Reviewer #2

Summary

This manuscript evaluates a long time series of lake water quality to learn about controls of short- and long-term changes in DOC concentrations. This is a good fit for HESS and of interest to a broader water science community.

I like the methodological approach and find results convincing. I have, however, some major concerns: The introduction does not clearly lead to the objectives, the results need improvement on their presentations and the discussion often lacks the clear relation to the results. This needs improvement.

Moreover, consider this thought:

The model describes the dependencies of a set of water quality variables but is partly looking at a hen-egg problem. Are high DOC concentrations triggered by high TP concentrations within the lake or are they responding to the same water source coming into the lake? The GAM cannot clarify cause-effect relationships – so it can be used for system understanding but not for predictions. This can be made more clearer in the manuscript.

For more specific results see my comments below.

It is nice to learn that the reviewer sees the results of being convincing and the work as being of broad interest. We thank the reviewer for the encouraging feedback and useful questions raised. We will be explicit that the GAM is not intended to be used for making predictions.

Specific comments

Abstract

L9: Better be specific and name it “concentration“ trends or “flux” trends, if this is the better fit.

Yes, we can be specific about the type of trends.

L14ff: While the title puts stress on the novelty of the statistical approach, the abstract is not doing this. Would it make sense to add this aspect here?

Yes, we can include this in the abstract.

Introduction

I am not fully convinced by the line of argumentation in the introduction that does not define the lack of knowledge that is addressed by the objectives. Similarly the methodological approach is not clearly motivated by the problem that is addressed here. Finally, the hypothesis is not grounded in the state-of-the-art knowledge described before.

Yes, the hypothesis was also flagged by reviewer 1, and we will modify this. We can also refine the introduction to better lead the reader to the objectives we address through this work.

L29: I am not sure what “across systems” means here? Across regions? Across water compartments?

We will clarify that we mean across studies of different watersheds.

L32f: If “across regions” is meant, Temnerud and Winterdahl are maybe not the best fit as a reference as they look at Sweden only.

We can clarify here. The first part of this sentence is building on the previous, while the references relate to the search for common drivers.

L35ff: For hydrological changes this would be a potential additional reference: 10.1002/2017GB005749, for Nitrogen deposition this one: 10.1111/gcb.13758

Thanks for these suggestions, we will work to include them.

L47: Is landscape complexity the best term here? I understand landscape complexity more as the complexity and heterogeneity of a given landscape and not of complexity across landscapes. This may be termed more clearly.

Yes, thanks for this suggestion, we can differentiate between complexity and diversity here.

L59ff: DOM (why not DOC?) as a “master variable” and “limnological behavior” deserves more explanation.

It is more appropriate to use DOM as a master variable as it captures DOC and a broader assemblage. We can expand to be more explicit about limnological behaviour.

Methods

L84: What is the dominant land use here and is that related to the nutrient rich soils? Are there people living in the catchment and what happens to their wastewaters?

We will expand description to talk about the importance of agriculture as the dominant land use. There are no substantive sewage sources into the local area.

L85ff: Does the non-contributing areas are totally disconnected or connected via a groundwater pathway?

Groundwater flows are generally not important at the catchment scale here. We can clarify.

L90ff: Is there any regulation of water level/ flow at the dam of Buffalo Pond or is everything managed upstream only? Additionally, Eyebrow Lake is not mentioned in the text. Does this lake play a role in this complex lake system?

We can speak to Eyebrow Lake, which not a true lake (and thus a misnomer). This feature is considered in the derivation of Q-BP flows as the change of storage here is reflected in the flows entering Buffalo Pound from the upstream. We can also expand description of lake level regulation.

L97f: This last sentence does not make sense for me when not underpinned with facts and referenced.

We can include DOC info to better support this.

L109ff: I miss information how samples where taken and how often. How many samples are averaged on the mentioned monthly base and how was averaging done?

We will add a sentence on the collection procedure.

Fig. 1: No need to write where the middle of the lake is when coordinates are given on the axis. Maybe mention the yellow intake point in the captions and define its abbreviation.

We will make these changes to the caption.

L138ff: I am a bit puzzled by the flow reconstruction. Why is BP inflow the reference point since the catchment contributing to the water in the lake seems to be larger (Fig. 1, areas contributing downstream of BP inflow). This needs further explanation. I note that in lines 166-174 there is a section on ungauged flows downstream of BP inflow but it is not explained how this QLC was used in the analysis. I note that this is part of chapter 2.4. … for the sake of understanding I suggest to first describe what is needed for the analysis and then to describe how this data is constructed.

We can provide additional detail here to clarify, and have identified above in response to reviewer 1 a need for a revised equation on Q-LC which will help in this regard. In effect, our analysis looks at inflows sourced from and upstream reservoir (Q-LD), local catchment inflows (Q-LC), and the major water source upstream of the lake (Q-BP), which represents a combination of those flows, but one that is dominated by Q-LD. In short BP is an important reference point because it represents gauged inflow nearest to the upstream inflow to the lake. It would be a shortcoming to not consider each of these independently in our analysis. We will describe the what and why of why each is considered in the analysis in the revised manuscript.

L184f: This text would profit from an explanation why GAM has been used. I note that this is described later but you also justify the use of wavelet analysis here – no reason not to do it for GAM as well.

Good suggestion, we will provide an explanation for our use of GAM here alongside that for wavelet analysis.

