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Discussions

Interactive comment on "Evaluation of a multi-satellite soil moisture product and the Community Land Model 4.5 simulation in China" by B. Jia et al.

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Received and published: 23 June 2015

The manuscript evaluates a satellite-based ESA CCI soil moisture product and a soil moisture product based on CLM 4.5 simulations using ground observations from approximately 300 meteorological stations in China. Classical metrics are used to evaluate the two products, e.g. Pearson's correlation, bias and the RMSD. In-situ observations are a valuable resource for assessing the performance of satellite and model products but unfortunately the manuscript fails to dig deeper into the causes of observed mismatches. Some of the metrics presented do not make much sense with respect to the evaluation of the ESA CCI SM products improve our dataset under-

standing at all, e.g. bias and the RMSSD. Especially given the fact that various similar CLM and ESA CCI SM validation papers over China have been recently published, more effort needs to be invested to make this manuscript unique.

My concerns are detailed below.

MAJOR

1) Several studies comparing soil moisture from land surface models and ESA CCI SM against in-situ observations in China have been recently published (e.g. Lai et al. (2015); Dorigo et al., 2015). To distinguish your work from these studies, you need to put more effort in explaining the differences will real evidence, such as ancillary datasets. In your study, the causes for mismatches observed remain rather speculative.

2) As described at http://www.esa-soilmoisture-cci.org/node/136 and in several publicaions (e.g. Liu et al., 2011, 2012; Dorigo et al, 2015) the methodology to generate ESA CCI SM involves a scaling against GLDAS-Noah to combine scatterometer and radiometer soil moisture products to produce a merged dataset in volumetric units. As a consequence, the mean and dynamic range of ESA CCI SM time series represent those of the GLDAS-Noah surface soil moisture product. Thus, metrics like bias and RMSD mainly reflect differences between GLDAS-Noah and in-situ observations and do not provide much interesting information about the satellite product itself. Therefore, I recommend to exclude them from the analysis and remove all related graphics.

3) It is stated (p5152.line10, p.5163.l15-17) that the high biases for CLM4.5 are caused by inaccurate descriptions of soil characteristics but nowhere in the text evidence is provided. This needs to be elaborated by additional analyses.

4) No details are given on how you match the in-situ observations and the gridded products. The ESA CCI SM datasets represents the upper $\sim$2 centimetres of the soil, just like the upper layer of the CLM product while, according to your description, the in-situ observations closest to the surface are taken at 10 cm. Thus there is a clear

discrepancy in depth which, in particular close to the surface may lead to significantly different dynamics. Therefore, you 1) need to specify which layers and observation depths were compared and 2) what impact the mismatch between depths may have on your statistics. If there is more than one in-situ station within a grid cell, do you regard the results as individual results? For more details on potential matching strategies see Dorigo et al., 2015.

5) The soil moisture observations of CMA were made by destructive sampling, which means that the sample each time is taken at a different location. Many studies (e.g. see work of Luca Brocca) have shown the enormous variability soil moisture can have even at local scales, in particular in absolute terms. The consequences of this sampling on your results needs to be thoroughly discussed.

6) It is not mentioned explicitly, but I understand that the precipitation measurements in ITP dataset used for forcing CLM include are taken at the same locations as the CMA sol moisture measurements. Therefore, it doesn't surprise me that the correlations obtained for the CLM simulations are higher than those obtained for soil moisture. To understand the real quality of your CLM simulations, the soil moisture fields should be validated at locations where no in situ precipitation measurements are made. You could do this by leaving the in-situ P measurements out, either entirely or by cross-validation (i.e. you use all in situ P measurements except the one made at the location of your soil moisture measurement).

7) The selection of comparison metrics needs to be reconsidered: as stated above, for ESA CCI SM metrics of "absolute" deviation (i.e. bias, RMSD, SDV, E) basically provide an indication of how well GLDAS-Noah soil moisture fits the in situ observations. The metric E corrects for differences in mean soil moisture (additive bias), but not for differences in the dynamic range (multiplicative bias). As measure measure on its own, it is not very valuable; it mainly makes sense as part of the taylor diagram. The ubRMSD as computed from the anomalies does not correct for differences in variances between the in-situ data and the other data. It would make sense to compute the Spearman

correlation coefficient in addition to the pearson R as a non-linear relationship between the in-situ data and the coarse scale products is expected because of differences in spatial support and depth (see Gruber et al. 2013, 2015 for valuable discussions on this issue.

