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Peer Review File

A Process Model for Leachate Treatment in Adsorbent-Amended Constructed Wetlands

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Associate Editor Summary (James Bays)

The manuscript entitled "A Process-based model for Leachate Treatment in Adsorbent-amended Constructed Wetlands" by Ishfaqun Nisa, Mauricio E. Arias, Lillian Mulligan, Xia Yang, and Sarina J. Ergas presents a process-based model designed to simulate ammonia and chemical oxygen demand (COD) removal in constructed wetlands treating landfill leachate. Four established ecological engineers and specialists provided detailed and thoughtful reviews. The study initially received mixed reviews, but the consensus leaned towards acceptance with revisions. The authors did an excellent job of responding to the comments.

The reviews indicate strong potential for the study, with commendations on the innovative approach and relevance to the field. Reviewer A praised the compelling research and experimental design but suggested further refinement to enhance the model's applicability.

Reviewer B took a more critical stance due to concerns regarding the clarity and depth of the methodology and analysis. They emphasized a better presentation of results to reinforce the study's significance. The authors responded diligently, shortening the introduction for conciseness while addressing suggestions for enhancing the clarity of the experimental setup and context in the results.

Reviewer C appreciated the comprehensive nature of the work and suggested a focus on reducing unnecessary details while clarifying certain parts of the methods. They also provided input on the role of vegetation in the model, which the authors acknowledged for future iterations.

Reviewer D's comments involved requests for further clarity and justification regarding model validation metrics and comparisons to literature benchmarks. They highlighted the importance of addressing potential limitations associated with the model's assumptions. The authors responded effectively by incorporating relevant references to bolster their claims and indicating their commitment to addressing these limitations in the manuscript.

Overall, author responses have been thorough and addressed the reviewers' concerns effectively. The revisions aimed to clarify the model's application, improve the presentation of data, and ensure that the manuscript aligns with both reviewer feedback and the journal's standards. Given the positive remarks from Reviewer A and Reviewer C, along with satisfactory revisions made in response to Reviewer B and Reviewer D's comments, I recommend that the manuscript is suitable for publication. With these final adjustments and refinements, the paper is positioned to contribute valuable insights into the field of constructed wetland research.

Thank you for the opportunity to review this interesting, relevant and well-informed paper.

Reviewer Comments and Author Responses

Thank you very much for your feedback and the opportunity to improve our manuscript. We have responded to all 54 comments from the reviewers and included in the document compiled by the editor. We also addressed 63 comments and suggestions from two of the reviewers who shared a tracked version of the manuscript, which were combined and addressed directly in the manuscript. We also reformatted the references reformatted and edited using The Council of Science Editors (CSE) Name-Year citation style in Zotero. Overall, we reduced the word count reduced from 7235 to 6355 (excluding title page, abstract and references). Please note that below the original reviewers' text is in *italics*, and our responses are in blue.

Reviewer A (Scott Knight):

Reviewer summary to be shared with the author and editors:

Interesting study and promising model development that could be used to estimate constructed wetland performance. Model results appear promising and provide a good basis for continued model refinement and calibration to other leachate systems. Research is compelling and the experimental setup appears well formulated for collecting data relevant to model development. Continued piloting in the same or larger mesocosms would demonstrate the applicability of the results to larger systems and longer-term and may allow for developing temporal variables to account for aging of the amendments that could affect performance.

Response: Thank you very much for your comments!

Detailed reviewer notes to be shared with the author and editors:

1. Section 3.4 (Line 528) compares the performance of VF and HF performance, however there does not appear to be consideration of the large differences in influent concentrations as the HF wetland was operated in series. May this have impacted the overall removal rate?

Response: Thank you very much for your comment. The removal analysis was done considering the difference in the influent concentration in the series set up. In Section 3.2 (Figure 3, 4 and 5) and Section 3.3 (Figure 6 and 7), the graph shows difference between the influent concentrations for VF-CW and HF-CW, and these values of influent concentrations were used for calculating removal efficiency.

