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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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General Comments

The paper presents a potential method for merging different estimates of soil moisture for use in real-time drought monitoring. The estimates are from passive microwave and thermal infrared remote sensing, and land surface modeling. The estimates are compared to estimates of precipitation from TRMM, vegetation from MODIS NDVI and groundwater from GRACE to show the qualitative evolution of these different indicators for the 2010-2011 Horn of Africa drought. The set of products provide complementary

information in terms of the temporal dynamics and representation of different parts of the hydro-ecological system. The authors note that the soil moisture products have their strengths and weaknesses, and propose a method to combine them via triple collocation analysis (TCA), which estimates the relative errors between the estimates and uses these to calculate a weighted merged product. The merged product provides a consensus view of drought evolution that also overcomes the spatial and temporal sampling issues of remote sensing and has potential to be used in realtime for drought monitoring.

The paper is relevant to HESS and makes a contribution to the application of remote sensing and model products to poorly gauged regions, building on previous work applied to the US. The manuscript is well written and technically correct. However, I think that the authors need to address some higher-level concerns and questions related to the suitability of the approach and the accuracy of the individual and merged products. These are given as speciïňĄc comments below

*** We thank Dr. Sheffield for his support of the overall value of the manuscript, as well as for the detailed, constructive suggestions that follow. Our responses to his specific comments are described below and incorporated to the revised manuscript.

1) An important question that is hinted at by the authors, is how you test this method? Obviously there are few if any ground observations of soil moisture in the region, and so there are no estimates of errors (relative to the truth) in each of the products. Rather the paper provides estimates of the errors relative to each other. Although the consensus product may be the optimal merged solution it will not be the truth. The authors argue that this is of value in unmonitored regions because the consensus view can be interpreted as a measure of conïňĄdence. This statement is difficult to justify, because two or all of the products can be biased in their absolute values and in their temporal dynamics, which can give incorrect assessment of drought when using the consensus view. Some more argumentation about this point would strengthen the paper. Are there ways of evaluating the merged (or any of the individual products), say against other ob-

servations or expected behavior? Each of the products has been evaluated against soil moisture estimates in the US (the current work builds directly from the same models and analysis methods over the US by Hain et al., 2011) but can these errors bet ransferred to east Africa and be used to give confidence in the merged product. The authors need to comment on the absolute accuracy of the merged product (and the individual products) and its suitability for drought monitoring, especially in the context of using a consensus product.

*** This comment addresses the single largest challenge for soil moisture drought monitoring in the Horn of Africa. Our group is currently working with collaborators in the region (primarily the Ethiopian NMA and the Nile Basin Initiative) to implement some form of systematic soil moisture monitoring in support of satellite product evaluation, but at present we have been unable to identify any reliable in situ measurement networks that can be applied to the present study. Given this limitation, we have attempted to contextualize our results and justify data merging through the following additions to the manuscript:

*** 1) Figure 12 and Table 6 have been added to the manuscript. Both address the matter of consistency between ALEXI, LPRM, and Noah within the Horn drought region, and they show that in this area, which is the primary focus region for the paper, there is reasonable consistency between all three independent soil moisture estimates and the merged product. This provides some confidence that in the region of interest the merged product behaves as expected and is not unduly influenced by one or two of the products. These results are introduced in the final paragraph of the Results and Discussion Section.

*** 2) The rationale for data merging is now stated transparently in paragraph 4 of the Conclusions section. Essentially, our argument is that: (1) similar seasonal and interannual patterns across the three independent products indicates that they are capturing similar processes w.r.t. drought, such that data merging to generate a spatially complete product is justified; (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 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.

*** 3) Consistency between the rank order of rainy season soil moisture deficit (Table 6) and the order of negative GRACE TWS and MODIS NDVI anomalies provides further confidence in the realism of the soil moisture products (in terms of variability, not absolute values). This is now noted at the end of the Results and Discussion section.