8) Performances over 8 sub-regions: The regions are boxes which only roughly follow natural climate or land cover zones. Why did you define the regions in this way? And why are some regions not included in the analysis (e.g. the Tibetan plateau)? More importantly, the average performance statistics per region should be computed in a different way: first you should compute the statistics for the stations individually (you already do this), and then you average the numbers. It doesn't make sense to average in-situ observations that may have completely different soil moisture dynamics. You even mention this on p5163.l24-25

9) There is some ambiguity regarding the terminology used: the metric E (centred RMSD) presented in Eq (5) is the same as the ubRMSD used in Albergel et al., 2013b, which again is the same as the RMSD of the anomalies (p5164.l26). So why do you present the same metric twice? In addition, the "real" ubRMSD should correct both for differences in the mean (additive bias) and the variance (multiplicative bias). Effort needs to be out in harmonising the metrics and terminology with existing literature.

10) p5166.l18: you speculate that the introduction of the ASCAT soil moisture product in ESA CCI SM may have caused the decrease in skill for the period 2007-2011. But how can you then explain that the skill dramatically improves again for the latest blending periods, which just as well integrate ASCAT observations? Besides, Dorigo et al. (2015) showed that it was not the quality of the ASCAT product as such, but the way in which it was integrated into the blended product what explained the decrease in quality.

MINOR

general: The official name of the satellite-based product is ESA Climate Change Initiative soil moisture, or in brief ESA CCI SM. Please replace all occurrences of ECV-SM

with the official name.

Line 4: you cannot speak about THE land surface model simulation from CLM 4.5 as the model can be forced in many different ways. Your output is only one of many possible outputs.

p5155.l12: Later it becomes clear that you'll only use 301 out of the 778 available stations (p5162.l4), so the number 778 needs to be replaced

What version of the ESA CCI SM product was used? I assume v02.0 or v02.1?

p5157.l19: were the flags also applied?

Section 2.4 Evaluation stretegy: details need to be given on how the data were assembled to monthly values: averages? Did you take into account the flags? Did you take into account differences in the number of observations per month? For each month, were the number of daily observations used to build the (supposed) mean values equal for both data sets? A valuable discussion on the importance of correct flagging and comparable temporal and spatial collocation of datasets was provided in Wagner et al., 2012, ISPRS

p5159.l16: The relationship between E does not seem to be correct: Check Taylor (2001) to verify this.

p5164.l16-18: Also the ESA CCI SM is sensitive to precipitation (see e.g. Brocca et al., 2014), so this is not a valid explanation. the most plausible explanation would be the use of forcing data that was measured at the meteorological stations themselves.

p5164.l20-24: It is not clear what you mean with this statement: why would vegetation attenuation lead to stronger anomalies?

p5166.l17: the period of the 6th blending period should be Jan 2007-Sept 2011 (not 2007)

REFERENCES:

Brocca, L., Ciabatta, L., Massari, C., Moramarco, T., Hahn, S., Hasenauer, S., Kidd, R., Dorigo, W., Wagner, W., Levizzani, V. (2014). Soil as a natural rain gauge: Estimating global rainfall from satellite soil moisture data. Journal of Geophysical Research-Atmospheres, 119 (9), 5128 - 5141

Dorigo, W.A., Gruber, A., De Jeu, R.A.M., Wagner, W., Stacke, T., Loew, A., Albergel, C., Brocca, L., Chung, D., Parinussa, R.M., Kidd, R. (2015). Evaluation of the ESA CCI soil moisture product using ground-based observations. Remote Sensing of Environment; 162, 380-395, doi: 10.1016/j.rse.2014.07.023

Gruber, A., Dorigo, W.A., Zwieback, S., Xaver, A. Wagner, W. (2013). Characterizing coarse-scale representativeness of in-situ soil moisture measurements from the International Soil Moisture Network. Vadose Zone Journal, 12 (2), doi:10.2136/vzj2012.0170

Gruber, A. Su, C.-H., Zwieback, S., Crow, W., Dorigo, W., Wagner, W. (in review). Recent advances in (soil moisture) triple collocation analysis. International Journal of Applied Earth Observation and Geoinformation, in press

Lai, X., J. Wen, S. Cen, X. Huang, H. Tian, and X. Shi Spatial and Temporal Soil Moisture Variations over China from Simulations and Observations, Advances in Meteorology. http://www.hindawi.com/journals/amete/aa/529825/

Taylor, K. E. (2001), Summarizing multiple aspects of model performance in a single diagram, Journal of Geophysical Research: Atmospheres, 106(D7), 7183-7192.

Wagner, W., L. Brocca, V. Naeimi, R. H. Reichle, C. S. Draper, R. de Jeu, D. Ryu, C.-H. Su, A. Western, J.-C. Calvet, Y. H. Kerr, D. J. Leroux, M. Drusch, T. J. Jackson, S. Hahn, W. Dorigo, C. Paulik (2014), Clarifications on the 'Comparison Between SMOS, VUA, ASCAT, and ECMWF Soil Moisture Products Over Four Watersheds in U.S.', IEEE Transactions on Geosciences and Remote Sensing, 52 (3), 1901-1906.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 5151, 2015.

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