2. Section 3.4 (Line 538) presents the model performance and error with a range of 0-3%, this does not appear to be the appropriate metric as it is looking only at cumulative performance rather than individual sample performance. Consider eliminating and reporting only the NRMSE.

Response: Thank you for your insight. The error metric has been eliminated both from the text and Table 4.

3. Line 203. Was it verified that the VF operated in an unsaturated condition based on water level measurements?

Response: The leachate remained in the VF tank so little that it was difficult to capture any ponding with the water level measurements (from pressure transducers). The system was fed 24 L of leachate for 15 minutes every 2 hours (see line 127). Considering that the vertical tank had a capacity of 250 L (line 24), this means that the VF tank was most certainly unsaturated most of the time.

4. Line 230. Was DO measured and calibrated for each wetland system? Did the HF wetland operate in an anaerobic condition as stated?

Response: Thanks for pointing this out. We did measure the DO concentrations in each wetland. The DO concentrations for VF and HF are 2-5 mg/L and 0.2-0.5 mg/L, respectively. The HF wetlands were fully saturated with highly low DO level (0.2-0.5 mg/L), which indicates anaerobic conditions. We input these ranges into the model to estimate the desired outputs (ammonia and COD), but DO was indeed an input.

5. Line 31. The model simulated mesocosm performance, assuming applicability for full-scale systems may not be valid without testing and demonstration.

Response: That is a valid point. This part has now been modified in the abstract and also added in Section 3.7 (Limitations) and Section 4 (Conclusions and Recommendations).

6. Section 3.6 (Line 562) discusses estimates for varying HLRs, but the range of simulated results extends beyond the experimental rates. Drawing conclusions based solely on the model seems problematic given the lack of testing and the displayed differences between experimental and simulated values.
Response: We understand the reviewer's concern, and we have updated the figure associated with this (Fig 9) to represent HLRs of 20 cm/day and lower. That said, let us clarify what the goal of varying the HLR was. The idea here was to understand how varying this important design experiment would affect the system performance. It is not to say that the systems can treat X or Y % of pollutants, but rather to see how the efficiency could change under a wider (but viable) range of operations than what we were able to test experimentally (each of the experiments for which we show data (see Fig 9) took place over months, whereas each model simulation takes a few seconds to run).

This is what we added in L 483-484: "Note that while the range of HLR simulated covered a range of typical operation conditions for CWs, these simulations cover a larger range than the experiments."

7. Line 577. The simulated value shows an increase for ammonia removal beyond an HLR of 20 cm/d. Was the basis for this increase considered?
Response: We did not interpret the removals associated with HLR higher than 17 cm/day as a significant increase in ammonia removal, but rather as a plateauing of the treatment capacity. Besides, and line with the reviewer's previous comment, perhaps it is best for us not to speculate too much about what happens for specific HLRs too far beyond the range of experimental values. That said, the figure has been modified to limit it to HLRs of 20 cm/day max.

Reviewer B (Anonymous):
Reviewer summary to be shared with the author and editors:
The paper describes the development and testing of a useful processed based model to predict N and COD treatment of landfill effluent with pilot scale VF and HF wetlands. This work is consistent with the goals of the JEED. With a couple of exceptions, the modeling equations and approach appeared reasonable and defensible, and the model appeared to represent treatment reasonably well. While the research appeared to be sound, I have some concerns about how the results were analyzed and reported. Overall, the paper is well written with very few grammatical errors, but a better background of the results of the field test, a clearer description of the methodologies, and clarification of some of the results must be provided before this article can be accepted.
Response: Thank you very much for the detail review of our manuscript.

