American Ecological Engineering Society

Journal of Ecological Engineering Design
journals.uvm.edu/jeed

Peer Review File

Evaluating and Enhancing the Design of Borrow Pit Wetlands for Managing Nutrients in Agricultural Runoff

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[1]Round 1
[2]Round 2

Round 1 Response

Thank you to the associate editor and all four reviewers for their thorough and insightful comments. We appreciate the opportunity to revise the paper. We have addressed all comments individually below, noting where in the revised manuscript changes were made.

Associate Editor Comments—Debabrata Sahoo

1. Please ensure that all figures (e.g., Figure 1) are clearly and meaningfully explained within the text, with sufficient context to guide interpretation.

   Response: We have added additional text describing Figure 1 (Lines 136-137) and clarified the figure caption.

2. Provide an appropriate reference to support the assumption of a 6 cm water depth discussed in Lines 250–255.

   Response: We have clarified in the text that this was based on observations during field sampling (Lines 274-275).

3. As noted by multiple reviewers, some sentences would benefit from additional clarity or elaboration. Please revise these sections as needed to improve readability and precision.

   Response: We have revised sentences as identified by the reviewers and made other edits to enhance clarity.

Review #134—Emily Nottingham Byers

Reviewer summary to be shared with the author and editors:
Overall, the subject matter of this manuscript is appropriate for the journal. The authors provided a case study for the use of borrow pits as constructed wetlands through the demonstration of field and modeled research. The study also explores additional design parameters to determine how these wetlands should be optimized. The authors demonstrate that understanding the system's hydrology and river dynamics play a crucial role in treatment efficiency of the wetland. Points of praise include the scale of the study (i.e., field-scale) to demonstrate the true treatment potential of these wetlands. The integration of culvert design and river dynamics are novel and valuable to field, particularly when thinking about how to repurpose important land area in levee construction. There are several grammatical errors (e.g., misspelled words, incorrect use of abbreviations) that need to be addressed throughout the manuscript, but the English is of a reasonable

standard. Overall, I recommend removing any information in the methods that are not discussed in the results. There also needs to be additional information presented about the original design of the wetland. There appears to be some discrepancies between what was observed in the field study compared to what the main model conclusions were that need further discussion. Lastly, I recommend additional discussion surrounding sizing of wetlands for hydrology in your conclusions. There are additional points for the authors to consider/address described below.

Response: Thank you for your detailed review and helpful comments. We have clarified the writing and addressed grammatical errors throughout and removed superfluous information from the methods. We also expanded the comparison of the field and modeling data and added recommendations to the discussion and conclusions. We have addressed individual comments below.

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

4. Lines 47-48: Is the model results only applicable to this wetland/Missouri River or could the interactions be extrapolated to other applications. Would it be more appropriate to say that "Results highlight the importance of considering hydrologic interactions between wetlands and river dynamics in CW design and evaluation."

   Response: We agree that this is not limited to this specific case study and edited this sentence to read "Results highlight the importance of considering hydrologic interactions between wetlands and adjacent waterbodies in CW design and evaluation." (Lines 47-49).

Introduction:

5. Lines 57-58: Is ag nutrient runoff really any more persistent than other landcover nutrient outputs? I assume you are referring to the persistence of P in the environment, but I'm not sure P from ag is any worse than P from industrial sources, for example. The first two points in your sentence along with the magnitude of applied nutrients I think are what is driving the difference of ag landscapes compared to other sources.

   Response: "Persistence" was an incorrect word choice. This has been changed to "ubiquity" as the scale of agricultural nutrient sources is one of its major challenges (Line 59).

Case Study Site:

6. Lines 122-124: Rather than saying Horse Creek was not monitored for wetland performance, could you please describe briefly why it was monitored here. If Wetland 2 and horse creek are not important for the study, I recommend removing them from the methods sections. If they are important findings, please make that clearer in your results section.

   Response: We have elected to remove descriptions of Horse Creek and Wetland 2 from this section as these data were not used in this study. We also updated Figure 1 to remove these two locations.

Materials and Methods:

7. Line 137: how do you know that nutrient removal was limited if you didn't monitor during the winter? It may be better to state, "We did not monitor during the winter because the wetlands were frozen, and nutrient removal was assumed to be limited with decreased biological activity."

   Response: We have made this change (Line 149).