*** 4)Limitations in this approach to merged product evaluation are now stated clearly in the Conclusions section.

2) There are many uncertainties in the processing of the data, especially the exponential filter for the LPRM soil moisture. Firstly, the LPRM is quite different to the other products, but how much of this is due to it being ïňĄltered and how much is because of errors (relative) in the original surface retrievals? You could look at the correlations between the Noah and LPRM for the surface soil moisture only, to see the contribution of these errors. Otherwise it could be considered an unfair comparison because the LPRM data is only a transformation of the surface soil moisture using another model.

*** The influence of the exponential filter on LPRM correlations does, indeed, require consideration. In keeping with Dr. Sheffield's suggestion, we have analyzed correlations between Noah and LPRM for surface soil moisture only, and this test confirms that the relationship between LPRM and other products largely reflects the information present in the surface zone measurements. Although including the rootzone filter changes the LPRM SM estimates to rootzone-surfacezone mixed measurements, the correlations and TC values change very little.

*** Correlations between pairs of products using only the LPRM surface zone are very similar to those using the exponential filter. The magnitude of correlation changes slightly for some regions, but the spatial pattern of correlation remains the same (See attached Fig 1., which is a reproduction of Figure 5 in the manuscript using only surface zone values for LPRM). Text summarizing these results has been added to the manuscript, (lines 510-519, second paragraph of section 3.2)

Secondly, does calibrating tau on the Noah data contradict the assumption of independence in the errors, because, for example, errors in the Noah forcings may filter down to the dynamics between the surface and root zone and thus the characteristic time? Furthermore, the use of a characteristic time that is highly model dependent as seen from NLDAS evaluations of soil moisture dynamics (e.g. Schaake et al., 2003; ShefïňĄeld et al., 2012) may cause the LPRM to be closer to the Noah data than would normally be expected and therefore may overwhelm any information in the original LPRM retrieval.

*** The reviewer has a point that calibrating the LPRM RZ filter using correlation with Noah RZ data has the potential to introduce artificial relationships. To test the sensitivity of the final TCA values (and therefore weights) to the chosen value of Tau in the exponential filter, TCA values obtained using the calibrated Tau were compared to TCA values resulting from using constant values for Tau. Fixing the characteristic time (Tau) at 8, 16 and 24 days resulted in TCA values that differed only marginally in magnitude and not at all in structure. Attached Figure 2 details the TCA values resulting from the calibrated Tau are compared to those from the Tau fixed at 8, 16 or 24 days. Summaries of the attached Figure 2 were added to the manuscript (lines 561-568, first paragraph of sect 3.3).

Page 4601, line 5-9. What happens if you do not normalize the data? For example, the correlations in Figure 5 indicate high correlation between the RS datasets and Noah, but lower between the two RS datasets. I could imagine that the correlations between

the original (unscaled) data would be lower.

*** The correlations calculated in this study are invariant to linear operators, such that correlation coefficients between datasets are independent of the normalization.

3) Can you add the version of the TMPA to the text when the dataset is ïňĄrst described?

*** Described at first mention of the dataset in lines 229-230 (first paragraph, sect 2.1.3)

I see that this is given in the caption for Figure 2 as the 3B42 research product, which is gauge corrected. What if you used the RT version (which you would have to for realtime monitoring), which is different from the research product in terms of its biases and their temporal consistency? For example, Bitew and Gebremichael (2011) showed that the TMPA 3B42RT is negatively biased in terms of evaluations of simulated streamïňĆow over Ethiopia but the rain gauge corrected research version 3B42 had worse performance and temporal inconsistencies. Can the authors comment on the use of the 3B42 product and whether the results are relevant in a realtime context?