Detailed reviewer notes to be shared with the author and editors:
Specific Comments:
An annotated file with embedded comments has been uploaded. The major questions and clarification that I think should be addressed are listed below. The remainder of the comments can be considered suggestions or editorial changes to be addressed at your discretion.
Response: In addition to the response to the comments below included in the reviewer's response, edits/comments in the annotated file were addressed (see "Reviewer" comments in the tracked version of the revised manuscript).

1. In the Introduction, I think it is critical that you describe the mesocosm-scale field project a bit more to capture the reader on the importance of this modeling study. Not everyone is going to the Yang, 2023 paper like me to learn more about it. Specifically, in addition to the suggestions I made to enhance your schematics with the amended layers and to report the plant types, you should convince the reader that this *study is important since you observed such good removal results from your mesocosm scale study, and that scale up seems possible but modeling is needed for future designs. Yes, you show the observed data later, but reporting a summary of the observed data earlier in the paper would enhance it greatly and make it seem less of a purely academic exercise.*

Response: We shortened the introduction substantially to meet word count requirements, but we included several (and additional) details related to the previous paper in the methods (see section 2.1) That said, we were careful not to repeat too much of what is already in Yang et al (2023).

2. Reporting of FA, ammonia-nitrogen, NH4+-N, etc. Most of what I think is described in this paper should be technically described as ammonium - nitrogen. I think it would be fine since you report the abbreviation COD, to replace ammonia-nitrogen with NH4+-N instead of ammonium-nitrogen, but either is OK.

Response: Free Ammonia is only used as a word in Line 70 (acronym deleted). Else in the text results are presented for NH_4^+-N.

3. Model development - while the equations seem appropriate, it is not clear how the model algorithm is constructed. While Figure 2 is OK to show the steps you took in the overall process, an equally important figure should be included be to show the algorithm (as a flow chart) of how each one of the equation "modules" fit together. It could be included in the text or in the supplemental information.

Response: A flow chart showing the overall connections between different modules have been included in the SI material.

4. In section 2.2.3 - it is not clear with the information provided how DO concentration feeds into the model predictions. Please describe this and/or incorporate it into the algorithm suggested above

Response: The availability of DO determined if the system is aerobic or anaerobic and thus, determined the values of rate constants (aerobic or anaerobic) used in all the other modules. We added this line in this section (Section 2.2.3).

5. Model Fitness Section 2.3.

a. The NRSME equation does not look like those defined in the multiple sources I checked. Is this a typo or is this a derivation that was used? Please explain.

Response: Please check the reference we have included to the equation we are using:

Ranatunga T, Tong STY, Yang YJ. 2017. An approach to measure parameter sensitivity in watershed hydrological modelling. Hydrological Sciences Journal. 62(1):76–92. doi:http://dx.doi.org/10.1080/02626667.2016.1174335.

b. Model validation (Lines 374-375). As described it was based on % removal efficiency. It is unclear how this was calculated. Was it against each daily % removal efficiency where there were daily observations? Or against some sort of mean? Please explain. This comes up later in the results section 3.4.

Response: Removal efficiency was calculated at each data point for both the simulated and observed scenarios. The mean of these removal efficiencies was then computed for both datasets. Using these mean values, the percent error between the observed and simulated removal efficiencies was calculated. We have added this line to the section (Section 2.3).

6. Lines 319-320. The Stover-Kincannon model seems pretty important to your study - a brief overview of it or at least the equation should be included. Did you use this model or base part of your models from it?

Response: The Stover-Kincannon model is used to calculate the rate of change in substrate concentration at steady state as a function of loading rate (Nga Dinh Thi, 2020). This is now stated in Lines 258-259.

7. Section 3.4 Removal Efficiency Validation. As mentioned earlier, how removal efficiency (RE) was calculated is not clear. When studying the validation periods the differences in RE for individual days appeared to far exceed the % error ranges reported in lines 531 and 540, as well as in Table 4. This makes the model appear more robust than perhaps it actually is, which would be misleading. I certainly do not think the authors intentionally meant to mislead, but some explanation/clarification of how these REs and the error were calculated is warranted to address this to remove any doubt.
Response: Response to 5b explains how the percent errors in mean RE were calculated.

