Hydrol. Earth Syst. Sci. Discuss., 9, C1922–C1928, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C1922/2012/

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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Received and published: 30 May 2012

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 specific comments below.

Specific Comments

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 confidence. 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 observations 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 be transferred 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.

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 filtered 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.

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; Sheffield 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.

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.

3) Can you add the version of the TMPA to the text when the dataset is first described? 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 real-time 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 streamflow 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?

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; Sheffield et al., 2012). This may have been fixed 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?

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?

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 confidence 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?

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.

Schaake, J. C., Qingyun Duan, Victor Koren, Kenneth E. Mitchell, et al., 2004: An inter-comparison of soil moisture fields in the North American Land Data Assimilation System (NLDAS). J. Geophys. Res., 109, D01S90, doi:10.1029/2002JD003309.

Kumar, S., and V. Merwade, 2011: Evaluation of NARR and CLM3.5 outputs for surface water and energy budgets in the Mississippi River Basin, J. Geophys. Res., 116, D08115, doi:10.1029/2010JD014909.

Bitew, M. M. and M. Gebremichael, 2011: Evaluation of satellite rainfall products through hydrologic simulation in a fully distributed hydrologic model, Water Resour. Res., 47, W06526, doi:10.1029/2010WR009917

Sheffield, J., B. Livneh, and E. F. Wood, 2012: Representation of terrestrial hydrology and large scale drought of the Continental U.S. from the North American Regional Reanalysis, J. Hydrometeor., doi: http://dx.doi.org/10.1175/JHM-D-11-065.1

Minor Comments

Page 4589, Line 11-16. Suggest breaking long sentence into two parts

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

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?

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)

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

Liang X, Lettenmaier DP, Wood EF, Burges SJ (1994) A simple hydrologically based model of land surface water and energy ïňĆuxes 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.

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?

Page 4600, line 10. Should this be the Fc weighted sums of SF and RZ, rather than the RZ?

Page 4602, line 5 and eqns 9-11. Should this be a single prime superscript on the theta values?

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”

Page 4604, line 22. Isn’t this to be expected, given that drought conditions cannot be alleviated during the dry season?

Page 4606, line 24. Need a period after “0.60”.

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.

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

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.

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