*** The reviewer is correct that as an operational tool, a merged product would have to use a near real-time operational forcing. As demonstrated by Bitew and Gebremichael (2011), The TMPA 3B42 RT product has different biases and temporal consistency from those of TMPA 3B42 and these differences would need to be investigated in the transition to operational, near real-time mode. That said, previous experience with TMPA research product vs. TMPA-RT has shown that the relative performance of each can change substantially with small updates in the algorithm: the switch from TMPAv6 to TMPAv6A in 2007, for example, introduced a small dry bias in portions of East Africa (apparently altering, temporarily, the relative product biases reported by Bitew and Gebremichael 2011), but this bias appears to have been remedied in the new TMPAv7. Given the evolving nature of TMPA and TMPA-RT (and other satellite-derived products, for that matter), we feel that the results using the research product are relevant to the development of an operational monitoring system - Noah forced with the RT dataset will produce a different merged final product, but so will Noah forced with v7 vs. v6 of either product, and the TCA analysis framework proposed in the paper can incorporate any of these various versions of TRMM-derived precipitation.

4) The choice of the Noah dataset should not matter to the TCA, but reading through the manuscript there seems to be an underlying assumption that Noah is closer to the truth than any of the other products. Although the errors in the LPRM and ALEXI are evaluated in relative/consensus terms, they are also evaluated in terms of their process representation with reference to the Noah model, presumably because the Noah model explicitly represents the surface and root zone processes and for dense and sparse vegetation, which the other products do not do completely. I think this is problematic, because although the attribution of the errors is intuitively correct, you have no way of knowing this for sure. For example, the TMPA data is likely biased at different scales, which will propagate through the Noah model, or the soil moisture in Noah may have biased dynamics. The latter has been shown in previous versions (v2.7 and earlier) because of too high evaporation and low coupling with soil moisture (Kunar and Merwade, 2011; ShefïňĄeld et al., 2012). This may have been ïňĄxed in version 3.2. If you chose the ALEXI instead as the reference what would the errors look like and would the physical interpretation of the relative errors be the same?

*** We agree with the reviewer that Noah should not be assumed to be closer to truth than either of the remotely sensed products. We have revised statements in the text that give the impression that Noah is a reliable reference against which the other products can be judged.

*** Most importantly, we have revisited the portion of the discussion that compared the two RS datasets to Noah and proposed physical interpretations of the relative errors based on that comparison (Figure 6 and associated text). To rebalance this discussion, we have updated Figure 6 to reflect the results when ALEXI is used as a point of comparison as well (see attached Figure 3, the modification of Figure 6 in the manuscript).

*** The expansion of Figure 6 to include ALEXI as reference is intended to provide further insight into the performance of the sensors and to aid the reader in interpreting results. While the figure itself is changed, the physical interpretation of the correlations remains consistent with the previous Figure 6: the LPRM product correlates less strongly with ALEXI in areas of moderate to high vegetation than does Noah. In areas of low fractional vegetation, the LPRM product and Noah correlate equally well with ALEXI. Including this additional figure further details the relative performance of each sensor over high and low fractional vegetation.

*** Text to this effect has been added to lines 528-539, 551-554 (last two paragraphs, sect 3.2), and Figure 6 has been updated in the manuscript.

*** With respect to the merged product: the merging calculation depends on the relative magnitudes of differences between products, so while the choice of the reference dataset determines the total magnitude of errors, it does not affect relative weights. Standardized anomalies of a merged product that uses ALEXI as the reference product for error calculations would be identical to the standardized anomalies of the merged product that uses Noah as the reference.

5) I think you mention somewhere that the errors may be seasonally varying, but the full time series of the data are assessed together, likely because of the short record of the ALEXI data. Can you comment further on this, such as whether the errors may change with time and therefore whether the weights should be seasonally varying?

*** The reviewer is correct that this point should be further discussed in the manuscript. Text has been added to lines 587-591 and 609-612 (end of first paragraph, sect 3.3; beginning of the 4th paragraph, sect 3.3.):

*** The errors may in fact vary with time, and would be expected to vary seasonally. For example, the errors during the rainy season would be expected to be larger simply because the magnitude of soil moisture during rainy events is larger. For this study the errors were assumed constant in time due to the short time series of available data.