   Lines 141: It sounds like you are describing upstream and downstream as wetland cell locations. It would be better to state that water samples were collected in Wetland 1a, Wetlands 1b, Wetland 2, and upstream and downstream of Wetland 1 (or removed wetland 2 from your methods).

   Response: We have made this change (Line 153).

8. Line 142: By composite water samples are you saying that they were composited over the different sampling locations or did you take multiple samples at each location, and those samples were composited? Please describe it in more detail.

   Response: Three samples were taken in each sampling zone (e.g. Wetland 1b) and composited. We have added additional clarification in the text (Lines 158-159).

9. Were the "upstream" and "downstream" samples taken from Horse Creek? Please describe.

   Response: No, samples were taken from a single location on Horse Creek. However, we have removed the description of Horse Creek as the data were not used in this study.

10. You can use N and P rather than nitrogen and phosphorous since you already defined them during the introduction.

    Response: We have made this change.

11. Line 143: Please state the lab used and the method/instrumentation used for nutrient analysis if possible.

    Response: We have added the lab name and the standard methods used for nutrient analyses (Lines 155-157).

12. Please describe in more detail how you collected composite soil sample (e.g., depth of sampling, how many samples were composited). Also, show results of this analysis if presented in the methods.

    Response: We removed reference to soil samples as these data were not used in this study.

13. How long were samples stored until analyzed? How were they stored once collected?

    Response: Samples were placed in a cooler immediately after sampling and shipped to the lab for analysis. Additional details were added to the text (Lines 159-161).

14. Did you account for wetland vegetation between the different cells?

    Response: Not explicitly, no. However, wetland vegetation was noticeably different between Wetland 1a (shallower with more emergent vegetation) compared to Wetland 1b (deeper with less vegetation). We did not quantify these differences, however. We have added additional description of the wetland vegetation to the study site description (Lines 122-130).

15. Line 166: same as comment 8, once you have defined your abbreviation you can use the abbreviated version moving forward.

    Response: We have made this change.

16. Could water depths be related to discharge and used to help calibrate flow in the ungauged watershed? Or how were water depth measurements used in your analysis (beyond for wetland mass balance)?

Response: We had hoped to develop a stage-discharge rating curve for the stream flowing into the wetland. However, the small size of the channel and limited observed flow during the monitoring period (many times essentially stagnant water) made it impossible to get accurate flow data. Similarly, we attempted to use hydraulic modeling to relate depth and discharge, but could to obtain a reliable relationship to be used in hydrologic model calibration. Therefore, depth measurements were used only wetland water balance calibration.

17. Lines 227-230: At what climatic/wetland conditions would you get surface outflow of the wetland?

    Response: Surface outflow from the wetland occurs when the wetland water level is above the invert elevation of the culvert and the Missouri River water level is below the invert elevation on the riverward side.

18. In general, it would be better to describe the design and flow of the wetlands in more detail (e.g., HLR, HRT, design depth, main vegetation present, % of wetland area vegetated vs open water, main soil type/class).

    Response: We have added more description of wetland characteristics in the study site section (Lines 110-130).

19. Line 270: Justification for assuming 50% of the nutrient load was attenuated is needed.

    Response: We have added additional context and references here (Lines 294-299). Nutrient retention can vary widely depending on many site-specific factors. We assumed a single value for the sake of modeling, but recognize that this introduces uncertainty into our analysis.

Results:

20. Line 340: gradual spatial decline or temporal decline in nutrient concentrations?

    Response: Temporal decline. This was clarified in the text (Line 370-371).

21. Line 345: During April it looks like TN increased in the wetland compared to what was coming in. How can you explain this?

    Response: Higher concentrations were observed in the wetland compared to upstream for the first couple sample events. This is likely due to loads that came into the wetland before sampling began in the spring, which were still being processed by the wetland.

22. What were the predominant N species contributing to TN during this time? You show them in supplementary materials but a short description in the manuscript would be helpful in explaining what was observed.

    Response: Most nitrogen was organic. We have added reference to Table S4 in the text (Line 378).

23. Why do nutrients increase in wetland b compared to a?

    Response: This was likely due to differences in vegetative cover, with 1a being more vegetated than 1b. We have added this to the text (Lines 376-377)

24. This study took place approximately 10 years after the construction of the wetland, correct? How does wetland age play a role in your observed results and integrated into your modeled results?

