Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/hess-2021-52-RC2, 2021

Comment on hess-2021-52

Anonymous Referee #2

Referee comment on "Assessing interannual variability in nitrogen sourcing and retention through hybrid Bayesian watershed modeling" by Jonathan W. Miller et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-52-RC2, 2021

There are several areas of the manuscript that need improvements which require Major Revision.

(1) The abstract notes that the main contribution addresses the point that the "statistical calibration of loading models does not always yield plausible results"(lines 10-11) but it was difficult to see this aspect addressed in the manuscript as the broader contribution of the manuscript beyond the study area. However, later in the introduction it appears the main contribution is to improve the understanding of nitrogen export specifically for the two highly managed basins in North Carolina, USA (lines 45-46) using a smaller study with more dense monitoring than a previous study (Strickling and Obenour, 2018) over the same area (lines 40-53). This, in my opinion, is the weakest part of the paper and potentially makes it unsuitable for HESS. Strengthening is needed in the introduction to understand what broader research gap is being filled here, given the manuscript expands on an existing model of the study area.

Later in lines 345-349, there are some statements that could indicate that these results could have potential for other studies related to nutrient loading due to agriculture. Perhaps normalizing your results by drainage area could make some of your results generalizable? In my reading, it seemed as though livestock played a smaller role because they occupy a smaller area of the basin.

(2) There are numerous areas where the methods are not fully explained or choices are not justified. A reader would not be able to reproduce the study from the details provided solely in the manuscript.

(a) Line 66: The minimum data requirements for WRTDS seem incorrect. Please provide a citation here to support where you found the requirements to be a minimum of 5 years and 50 water quality samples. The original WRTDS paper

(10.1111/j.1752-1688.2010.00482.x) states that one needs a minimum of 20 years and at least 200 samples.

(b) It is not clear what is the difference between incremental watershed and subwatershed throughout the manuscript (Section 2.3). What is each supposed to represent, in hydrologic terms? These need to be better defined.

(c) Line 145: Explain and justify why the period of record was split into two and why that is a good choice.

(d) Lines 149-151: How did you perform the preliminary analysis? I believe this belongs in the supplementary analysis.

(e) Provide justification for equation (1) and why this calculation is needed as part of the workflow.

(f) Section 2.7 needs much more explanation. This seems to be the novel part of the work and what is different from Strickling and Obenour (2018). It was difficult to understand how the coefficients are determined and how negative loads are accounted for.

(g) Line 203: I do not believe NHD+ is spelled out before its first use.

(h) Eqn. 6: I do not think all of the variables are defined after the equation.

(i) Eqn 7a: I could not find where the calculation for tau(z) is described.

(j) Line 212: I do not believe PIC is spelled out before its first use.

(k) Section 2.9: The selection of 4 different urban land-use splits is not well justified in the text. What is the hypothesis or scientific reasoning for the splits? Otherwise, it seems like the scenarios were made up with a trial and error to get the most attractive results.

(l) Lines 226-227: The “degree of overlap” was used to compare model fits but there is quantification given of this metric or objective reporting on why the particular scenario was selected.

(m) Section 3.1 needs references to figures or tables to support the statements made here with figure and panel references.

(n) Section 3.2: A statistical test appear to be mentioned here but the test is not described nor is the null hypothesis so a reader cannot evaluate the validity of the test or the results.

(o) Line 268: I do not believe EC is spelled out before its first use.

(p) Section 3.4: Few sentences offer supporting evidence for the statements made. Add references to figures or tables after each sentence to support these statements of fact. No proof appear

(3) The figures with multiple graphs need to have each panel labeled and referred to in the text. It was difficult to understand where to look for supporting evidence when only “Figure 2” is referenced but Figure 2 has 9 panels. This should be done for all figures with multiple panels.

(4) It was difficult to follow the justification for the role (or lack thereof) that both soils (Section 3.4) and aging infrastructure (Section 4.1) play in this analysis. There seems to be a lack of clear supporting evidence showing why or why not this is the case. There needs to be a more defined logical path in the text or these statements need to be removed.

(5) The data statement is no longer acceptable. It is now commonplace to have your data served on a publically available website. Even if HESS allows this outdated practice, major scientific organizations and publications - such as AGU - no longer allow statments that the data is available upon request. It is good practice - - to serve the data using its own doi or as supplementary material.

Minor comments:

Line 217: Change to “distributions”

Line 380: Change to read “At the same time, loading attributable…”