The weights will vary accordingly.

6) The discussion of the merged product is weak. Presenting the errors in the individual products versus the merged product and showing the evolution of the merged product during the drought does not provide much conïňĄdence in the merged product. One suggestion is to show the error in drought monitoring if you used just one of the products, for example, given a threshold for drought that would induce a trigger, how would each of the products fare against the merged product if it was considered the truth?

*** Quite agreed. Please see our response to Comment #1 for our updated treatment of the merged product. While we like the idea of applying a threshold analysis to each product, we have opted to leave that analysis for future work: defining triggers in the context of the internal error characteristics of each product warrants its own devoted analysis, and it would add to the complexity of this already lengthy manuscript.

7) What are the challenges of doing this in realtime? There are latency and availability issues in many of the datasets that go into these products. For example, the realtime TMPA has different error characteristics than the research product that may also vary in time if the realtime calibration changes

*** The reviewer addresses two critical considerations for real time applications of the systemâĂŤdata latency and changes in realtime retrieval algorithms. In addition (as we learned the hard way while preparing this manuscript) data continuity is a challenge for any monitoring system that relies on research grade sensors like AMSR-E. We have added a paragraph to the Conclusions section to acknowledge these challenges. We hope to perform a full, practical assessment of operational limitations in the near future, when we are able to test the monitor using data from the new AMSR-E sensor in combination with ALEXI and updated TMPA-RT.

Minor Comments Page 4589, Line 11-16. Suggest breaking long sentence into two parts *** The long sentence has been split into two sentences.

Page 4590, Line 10. The website is http://hydrology.princeton.edu/monitor and a better reference is Sheffield et al. (2008) *** Website and reference updated

Sheffield, J., E. F. Wood, D. P. Lettenmaier, and A. Lipponen, 2008: Experimental Drought Monitoring for Africa. GEWEX News, 8(3).

Page 4590, Line 17. Do you have a reference or link for the Global Drought Monitor? C1926 *** A link has been provided.

Page 4590, Line 27. Suggest adding “University of Washington” before “Experimental Surface Water Monitor”. Also, this monitor is multi-model (VIC plus Noah, SAC, CLM, CATCHMENT) *** The suggest text has been added to the manuscript

Page 4591, Line 18, The VIC reference should be Liang et al. (1994) *** Reference updated

Liang X, Lettenmaier DP, Wood EF, Burges SJ (1994) A simple hydrologically based model of land surface water and energy ïnËĞC ÌĄuxes for GCMs. J Geophys Res 99(D7):14,415–14,428

Page 4591, Line 24. The NLDAS references should be Sheffield et al., (2012) and Xia et al. (2012), not Mitchell. *** References updated

Sheffield, J., Y. Xia, L. Luo, E. F. Wood, M. Ek, K. E. Mitchell, and the NLDAS Team, 2012: Drought Monitoring with the North American Land Data Assimilation System (NLDAS): A Framework for Merging Model and Satellite Data for Improved Drought Monitoring, in "Remote Sensing of Drought: Innovative Monitoring Approaches", B. Wardlow, M. Anderson and J. Verdin (eds.), p. 270, Taylor and Francis, London, United Kingdom

Xia, Y., K. Mitchell, M. Ek, J. Sheffield, B. Cosgrove, E. Wood, L. Luo, C. Alonge, H. Wei, J. Meng, B. Livneh, D. Lettenmaier, V. Koren, Q. Duan, K. Mo, Y. Fan, D. Mocko, 2012: Continental-Scale Water and Energy Flux Analysis and Validation for the North-American Land Data Assimilation System Project Phase 2 (NLDAS-2), Part 1:

Intercomparison and Application of Model Products. J. Geophys, Res., Vol. 117, No. D3, D03110, http://dx.doi.org/10.1029/2011JD016051

Page 4596: line 9 can you provide the spatial resolutions of TMPA plus the version number? *** Version and resolution added

Page 4600, line 10. Should this be the Fc weighted sums of SF and RZ, rather than the RZ? *** The typo has been corrected.