    Response: Yes, field data were collected approximately 10 years following wetland construction. This could influence our monitoring results, as nutrient removal may either increase or decrease (e.g. organic matter accumulation enhances denitrification, or soil P saturation limits further P removal). We did not incorporate wetland age effects into our modeling. This would require temporally-variable removal rates (k values), but we had no data to justify this and therefore assumed constant removal rates for the whole modeled period. We have noted this limitation in the results (Lines 413-415).

25. Line 401: what are the average water temperatures in December? I thought you couldn't sample over the winter due to frozen water, so why would the warmer temperatures increase the rate of N removal in late fall/early winter?

    Response: This sentence was poorly written. Inflows are low through summer and fall, but high temperatures in summer only contribute to high N removal. This was revised in the text (Lines 442-444).

26. Lines 409-412: Isn't N storage in the wetland the goal for treatment? Could you clarify why "storing" more N when there were high river water levels, decreased N removal rates? How could you have decreased N removal, when there was less leaving the wetland that year?

    Response: We define N stored as the mass of N remaining dissolved in the wetland at the start of winter. Some of this N will be exported from the wetland as it drains, but we can't track what happens since we did not model wetland dynamics during the winter. N removal was quantified as the amount of N removed via uptake only. The second sentence is reporting average removal rates for the whole modeled period, not just 2018 and 2019. These sentences were revised to clarify these points (Lines 449-451).

27. Lines 413-415: are you discussing total average over the whole study or for 2018-2019?

    Response: These are averages for the whole study (see Response to comment #26, above). This has been clarified.

28. I would have expected higher N removal, especially at the concentrations observed. A little bit more discussion explaining this would be beneficial? Particularly compared to your field data.

    Response: Our N mass removal rates were lower than reported in the literature, but this is to be expected with low loading rates. Removal efficiencies (average ~36%) were relatively typical for nitrogen, although it is on the low end of the literature range. This is likely in part due to the high organic fraction, which is harder to process/remove. Furthermore, with the exception of spring, our monitored incoming TN concentrations were low (near background) which limits the removal that can occur in the P-K-C* model. We have added text explaining these points to the results (Lines 460-465).

29. Line 429: How was soil storage considered? Typically, the predominant removal processes for P are physical sorption to soil, not uptake.

    Response: We did not model specific removal processes (e.g. sorption, plant uptake, denitrification) separately, but instead lumped all processes into the calibrated P-K-C* model. This was clarified in the methods (Lines 302-304). We also changed references to "uptake" to "removal" throughout the text and figures.

30. In lines 463 you show that nutrient removal efficiency increases with larger loads but in lines 418 and in Figure 5 you state the opposite that removal was higher with less N coming in? How do you explain this discrepancy? You seem to make the argument that the wetland has sufficient capacity to remove more TN and TP than what is observed, yet you still modeled relatively low removal efficiencies for TN.

    Response: This discrepancy is due to how the increasing loads scenarios were modeled. In these scenarios, total incoming nutrient loads were increased without changing incoming flows (and therefore not changing hydraulic loading rate). This allowed for more efficient nutrient removal in these scenarios (in line with the literature). In the baseline scenario, on the other hand, years with higher nutrient loads also had higher hydraulic loading rates. These higher hydraulic loading rates are what reduced nutrient removal efficiency for these years. This was clarified in the text (Lines 527-533).

31. Line 537: what types of ecological benefits are directly provided by passive depth control?

    Response: Increased vegetation and waterfowl species diversity, per Carpenter and Bledsoe (2025). This was clarified in the text (Line 607).

Figures/Tables:

32. Figure 1. I don't see the study wetland outline in orange. There is a red rectangle on panel A and B that is not described in the legend, (assuming that's the same area shown in C?). Please include the watershed in the legend. Please make the two wetland areas clearer in panel C.

    Response: We have clarified the red rectangles are extents of the subsequent map panels, included the watershed in the legend, and add extents of the two wetland cells, 1a and 1b.

33. Figure 4. Define your abbreviations in the graph

    Response: Abbreviation definitions were added to the caption (Lines 435-437).

34. Table 1. It would be helpful to add in a row that shows your data for direct comparison with your review. If you are looking to save space, you could probably just provide the state/country rather than the name of the wetland or remove that column completely. I'm not sure how much it adds, particularly since there are several studies included that don't specify the location.