8. Effects of Media on Removal - Section 3.5.
a. The % enhancement of the amended HF-CW seems underestimated, since the effluent in the unamended tracks around 100 mg/L and amended tracks around 50 mg/L. Can you explain how you arrived at these values?
Response: The value was a typo in the analysis, it would be 46.6%, not 10.6%. It has been corrected in the manuscript.

b. Also report the % change in COD in the VF-CW rather than say it did not vary much.
Response: This has been included as follows (line 474) "However, simulated COD removal efficiency did not vary much between the amended and unamended VF-CW (16% vs 17%).".

c. Additionally in Figure 10 - In the figure title I think you mean that the data represents effluent concentrations rather than removal, correct? Please modify this heading if that is correct. Response: You're right, it has now been corrected.

Reviewer C (Brandon Winfrey):
Reviewer summary to be shared with the author and editors:
The manuscript presents a process-based model for simulating ammonia and COD removal in two types of constructed wetland treatment trains, focusing on effects of adsorptive additives to the media layers in vertical flow and horizontal subsurface flow wetlands. The paper is well-written, comprehensive, and clear. The contribution is significant and novel. There are some aspects that, in my opinion, could be covered more efficiently so as to reduce the length of the paper and some sections the authors could further elaborate on so their work could be replicated. Most of my detailed comments are in the Methods section. One suggestion for the next iteration of the model is to pay closer attention to role of vegetation. The plant uptake component of this model was a simple 1st order rate model. However, the rate constants may be insufficient to represent plant uptake at these high concentrations of ammonia. In addition, there has been some recent work on process-based modelling of plant uptake of different forms of nitrogen which may have relevance here (see https://doi.org/10.1016/j.scitotenv.2019.133633). Just so the authors know, this work I have linked is not mine.
Response: Thank you so much for your detailed review and suggestions, the references have been noted down for future iterations of the model.
Detailed reviewer notes to be shared with the author and editors:

1. *Introduction*
In the Introduction section, the authors should consider generalising their summary of wetlands and amendments, seeing as the paper focuses on the model. The manuscript is about 1000 words too long, so this might be a good place to briefly introduce the topic of constructed wetlands for landfill leachate treat-

ment and summarise only the key processes of concern before focusing most of the Intro on the modelling. I think the level of detail for identifying the knowledge gaps with respect to process-based models of CWs was good.

Response: We edited the introduction to reduce the text describing general information about CWs and specifics about the adsorbents.

2. *Methods*

In the Methods section, the authors should add some more detail about the mesocosm setup relevant to the hydraulics. For example, what were the depth (or length) of the layers of media and porosity of each media layer? For how long were the CWs dosed and when did data collection start relative to the start of dosing? Were plants used or were these non-vegetated?

The diagram (Figure 1) could be improved. It appears in the VFW that the outflow comes from the side of the tank, does this mean it does not travel through the gravel layer at all? Also, is the gravel layer half the depth? The media layers are not labelled or described in the caption.

Regarding the aerobic/anoxic condition modelling for VF and HSSF systems, do the data support modelling them in this way? If DO was only measured at the outflow, can you know whether you had aerobic conditions in the VF CW? Were the O2 levels low enough in the HSSF to support modelling as anoxic condition?

Response: This comment was addressed in two ways. First, a modified schematic of the mesocosm system was introduced (see Fig 1). Also, we added more details regarding the dosing of the system (24 L of raw leachate during 15 minutes every two hours, see lines 126 and 159).

To get the manuscript more in line with the expected 6000-word limit, the authors should consider summarising the background information on the nitrogen cycle in section 2.2.4, as most readers will be familiar with these concepts.

Response: We have reduced this section as much as possible, but please bear in mind that meanwhile, another reviewer asked to elaborate more on details related to nitrogen cycling.