Page 4602, line 5 and eqns 9-11. Should this be a single prime superscript on the C1927 theta values? *** No, the single prime refers to the composites before rescaling, while the double prime refers to the rescaled composites. The wording of that sentence was unclear and has been clarified.

Page 4604, Line 7. This is not quite true, the figures shows that the AMSR-E and GRACE data are not the most negative. Suggest changing this to "were the most severe or close to the most severe" *** Noted, the text has been updated

Page 4604, line 22. Isn't this to be expected, given that drought conditions cannot be alleviated during the dry season? *** The reviewer is correct that it would be expected, but it is worth noting explicitly because the phenomena was particularly important for the 2010-2011 drought due to the two-season nature of the drought.

Page 4606, line 24. Need a period after "0.60". *** Corrected

Page 4607, lines 7-8. Why are there high correlations in highlands and coastal areas and why is this a problem? You attribute this to the short record length. But I do not understand why this should be. This is not explained very well. *** The short record length is not meant as an explanation for the TC values in the coastal areas, this was unclear due to poor sentence structure. The paragraph has been clarified.

Page 4608, line 5-6. Suggest include a reference to Figure 7, which has the locations of the four regions. *** A reference to Figure 7 has been added

Figure 4 and all other map figures. Suggest adding a label above each of the panels that gives relevant information such as the product name and time period where appropriate. Otherwise it is difficult to determine which is which from the figure captions alone. *** Product name and time-period has been added to all map figures.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C2620/2012/hessd-9-C2620-2012-supplement.pdf

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

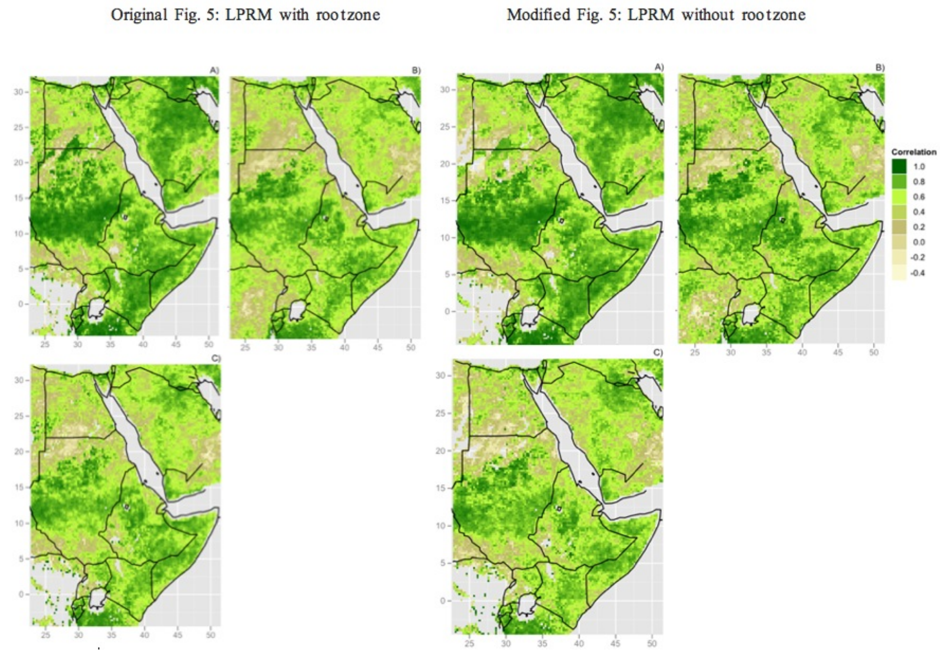

Fig. 1. Temporal cross-correlation of rescaled soil moisture anomalies for Jan 2007 – Jun 2010 computed between A) LPRM and Noah, B) ALEXI and Noah, and C) ALEXI and LPRM

