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Interactive comment on "Using Satellite-Based Evapotranspiration Estimates to Improve the Structure of a Simple Conceptual Rainfall-Runoff Model" *by* Tirthankar Roy et al.

Tirthankar Roy et al.

royt@email.arizona.edu

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We thank the reviewer for reviewing our manuscript and providing his/her valuable feedbacks. We have now addressed all of his/her comments and discussed them in the following. As the reviewer mentioned, there were some places in the manuscript which created confusions and the concepts seemed circular. We agree with the reviewer on that. These were mainly due to the lack of sufficient care in the use of terminology. We have revised the manuscript to resolve these issues and make our message more clear-cut. Thanks to the reviewer's feedback, the paper is now much improved.

NOTE: [1] The manuscript with tracked changes is uploaded in the form of a supplement. [2] Page and line numbers mentioned in the response correspond to the revised

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Discussion paper

manuscript. [3] We have added an additional figure (Fig. 4) to demonstrate the structure of the model.

Reviewer Comment 1: This paper presents results from a study examining the use of satellite estimates of actual evapotranspiration (SET) to firstly constrain and secondly modify a HyMod model of Nyangores River Basin in Kenya. Although the ideas presented here are interesting, I found that the reasoning used in the study was circular and I'm not convinced by the results. I think the presentation of the material is too much like a report and the method and results are often mixed up, with the vast majority of the method discussion provided in Section 3 which is nominally the results section. The paper also refers to another publication in preparation by the same authors on this catchment and without seeing this it is difficult to understand the similarity and any potential overlaps between the two publications. It's not clear why this paper would be presented first. I recommend that the paper is rejected and the authors undertake more extensive validation of the method in a catchment where there is data other than the SET to allow comparisons.

Author Response 1: The manuscript is designed such that all the analyses steps are clearly stated and their results are thoroughly discussed. This is important since we recommend this approach for similar investigations, due to the fact that it's inclusive. It takes into account several important issues, including process constraining, use of constraint adjustment, usefulness of model (re)calibration, information assimilation (from satellite-based sources), diagnostic model structural improvement, and uncertainty analysis. However, as pointed out by the reviewer, we do see that some method discussions could be removed from the results section and put back to the methods section itself. We have now taken care of this issue in our revised manuscript.

Regarding the point on the second publication, we do have another manuscript under review, however, we would like to clarify that the objective and scope of that manuscript are quite different as compared to this one. That manuscript reports on the development of a multi-model and multi-product (satellite)-based probabilistic operational streamflow forecasting platform for sparsely-gauged basins and does not in any way address the problem of model structural correction/improvement. We are ready to share the manuscript with the reviewer and editor personally if necessary to resolve this concern.

Since the other manuscript is under review, we are not citing that anymore in this manuscript.

Regarding the comment on the other available data for comparison, note that the dataset (GLEAM) we are using has already been validated in several recent studies. Although we didn't include the detailed discussion on validation in our initial manuscript, we have now included that part in our revised manuscript (Page 2 Line 21 - Page 3 Line 4). GLEAM has already been evaluated both at local (eddy-covariance towers) and global scales. There have been projects that have focused on the topic of the evaluation of GLEAM, e.g. The WAter Cycle Multi-mission Observation Strategy-EvapoTranspiration (WACMOS-ET), Global Energy and Water Cycle Exchanges (GEWEX) LandFlux Project, etc.

All the studies cited in our revised manuscript (one book chapter and four paper) found GLEAM to be one of the best ET products. Therefore, we don't think it is necessary to carry out an additional evaluation of GLEAM, given the fact that other studies have already focused on that part. This also does not fit well with the main goals of this manuscript. Moreover, an evaluation study of this kind would stand out on its own as an independent paper, which is clearly beyond the scope of this manuscript.

