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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.

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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.

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.

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.

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? 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.

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.

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.

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