

Interactive comment on “Assessing the model performance of an integrated hydrological and biogeochemical model for discharge and nitrate load predictions” by T. Pohlert et al.

Anonymous Referee #2

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In this paper, authors applied their integrated model (submitted elsewhere) to a rural catchment and compared simulated results with observations. Although their model application procedure seems adequate, this paper is not ready for publication in the present form. The biggest reason for this opinion is the lack of contribution. I do not see any significant scientific question addressed or novel concept/idea proposed. If the purpose of this article is simply 'to test a newly developed model to a real catchment,' then why don't authors present the results in their other paper (submitted elsewhere) which deals with the new model? Authors also should address following major and minor comments.

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Major comments

1. Comparison with earlier models. Page 2816, paragraph 2, authors listed earlier process-oriented models such as HBV-N and LASCAM. However, this paragraph has no justification of the necessity of the new model. What motivated authors to develop a new model unless they just wanted to develop 'another' model? What is the advantage of SWAT-N over these earlier models? Authors should include the comparison of the new model with earlier ones.

2. Questions in calibration. As explained in pg. 2825, log-transformed stream flow data is desired since low flow is quite important. However, authors used original data for calibration (in the SCE-UA algorithm). I would like to ask why don't they use log-transformed data for calibration? Authors may need to test the case with log-transformed data. In Table 1, authors listed the lower and upper limit of 6 calibrated parameters. However they do not provide the reference for these limits except stating 'physically reasonable spans (in pg. 2823, Line 28).' Validity of these limits is curious since calibrated values of 4 out of 6 parameters are very close to (or the same as) the upper limits. Authors should justify this.

3. Lacking interest. This paper is not particularly well written. Authors can do a better job in writing an interesting paper. As an example, authors should consider moving section 2.6 to the beginning of Ch.2. The focus of this paper is on the new model, SWAT-N. So, it is natural to inform readers this new model before going into the details of study area. The section 2.6 should be revised. It is not clear what is the difference between the new model and the original SWAT. Authors should clearly explain the differences instead of citing pieces of previous works.

Minor comments

1. Pg. 2818, Line 12: Please avoid the abbreviation 'a.s.l.'
2. Pg. 2824, Line 1 & 21: 'Only six parameters.' What are they?

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3. Pg. 2824, Line 25-28: It is not sure how authors varied the single parameter value over the entire basin. Explain more detail.

4. Pg. 2828, Line 3-7: I encourage authors to consider the fact that groundwater divide is not necessarily the same as surface watershed divide. In fact, they can be quite different. I guess that the model assumes groundwater divide the same as surface watershed divide (in each HRU). The consequence of this difference can be discussed.

5. Fig.4: Avoid duplicate figures. Remove either log-transformed figures (b and d) or figures with raw data (a and c).

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