The sensitivity analysis, Sobol method, appears to have only been done on one component of the model. The authors should elaborate on whether parameters related to other model components (e.g., hydraulics, N transformation processes) were also subject to a sensitivity analysis.

Overall, the methods section gave a good overview, but it would be difficult for someone to replicate this work without addressing the comments above.

Response: The sensitivity analysis was done on 11 parameters involved in ammonia and COD transformation and adsorption processes and were not applied to the hydraulics parameters, which has been mentioned in Section 2.4.

3. *Results and Discussion*

The results and discussion section was well presented. No major comments here.

Line-specific notes:

4. *no need to capitalise 'constructed wetlands' (ln 20)*

Response: Done

5. *spell out COD (ln 27)*

Response: Done

6. *replace 'soil surface' with 'media surface' (ln 47)*

Response: Done

7. *Authors should consider elaborating on explanation that VSSF wetlands can operate as aerobic systems by explaining that this is achieved by pulsing/batching inflow rather than using a continuous flow regime (ln 61)*

Response: Done. Sentence in lines 47-51 now reads: "because oxygen diffuses into the unsaturated media, particularly when operated in batch and/or with pulses of influent. HF-CWs are saturated and develop anoxic conditions that favor denitrification. However, due to the limited adsorption capacity of gravel substrates, the performance of gravel-bed CWs in wastewater treatment is frequently insufficient (Kizito et al. 2017).".

8. *Authors should specify whether high free ammonia levels are typically present in leachate with a citation. The term 'wastewaters' is used here, but it's not clear whether this is common in leachate specifically (ln 70)*

Response: The following reference has been added: Kim, D.-J., Lee, D.-I., Keller, J., 2006. Effect of temperature and free ammonia on nitrification and nitrite accumulation in landfill leachate and analysis of its nitrifying bacterial community by FISH. Bioresource Technology 97, 459–468. https://doi.org/10.1016/j.biortech.2005.03.032

9. *This is mostly personal preference, but consider using acronyms sparingly in your manuscript. For example, there's no need to use the acronyms 'FA' or 'IX'. These terms are used again, but it's much easier to read the phrase than it is to remember what these acronyms stand for (ln 71 and elsewhere)*

Response: Done.

10. *Double check all references are listed in the Reference List. The citation 'Payne et al. (2018)' was used, but there is no reference listed for 'Payne' in your list (ln 99 and possible elsewhere). It might have been mixed up with Apontes-Morales? In any case, double check the reference list, as well, because the first listed item is not in alphabetical order.*

Response: We corrected this issue. These are two separate references.

11. *Replace 'have' with 'has' (ln 107)*

Response: Done.

12. *Citation format of Kadlec and Wallace should be fixed (ln 112)*

Response: We have formatted all our references now using Zotero so that the format should be consistent across references and in line with the journal's requirements.

13. *Throughout, check for minor grammatical errors. E.g., 'numerical process models' should be 'numerical process model' (ln 138, and elsewhere).*

Response: Thank you, minor grammatical errors have been fixed.

14. *Indicate at what levels pH affects biodegradation rates according to K&W. Is that pH relevant to the observed levels in your systems? (ln 185)*
Response: As stated in the paper (eg., lines 139 and 505), pH was assumed to be constant and neutral in our model.

15. *Define 'a' in the equation (ln 207)*
Response: Done.

16. *Authors should comment on variability of inflow concentrations where the weekly pollutant concentration sampling frequency is mentioned (ln 367). Because the nominal HRT is 11 days, if the pollutant concentrations of the inflow vary significantly, the assessment of removal efficiencies could be skewed by highly variable inflow concentrations when outflow concentrations are assessed at the same time.*
Response: We supplemented this information in Section 2.3.

17. *Authors should provide standard deviation of inflow concentrations as well to give readers an idea of the variability since raw leachate was used rather than a standard synthetic influent mix (ln 416).*
Response: Added in Section 2.3.

