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Hydrology and Earth System Sciences Discussions

Interactive comment on "Towards an integrated soil moisture drought monitor for East Africa" *by* W. B. Anderson et al.

Anonymous Referee #1

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Anderson et al. analyse the sensitivity of three different soil moisture related products (2 derived from earth observation data, 1 model data set) over the Horn of Africa for drought monitoring purposes and apply a method to objectively merge the products into an integrated soil moisture drought monitor.

The paper does not present an original idea nor does the analysis of the data provide new insight in the performance of satellite retrievals or land surface modelling. I therefore recommend to reject this paper. In particular the paper fails to address following issues:

1. The analysis of the three data products is superficial. Errors and correlation coefficients are presented without an in-depth analysis of the observed error characteristics

and patterns. In view of the latter merging of the products this would have been an essential prerequisite. In particular the authors failed to analyse systematic differences in the datasets. The authors assume that calculating anomalies and rescaling the data removes all biases. This assumption requires thorough checking. The very different physical characteristics of the different datasets give raise to the concern that biases are not only linear but also of higher order. As was shown by Zwieback et al. (Nonlin. Processes Geophys., 19, 69–80, 2012) unresolved biases invalidate the TCA approach.

2. TCA only provides valid error estimates if the three input datasets represent the same process. Otherwise apples are compared to oranges and the retrieved errors are physically meaningless. It is not clear if this is the case in the current analysis. Although the authors spend a lot of effort in unifying the datasets but the validity of this approach is not further investigates.

3. The merging approach appears bizarre and the benefit of it is not clear. The authors apply a very complex process to unify the datasets (involving corrections for vegetation cover and different soil layers). This not only raises my concern expressed above but it also appears unlogic. As the authors use a land surface model the observations could be assimilated directly into the land-surface model which is the statistical correct solution and would lead to a more consistent analyses (correctly considering the different physical meanings of the observations and different observation times).

4. Finally the authors fail to seriously analyse the merged dataset and to ultimately justify their approach. Apart from an obvious improvement in the sampling the benefit in terms of reduced error is not further analysed.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4587, 2012.