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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.

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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:

*** While we appreciate the reviewer's specific comments on the manuscript, which are addressed below, we fundamentally disagree with his/her perspective on the role of peer reviewed literature in remote sensing and hydrology. Our manuscript, like a great many papers in the literature, attempts to adapt methods developed in a data rich environment (the United States) to socially relevant applications in a data poor environment (the Horn of Africa). In doing so, we implement analytical methods in a region for which they have not been applied previously, describe the characteristics of these techniques in that region, identify challenges associated with data limitations, and, where possible, propose ways to evaluate the utility of these products in a place that will not have extensive in situ monitoring networks any time soon. The application of satellite monitoring techniques to data poor regions is a major thrust in applied remote sensing sciences, and the 2011 drought in the Horn of Africa is a salient example of the kind of humanitarian crisis in which satellite-based monitoring and early warning might be applied to improve disaster response. The reviewer seems to feel that HESS should only publish papers on methodological advances (presumably in data rich environments) and should not entertain applications papers. This is an exceedingly narrow view of the earth sciences.

1. The analysis of the three data products is superficial. Errors and correlation coefïňĄcients 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.

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*** Line 471-494 discusses the errors while correlations are discussed between lines 415-436. We believe that these passages, together with the extensive discussion of spatial patterns found throughout the manuscript, do provide a thorough description of both errors and correlations. It is not clear to us what more the reviewer would like to see by way of error analysis.

*** The reviewer is correct that unresolved biases invalidate the TCA, therefore we have applied bias correction to the mean and the standard devation (via scaling) of the datasets. These are precisely the same type of biases that Zwieback et al. (2012) discuss under the section "2.2 Bias model" of their paper.

*** By calculating the anomalies the systematic differences between the mean values of the datasets are removed, and by rescaling the datasets the systematic differences in the magnitudes of the datasets (i.e. due to unit differences) is removed. After taking care of these two bias components, the only two moments that are left unmatched are the skewness and the kurtosis. To date, there are not many studies investigated the effect of these higher order moments in hydrological variable rescaling related studies, let alone the few triple collocation studies. In fact, the only relevant study (as far as we are aware), Yilmaz and Crow (2012; Manuscript available upon request), found that rescaling via correcting for only the mean and the standard deviation resulted in almost identical assimilation results with rescaling via cdf match (which also corrects for the skewness and kurtosis). Hence we believe correcting the products for their mean and standard deviation resolves all the linear differences that may exist between the products, and it is sufficient in triple collocation studies.

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.

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*** The reviewer is raising a fair point, and we agree that input datasets need to represent the same process for TCA methodology. In this study we have selected a surface-rootzone mixed soil moisture information. TCA assumes a linear relationship exists between the products, while correlation is a very suitable statistic that measures the linear relation between the products. Based on the very high correlations found between the products, we conclude that there is very high linear relationship between the products, hence they are perfectly consistent and suitable in a triple collocation analysis framework. We clarified this point in the manuscript between lines 558-561 (Beginning of sect 3.3).

*** However, if the reviewer is raising the issue about the vertical support consistencies of the three soil moisture datasets, then this issue is extensively studied in another study (Yilmaz et al., 2012), which shows that the applicability of TCA using products that has different vertical support information depends on the linear relationship between soil moisture at different soil depths (i.e. surface, vegetation-adjusted soil moisture, or root-zone). The depth variations will pose a problem if they manifest themselves in a nonlinear or a hysteric relationship; instead if the relationship is linear then it fits into the TCA framework. Therefore the impact of vertical inconsistencies will depend on the linear relation between the soil moisture values of different layers. Similar to what Yilmaz et al. (2012) have found over the US, we found a very high linear relation between the representative soil depths of the products we use (results not shown), hence we expect the vertical support inconsistencies are effectively handled via the linear rescaling performed in TCA equations. Perhaps this point was not clear in the manuscript and we now clarified this between lines 568-581.

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 solu-

tion and would lead to a more consistent analyses (correctly considering the different physical meanings of the observations and different observation times).