18. *It's unclear why Organic N data were limited in this study- are the authors referring to limited data in the literature describing ammonia concentration reduction or limited data from the study that provided calibration and validation data, meaning that study did not measure Org. N? (ln 423).*
Response: The latter. We were unable to measure organic nitrogen due to analytical equipment issues. We only measured inorganic nitrogen forms.

19. *Spelling of 'amended' is incorrect (ln 529 and 530, ln 543, double check throughout)*
Response: Done.

20. *Figure 10: Add pollutant name to y-axes and titles on plots rather than only mentioning in the caption. Also, caption says 'removal' but the figures show effluent concentrations (I assume).*
Response: The figure has been modified.

Reviewer D (Jay Martin):

Reviewer summary to be shared with the author and editors:
I think this is helpful study that does contribute knowledge related to Ecological Engineering. I do think it can be published after revisions to address issues that are generally noted before, and with more detailed comments on the attached documents. Some of the most important needed improvements are:

1. NRMSE values are calculated and cited throughout as evidence that the model accurately matches measured data. But, there are no thresholds cited from modeling literature to denote that these NRMSE values are sufficient. A value (0.3) is mentioned in the conclusion section, but this threshold is not related to any literature values. Also related to this topic, the figures (5,6,7, 8, 9) comparing the modeled results to the measured data do not convey confidence that the model is matching the data. Better ways to compare these values are needed to convince the reader that the model has a good fit to the measured data.
Response: As explained in lines 317-319, NRMSE is a standard metric to assess the accuracy of a prediction tool as the values are not unit dependent. NRMSE value of 0 indicates the model to be a perfect fit. There are numerous articles that have accepted NRMSE value below 0.3 as good model fit. We added following references to the manuscript:

1. Guoqing Lei, Wenzhi Zeng, Jin Yu, Jiesheng Huang,
A comparison of physical-based and machine learning modeling for soil salt dynamics in crop fields, Agricultural Water Management, ISSN 0378-3774, https://doi.org/10.1016/j.agwat.2022.108115

1. Mozhdeh Sadeghi, Mohammad Shayannejad, Ali Ashraf Sadraddini, Saeed Salehi,

Estimation of soil infiltration coefficient in the furrow irrigation using the combination of the hydrodynamics model and Richard's equation, Physics and Chemistry of the Earth, Parts A/B/C,
Volume 135, 2024, 103649, ISSN 1474-7065, https://doi.org/10.1016/j.pce.2024.103649

2. Verhamme EM, Redder TM, Schlea DA, Grush J, Bratton JF, DePinto JV. 2016. Development of the Western Lake Erie Ecosystem Model (WLEEM): Application to connect phosphorus loads to cyanobacteria biomass. Journal of Great Lakes Research. 42(6):1193–1205. doi:10.1016/j.jglr.2016.09.006.

However, we have included this in the 'Limitations' section to explore better ways to compare the results other than NRMSE metric.

2. More clear comparisons between rate constants used in the model and those from literature are needed. The large table in the SI, should be moved to main body of the paper, because of importance.
Response: There are three tables in the SI, two of which are of similar length, thus it is unclear which is the one the reviewer is referring to. That said, we are reluctant to move any material from the SI to the main text, as we are already being asked by the other reviewers to reduce the number of words and complementary aids (figures and tables).

3. Some of the figures currently in the paper do not add much and should be deleted.
Response: The figure explaining CSTR method has been deleted.

4. Assuming CSTRs makes sense for this model, but likely leads to overestimation due to transport limitations that should be described in the Limitations section.
Response: Yes, we are aware that CSTR could lead to deviation from actual performance (see statement in line 220). That is exactly why we used a steady-state CSTR in-series model (or TIS model) instead.

