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Discussions

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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[REVIEWER COMMENT 1] The paper deals with the use of satellite-based evapotranspiration estimates (GAET) to improve results of a simple hydrological model. The general idea of the paper is sound and potentially useful for the hydrological community. Unfortunately, I see a number of problems with the paper. The main problem for me is the unclear rationale of the methodology. GAET is used in two ways to improve the hydrological model, and the two procedures have problems.

[AUTHOR COMMENT 1] We thank the reviewer for his/her valuable comments and

acknowledging the importance and relevance of our paper by stating that 'the general idea of the paper is sound and potentially useful for the hydrological community'. We have now thoroughly addressed all of his/her comments and concerns in our response.

[REVIEWER COMMENT 2] The first procedure "constrains" the hydrological model estimates of evapotranspiration HAET forcing them to be more similar to HAET. This is done in a very prescriptive way, and to some extent may contradict the whole physical basis of the model. The results of this exercise are not successful, as shown by the poor performance of the model in terms of streamflow. There are other ways of constraining intermediate model results, which are more formal and do not compromise the model physics (for example calibration optimization with side constrains). I believe that the first procedure does not present any novelty in terms of ideas or techniques. A thorough justification of why it should be included in the paper based on similar procedures applied successfully elsewhere is needed here.

[AUTHOR COMMENT 2] The reviewer expressed two main concerns in this paragraph. First, the way the method is implemented and second, the performance improvements. We fail to agree with the reviewer on either of them.

Regarding the first point, our constraining scheme is conceptually analogous to any filtering technique, where the main goal is to fix the behavior of the model, not its structure/process parameterization. The model state at any time step is adjusted based on the observation from that time step so that the model behaves more 'accurately'. A filtering cannot directly correct the model structure. Likewise, in our constraining approach, we try to fix the model behavior without modifying its structure. We modify the structure diagnostically in the next step (Step II).

The constraining approach corrects the model behavior in a physically-based manner (using new information from the satellite-based actual ET, GLEAM), which is exactly what we want. The water balance, as expected, is also preserved. Therefore, we don't agree that the constraining approach contradicts the physical basis of the model. To our opinion, it actually corrects the model behavior.

The reviewer mentions ‘calibration optimization’, however we are not sure what he meant by that. Calibration itself is optimization. To note, we are already performing calibration (using SCE-UA which is a global optimization algorithm) using two different types of constraints, one on the parameters (their ranges) and the other on the ET process. This should result into a more physically-consistent model and not ‘contradict the physical basis of the model’.

We think that the constraining is an important part of the paper and should remain in it.

Regarding the second point, we did have performance improvement in constraining, although not as much as the second approach, where we changed the model structure. In Table 5 (revised manuscript) it can be seen that in many cases the error statistics improve from Step-I to Step-IV. In calibration, NMSE changes from 0.56 to 0.43, NBσ changes from -0.12 to -0.06, and R changes from 0.76 to 0.83. In Figure 9 (revised manuscript), we do see improvements in the streamflow simulations (compare the blue and green lines in the transformed space to see the improvements more clearly).

[REVIEWER COMMENT 3] The second procedure modifies the structure of the model by multiplying the ET equation by a factor. Different formulations are used for the factor, which try to capture more of the physics of the problem. This last point is not clearly explained or justified by the authors. The formulations are tested against GAET estimates and the more complex formulation gives the best results. That formulation is then used to predict discharges, which shows some improvement of the model results. A major problem with the procedure is that the model produces a value of the soil moisture storage capacity H that is totally unrealistic (H=12.8 m). The authors do not report the value of H for the original model without the “improvements” using GAET, but my impression is that it may have been more physically adequate. I think that

the author’s claim about the advantage of physically-based over data-driven models is weakened by this outcome.

[AUTHOR COMMENT 3] We thank the reviewer for helping us improve our explanations. As suggested, we have now included the idea (i.e. the formulations ‘try to capture more of the physics of the problem’) into our discussion.

There has been a misunderstanding here. The reviewer pointed out an H value of 12.8 m, however, our final model DID NOT produce an H value of 12.8 m. Please refer to Appendix C where we provided a table with all the calibrated parameters. The column with the final model in the second step (Step II-1d) shows an H value of 866 mm or 0.866 m, which is conceptually realistic. The value the reviewer mentioned is from another step (Step II-3) which is not the final step. Please note that the H values from all different models (including the original one) are already reported in the table in Appendix C.

