

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/hess-2021-621-RC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on hess-2021-621

Anonymous Referee #2

Referee comment on "Seasonal forecasting of lake water quality and algal bloom risk using a continuous Gaussian Bayesian network" by Leah A. Jackson-Blake et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-621-RC2>, 2022

General comments:

This study presents an application of a Continuous Bayesian Network (CBN) to seasonal (6-month average) algal forecasting in a northern lake. This is likely the first use of CBN for this purpose. In general, the model performs similarly to a traditional (discretized) BN and a naïve model (using the mean from the previous year). It could be a good fit for this special issue, but I do have several concerns, as outlined below.

I'm not really sure that there is a strong contribution, as the CBN does not perform particularly well. Also, the model appears to be based on existing software (an R package), so there isn't new methods development. If the objective of the study is to provide a thorough demonstration of CBNs for algal bloom modeling, that could potentially be an important contribution. In this case, I'd like to see more demonstrations of how the CBN approach (e.g., Figure 7) can be advantageous for studying a system or supporting management. In my opinion, the current discussion is too focused on skill assessment (e.g., R2), which probably doesn't do justice to the CBN approach. Also, probabilistic predictions using various linear covariates can also be obtained through multiple linear regression (frequentist or Bayesian), so why use a CBN? I think there are potentially good reasons for using a CBN, but they aren't compellingly demonstrated in the current manuscript. Also, I'd like to see more discussion of how this effort compares to other CBN (or BN) applications for water quality or environmental sciences, more broadly.

Major comments:

The paper includes a tangential analysis on making predictions at smaller time scales (e.g., Lines 208-215). I recommend removing this material, as it doesn't seem relevant to the main focus of this paper (no CBN was used). Furthermore, this additional analysis doesn't provide new insights (that aren't available through existing phytoplankton literature). It seems a bit "tacked on". If you do keep this analysis, the data should be presented (as in Figure 2 for the six-month model).

The variable selection process seems ad hoc (Section 3.1.1), making it somewhat hard to follow and likely difficult to reproduce. Some of the explanations seem questionable. For example, the article cites previous literature showing that "windier summers" are relevant, but the CBN uses winds from the previous 6 months (prior to summer), right? I have two general suggestions. First use clear and consistent terminology that clarifies which time periods you are talking about (also use consistent notation across the different figures and tables). Second, drop wind from the 6-month analysis altogether. Much of the text is a somewhat tortuous explanation (at least for this reader) of reasons to include/exclude wind speed, while in reality, the authors readily acknowledge that wind speed is only relevant at smaller time scales (e.g., Lines 443-445: "wind would likely only have an immediate and relatively short-lived effect..."), not ~6 months in advance.

Detail-oriented comments:

Line 11: Clarify in the abstract that you are predicting a May-October average (rather than daily predictions).

Line 20: The term “purely parameterized” is used multiple times throughout this manuscript, but I don’t understand what it means or how it is justified. As noted above, the parameterization process seems somewhat ad hoc to me.

Line 23: Suggest clarifying what is meant by a “naïve forecast” here.

Line 44: Models for Lake Erie cyanobacteria blooms (including Bayesian models) predict the maximum bloom size months in advance.

Line 56: Could you explain why “colour” is particularly relevant to water treatment or provide a reference?

Figure 1: Suggest including arrows to show dominant flow directions.

Table 2: Clarify what averaging periods were used.

Line 273: Clarify what normality test was used.

Figure 3, 4, 5: Clarify why only certain features are shown in each figure.

Table 4: The "Feature subset" column is confusing. Use consistent terminology and explain in the caption.

Line 370-371: Revise for clarity.

Line 422: Suggest "wind-related" instead of "related" for clarity.

Line 458: The term "credible" usually refers to the uncertainty in a parameter. It could be good to present actual parameter uncertainties (e.g., credible intervals). Also, I don't think relationships matching the simple bivariate correlations necessarily makes them "credible" in any sense. For example, see literature on Simpson's Paradox.

Line 470: Again, I'm not sure using simple bivariate correlations to evaluate a more sophisticated model makes sense.

Table 6: To me, making some numbers bold isn't effective for highlighting unexpected results. It really depends on which particular pair of numbers is being compared. Also, I wouldn't describe some of these relationships as a "physical" response.

Line 569: This statement seems too strong and/or requires clarification.

Line 644: This is clearly true (based on the general nature of a GBN), but it wasn't really explored in this study. I'm not sure why it is a conclusion.

Line 659: This seems like a bit of a stretch. I'm not sure that any "expert" can predict an extreme event ~6 months in advance. Maybe the authors mean something else, but I can't imagine what.