*** We appreciate the reviewer's suggestion to try DA approaches, as several of us are actively involved in advancing multisensor soil moisture data assimilation algorithms. We agree that these approaches have great potential and should be pursued in the future. We disagree with the reviewer, however, in his/her conviction that offline data merging is "very complex" relative to DA approaches-which certainly don't lack for complexity!-and we are unclear as to why the reviewer views DA as the "statistically correct" approach relative to offline data merging; Our method is statistically correct as well. There are statistically correct and incorrect ways to merge data streams either in the DA or the offline context and, as those of us involved in DA algorithm development are aware, there is tremendous value in offline data merging methods that can be evaluated against DA techniques.

*** For example, in land data assimilation techniques, the spread of the forcing datasets (i.e. precipitation) tends to dominate the error covariance estimates, which is the most important parameter (together with the observation error covariance) in the calculation of assimilation increments. However, the majority of existing studies set forcing perturbation parameters using subjective or semi-empirical methods based on prior experience. This is the case even in data-rich environments, and it is particularly problematic in regions for which the error characteristics of forcing data and observations are poorly constrained. This limitation in DA techniques doesn't mean that DA isn't useful, but it does mean that most DA studies involve a subjective element, the implications of which are a matter of ongoing research. The TCA data merging technique used in this study is, in this sense, a more rigorously objective approach in that it does not require any user-specific parameter selection. In this way the TCA offline technique provides a methodologically independent method to which emerging DA algorithms and DA update parameters can be compared.

*** Perhaps the reviewer wanted to emphasize that the assimilation studies could po-

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tentially generate more accurate results in that the time-evolution of the updated analysis may yield better forecast through the time-dependence. This could be true, however, as the drought monitoring studies are concerned with longer time-series (here 8-daily) than majority of the soil moisture assimilation studies (i.e. hourly), the added benefit of the data assimilation studies over 8-day averaged products would be reduced as well (i.e. innovations are required to be white, and in 8-day time-frame the innovations would cancel each other). Additionally, given the methodoogy applied here is more objective, here in this study we have selected this least-squares based merging rather than a data assimilation study. This point perhaps was not clear in the earlier version of the manuscript, hence in this version we clarified these points via addition of the text at line 155-158 (last lines of the intro).

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.

*** The reviewer is raising a fair point that the evaluation of the merged product is not as robust as the methodology. To a great extent evaluation of the merged product-and of any drought monitoring system in this region-is limited by the almost total absence of in situ monitoring networks. Nevertheless, we believe that the generation of a merged dataset can be justified for the following reasons:

*** 1) Similar seasonal and interannual patterns across the three independent products (see new Figure 12, Table 6) indicates that they are capturing similar processes w.r.t. drought, such that data merging to generate a spatially complete product is justified. It may be obvious that there is an advantage in sampling coverage, but it is not trivial.

*** 2) Given that data merging is justified, the fact that spatial patterns of TCA values follow an expected and well-understood pattern-i.e., passive microwave measurements appear to degrade relative to other products in densely vegetated areas while thermal RS techniques are prone to error in desert regions-suggest that a consensus-weighted

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merging process has an advantage over simple averaging

*** 3) As the relative ranking of drought years is similar across the independent products and the merged estimate, and all capture the relative intensity of the 2010-2011 drought, data merging does not lead to any radical departures from the drought rankings produced by each of the products on its own. This doesn't prove that the merged product is any better than the independent products, but it does provide confidence that merging is not introducing any major biases or artifacts relative to any one of the independent products.

*** This rationale is now described in the manuscript through Figure 12 and Table 6, text in the final paragraph of the Results & Discussion section, and text in the rewritten conclusions section. We readily acknowledge that this reasoning does not provide a copacetic validation of the merged data product, but we argue that it justifies the data merging effort as a step towards spatially complete drought monitoring that incorporates available information from independent microwave remote sensing, thermal remote sensing, precipitation remote sensing, and hydrological modeling techniques.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C2635/2012/hessd-9-C2635-2012-supplement.pdf

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