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Hydrology and Earth System Sciences Discussions

Interactive comment on "A multitemporal remote sensing approach to parsimonious streamflow modeling in a southcentral Texas watershed, USA" *by* B. P. Weissling et al.

Anonymous Referee #2

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General Overview: Before I begin to elaborate on my review, I would like to highlight to the editor and the authors that remote sensing of biogeophysical parameters (other than hydrologic remote sensing) is not my area of know-how. My understanding is that the work of the authors is significantly applied to require only a general understanding of this particular domain of remote sensing for a useful review.

At this stage, there are a sufficient number of conceptual weaknesses of the paper that I feel is necessary to highlight (rather than a detailed review). These border on the philosophical nature/motivation of the study. The spirit of authors is good - they address a long realised problem of not having adequate AMC for streamflow modeling,

given how sensitive and non-linear a watershed is to this condition in its rainfall-runoff transformation. Having said that, I think the objective of the authors to develop a statistical regression model based on MODIS parameters as a proxy for soil moisture is like a step backwards in hydrologic science, but perhaps temporarily a step forward in bridging current practical gaps with data for real-time decision making.

Two critical questions came to my mind while reading:

Question 1: Why not use the more frequently available rainfall data from remotely sensed platform in land surface models that simultaneously solve energy and mass balance for a derivation of the soil moisture condition in the effective soil column?

Question 2: Remotely sensed soil moisture is available from AMSR-E, of course the frequency may not adequate for dynamic modeling of flood events. But have the authors assessed their approach in context of what is already available to present a compelling justification of a 'physically backward' multi-regression model?

Comments:

1) In general, the paper does not very clearly articulate the scientific objective of the study and present the critical science question that is being answered here. One has to sieve through the paper and that leaves a lot to the guesses of the reader. I think the authors need to work a lot more on honing their introduction and motivation in to a more crisp stage.

2) Details about the regression model development are very fuzzy - the model itself is not mathematically formulated in the paper. I was hoping to see how that statistical model would look like as a y=f(x) type function.

3) Why are all assessments done in volumes and not in fluxes (L/T)? For streamflow modeling, being dynamic as it is it, wouldn't assessment of time to peak, runoff volume, peak runoff etc. be more worthwhile to the hydrologist who is considering using the author's approach?

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4) The exponential decay function for curve numbers between AMC classes is not well spilled out. At least I couldn't figure out how exactly the smoothening was done functionally (see Figure 3). Authors should address this. But, the smoothening is a good idea! It would be nice to see how much more physically realistic the CN simulations become with this approach?

5) Table of comparative performance is needed. Authors are using their calibrated CN method as the benchmark to assess the value of the regression model that uses as input the MODIS type variables and precip to estimate Q.

Minor comments:

6) Typo in Figure 1 - 'predominantly' 7) Figure 2, the precip hyeotograph on the upper x-axis is disproportionately larger than streamflow bar chart. Also, Q is better off shown as a smooth line, after all it is a continuous random variable, as opposed to precip that has intermittency.

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