Results

L262: Consider a different header here. “Temporal parameters” sounds not too good for me.

We can change this to “Flow and water chemistry patterns”

Fig. 2: At this multi-annual scale it is hard to see the timing of the seasonal dynamics. Panels a and b are, to my understanding, managed flows to meet the water demand while panel c is a natural seasonal dynamic. Any idea how to show these differences? Maybe by a plot as day of the year in the SI? Consider to use the same y-scale for all discharge plots.

We can include a plot in the SI similar to the one used for Fig 3b to show within year variation.

L273: DOC concentration is described after discharge in the text but shown in Fig. 3 after showing all other constituents. Maybe show DOC (as the master variable here) earlier?

We feel it is important to describe flows at the start as this system is unique both due to region and management regime, so we are not keen on reversing Fig 2 and 3, but will ponder on this.

L300-301 and 303-304: Two sentences saying the same thing here. Maybe combine both.

We can revise the text to make more concise here.

L325ff: The text reads as if it is given that there is a clear driver-response relationship between predictors and DOC concentrations. However, for the constituent you partly look at a set of potentially connected variables. E.g. TP and Chl a can both describe algae biomass. NH4 may occur because algae break down.

DOC may be excreted by algae. TP may decrease when flow increases due to a dilution effect of wastewater sources… All these interactions mean that predictors are not independent and you partly look at a hen-egg problem. This is more part of the discussion but I suggest to spent effort in this text to avoid this clear driver-response style of writing.

We can revise the text to be more intentional in our description as predictors rather than drivers (at L335).

L339: What is the <~7 mg/L referring to? Root mean squared error?

No, this is a threshold where we noted a change in model performance, as in S4. We will include 'threshold' to be clearer in this regard.

Fig. 5: Consider to keep same colors for same constituents across the figures. Impressive fit of the observation by the way.

Good suggestion, we will update colour scheme here to correspond to that used for DOC in Figure 3.

Discussion

L355ff: This chapter 4.1 reads like a summary and conclusions. You make statements that are justified in later chapters. I find it more appealing for the reader to first argue and discuss and then make statements. From this text alone, the reader does not know the basis of your statements (which analysis the statements are referring to and how they were interpreted).

We can include in this text more explicit pointing to our results to better solidify the basis for the statements we offer in this section.

L369: Not a good sign when the discussion is the first time when the reader learns that there is agriculture in the catchment.

Yes, we note a previous question above about land use, which we will describe in the methods to be sure the reader better understands context around this system.

L384: Same is true for the lake residence time – this needs to come earlier.

Yes, we can describe residence time behaviour in our methods.

L392: This needs to be also part of the site description.

Yes, we can address this along with changes at line 97, about the nature of upstream flows that are diverted to Buffalo Pound Lake.

L394: That DOC can be allochthonous and autochthonous in lakes should be more explicitly part of the introduction.

We will expand the introduction to touch on the nature of DOC sources.

L395ff: Have you tried a DOC mass balance? This could strengthen the argumentation here? How high need concentration in the QLC be to be visible in the lake when most of the water is coming from LD?

Yes, this has been considered, and attempted to some extent, but we do not have robust flow data from the catchment, and residence times vary according to flows and ET losses, so this can not be done with satisfactory confidence. (there is limited monitoring data from the catchment, so that's a key gap).

L403ff: It would be helpful to learn about correlation among the predictors. You state that water inflow to the BP lake comes often from LD. So, how is QBP correlated with QLD and what are cumulative fractions of the lake water balance (eq. 1)?

We disagree about any value this would add (and refer to the rationale in our methods as to why coherence rather than correlation is the framework used). Our analysis highlights the importance of lags for the predictors, and these lags tell us that correlation is not the best approach to investigate these patterns. In the case of Q-LD and Q-BP specifically, the data in Figure 2a,b,c show quite clearly that Q-BP is dominated by Q-LD.

L427ff: This section reveals a large number of formerly undescribed data. I see that this data is not your own result but it may be helpful if there is an overview on average water quality from these unpublished sources as a table somewhere above and/ or the SI.

These data are now published, so we can update with citations.

L445ff: This section describes relationships between DOC and TP as source-driven mainly. Are there additional in-lake mechanisms as well? Joint release of DOC and TP from lake sediments under iron-reducing conditions? Or is this what you mean in L450f?

This is not what we mean at L450, but new work on TP release from sediments in the system has been done, and we can describe this as part of in-lake mechanisms.

L480ff: Brining in new data here in the late part of the discussion is not good. Your discussion should be based on everything you describe in the results plus literature.

This is not new data to the analysis, but rather data conducted through the broader research project. We will remove the supplementary figure here to help in this regard, and instead cite that work.

L475ff: The whole chapter 4.3 seems to be very detached from the results and discussion above. What of this information is directly related to your findings. I suggest to shift part of this to the problem definition in the introduction, to try to discuss how your results help with water management and omit the rest.

Thanks for this comment. We agree that the introduction should introduce the idea of flow management, as this would allow us to better tie in this section, and we will revise accordingly. Ultimately it is this section that is the ‘why we care’ of this work, as the DOC behaviour in the lake and how it is managed have tangible outcomes for humans relying on this water body for different purposes, so this section is key to closing that loop.

Conclusions

L519: This reads as if the upstream lake flushes DOC into the lake BP. Better use “diluting” here and name the source of the high DOC water.

Yes, we will revise the wording to improve clarity on the role of the upstream reservoir.