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Hydrology and Earth System Sciences Discussions

Interactive comment on "Combining surface reanalysis and remote sensing data for monitoring evapotranspiration" *by* M. Marshall et al.

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Major Comments:

The paper is long, as reviewers in the past have requested that we enumerate its limitations. There are several, but I think that there are some interesting conclusions that can be drawn from the paper that make this an important contribution: 1) Demonstrates how a dynamic vegetation component can be integrated into an operational LSM, which can be particularly valuable in semi-arid regions 2)Greatest inconsistences in GLDAS data and how they can impact ET models a.Model still underperforms in arid regions: Priestley-Taylor is not appropriate in semi-arid regions where advection can contribute a significant portion of moisture flux and the large negative bias in GLDAS net radiation, which drives P-T leads to large underestimates in these areas

It may seem arbitrary that the GLDAS realization of NOAH was chosen, but in fact, it was chosen, because it has been evaluated at sites representing various climatic zones, has a well-established formulization for wet canopy and soil evaporation, and it is readily available over time series long enough for trend analysis in Africa (ultimate goal of using the model). There are decent precipitation datasets available for Africa (TRIMM, RFE2, etc) and researchers are currently looking at assimilating these data into the NASA LIS to drive operational LSM's, such as NOAH. GLEAM and other models perhaps have better formulizations, but also require more inputs not readily available in Africa and they would still introduce additional biases- more than NOAH. CRU and other long-term climate data sets may be more accurate than the reanalysis, but do not cover the range and depth of the reanalysis data and therefore we did not explore this as an option. I think it is important that we test how models that perform well with flux tower data do when driven by coarse resolution datasets. As this paper has shown, the PT-JPL model itself is good, but certain components when driven by potentially erroneous coarse resolution data perform poorly-perhaps even more poorly than a simple ET model driven by one variable. I will add text to reaffirm our rationale and defend the benefits of the PT-JPL model.

Minor Comments:

P1549L19&21: I will reword this to reflect that ET and Prcp (ET to a lesser degree) are important components of the atmospheric and land hydrologic cycles.

P1550L6: I will insert the following citation: Wani, S., T. Sreedevi, J. Rockstrom & Y. Ramakrishna (2009) Rainfed Agriculture - Past Trends and Future Prospects. Rainfed Agriculture: Unlocking the Potential.

P1550L6: I will insert the following citations: Mueller, B., Seneviratne, S.I., Jiménez, C., Corti, T., Hirschi, M., Balsamo, G., Ciais, P., Dirmeyer, P., Fisher, J.B., Guo, Z., Jung, M., Maignan, F., McCabe, M.F., Reichle, R., Reichstein, M., Rodell, M., Sheffield, J., Teuling, A.J., Wang, K., Wood, E.F., Zhang, Y., 2011. Evaluation of global observations-based evapotranspiration datasets and IPCC AR4 simulations. Geophysical Research Letters 38: L06402, doi:10.1029/2010GL046230. And Jiménez, C., Prigent, C., Mueller, B., Seneviratne, S.I., McCabe, M.F., Wood, E.F., Rossow, W.B., Balsamo, G., Betts, A.K., Dirmeyer, P.A., Fisher, J.B., Jung, M., Kanamitsu, M., Reichle, R.H., Reichstein, M., Rodell, M., Sheffield, J., Tu, K., Wang, K., 2011. Global intercomparison of 12 land surface heat flux estimates. Journal of Geophysical Research 116: D02102, doi:10.1029/2010JD014545.

P1550: I will reword this paragraph and provide an example of a purely empirical approach (Wang et al. 2007), but "remote sensing models which derive flux directly from empirical relationships of meteorological and remote sensing data, are not considered in this category." I will also differentiate between the energy balance approaches.

P1551: I will rephrase- operational LSM's that can also provide, like energy-balance approaches, near-real time estimates.

P1552L3: The model will be referred to as PT-JPL throughout the paper. I will also reword any mention of the NOAH LSM as GLDAS realization of NOAH or GNOAH.

