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Hydrology and Earth System Sciences Discussions

Interactive comment on "On the utility of land surface models for agricultural drought monitoring" *by* W. T. Crow et al.

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We thank the reviewer (Dr. Heye Bogana) for his very helpful review of our manuscript. His comments have led to major modifications of our manuscript.

1. The method chosen to validate global land surface models (LSM) was already applied to the European continent and is for the first time applied globally. However there is also no verification that this method is valid for the global scale. This could be accomplished using global soil moisture products from satellite missions (e.g. SMOS, ASCAT).

Response: This is a very good point. In response, we have completed a new analysis

in which we compared our existing results - derived using MODIS-based NDVI as the target variable - to a completely new set of results derived using AMSR-E based surface soil moisture retrievals as the target variable. Note that AMSR-E retrievals were used since they provide a sufficient data record (mid 2002 to late 2011) to accurately capture monthly inter-annual ranks required for our analysis.

Figure 1 below contains a preliminary summary of these new results. The right-hand column of the figure shows rank correlation results between model-based soil moisture for month i and NDVI for month i+1, and the left-hand side shows results versus rank correlation results between model soil moisture for month i and AMSR-E based surface soil moisture for month i. Comparable rank correlation results for API have been subtracted from all results and all differences have been normalized into statistical Z-scores. Therefore, red areas are locations where various LSMs outperform API and blue areas locations where API outperforms a particular LSM. Note that while differences exist between columns, our new AMSR-E surface soil moisture analysis provides an independent verification of NDVI-based results contained in our original manuscript. In particular, it re-confirms the lack of any substantial global-scale advantage associated with modern LSMs and demonstrates a surprising amount of geographic consistency with our NDVI-based evaluation (note, e.g., the lack of difference between Noah results shown in the top row of the figure). A complete comparison of results from both evaluation strategies will be added to the revised manuscript.

2. The reason for choosing this indirect method is the supposed lack of large scale soil moisture measurements. However, recently several soil moisture data products from satellite missions are available, which already have been used to evaluate modeled soil moisture products (e.g. Albergel et al., 2012). Such data sets have sufficient time (e.g. 3-5 days) and spatial resolution (e.g. 50 km) for the validation of global LSM.

Response: As noted above, we agree with the reviewer's general point here and will add a comparison to satellite-based, surface soil moisture retrievals to the revised manuscript. However, we disagree with the characterization that our original NDVI-based evaluation method is somehow more indirect (and therefore less valuable) than comparisons against satellite-based, surface soil moisture retrievals. Here, our focus is on evaluating the contribution of LSM root-zone (surface to 1-meter) soil moisture estimates for regional-scale agricultural drought monitoring and forecasting applications. In such applications, root-zone soil moisture estimates for month i are used to forecast vegetation status on months i+1, 2, and 3. Therefore, the lagged rank cross-correlation analysis we perform between current soil moisture and future NDVI provides a direct evaluation of how much value a given soil moisture product contributes to drought monitoring and forecasting applications. Note that, in such applications and in our analysis, the ultimate interest is on vegetation condition and productivity and not soil moisture itself. Soil moisture is simply a means to an end.

From this point of view, comparisons against satellite-based, surface soil moisture retrievals are less valuable in two key regards. First, they are not directly reflective of the true goal of an agricultural drought monitoring system (i.e., vegetation condition and productivity). Second, they are inherently superficial observations describing only soil moisture conditions in the top 1 to 3-cm of the soil column. Consequently, they are not fully descriptive of integrated root-zone conditions impacting vegetation productivity. As a result, one can make a strong argument that our original NDVI-based approach is actually the superior evaluation method.

That said, the best path forward is clearly to follow the reviewer's advice and describe both types of evaluation approaches in the revised manuscript (which we will do). In addition, we will add new text to the revised manuscript to better motivate our existing approach (following along the lines of the discussion presented above).

