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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.

B. P. Weissling et al.

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General

The three reviewers of this paper all agreed that our efforts to address the long-standing problem of a watershed's runoff response to antecedent moisture condition through the development of a minimally parameterized, operational streamflow model based on remote sensing and precipitation data is both encouraging and potentially useful if
a. the model were sufficiently developed and validated with other time periods and with a proxy watershed
b. the model parameters (land surface temperature and EVI) were physically interpreted in the context of the spatial-temporal variability of soil moisture

c. the authors were to provide a more concise and articulate description of the motivation and objectives of this study and the science question that is to be answered.
The authors wish to thank the reviewers for the thoughtful and critical analysis of this discussion paper as we feel that such criticism will ultimately serve to improve both our model and the subsequent reporting of it.

As a framework for responding to the three reviewer's comments and criticisms we will take this opportunity to clarify our motivation and objectives of this study. Our study originated with an attempt to improve on the generalized 5-day antecedent moisture condition model so commonly employed in the NRCS curve number method for runoff estimation. We postulated that remote sensing could offer both a spatial and temporal component to the characterization of landscape moisture status, potentially adding another distributed dimension to the curve number method in addition to soils and land use land cover data. We were not striving to develop an end-all hydrologic model for this particular watershed, recognizing (but not adequately clarifying in the paper) the limitations of this approach for general streamflow modeling. Instead, we endeavored to demonstrate, for a particular calendar year of precipitation/streamflow event data, that at least two biophysical parameters derived from remote sensing imagery were capable of explaining a significant percentage of the variability of streamflow response to precipitation events. However, if demonstrated to be reasonably successful by validation with other time periods, we believe that this approach has ramifications for water balance modeling, modeling recharge potential, extending the flow records of watersheds with discontinued gauge stations, or providing a reasonable alternative to modeling streamflow in ungauged watersheds (proximate to a watershed for which the estimation equation is built and validated) when the CN method is not applicable.

Reviewer 1

The reviewer points out that the manuscript read too much like a logbook, was not concise, and that insufficient attention was given to the review of watershed modeling literature.

We agree that the manuscript needs improvement and we'll make every effort to clarify both the general text as well as the research study objectives. We realize that our review of watershed modeling was insufficient, in particular as regards to the modeling of antecedent moisture condition (AMC) and the role of AMC characterization in the rainfall/runoff transformation process.

The reviewer expressed disappointment in our selection of the NRCS curve number (CN) method as a standard of comparison to our model although he acknowledged that our model might be useful in regions of the world where the CN method could not be employed due to a lack of data. The reviewer also questioned the validity of a regression model approach in ungauged watersheds because observed streamflow is required.

We chose the NRCS curve number method as a basis of comparison to our remote sensing/AMC modeling approach for two reasons. One, the CN method, in its modern implementation, is dependent on the spatial characterization of land use land cover (LULC) through remote sensing techniques. Second, the method is dependent on an antecedent moisture condition model that has no spatial component and only a crude temporal component - issues that we felt our model could perhaps improve upon. For these reasons, and for the simple fact that the CN method is commonly employed to transform rainfall to runoff in a variety of streamflow models, we felt that the comparison was both valid and appropriate.

It is not entirely true, in our opinion, that it would not be possible to develop the model for ungauged watersheds given the pre-requisite for regressing observed streamflow.

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Within a given basin, even in developed countries, it is probably not feasible and certainly not economically justifiable to gauge all catchments of even appreciable size - greater that $500 km^2$ for example. Moreover, in times of shrinking budgets, the trend is toward de-commissioning gauge stations as opposed to building new ones. Within the Guadalupe River basin, of which Sandies Creek is a part, there are regionally proximate ungauged watersheds with similar climatology and land use land cover (LULC). With model testing of our Sandies Creek streamflow estimation equation on neighboring gauged watersheds, along with an evaluation of how regression coefficients might change for new regression models for these watersheds (considering varying spatial dimension), it is certainly possible that a reasonable empirical equation could be developed for ungauged regionally-proximate watersheds. Of course, if a CN method is an option, one might ask what is the point of our model. Soils data for many parts of the world are incomplete or non-existent. Moreover, the other dataset essential for a proper application of the curve number, LULC data, as provided by the Landsat program, is currently threatened by a crippled Landsat 7 satellite, and an aging Landsat 5 satellite - both satellites serving as the foundation of landcover imaging (at the spatial resolution necessary for assessing LULC). There is currently no Landsat replacement waiting to be launched. The uncertainties of future LULC monitoring and the respective impacts on a common empirical approach to runoff modeling (the CN method) suggest that alternate methods of parsimonious streamflow estimation must be explored and evaluated. Our approach represents basic research into the potential of high temporal resolution imagery and ultimately remote sensing derived precipitation to elucidate the runoff response of a watershed to precipitation events. This paper represents the first steps of such research.