    Response: Our data are included in the last row of Table 1.

Conclusions:

35. In theory, we don't have control over the amount of nutrients entering the wetland, so according to your results, the best option is to increase the wetland size? It would be helpful to briefly discuss the benefits and limitations of increasing the wetland size.

Response: Yes, increasing wetland size would increase the total mass of nutrients removed (even if per area rates would decrease slightly). We have stated this more explicitly in the end of the discussion (Lines 599-604) and the conclusions (Lines 631-632).

36. In other words, are you showing that the wetland was undersized based on the hydrology not the nutrient loads?

    Response: Yes, in part. We show that wetland performance is more sensitive to HLR than nutrient loading and that increasing wetland area could improve performance. This was clarified in the discussion (Lines 575-581).

37. Could you state more about how river dynamics should be incorporated in the original design of the wetland? This would be beneficial for being able to properly design these borrow pit wetlands in the future.

    Response: We have added additional text on incorporating river dynamics into wetland design into the discussion (Lines 609-614).

38. Supplementary Data files: please ensure abbreviations in figures are defined for clarity and ease of viewing.

    Response: We have added abbreviation definitions to figure captions.

Review #136—James Bays

Reviewer summary to be shared with the author and editors:
Thank you for the opportunity to review your paper on the performance of the borrow pit wetlands along the Missouri River. I found the paper to be resourceful, well-written and detailed appropriately. I've attached a short list of comments and corrections for your use. As a general summary of areas for your consideration, I would have liked to have seem more detail about the wetland, i.e., morphometry, hydraulic assessment, vegetation coverage and composition. These details pertain to the hydraulic characterization of the wetland, as well as a potentially useful point of comparison for other wetland researchers. You will also see a general theme that when we rely on percent reduction or percent efficiency, we sometimes may obscure or diminish the actual magnitude of mass reduction, particularly for highly-loaded systems. In general, please accept the observations as points to consider. This manuscript is ready to publish, pending some minor typo corrections and, hopefully, some consideration of a few of my observations.

Response: Thank you for your review and detailed comments. We have added additional information about the wetland and noted different trends in percent removal and mass removal rates. We have addressed specific comments below.

Detailed reviewer notes to be shared with the author and editors:
Thank you for the opportunity to review this paper. The topic of using oxbow wetlands, or their constructed equivalent, has been in the literature awhile. However, this manuscript does a creditable job of assessing the many inflows and loads that are commonly difficult to measure. I do have some observations pertaining to mass reduction, model calibration and how these results fit into the wider universe of wetlands, but they are for the author's consideration.

File	Page	Figure	Suggested Change
Supplemental		S1	depths ~~a~~ in the study wetland **Response:** Fixed.
Supplemental		S2&S3	modest agreement between simulated and observed **Response:** This was added to the figure captions.
Supplemental		S4	good agreement in predicted and observed water levels **Response:** We noted modest agreement for this figure in the caption.
Supplemental		S5	Good agreement in predicted and observed wetland TN and TP in second year **Response:** We have added text noting good agreement for 2024.
Supplemental		S5	Predominant concentrations in TN and TP are at C* after initial removal, suggesting internal process dominance during storage years. **Response:** This is a good point and we have noted this in the results (Lines 463-465).
Supplemental		S9	Groundwater **Response:** This figure was updated.
Supplemental		S9	Water Left = Storage (check definition) **Response:** This figure was updated.
Supplemental		S10	For model scenarios, caution on predicted outflow TN and % removal, given predominance of organic nitrogen **Response:** We have noted the potential difficulty in removal of organic N in the results (Lines 461-463).
Manuscript	2		Decreasing wetland area...decreased nutrient removal efficiency (but could increase mass removal). Consider reducing importance on nutrient removal efficiency.