To be clear, the main objective of this study is NOT to validate/compare actual ET products, which is an interesting topic, but appropriate for a different manuscript. In this study, we explore different structure-related methods (including process constraining) to improve the performance of a rainfall-runoff model, and we have an inclusive design to organize all the steps in a systematic manner. We show how the model deficiencies could be overcome by using new sources of information.

Reviewer Comment 2: If I understand the method properly, in Case 1 HyMod is run and the AET from the model is found to be different from the SET estimates. So the model is run using SET to constrain the AET in the model by setting the requirement that the AET <= SET. However then the model parameters are found to be unrealistic so the SET is bias corrected so that when the model is constrained to have AET <= SET, the model parameters are more realistic. In all of this there is no evaluation of the SET itself and the bias correction step implies that there are problems with the SET. So you're trying to match a model to a biased quantity and then changing that quantity and then still trying to match it. It just seems very circular to me. Case 2 follows much the same logic except rather that using the constraint that AET <= SET, the model structure is changed with a variety of different equations that factor the evaporative demand ratio. Finally in Figure 9 the model is compared back to the SET which was used to correct the model I just don't understand how you can accept the SET data without having an external validation. I accept that this is unlikely to exist for the catchment you have chosen but I think you then need to test your method in a more instrumented catchment where you do have external validation data and once you have confidence in the method then you can apply to a poorly gauged basin.

Author Response 2: This is a very interesting point which we unfortunately did not explain well in the original manuscript. We should point out that there is no 'bias correction' in this study. For a proper correction, we need the 'ground truth' which we don't have in our case for ET. Therefore, the term 'bias-correction' was wrongly used and we have now changed that. We are now calling it 'constraint adjustment' because that is what it is actually doing. In Stage 1, the model structure is fixed. When GAET is used as a constraint in the ET process within the model, it introduces bias in the streamflow. Therefore, we adjust the constraint such that that bias is removed. Note that this is NOT indicative of the presence of any actual bias within the GAET estimates. The constraint adjustment factor is a model "parameter" which corresponds to the structural deficiencies of the model. It may or may not be necessary as the structure changes. In Stage 2, we saw that when the structure was improved (deficiencies reduced), ET constraining became irrelevant.

Regarding the point of external validation, please see the last three paragraphs of our first response.

Reviewer Comment 3: Page 2 – paragraph 3 – at this stage its not clear how ET can be a model target – I think you need to make it clearer at this point that PET is forcing data and AET is a model state.

Author Response 3: We consider precipitation and PET as the forcings. Note that the precipitation is the only input to the water budget of the model, PET is a constraint to set the upper limit of the actual ET. The model produces both discharge and actual ET as outputs. Therefore, we don't see why AET needs to be considered as a model "state" (as conventionally defined). It is clearly a model simulated "output".

Reviewer Comment 4: Page 2, line 15 – good correlation of the SET does not give me confidence that the property is not biased which is key for this method and even line 23 where the annual bias is low doesn't guarantee that there are not other biases that are cancelling out throughout the year.

Author Response 4: We agree we had a very brief discussion on the comparison/validation of the ET products in our initial manuscript. We have now expanded that discussion in our revised manuscript, where some additional error statistics (apart from correlation coefficient) are also reported.

