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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Multi-source hydrological soil moisture state estimation using data fusion optimisation" *by* Lu Zhuo and Dawei Han

Anonymous Referee #2

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The authors have suggested a method for creating a soil moisture product for hydrological applications using multiple data sources retrieved from three sources (SAC-SMA land surface modelling product, MODIS satellite-retrieved land-surface temperature, and Soil Moisture and Ocean Salinity (SMOS) project data) using the Gamma-test and the Local Linear Regression techniques. The accuracy of the produced soil moisture data was evaluated against the Xinanjiang (XAJ) hydrological model's soil moisture simulations. The authors have concluded that "together with the chosen data inputs can be used with high confidence to estimate an unintermitted hydrological soil moisture product, and the proposed method could be easily applied to other catchments and fields". The topic is of current scientific interest and the manuscript is overall well prepared. However, there are some general points that need to be clarified and at some points more detailed information or analysis is necessary. The following gen-

eral and specific comments should be addressed before accepting this manuscript for publication.

General Comments

1. Duration of the study period is two years: from January 2010 to December 2011. In my opinion, the presented calibration/validation results don't allow a reader to evaluate the model applicability and the aforementioned conclusion looks too optimistic. The point is that the study period is too short, and the presented results of the model validation are deficient. It means that the overall model performance based on these results is very sensitive to the meteorological conditions of the study period and the performance assessments are rather casual. This fact limits opportunities for application of the proposed method "to other catchments and fields". The conclusion on the model applicability would be more convincing if the authors evaluated the model against hydrological data for a longer period. According to the USGS Water Resources webpage, streamflow data series exceed 10 years for the Vermilion River at Pontiac, IL.

2. The Pontiac catchment is characterized by frequent soil freezing events in winter seasons. During freezing events, soil moisture transfer fundamentally differs from the unfrozen conditions (e.g. Gelfan, 2006). To my knowledge, the lumped XAJ model does not consider soil freezing, thus SMD simulations can be inaccurate for winter seasons. Please clarify. Gelfan A. N. (2006) Physically based model of heat and water transfer in frozen soil and its parametrization by basic soil data. IAHS Publ., 303, pp. 293-304.

3. The authors argued that "only the surface SMD referring to the vegetation and the very thin topsoil, is utilised as a hydrological soil moisture target". Does the XAJ model allow one to simulate SMD in the "very thin topsoil"? If no, this should be clearly pointed out in the manuscript, and the simulation results' interpretation should be corrected.

4. I fully agree with the authors that the results are "model parameter dependent" (line 486). But I disagree that the proposed NHSMS indicators allow one to obtain

independent results. I think that Figure 13 can not be considered as an evidence of such independence because of at least two reasons: (1) only one parameter has been changed; (2) the obtained closeness of the two curves is shown for only 4 months (of 2 years) with mostly high SMD values. Thus I believe that the results presented in the manuscript are dependent on the XAJ model structure, parameters and inputs. Please give a comment.

5. Conclusion, lines 539-541. I do not share the optimistic view of the authors on the perspective of the proposed fusion technique. Yes, the data sources contain part of useful (and probably independent) information. However, these data contain their own large measurement errors and error's synergy can result in dramatically increase of the presented results' uncertainty. I would like to read the authors' comment on this topic.

Specific Comments 1. Lines 133-136: The phrase beginning from the words "It is worth noting that. . ." looks unnecessary in scientific (non-popular) hydrological text 2. I suggest removing Fig. 3. This figure has been already demonstrated in three (at least) recently published papers of the authors (Zhuo and Han, 2016, 2017; Zhuo et al., 2015) 3. Line 165: There are 17 parameters in Table 1 4. Eq. 1: M is not defined 5. Lines 495-496: The statement "all hydrological models are driven by the same hydrological inputs (precipitation, evapotranspiration and flow)" is misconception. Please be more precise.

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