The reviewer commented that the 8-day timestep employed in our model renders the model useless in predicting peak flows and that our model really just describes water balances. The reviewer further expressed an interest in knowing how daily streamflow would relate to the predictor variables and if those relationships might degrade with

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increasing lag after the remote sensing data.

We agree that the 8-day time step, as currently employed in our model, is not conducive to peak flow determinations. While predicting peak flow would certainly be advantageous in a region characterized by high intensity rainfall, it is not the only hydrologic modeling objective. An 8-day time step could perhaps be entirely appropriate in modeling water balances, recharge potential, flood vulnerability, or water quality constituent loads. A recent USGS study, utilizing the HSPF conceptual model (USGS Water Investigations Report 03-4030, 2003), described a physically parameterized streamflow estimation model of five headwater catchments in the upper San Antonio River basin, a basin immediately adjacent to our own study area. This model estimated mean streamflow at catchment outlets, associated with active USGS gauging stations, from 1997 - 2001, on a 7-day timestep. The overall objective of the study was to estimate streamflow constituent loads at the catchment outlets in an assessment of water quality. The validity and use of a multi-day timestep clearly has ramifications beyond that of just describing water balances.

However, the 8-day time step represents a starting point for model development - a time step entirely based on the utility of finished data products from MODIS. Finer temporal resolution could be achieved by the generation of our own aggregate data products from MODIS Level 1 products (from daily raw reflectivity products). Unfortunately, a daily time step is not a reasonable goal as the daily precipitation/runoff events one is trying to model can't be modeled due to the inevitable gaps in data coverage as cloud cover prevents image acquisition.

The authors were criticized for failing to explore the physical links between land surface temperature, the vegetation index EVI, and streamflow. The reviewer suggested that the general applicability of the model be assessed in the context of our assertion that vegetation and ambient surface temperature retain a memory of antecedent moisture, as suggested by the regression results.

In hindsight, the authors acknowledge that our statement that the significant correlation serves to “confirm the results” of other studies that a “memory” of antecedent moisture is retained in the land surface temperature and vegetation was premature. Recognizing now that if such a “memory” does exist and its nature can, at the very least, be described statistically, then an exploration of the physical link between these descriptive parameters and streamflow should be more adequately developed in this study.

Finally, the reviewer expressed disappointment that the investigation of the relationship between MODIS data and hydrology was not fully explored, and suggested that other time series of reflectance other than prepackaged vegetation indices be explored for their potential links to the processes of interest.

In addition to an examination of MODIS land surface temperature and EVI products the authors investigated the Normalized Difference Vegetation Index (NDVI) and the Albedo products for their potential correlation to streamflow. These latter two products were not significant in the regression model for the period evaluated (calendar year 2004). At the time of submission of the manuscript, the authors were beginning an exploration of other spectrally relevant time series indices (user-derived rather than MODIS products) with potential for correlation to streamflow, albeit they too are predominately linked to the reflectance characteristics of vegetation and a vegetation canopy’s water content. It is precisely these vegetation indices that are of interest to this study for the fact that they are sensitive to spatial and temporal change in water balance. Given a catchment with little soil exposed to the synoptic eye of the remote sensor (and specific passive microwave applications), any investigation for the illusive soil moisture state of a landscape must necessarily lie with the vegetation canopy, land surface temperature, albedo, and other biophysical responses to the influx, interaction, reflectance, and emission of short and long-wave radiation.

Reviewer 2

Conceptually, this reviewer questioned the objective of the authors in developing a statistical model utilizing remote sensing parameters as a proxy for soil moisture. He felt that such an approach was a step backwards in hydrologic science.

