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**Hydrology and Earth System Sciences Discussions**

# *Interactive comment on* "Evaluation of PERSIANN database in the framework of SMOS Calibration/Validation activities over Valencia Anchor Station" *by* S. Juglea et al.

**Anonymous Referee #2**

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Main comment

The paper is about using the PERSIANN rainfall product to simulate soil moisture over the Valencia Anchor Station in Spain. The context is the SMOS calibration/validation process. The use of satellite precipitation products to force hydrological models is quite a recent field of research which has to be developed as more and more satellite products will be available in the coming years and have to be evaluated and validated according to the requirements of hydrological modelling studies. In that sense the paper is in an original and interesting context. However the paper has too many weaknesses. In my opinion it has to be significantly improved before thinking about a publication in



HESS.

I'm particularly concerned by the three following aspects:

1. The objectives of the paper are not clear at all 2. Conclusions are not always convincing and not supported by the discussion of the results which seems to have been rushed. 3. Some explanations and illustrations are missing to understand the methodology and to analyse the results.

These three points are detailed hereafter:

Point 1.

The context of the paper is the calibration/validation (cal/val) activities of SMOS but this does not make a scientific objective on its own. What are the scientific questions?

–>If the objective of the paper is to provide a reference soil moisture to be compared to the SMOS estimates for the cal/val process: - What are the scientific issues that the authors will have to face during the CAL/VAL process? - Do these issues justify the use of a hydrological model? Why not using the data from the soil moisture probes (mentioned p.1146 l.11)? - p.1148 l18-19 the authors "obtained good estimation of the distribution of soil moisture over the entire VAS area" which means for me that the rain gage network provides a sufficient information to be used routinely for the cal/val process. So why using satellite precipitation data?

–>If the objective is to evaluate the performance of a hydrological model for different rainfall inputs: - How do the authors define the performance? Do they have any soil moisture reference that they could use (see my comment on soil moisture probes)? It seems that the authors assume that soil moisture simulated by using rain gages data is the reference. How can we trust this assumption? - The results should be discussed according to the hydrological processes and the way they are represented in the model. However are provided neither a description of the model, nor a fine description of the main processes in the studied region that could help the reader in understanding the

sensitivity of soil moisture to rainfall variability.

–>If the question is to “improve the soil moisture modelling in situations where there are few or no rain gauge data to allow reliable estimates of spatial rainfall” (as written p.1146 l27-27) - Some aspects are missing to comprehensively address this issue. For which network configuration is it preferable to use satellite data instead of rain gages data? Is a there a critical density of rain gages under which satellite data must be preferred? More generally are there some critical scales in space but also in time to be taken into account to properly simulate the soil moisture? The last sentence of the abstract suggests that these scale issues are addressed in the paper: “Having an accurate estimation of the amount and temporal/spatial distribution of precipitation is a critical issue so as to have a faithful representation of soil moisture distribution.” However this question is actually not quantitatively treated. Some crucial information is missing: what is the temporal resolution of the PERSIANN product used? What is the time resolution of rain gage data? What is the time step required by SURFEX-ISBA?

Point 2.

I’m not always convinced by the results and conclusions of the paper.

–>Rainfall comparison

The robustness of the presented results is criticisable as the analysis is carried out for only one year and with only one satellite pixel and two rain gages. It would be useful to see at least the behaviour of year 2007. Figure 1 and 2 are not sufficient for a clear comparison. I would expect scatter plots and/or distributions of the rainfall intensities or a contingency table with non-rainy and rainy days. Moreover the colorbar chosen for the plots is not suitable as non-rainy and rainy periods are not distinguishable.

–>Soil moisture simulation

Over the Valencia area the main problem of PERSIANN rainfall estimations seems to be the significant overestimation of rainfall from September to October. This overesti-



mation directly impacts the simulated soil moisture which is significantly overestimated from September to October. The point here is that September-October period is the rainy season which is obviously critical in a hydrological context. Mediterranean regions experience extreme events (rainy events and flash floods) mostly during this period. In my point of view, the estimation of rain and soil moisture for these two months should be a prior target. This aspect is a bit underestimated by the authors. The authors do mention the discordances during this period of time, but they mainly base their conclusions on the good accordance between satellite and rain gages from May to August, which is actually the dry season. I find thus quite optimistic to conclude about the “potential” (p.1155 l. 3), of the PERSIANN product for soil moisture simulation both for point and areal simulation. I don’t agree with the conclusions of Section 4.2 (p.1152 l. 17-18). Where is the demonstration of the interest of using PERSIANN together with rain gauges?

A deeper and more objective discussion is expected about the significance of the results obtained in Section 4.1 and 4.2.

Point 3.

–>What is the contribution of the study according to the existing literature? A review is missing. Only 4 papers are reported as similar studies. There are many studies dealing with the issues of using satellite rainfall for hydrological modelling, not only in Southern Africa.

–>A minimum presentation of the Valencia Anchor Station would be expected to clearly understand the study. Two important illustrations are missing: - a description of the climate and particularly the seasonal variability of rainfall. - a map with the rain gage network and the satellite grid is missing. Table 1 is not reader-friendly.

–>The choice of the satellite rainfall product is not clear. Why PERSIANN? What about using other satellite products? It is not clear which one of the satellite rainfall products of the PERSIANN database is used, PERSIANN CCS is mentioned p.1145 l. 6. Is it

the product used? What are its specific characteristics?

–>Some descriptions of the hydrological models are missing. The paper is too dependent from Juglea et al., 2010. What are the differences between SURFEX and ISBA? What is the time step of the simulations?

–>As a direct consequence of the unclear paper objectives, I really don't understand the objective of Section 4.4. Why making a comparison with AMSR-E? This section is not discussed and does not even support the conclusions of the paper.

Other comments

p.1145 l. 10. It's not obvious to me. Do you have a reference? What about the effect of soil characteristics?

p. 1147 l. 1-2. Is there a reason to use Inverse Distance Weighted compared to other interpolation techniques (nearest neighbour, kriging,. . .)?

p. 1147 l. 9. What is meant by "optimum"?

p 1147 l. 18. What is "a good estimation of the distribution of soil moisture"? Please give some illustration of the simulation performances?

p. 1149 l. 18. the nearest PERSIANN pixel?

p. 1151 l. 5. Please remove "Anyway". I don't agree with this explanation. On contrary, spatial aggregation leads to an underestimation of rainfall intensities.

p. 1151 l. 27-28. Please rephrase.

p. 1154 l. 23-24. "Anyway" sounds weird for a scientific discussion. Please discuss objectively your results.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1143, 2010.