3. Three different LSM have been chosen for this study, but no specific reason for this selection has been made. Also, not the latest versions have been used. For instance, the CLM 2.0 version instead of the actual 4.0 version was used. This unnecessarily decreases the significance of the results.

Response: The three different LSMs examined here (Noah, CLSM, CLM2.0) were selected based on their availability within the NASA Land Information System (LIS). The LIS system provides a unified data driver for LSMs and thus greatly facilitates the side-by-side comparisons of multiple LSMs. Unfortunately, CLM4.0 is not currently implemented in LIS. This rationale for selecting particular LSMs will be clarified in the revised manuscript.

Given the extremely large number of different LSMs (and LSM version numbers) currently in use, any analysis involving LSMs can always be criticized as the basis that it does not include a particular LSM version. It is our contention that the three LSM's examined here are reasonably representative of LSM's currently used by the land surface hydrology and climate communities. But, naturally, the results presented here cannot be assumed strictly valid for all LSMs. The revised manuscript will note this limitation.

4. The study showed that the oversimplified API-approach led to better drought forecast than using LSMs. However, since the API-approach uses a spatially constant parameter it should yield highly unreliable soil moisture predictions depending on the local rainfall amounts. Therefore this result could also hint to the fact that the validation approach is not reliable. Again, satellite soil moisture products would be needed to disprove this suspicion.

Response: The assumption that "over-simplified" modeling approaches like the API should lead to lower-quality predictions is a natural one to make. However, it is an assumption that has not been verified in the application of LSMs to monitoring soil moisture at large spatial scales and cannot be assumed as a given. Since we have no reason to question the objectivity of our NDVI-based evaluation system, we assert that our results point to the lack of added value in existing LSMs (for this particular application) and not a problem with our evaluation system.

Nevertheless, we fully agree that our manuscript would be strengthened if our faith in an NDVI-based verification approach were justified by some kind of external verification. In the revised manuscript, we will provide such verification using the reviewer's suggestion of comparison against satellite-based surface soil moisture retrievals. See response to major point #1 above.

Specific comments:

P1 L5 I think you mean "formulations"

Response: Yes, this was a typo. Will be fixed.

P2 L56 The CLM version 4.0 is now standard

Response: See response to major point #3 above. In order to clarify which CLM version we are using, all references to "CLM" in the revised manuscript will be replaced with references to "CLM2.0."

P3 L86 The formulation "diagnosed from the: : :" is unclear

Response: The offending sentence will be re-worded.

P3 L87 What is the start value of theta?

Response: The initialization of all models is described at the end of the first paragraph in Section 2. In the revised manuscript, the text will be clarified to stress that the initialization procedures applies to all models - including API (the model alluded to by the reviewer here).

I would have expected that a spatially constant parameter will lead to large over and underestimations of soil moisture depending on the local rainfall amounts. Did you check whether the modeled soil moisture values are realistic at all?

Response: Yes, this is the exact purpose of the paper. We evaluate the "realism" of relative variations in API soil moisture predictions by seeing how well they cross-correlate with relative variations in independent NDVI observations and – in the revised manuscript – with relative variations in independent satellite-based surface soil moisture retrievals as well. It is our contention that any lack of realism in the API predictions should be reflected in these comparisons.

P4 L106 How did you disaggregate the SMAP data.

Response: We believe the reviewer is referring to CMAP precipitation totals (the acronym SMAP was not used in the manuscript). The disaggregation was performed via a simple procedure whereby higher-resolution GDAS data was rescaled to match coarser-resolution CMAP data. In this way, the high-resolution GDAS data is taken to be a down-scaled version of the CMAP data. We will clarify this point in the revised manuscript.

P4 L121 "uses" instead of "input"

Response: Agreed. Will be changed in the revised manuscript.

P4 L127 Delete "assumed"

Response: Agreed. Will be deleted in the revised manuscript.