It was not our intention to investigate MODIS biophysical parameters as a proxy for soil moisture. Our intention was to investigate high temporal resolution MODIS imagery for its ability to provide any additional information, indirect as it might be, on the spatial-temporal moisture status of the landscape. Since the vegetation canopy itself is coupled to soil moisture status and that changes in vegetation status (LAI, leaf water content, APAR, biomass) can be assessed from its spectral reflectance, it can be reasonably assumed that for a lumped parameter statistical model, that a metric of vegetation status (antecedent or synchronous with the precipitation event) will explain some percentage of variability in the rainfall runoff transformation. Land surface temperature from MODIS is another parameter coupled to soil moisture status, most likely from a surface heat flux shift from latent to sensible heat brought about by changes in evapotranspiration.

The reviewer posed the following questions.

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?

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For the first part of the question, the authors assume the reviewer is referring to NEXRAD radar as the remotely sensed rainfall data. Spatially distributed precipitation estimates certainly play a role in land surface energy and mass-balance models and such models can be used to back-calculate estimates of relative soil water content of the root-zone soil from relationships of ET and PET, such as in Pike's equation. However, it was not our goal to derive a soil water profile, but to evaluate the potential of remotely sensed biophysical parameters sensitive to landscape moisture condition to explain the variability of streamflow subsequent to precipitation events.

Question 2: Remotely sensed soil moisture is available from AMSR-E, of course the frequency may be 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?

AMSR-E soil moisture products have in fact demonstrated their utility in numerous hydrologic studies and have validated well with field scale soil moisture studies. However, passive microwave estimations of soil moisture is limited by masking effects of vegetation and landscape roughness - hence the majority of successful studies have been conducted in arid or semi-arid regions with minimal vegetation. The spatial resolution of all passive microwave sensors is also a considerable limitation. AMSR-E's pixel resolution of 25 km is really only appropriate for basin scale studies. So for a study watershed (Sandies Creek) comprising less than 3 AMSR-E pixels with minimal exposed soil surfaces, a passive microwave approach was not the most promising option. As described in this paper, a user-derived, 8-day, AMSR-E soil moisture product was evaluated in our model and was found to be insignificant. With a larger, less vegetated watershed, such as in the neighboring Nueces River basin in South Texas, AMSR-E data could be evaluated in this statistical approach.

In regards to a justification of our approach in context of what is already available, we acknowledge that the manuscript fell short on both a review of alternative modeling approaches and a clear concise explanation of the study objectives. While it may have appeared that the modeling of streamflow in Sandies Creek watershed was the primary objective, we should state now that that was secondary to demonstrating the potential of remote sensing to form the basis of a minimally parameterized streamflow estimation model. Developing a physical or conceptual model of the Sandies Creek watershed was certainly an option, if our objective had been just that. In this paper's revision we will strive to clarify our objectives and to temper our enthusiasm for this promising but not-yet-mature approach.

Minor 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.

We appreciate this comment. However many times we reread and revise our manuscript, we are inherently biased by what we've already written. We will work on more clearly articulating our objectives and making the prose more concise.

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.

Next revision we will strive to clarify the regression model development with a hydrologic interpretation of the parameters.

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?

A table of comparative performance for the three AMC models and the remote sensing model will be prepared in the next revision with fluxes reported in place of volumes.

With this 8-day time step (upon which the model is based), it would not be possible to generate "time to peaks" or "peak runoff" for a single precipitation event. Once again, our intention was not to develop a model that would compete with standard conceptual and physical approaches but to demonstrate the potential of a remote sensing approach to empirically estimating streamflow in a watershed for which there may be no reasonable alternate method.

4) The exponential decay function for curve numbers between AMC classes is not well spelled out. At least I couldn't figure out how exactly the smoothing was done functionally (see Figure 3). Authors should address this.

We followed a simple exponential smoothing scheme, implemented in an EXCEL spreadsheet, whereby past observations are weighted with exponentially decreasing weights to forecast future values.

For a time period *t*, the smoothed value S_t is found by computing

$S_t = \alpha y_{t-1} + (1-\alpha) S_{t-1}$
where α represents a damping factor, $0 < \alpha < 1$ and y represents the original observation

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We utilized a damping factor of α = 0.3.

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 precipitation to estimate Q.

Refer to author response in question 3 above.

6) Typo in Figure 1 - 'predominantly' 7) Figure 2, the precipitation hyetograph 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 precipitation that has intermittency.

These will be corrected in the revision.

Reviewer 4

Reviewer 4 acknowledged that our modeling approach is an encouraging one but that as presented fails to verify our argument that streamflow can be reasonably estimated from remote sensing parameters and a precipitation record. The reviewer's primary criticism was that the study was conducted on a single year data record, and that any results or conclusion drawn based on a single year cannot mean much.

