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Hydrology and Earth System Sciences
Discussions

Interactive comment on "SWAT Modeling of Water Quantity and Quality in the Tennessee River Basin: Spatiotemporal Calibration and Validation" *by* G. Wang et al.

Anonymous Referee #2

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In this manuscript, the authors applied the SWAT model to the Tennessee River Basin to simulate water quantity and quality. Statistical modeling results were used to evaluate model performance. They found that model simulations were improved after parameter calibration. Correlation analyses were conducted to analyze the impacts of watershed attributes on water qualities. The authors have done lots of work in model simulation, calibration, and analysis. However, I think the manuscript needs to be substantially revised for publication. Here are my major concerns: First, I do not quite agree about the way how model performance evaluation is conducted. Although there are some difficulties in collecting observational data for model evaluation, field data should be the most valuable and reliable material for benchmarking. However, the

authors mainly used estimates from statistic models to calibrate and evaluate their modeling results. Since there are significant uncertainties in these statistic models, particularly LOADEST, comparing you model with these modeling results introduces additional uncertainties to calibration and validation of this work. As a result, the authors should include comparison with streamflow records, and concentrations of different elements from the USGS gauges in their work. Temporally explicit observations are limited, particularly for water quality variables, but the authors should at least compare the long-term averages. Second, although the authors claimed they did 'spatiotemporal 'calibration and validation in this work, I do not think this is well achieved. Only one gauge station was used, and I did not see any regional comparison maps between this study and SPARROW/regional runoff products. Instead, only regional means (figure 5) were compared and the spatial distribution of the selected variables from this work and the previous studies were not presented, which make it hard to validate results of this study. Third, spatial correlation analysis was not clearly introduced. I am wondering how the authors calculated the correlation coefficient? Did they use bivariate correlation or multiple linear regression? Did they consider the collinearity among the independent variables? Why only r was used to measure significance of the correlation, not P values? Finally, interpretation of the results, particularly the correlation analysis is insufficient. In addition to report significant correlations, the authors should explain the underlying mechanisms responsible for the correlation, and be cautious with non-causative correlations.

Specific comments:

Page 5, Line 97: but later you mentioned that only one site, close to the outlet of the basin, was used for model calibration Page 5, Line 106: you already provide the full name of this acronym on page 1. Page 7, Line 147: as far as I know daymet is a modeling dataset. Does it also provide original site level observation?

Table 1, it will be more helpful if you provide your calibrated parameter values, rather than providing the input file and fortran code.

Figure 6a is confusing. Consider to label variables in a different way.

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