P1552L3: The main point of the exercise is to see if benefits of each approach can be combined to develop a superior "hybrid" model. From the outset, we thought that the canopy component driven primarily by a seasonal average in the LSM's could be replaced by a dynamic vegetation component to provide better flux estimates in the Sahel. This ended up not being the primary focus of the paper, but was the original reason for combining the two. This paragraph shows the weaknesses of each model and where we might expect improvements from a hybrid. I will reword this paragraph to clarify.

P1552L9: I will rephrase- they are statistical relationships based on theory. The point here and later in the paper is that these relationships are power functions and when driven by erroneous data exacerbate the errors. A simple linear relationship would produce fewer errors, even though it may not have the theoretical justification.

P1552L11: It is difficult to know if overestimation in one model could counter underestimation in the other, but the results show which parts of the models work and how substituting these parameters with NOAH or another ET product could produce better estimates. There is also the danger of increasing df with a new parameterization and I will address these briefly in the discussion.

P1553L7: I will reword

P1554L3: Ground heat flux is available

P1555L8: I will eliminate "they show better results"

P1557L8: "dekadal" is correct- 10 day timestep

P1558: The observed data is not corrected and I have included a thorough discussion of energy balance closure problems and published biases when available. I did not have access to many of the data from the towers, which made a more thorough analysis difficult. P1559: I do not think it is necessary to show an NDVI or EVI time series. They would not add any new information, as the canopy component (already shown) is driven primarily by EVI.

P1559: Soil LE accounts for the majority of LE following a rain event due to the high thermal capacitance of soil. It is generally assumed that during dry periods, the contribution of soil LE is low. I agree that the comparison is not 1:1 at lag t, but there should be some relationship, as is between runoff and precipitation. For more information, please refer to Nagler, P. L., Glenn, E. P., Kim, H., Emmerich, W., Scott, R. L., Huxman, T. E., and Huete, A.R.: Relationship between evapotranspiration and precipitation pulses in a semiarid rangeland estimated by moisture flux towers and MODIS vegetation indices, J. Arid Environ., 70, 443–462, 2007.

Very recent work (in press) has shown that soil LE can be significant outside the rainy season for forested sites and I can insert this citation and a sentence as reasoning for the poor qualitative assessment between soil LE and precipitation at the forest sites.

Table 1: This is based on an email I had with Kevin, because it was not clear to me in the paper. From Gao et al. (2000), fAPAR = 1.2*EVI- this is what I used in this paper. In other papers where I used SAVI instead, I used the formula fAPAR = 1.2*(1.136*SAVI-0.04). For NDVI I retained 1.0. m2 is incorrect and I will change this. I tested the power of 2, 4 (original) and 10 (you recommended 10 to me) and the correlation with the power of 10 was higher for all the sites. By increasing to the power of 10, the fwet function shuts off (not wet) at a higher relative humidity. I think this is reasonable given the high temperatures and PET of the sites compared to mid-latitudes. I can insert more details into the methods on my rationale for choosing 10.

Table 5: This table shows the sensitivity of the PT-JPL model to the various GLDAS forcing data. The slope is the most important attribute- EVI has the highest slope, so it accounts for most of variability (sensitivity) in the PT-JPL model. Figure 3 shows a similar pattern, just approached differently. In Table 5, only the test variable is perturbed, while in Figure 3, combinations of the variables are perturbed. This chart reveals any syngerstic or suppressive effects of the variables- e.g. EVI might be the most important variable in the PT-JPL model, but only because temperature increases its sensitivity.

Figure 2: The X-axis is observed data and the Y-axis is GLDAS data. I only compared the variables used to drive PT-JPL. I will clarify.

Figure 3: The point is to look at multi-collinearity. Just because p and q show no change in one simulation, does not mean that another simulation (combination of variables) will produce no change. I can remove this figure if you prefer, because it does not add anything after table 5, save revealing the robustness of the sensitivity analysis.

The initial analysis (Figure 1) was produced with observed data. The remaining analysis was performed with GLDAS. To reiterate, the point is to see how well the model performs when forced with GLDAS data. I can include a line in the discussion that states other forcing data (e.g. CRU) should be looked at as well. The regression plots of GLDAS with observed data shows where the largest biases in the forcing data are

and when used in the model, how they impact the results.

No worries- I hope I was able to answer your questions. It was a difficult paper to write, given the inconsistencies with the data.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1547, 2012.