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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "A predictive model for spatio-temporal variability in stream water quality" *by* Danlu Guo et al.

Anonymous Referee #1

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This manuscript presents a Bayesian-based approach to analyze spatio-temporal variability in stream water quality. The approach is demonstrated with an application to a large set of monitoring data in Australia. Overall, I think the manuscript is well written and will become a worthwhile contribution to the hydrological community. The proposed method also has the potential of being applied to monitoring data elsewhere. I do have some major and specific comments for the authors, which I hope can help improve the manuscript. I recommend its publication after the following comments are addressed.

General comments:

1. On model applications: I recommend the authors to add a separate sub-section to provide some guidelines to potential users of the proposed approach, including at least

the computer running time of the model, the required no. of stations and required no. of water-quality samples for running the model, as well as approaches to evaluate if the model does a reasonable job.

2. On calibration/validation analysis: The authors randomly selected 80% of the sites for calibration and used the remaining 20% for validation, and repeated this validation process for five times for each constituent, in order to evaluate the sensitivity of the model to the monitoring sites. Could you justify the use of five times for each constituent? If this cannot be easily justified, I recommend the authors to increase the replicates from five to a larger number (say 30 or 50). The results may be summarized as boxplots instead of Table 2, which can provide an overall evaluation of the model's ability to capture the dynamics of the different constituents.

3. On the below-LOR data: The authors argue that the model performance is related to the proportions of below-LOR data. The results appear to support the argument that model works better when the proportion of below-LOR data is low. Can you further prove this? The authors may quantify the proportion of below-LOR data for each monitoring site and conduct a separate analysis for sites of low proportions vs. sites of high proportions (perhaps 50% of sites for each group?) and see if the performance varies significantly between the two groups. This analysis may be implemented for each constituent.

4. On monitoring data: In this pilot application of the proposed approach, water-quality variability is modeled based on monthly monitoring data. First, I think the authors have made a good point that high-temporal-resolution data can further strength the model capacity to explain temporal variability in water quality. Second, I think the approach's ability to reasonably capture that variability based on just monthly monitoring data is a big strength of the proposed approach. After all, a lot of the monitoring records at many locations are based on a monthly sampling scheme. This aspect should be more emphasized. Third, how about high-flow sampling? Many monitoring programs supplement regular sampling with targeted stormflow sampling to cap-

ture concentration variability during storm events (e.g., Chanat et al., 2016; Zhang et al., 2017). It is widely acknowledged that sediment and particulate constituents are heavily affected by storms. However, I cannot find any discussion of this aspect in the manuscript. Would you expect the models to be further improved if the monitoring data contain targeted stormflow samples? References: âĂć Chanat et al. (2016) (URL: http://dx.doi.org/10.3133/sir20155133) âĂć Zhang et al. (2017) (URL: https://doi.org/10.1016/j.jhydrol.2016.12.052)

5. On key controlling variables: Table S5 and Table S6 may be combined to a single table and moved to the main text. I think this information is critical and deserves to be placed in the main text.

Specific comments:

6. The term “filterable reactive phosphorus (FRP)” may be replaced with “soluble reactive phosphorus (SRP)”. I think the latter is more widely used.

7. L46: Add a few more references to support the argument “differ significantly”.

8. L56: Provide some specific examples on “other catchment conditions”. One could be antecedent condition, which is heavily discussed in the manuscript. In this regard, Zhang et al. (2017) (URL: https://doi.org/10.1016/j.jhydrol.2016.12.052) provides a study on how antecedent conditions affect the estimation of riverine constituent concentrations. This is also relevant to your discussion at L430.

9. L103-L107: These sentences can be removed. I think the subsection titles are already very clear.

10. Figure 1: Use a different color or a larger font for the dots to make them more clear.

11. L130: Add a few more references to support the argument “widely known to influence water quality condition”.

12. L131: “literature review” is vague. Could you briefly describe how it was con-

ducted?

13. L164: I do think one or two references should be provided for “Box-Cox transformation” to help readers. The meaning of the parameter lambda should be also briefly described.

14. L352: This ranking is roughly consistent with particular constituent vs. dissolved constituent. Any comment in this regard?

15. L366: The authors list here some processes for N. How about processes for P?

16. L206: What is the “Rhat” value? Please clarify.

Editorial comments:

17. L71: Fix the usage of “. . .not only. . .but also. . .” In addition, “limits” should be “limit”.

18. L76: The model built. . . –> The model was built. . .

19. Equation 3 and Equation 4: For the betas, consider using subscript instead of dash.

20. L180: “General speaking” –> “Generally speaking”

21. L317: Fix “a results of”

22. L382: Fix “oppourtunities”

23. L417: Fix “droguht”

24. L420: Similarly to –> Similar to

Comments on the SM:

25. Supplementary Materials lack of “title-page” information.

26. Table S4: Change “lambda” to its Greek form.

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