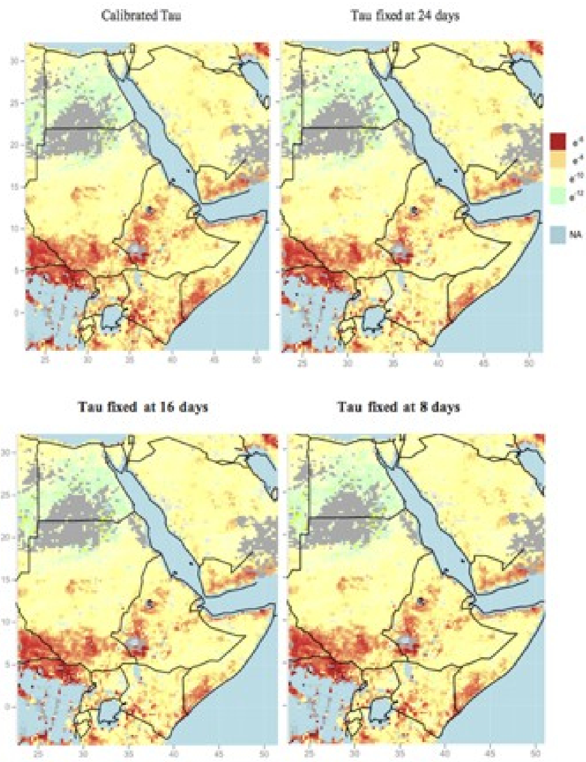

Fig. 2. TCA values resulting from the calibrated Tau compared to those from a Tau fixed at 8, 16 or 24 days

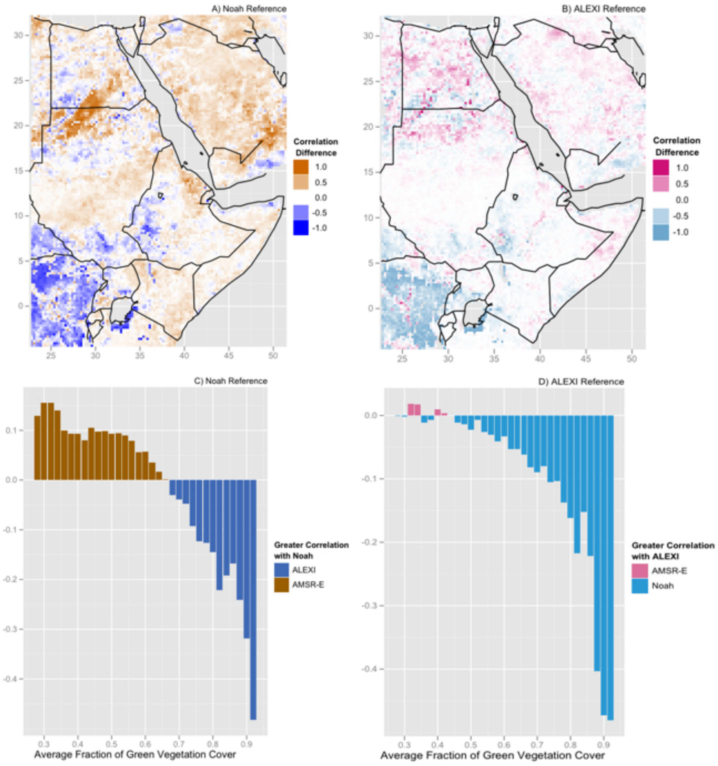

Fig. 3. Anomaly correlation difference using Noah (A,C) and ALEXI (B, D) as reference datasets. Areas shaded in brown or pink represent a greater correlation between LPRM and the reference dataset. A) and B)