concept of their studies on this topic.

And i am not surprised that they cannot find the controlling factors for the TB errors over IP.

I stopped further commenting this manuscript due to the perception of a wrongly conducted studies, as indicated from the above comments. Nevertheless, I do also provide the minor comments in the attached PDF.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C6839/2016/hessd-12-C6839-2016-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 13019, 2015.