The decision to report only the 2004 regression results was certainly not made based on any lack of understanding that the model would have to be validated and calibrated with other time periods. The inclusion and comparison of the CN model, a comparison that we felt was both informative and warranted, made for a long manuscript. The decision was made to report the validation phase (independent time periods and proxy watersheds) in a follow up paper. Given the obvious shortcomings of characterizing spatially varying precipitation within our watershed with one or two NWS gauge stations, we are currently re-building the model utilizing NEXRAD precipitation estimates for 2002 - 2005. The procedure for data-mining NEXRAD Stage 3 and/or MPE products for a specific time period and spatial location is a time consuming and computer-intensive process, requiring data reformatting, coordinate system reprojection, subsetting, aggregating, and quality control/assessment. To report on both the processing of these data and their results in the model, would have made the paper exceedingly long and unmanageable. However, given the concerns of this reviewer and reviewer 1, dropping the CN model comparison in lieu of validation results for other time periods is possibly warranted in the subsequent revision.

The reviewer questioned the validity of a regression model built on a single year, suggesting that if the model were built on a different year or on a longer period the regression parameters would differ considerably thus rendering the model useless.

The regression model was calibrated on a single year streamflow record as reported in the paper. We are currently testing and validating the model for 2002, 2003, and 2005 streamflow events. We did not attempt to report these ongoing efforts for the reasons described above. We do expect some variation of parameter estimates due to the outlier influence of extreme events, long periods of drought or excessive precipitation, or to the potential mismatches of precipitation events with their corresponding streamflow event. The latter is due to a precipitation event occurring in a different 8-day time period than the streamflow event, an unavoidable issue when aggregating to a multi-day time step. Several of the above-mentioned issues, as well as some preliminary validation, can be reported on in the ensuing revision of this paper.

Another concern of reviewer 4 was that the model was not verified on an independent watershed.

At this point in time, the authors have not conducted a validation of this model in a proxy watershed. We have chosen to focus our immediate efforts in model validation toward other time periods within the 2001 - 2006 period of MODIS data availability. As that work progresses, we will select at least two regionally proximate watersheds similar in both LULC and climate characteristics for further model validation.

Another issue raised by reviewer 4 was that the authors did not report on any uncertainty analysis of the three independent variables or of the four regression parameters.

We neglected to include any uncertainty analysis in this version of the paper, and we agree that the inclusion of such analysis is warranted. There is an inherent difficulty of assessing uncertainty with point-scale gauged precipitation measurements; at the very least we can report on the sensitivity of the estimation equation to varying precipitation. In regards to temperature, the MODIS land surface temperature product has been validated with an uncertainty of 1K. We can report on the sensitivity analysis of that product as well. Vegetation indices, as a pure construct of remote sensing, have no physical meaning and as such no proxy in the real world for comparative groundtruthing. So there exists no level of uncertainty in a metric of EVI. However, as with the other parameters evaluated we can provide a sensitivity analysis on the EVI product. In addition to the above, we can also report on the uncertainty of the regression parameters and the degree to which that uncertainty influences the overall modeled response for events distanced from the mean.

Reviewer 4 suggests that the usefulness of any hydrological model is not its ability in reproducing historical records which are already available, but that the model is used to the following three conditions,
(a) Filling up the gaps of the flow record or extending it.

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(b) Simulation/forecasting of the future event or record under stationary and/or changing climate.
(c) Generating flow series at ungauged site/watershed.

The three conditions listed as endproducts or goals of hydrologic models is, in our opinion, an incomplete list. One of our stated objectives is to extend this research toward the goal of generating flow series at ungauged regionally proximate watersheds. Even if we're unsuccessful there are multiple interim objectives (as stated in prior comments above), each of which could be considered prospective avenues of research to further hydrologic science.

The final comment of reviewer 4 concerns our use of the calibrated runoff of the regression model rather than an independent simulation period to compare with the NRCS CN model. The reviewer writes that,"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''

We compared our regression model results for 2004 with the CN model for the purposes of demonstrating that a remote sensing approach to empirically estimating streamflow is a distinct possibility. But we do agree that a better comparison would have been with a separate validated time period rather than the period upon which the calibration was built. Where the CN's applicability to any catchment is its strength, we feel that a reasonable estimation of streamflow based entirely on remote sensing is our model's strength.

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