P5 L154 "issues" instead of "things"

Response: Following the advice of Reviewer #3, "things" will be changed to "objectives." We agree that "things" was not quite right here but "issues" leads to awkward English.

P6 L174 What might be the reason for the higher auto-correlation of Noah and why is this a problem for the later analysis?

Response: The higher auto-correlation of Noah soil moisture could be due a range of reasons (e.g., lower ET, reduced drainage, more upward diffusion of soil water, higher water column storage, increased canopy interception, etc) and the exact cause is beyond the scope of this analysis.

However, we are concerned with the impact of anomalous auto-correlation results on our analysis since differences in auto-correlation structure could interfere with our interpretation of soil moisture/NDIV cross-correlation as a measure of model skill. For example, otherwise skillful variations in profile soil moisture estimates from a given model could be unfairly evaluated if sampled at a depth which did not adequately express their true cross-correlation with NDVI. We attempt to standardize the auto-correlation of all model estimates prior to our analysis so that any particular product is not unfairly affected by anomalous auto-correlation characteristics. Text at the start of Section 3.1 in the revised manuscript will be modified to clarify this key point.

P6 L182 Delete "in the"

Response: Agreed. Will be deleted in the revised manuscript.

P6 L192 "product"

Response: Agreed. Will be changed in the revised manuscript.

P7 L2 Why did you include API in the ensemble? I think it would better to exclude API since in the z-score analysis the ensemble is compared with API.

Response: Agreed. Our inclusion of API in the ensemble was somewhat misguided. In the revised manuscript, we will present new ensemble results using only the three "modern" LSMs (Noah, CLM2.0 and CLSM). This modification will not lead to any qualitative changes in results.

P8 L1-237 this belongs to the methodology section.

Response: Agreed. Will be moved in the revised manuscript.

P8 L259 The quality of model results largely depends on the quality of the input data ("rubbish in, rubbish out"). This is particularly true for physically based models. Therefore, also the quality of the input data should be taken into consideration (e.g. soil data).

P9 L276 Since rainfall quality will increase LSM as well as API model output. If

the data quality of other input sources would be increased (e.g. soil information) the performance of the LSM is likely to be improved relatively to API.

Response: Our analysis concerns itself solely with inter-annual ranks (e.g., how many Junes between 2000 and 2010 are wetter than June 2007 - see Figure 1). As such, it is sensitive only to relative inter-annual variations in soil moisture. Such variations are generally driven by dynamic (i.e., time-varying) model forcing (like micro-meteorology, radiation and rainfall) and are much less sensitive to variations in non-time varying data model inputs like soil texture Therefore, our strategy was to focus only on the impact of variations in dynamic model forcing capable of significantly affecting inter-annual rankings. This key point will be clarified by new text added to the revised manuscript.

Also note that, in response to a comment by Reviewer #2, the revised manuscript will also include a discussion on the impact of using time-varying (as opposed to climatological) AVHRR NDVI retrievals to parameterize vegetation. As a result, it will consider the impact of all key dynamic model inputs.

P9 L293 Figure 5b

Response: No specific comment was associated with this manuscript location.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5167, 2012.

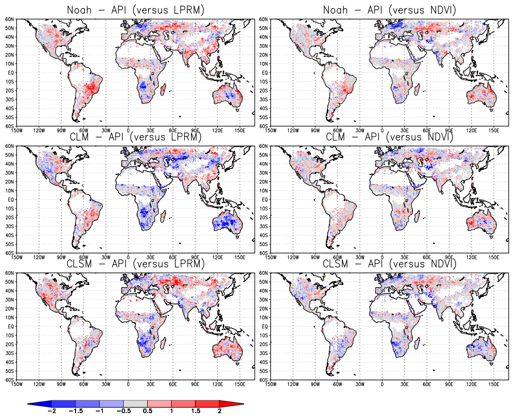

Fig. 1. Z-scores for differences between rank correlation results for various LSMs and an API baseline.