Page 2 Line 21 – Page 3 Line 4 "... Worldwide evaluations suggest that satellite-based ET estimates are strongly correlated (~0.83) with ground-based observations made at flux towers (Demaria and Serrat-Capdevila, 2016). We use the Global Land Evaporation Amsterdam Model (GLEAM) as the source of the satellite-based ET (SET) data for this study. In GLEAM algorithm, ET is computed using only a small number of satellite-based inputs, which is largely beneficial for sparsely gauged basins. Miralles et al. (2011) have shown that GLEAM estimates of evaporation are strongly correlated (0.80) with annual cumulative evaporation estimated via eddy covariance at 43 stations, and have very low (-5%) average bias. The correlations at individual stations are strong (0.83) for all vegetation and climate conditions, and improve to 0.9 for monthly time series (Miralles et al., 2011). McCabe et al. (2016) have found satisfactory statistical performance (R2 = 0.68; Root Mean Square Difference = 64 $Wm{-2}$; Nash-Sutcliffe Efficiency = 0.62) of GLEAM while compared against the data from 45 globally-distributed eddy-covariance stations. Michel et al. (2016) compared Priestley-Taylor Jet Propulsion Laboratory model (PT-JPL), Moderate Resolution Imaging Spectroradiometer evaporation product (PM-MOD), Surface Energy Balance System (SEBS), and GLEAM simulations against 22 FLUXNET tower-based flux observations and found GLEAM and PT-JPL to be more closely matching the in-situ observations for the selection of towers and the reference period (2005-2007). Their extended analysis with 85 towers had similar overall outcomes. Miralles et al. (2016) compared three process-based ET methods (PM-MOD, GLEAM and PT-JPL) against surface water balance from 837 globally distributed catchments, and reported that GLEAM and PT-JPL provide more realistic estimates of ET. They found these two products to provide superior overall performance for most ecosystem and climate regimes, while PM-MOD tends to underestimate the flux in tropics and subtropics."

Reviewer Comment 5: Page 4 – paragraph 12 – TRMM data is no longer available so not clear why you say that it is available to near-present? The study period is not clear from Section 2 in any case.

Author Response 5: This is a good point. We have now included this information into our revised manuscript. We are using the TRMM Multi-Satellite Precipitation Analysis (TMPA-RT) dataset which is still available. This is a merged dataset. TRMM Microwave Imager (TMI) was a part of it, which is no more operational (since 8 April 2015) because of fuel and battery issues with the satellite. As mentioned by the developers, the absence of TRMM is not crucial to the production of TMPA and TMPA-RT data.

We discuss the time periods in Section 2.5:

"The model was run continuously for the 7.5-year period Jan 2003 to June 2010, with the first 4 years (2003 to 2006) used for calibration and the remaining 3.5 years (2007 to mid-2010) used to provide an additional assessment of model performance. Results are shown for the "calibration (4-years)", "evaluation (3.5 years)" and "total (7.5 years)" simulation periods."

Reviewer Comment 6: Page 4, line 34 – here you describe Stage 1 as "constraining" and you are at pains to point out that it is not assimilation and yet in the remainder of the manuscript you continue to use the term assimilation – I think you need to be more careful with the terminology e.g. Page 8, line 23; Page 12, Line 24

Author Response 6: Thank you for pointing this out. We have now removed the term 'assimilation' wherever required.

Reviewer Comment 7: Page 5, Step 1-2 – given this is the method section, there are no details here of the actual constraints. These are provided in the results section. I think this makes the presentation quite confused and doesn't provide the reader with much of a sign post or guide as where the research is heading. Similar comments for Step II-1 where the four equations are mentioned.

Author Response 7: We agree with the reviewer on this and have now restructured the methods and results sections in our revised manuscript.

Reviewer Comment 8: Page 7, Line 24 – I don't understand why you validate your water balance using satellite precipitation which has its own concerns. Why not use some ground based data as well?

Author Response 8: Note that the TMPA data used in this study has been bias corrected using rain gauge measurements from the study area. The detailed methodology is discussed in the other paper. As mentioned earlier, we are ready to share the other manuscript personally with the reviewer or the editor.

Reviewer Comment 9: Page 7, Line 27 – "based on our expectation of how it would behave" – this comes to my concern about the validation. We generally expect a more robust validation than just a sense that the soil moisture should be smooth. Why should it be smooth for this catchment? You don't appear to have any soil moisture data to validate this statement.

Author Response 9: Thanks for pointing this out. We agree that in order to make this statement, the soil moisture data need to be studied first. Therefore, we have now removed this sentence from our revised manuscript.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-413/hess-2016-413-AC2-supplement.pdf

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