Additional Reviewer D comments:

1. *Line 187-Rationale should be provided for assuming a neutral pH? Is there no information about how variation pH impacts the simulated process? Or???*
   Response: The focus of our research was on adsorbent materials, and the bulk of the efforts were placed on representing biogeochemical processes that most influence adsorption. As stated in line 168, the pH in the monitored CWs varied from 7.69 to 8.36, which are pretty mild alkaline conditions. Overall, we acknowledge that constant pH neutrality is a limitation of the model currently, and future research will focus on this aspect in the future,

2. *Why is the process of ANNAMOX not considered? I believe it has been documented as a process occurring in the wetland nitrogen cycle.*
Response: Our broader research program is actively looking at ANNAMOX in filtering media with adsorbents. However, this is ongoing work of another grad student, who may be able to integrate those findings into models, eventually, but that was within the scope of the research already completed for this paper.

3. *Why are NRMSE values cited in calibration section favorable results? Are there recommended thresholds for the values that indicate a good fit of model results to observed data. This needs to be specified to convince reader that the model is predicting accurately. Line 612 and in Results.*

Response: NRMSE has been a widely used metrics for evaluating accuracy of prediction tools. We added references to this in Section 2.3 (Model Calibration and Validation).

(Ranatunga, T., Tong, S. T. Y., & Yang, Y. J. (2016). An approach to measure parameter sensitivity in watershed hydrological modelling. Hydrological Sciences Journal, 62(1), 76–92. https://doi.org/10.1080/02626667.2016.1174335)

Please refer to response D-1 (Reviewer D, Comment 1) where this has been further explained in detail.

4. *One thing that needs to be added is how the first-order rates used in the model for process compare to those used in other models, or measured in labs/field research. The Table currently in the SI should be moved to the main paper and cited in these instances.*

Response: We are doing anything we can to keep the manuscript length at the appropriate journal length requirement, and bring these (2 tables, not 1) from the SI would extend the manuscript. Thus, we would opt for keeping those tables in the SI.

5. *Line 450 and elsewhere: How do the concentrations predicted by the model for NO3, and other variables compare to measure data? These comparisons would be make good graphs/figures.* Response: Figure 4 shows a comparison of simulated nitrate vs observations for the unamended system. As the media-amended system was targeting ammonia and COD, figures were presented for both of these indicators. However, nitrate was not a target of the media amendments and results were not discussed in section 3.3. Thus, we opt for not introducing another figure.

6. *Line 459: Need to quantify the alignment between data and model.*

Response: The sentence in line 398 has been edited, now it states "This aligns well with the observed data, which shows a mean COD removal of 16.5% in the VF-CW and 11.5% in the HF-CW."

7. *Figures 5,6,7, 8, 9: These are not very convincing that the model is working well. Are there other and better ways to compare the model out to the observed data? Maybe a plot of predicted vs observed on X and Y axis and see how close this is to 1:1. This would also indicate if model was performing better at higher concentrations/lower concentrations. Need to explore other methods of evaluating model performance to better evaluate performance.*

Response: We have used NRMSE as model fitness metric as this is a widely used metric predicting model accuracy (please refer to Additional Comment 3, where this has been explained in detail, and additional references added). However, we appreciate your suggestion about exploring other methods of evaluating model performance and have included in the 'Limitations' section.

8. *I don't disagree with choice of CSTR representation. But, wetlands have been identified as transport limited and diffusion limited systems. So, I am concerned that the CSTR assumption would likely lead to an overestimation of rates for processes in the model that could be limited by diffusion, like dentitrification. This should be a point in the limitation section.*

Response: This is the same issue brought up by reviewer D. Yes, we are aware that CSTR could lead to deviation from actual performance (see statement in line 220). That is exactly why we used a steady-state CSTR in-series model (or TIS model) instead.

9. *Citations in SI should also be included in references for the main paper, as they contribute to the overall study.*

Response: Citations in the SI are now also cited in the main paper and included in the reference list.