			Response: We have noted the potential for mass removal rates to increase in the abstract (Line 47).
Manuscript	4		Consider including a description of the vegetation communities and distribution in the wetland. It's described as a borrow pond, but the amount of littoral vegetation, and typical composition, could be a useful frame of reference for other researchers or practitioners. This could include both submersed aquatic and emergent vegetation communities. **Response:** We have added more details about wetland vegetation to the study site section (Lines 122-130).
Manuscript	5		"...nutrient removal was limited by low temperature". This presumption, although true, neglects other processes, include internal processes related to stratification, which may be important during spring turnover. **Response:** This is a good point. However, we did not have any concentration or water level data during the winter (when wetland was frozen) and therefore didn't feel confident including this season in our model.
Manuscript	4		I may have missed it but I didn't seen any description of the average and/or maximum depth of the borrow pits. It would be relevant to others to know the hydraulic residence time and depth of distribution of vegetation, particularly as it seems to vary with season. **Response:** The wetland was not designed for a specific depth, but was generally relatively shallow to promote submerged and emergent wetland vegetation. Depth can vary significantly throughout the year (as seen in the monitoring results) due to fluctuations in inflow and Missouri R. water levels. Additional description of the wetland design was added to the study area section (Lines 110-121).
Manuscript	11		The assumption of 50% reduction through the levee may be defensible from an operational perspective of model construction, but I feel confident in saying that it will depend upon flow rate, flow path length, pollutant type and concentration. Consider adding a reference or two for support. **Response:** We have added additional context and references here (Lines 294-299). Nutrient retention can vary widely depending on many site-specific factors. We assumed a single value for the sake of modeling, but recognize that this introduces uncertainty into our analysis.

Manuscript	11		Consider including a description of the Θ calibration. This value will vary depending upon the parameter being modeled. **Response:** We calibrated θ along with k20 based on observed TP and TN data. This was clarified in the text (Lines 313-314).
Manuscript	11		Consider including a consideration of the hydraulic characterization of the wetland as a combined tanks-in-series. This may be important for wetlands achieving very low concentrations hovering near C*, and can affect full-scale predictions of area and performance. **Response:** We modeled nutrient removal in the wetland with the P-K-C* model, which uses the tank-in-series approach. The hydraulics were modeled using a standard water balance, treating the whole wetland as a single unit. Future work could use more granular hydraulic modeling.
Manuscript	14		The calibrated hydraulic conductivity of 14 m/day is indeed high, in my experience, but is consistent with sandy soils. Consider strengthening this statement by including local or regional soils data measurements that may not be readily available to the interested practitioner. **Response:** We have added additional studies showing this is within the range of K values for the MO River alluvial aquifer (Lines 400-403).
Manuscript	15		The calibrated TN k rate of 5.0 m/yr may be in the 20th-percentile of calibrated wetland values, but is most consistent with organic or TKN rate constants. Consider including a statement that this value is consistent with calibrations from wetlands receiving water with high organic N. In other words, it is not that the removal rate is conservative, but instead is consistent with high organic N composition of the wetland water source. **Response:** Based on values published in Kadlec and Wallace (2009), a k value of 5 m/yr is around the 20^{th} percentile for TKN and 10^{th} percentile for organic N, similar to total N values.
Manuscript	16		Line 401: consistent annual patterns **Response:** Fixed.
Manuscript	16		From my outsider's perspective, I see essentially a batch type operation: An initial period of loading in the spring, and a subsequent period of largely internal cycling nutrients to approximately background (C*) levels.

			Response: That is largely accurate. Most of the inflow occurs in April-June. As can be seen in Figure 5a and 6a, there are many inflow events in individual years during the spring and early summer, but very little during the remainder of the year.
Manuscript	17		Consider supporting your characterization of internal nutrient retention as "plant uptake" by referring to Kadlec's characterization of wetlands as vegetative vs. microbial. **Response:** The low loading rates do suggest that removal is vegetation dominated. However, we did not quantify specific removal pathways and have changed our characterization to general removal (by multiple process) rather than just plant uptake.
Manuscript	17		Consider pointing out that the mass removal rates were highest during the period of high inflow loading, and lower during the period of low inflow loading. The percent removals may be lower during the latter period, but the total mass removal is greater in the former. This may be important in future placement of borrow pit wetlands where land area maybe limited and flows are high. **Response:** We have clarified here that percent removal rates and mass removal rates showed opposite trends (Lines 458-460). The effect of hydraulic loading rate was also important, which we explained more later (Lines 527-530).
Manuscript	17		Line 419: Consider supporting this statement of literature values by including a key reference or a brief comparison to actual literature values. **Response:** The 100-400 kg N/ha/yr is a published range from Mitsch et al. (2000). This was clarified in the text (Lines 452-453).
Manuscript	19		Line 433: One reason why TN removal efficiencies are lower than for TP is the composition of N in the system, whether predominantly organic or inorganic. In this case, the potential for high N removal efficiencies is different for TP because of the high TN C* and naturally low TN k values. **Response:** Thank you, we have noted this in the results (Lines 461-463).
Manuscript	20		Of the many useful papers cited (thank you for this diligent summary), my thoughts were that the created oxbow wetland studied by FInk and Mitch (2007) at the Olentangy River seemed a most useful comparison. They identified the same treatment