Let us try to explain our methodology once again. There is a reason why we designed the study the way it is there in the paper. It is an inclusive design that unifies several different aspects. We have two main steps, Step-I and Step-II. Step-I is based on process constraining, whereas Step-II is based on diagnostic structural modification. In each step, we have five main sub-steps (1 to 5). The first sub-step (Step I-1) is the benchmark (calibrated model WITHOUT constraining or structural modification), the second (Step I-2) is based on imposing the ET constraint on the model ET process without any recalibration (note we still use the calibrated model from the benchmark but we don’t recalibrate it), the third (Step I-3) is based on recalibrating the model with ET constraint, the fourth (Step I-4) is based on constraint adjustment, and finally in the fifth (Step I-5) we remove the ET constraint to see how sensitive the performance of the new model is when the satellite ET data become unavailable (note this is no longer the benchmark model since we recalibrated the parameters in Step I-4). We follow the exact same steps in Step II (Step II-1, Step II-2, Step II-3, Step II-4, and Step II-5). The only change here is that we have four more sub-steps (a-d) in Step II-1. These are based on the different structures we used. We select the best structure from Step II-1 and then carry out the other steps to see if there is any benefit in constraining the modified model itself. Our results indicate that there isn't much benefit in constraining the modified model and so we select the model with the best structure (without constraining) from Step II-1. Thus, our final model is the one from Step II-1d.

[REVIEWER COMMENT 4] In the middle of all this there are a number of methodological details that are also of concern. For example, model calibration is done using the SCE-UA algorithm, which essentially consists of a global optimization method. Since the formulation of the second procedure involves more calibration parameters, how does that affect the optimization?

[AUTHOR COMMENT 4] All the calibration runs were carried out with the same settings of SCE-UA, i.e. with the same number of complex and loops, in order to nullify the effects of the optimization algorithm itself. The calibration runs were successful in all the cases. We have already reported the calibrated parameters from all different calibration runs in the appendix.

[REVIEWER COMMENT 5] Also, there are ways of optimizing parameters with constrains that could be explored as a more formal way of incorporating the additional information from the GAET.

[AUTHOR COMMENT 5] We have already addressed the issue of calibration with constraints in one of our previous paragraphs (see Author Comment 2). The constraint could be on the parameters or it could be on the processes. We are already applying constraints on the parameters by setting their limits. We are also imposing constraints on the ET process within the model using the satellite ET data. Regarding constraining with ET, we found two very good papers (Winsemius et al., 2008, van Emmerik et al.,

2015, already cited in our paper) where the authors constrain the model parameters sensitive to ET using the ET data. Note that in this study our approach (and goal) is different. We impose the constraint on the ET process and also modify the model structure.

[REVIEWER COMMENT 6] Organization is also an issue. There is material in the results that should be in the methods (for example most of 3.1.2. in the results is about how to implement the "constrain" in the model and should be moved to 2.4. study approach). There is also an excessive use of subtitles and dot point type paragraph, which results in a lack of flow throughout the paper.

[AUTHOR COMMENT 6] We thank the reviewer for pointing this out. This was also pointed out by Reviewer #2. We have already taken care of this issue in the revised manuscript (also uploaded on the discussion forum).

[REVIEWER COMMENT 7] One lingering question that I have after reading the paper is why this new methodology was used in a study case with limited data and not on a catchment with extensive data where more verification and checks could be done. After all, the essence of the paper to me is the new formulation to improve an existing hydrological model and from that point of view a better set of data for validation is necessary. I would also add that the application to just one catchment may not be enough to demonstrate that the new formulation is better.

[AUTHOR COMMENT 7] Please note that one of the main purposes of this study is to develop models for sparsely-gauged basins. That is why we are using the satellite-based actual ET data in the first place. In a well instrumented catchment we could instead use flux tower data directly. A method/model that has worked well in a highly-instrumented catchment doesn't necessarily guarantee that it will also work well in a sparsely-gauged catchment.

The main focus of our project (NASA SERVIR) has been in solving water resources problems in sparsely-gauged basins using observations from space, and our current study is well aligned with the objective and the scope of the project.

We understand the usefulness of testing a model/method in multiple catchments (see the paper Gupta et al. (2014, HESS) by one of the coauthors of this paper), however, doing that was beyond the scope of this study. A rigorous testing is one of our future plans.

Please refer to the paragraph in '4.3 Overall Outlook' where we discuss this issue:

'Note that this study is based on testing of a single catchment scale conceptual rainfall-runoff model on a single basin, using a single satellite-based precipitation product and a single satellite-based AET product. While not demonstrating universal applicability, the results are clearly indicative and the methodology illustrates how such data can be used to investigate potential improvements to the structures of simple catchment scale models used for hydrologic studies in data scarce regions. For more detailed process-based models, the ET process parameters can be calibrated against some reliable SET estimates (e.g. GLEAM), or the process representation itself can be improved by adapting some similar strategies the SET products are based on.'

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-413/hess-2016-413-AC10-supplement.pdf

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