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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 #4

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General: The paper addressed a common problem facing the modelers of rainfall runoff processes that is the difficulty in quantitatively determining the spatial and temporal distribution of soil moisture condition antecedent to a rainfall runoff event. The study assessed the potential to parameterize a regression streamflow prediction model using precipitation records and multi-temporal remotely sensed biophysical variables. The calibrated results of the regression model were compared with the SCS CN model on a 1420km2 rural watershed in southcentral Texas. The comparison was made on an 8-day interval in one year. The attempt is encouraging which provides an alternative way in solving the difficult problem mentioned above. However, I find the study as presented in the paper is not adequate and fails to verify the authors' argument that a

watershed's streamflow can be reasonably estimated from readily available temporally continuous remote sensing parameters and a precipitation record.

Specific: Besides the comments raised by reviewer 2, I have the following comments and questions for the moment:

1. The study was conducted on a single watershed in a single year. It is well known that the rainfall and runoff processes and their relationship vary greatly both spatially and temporally. Any results or conclusion drawn based on a single year cannot mean much.

2. The multiple regression model containing three independent variables and four regression parameters was only calibrated on a single year runoff data on an 8-day interval. If it would be calibrated on another year or on a longer record the parameter values would differ very much. Then, what is the use of the calibrated regression model?

3. More importantly, the calibrated model was not verified on an independent time period or another watershed. The proxy basin test and split-sample test are the very very basic requirement for any model that will be useful, and this is not done.

4. Taking into account of the uncertainties involved in determining the three independent variables (P, EVI, T) and the uncertainties of the four regression parameters, great uncertainties in the simulation results would be expected when the model is used for simulation of runoff in an independent period or on another watershed. These uncertainties are neither studied nor discussed.

5. The usefulness of any hydrological model is not its ability in reproducing historical records which are already available, the model is used to the following three conditions, none of them was tested in the present study:

(a)Filling up the gaps of the flow record or extending it.

(b)Simulation/forecasting of the future event or record under stationary and/or changing climate.

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(c)Generating flow series at ungauged site/watershed.

6. The calibrated runoff of the regression model rather than an independent simulation period was compared with the SCS CN model. It is well known that the CN based model does not provide better or even equally well results as compared with any rainfall runoff model, with which the parameter values are calibrated. The advantage of the CN model is not its better accuracy (it is of course generally ok); applicability to any catchment without a need of calibration is its strength.

I think lots of work need to be done before it becomes good enough for publication.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 1, 2007.