			removal benefits of borrow pits created for levee construction and may be worth focused consideration. **Response:** Due to the length of the paper, we have elected to not add a more detailed comparison with the Olentangy wetland complex. We did however, note the hydraulic connectivity of these systems differ (Lines 566-569).
Manuscript	22		Figure title, upper right: Phos~~h~~phorus **Response:** This was fixed.
Manuscript	23		Again, consider noting that while decreased area corresponding showed increased loading rates, the actual mass removal is greater, although percent efficiency decreased. It may be more to important to emphasize the increase in mass reduction vs the decrease in efficiency. It is worth mentioning that the expected increase in TN mass reduction may be more readily observable in systems dominated by microbial transformation, as Kadlec has described. **Response:** We have clarified that mass removal rates may increase, even if percent removal decreases (Lines 575-579 and 600-603). We also added mass removal rates to Table 2 to show this more explicitly.
Manuscript	25		The "real world" potential role for wetlands noted (i.e., 100 acres of borrow pit per mile of levee) is very compelling. This paper does a good job of highlighting the benefits. Thank you for pointing this out! **Response:** Thank you!

Review #142—Anonymous

Reviewer summary to be shared with the author and editors:
Overall a fairly thorough and well-written manuscript. Contributes positively to the broader field of Ecological Engineering through both field monitoring and modeling of a borrow pit constructed wetland approach for water quality improvement. The research also covers modeling of different scenarios to optimize performance of borrow pit CWs. This work is thorough and fairly comprehensive. Some reasonable assumptions are made for purposes of modeling. See detailed reviewer notes for additional comments related to justification or contextualization of assumptions. In general, captions could be improved to allow them to stand alone. Discussion does provide appropriate comparison to relevant literature. One area for improvement is more discussion of differences between monitoring and modeling results from this study. If implemented, this study will positively contribute to ecological

engineering design and implementation of nature-based solutions for water quality improvement in agricultural areas.
Response: Thank you for your review and detailed comments. We have updated figure captions and added additional discussion comparing the modeling and monitoring results. We responded to each of your specific comments below.

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

39. Length – 6833 words plus two tables and seven figures. While the word count itself is reasonably close to the target, the number of figures add to that word count equivalent.

    Response: JEED does not use a word count equivalent, only the total number of words excluding figures, tables, and references. However, we have tried to reduce the word count some (by streamlining results), but also needed to add text to respond to reviewer comments.

40. Lines 135-147 – Some confusing language and comparison to Figure 1. Does bi-weekly mean every two weeks or two times per week? Sampling frequency for water depth (continuously is stated but was this 1 minute, 15 minute, hourly?)? Description about composite sampling is confusing, five locations but which samples were composited? This could be important as it could lead to different results or interpretations depending on the flow path through the wetland.

    Response: These points were clarified in the text. Biweekly refers to every other week. Water levels were recorded every 15 minutes. Three samples were collected in each sampling zone (e.g. Wetland 1a) and composited (Lines 150-161).

41. Lines 234-246 – Provide additional justification and clarification on calibration of K and uncertainty. Provide explanation of estimation of how datum was set and why wetland water elevation was determined using modeling instead of observation.

    Response: Additional details were added. Modeled water surface elevations were used because these were calculated based on all modeled components of the water balance. These were compared to measured values to calibrate the K value. The aquifer datum was assumed based on a reasonable aquifer thickness. Assuming a different datum would change the calibrated K value, but would not change the simulated groundwater flux (Lines 261-266).

42. Line 263 – Please include another sentence of the assumed 500 μg/L, is this considered conservative, is there a range, how sensitive is the balance to this assumption?

    Response: We have added additional context here and noted that since atmospheric deposition was a relatively small component of the mass balance, our assumed value should have little impact on the results (Lines 284-287).

43. Lines 263-272 – Please justify or contextualize this assumption of 50% reduction.

    Response: We have added additional context and references here (Lines 294-299). Nutrient retention can vary widely depending on many site-specific factors. We assumed a single value for the sake of modeling, but recognize that this introduces uncertainty into our analysis.

44. Line 371 – Why do you think the model overestimates in the summer?

    Response: This is likely a result of calibrating hydraulic conductivity K. Lowering K results in slightly better predicted water levels in 2024, but less accuracy during 2023. We have added a sentence to note this (Lines 405-406).

45. Lines 417-418 – Seems to be against what is stated in the abstract lines 42 and 43. Please explain further.

    Response: See response to comment #48 below.

46. Figures 1 and 2 – Please indicate where in the wetland the model the model is showing.

    Response: This comment is somewhat unclear. We have updated Figure 1 to more clearly show the two wetland cells (wetland 1a and 1b). Figure 2 shows components of the wetland model (water balance and nutrient mass balance) as green rectangles.

47. Table 1 – HLR is not defined

    Response: A definition of HLR was added to the table caption.

48. 460-469 – Seems to be against the monitoring results indicated in lines 417-418. Please explain further.

Response: The results from lines 417-418 (original manuscript) is based on baseline modeling (not monitoring) of historic wetland performance. This is compared to results from model scenarios where we artificially increased incoming nutrient loads (lines 460-469 in the original manuscript). This discrepancy is due to how the increasing loads scenarios were modeled. In these scenarios, total incoming nutrient loads were increased without changing incoming flows (and therefore not changing hydraulic loading rate). This allowed for more efficient nutrient removal in these scenarios (in line with the literature). In the baseline scenario, on the other hand, years with higher nutrient loads also had higher hydraulic loading rates. These higher hydraulic loading rates are what reduced nutrient removal efficiency for these years. This was clarified in the text (Lines 527-533).

49. Lines 460-478 – Please provide a discussion comparing the modeling results to the monitoring results.

Response: We have added additional text describing differences between the increased loading scenario results and the baseline scenario modeling results (Lines 526-532). We also reference monitoring results more in our discussion of baseline modeling results (Lines 463-465).

50. Lines 493-508 – Does the MO River depth play a larger role that passive depth control? Discuss how this would be different in other river adjacent wetlands.

Response: Yes, the MO River water level has a larger influence than adding the culvert in that the river water completely restricts surface outflow when it is above the culvert elevation. This is somewhat unique for the study wetland that is located on the landward side of a levee. Wetlands on the riverward side (or wetlands in areas with no levee) would be inundated during periods with high river water levels. Other river-adjacent wetlands in the literature are less connected to the river (e.g. receive pumped water rather than natural inflows). This additional context was added to the text (Lines 566-569).

51. Conclusion – Any limitations to the study or gaps that still need to be answered? Recommendations for future work? Second paragraph would almost fit better in the introduction. References traditionally not included in conclusion.

Response: We have added recommendations for future work to the conclusions (Lines 587-596). We have elected to keep the final paragraph in the conclusions to reinforce the potential scale and applicability of this approach for managing runoff to these large river systems.

Review #139—William Strosnider

Reviewer summary to be shared with the author and editors:
This study describes a coupled field-data and modeling approach for better understanding the water quality improvement capacity and hydrology of borrow pit wetlands in floodplain settings. The study is well written, all Methodology seems strong, and all conclusions are supported by theory and the data. Therefore, I recommend this paper be accepted either “as is” or with minor revisions. I have never suggested a paper being accepted “as is” in roughly 100 reviews over my career. This is a high quality submission and as a reviewer I am appreciative of the authorship team’s care in producing this manuscript.

Response: Thank you for your kind words. We have addressed your specific comments below.

Abstract:
Nice work
You should add a mention of the ubiquity of borrow pits in these settings for USACE purposes. This underscores the applicability and importance of your work.
Response: We have noted this in the last sentence of the abstract (Lines 49-51).

Introduction:
Good job contextualizing the work

Methods:
Seem strong and reasonable.

Results:
At your pH, you didn’t have NH_3, you had NH_4.
Response: This was changed throughout.

Nice job comparing to Kadlec and Wallace percentiles!
Yes, your lower removal rates are due to relatively low incoming loads. You can state this with more certainty.
Response: We have stated this more confidently (Line 454-455).

Line 435: values, not value
Response: Corrected.

Conclusions:
What research should happen next? Perhaps monitoring of other similar sites to create a more generally applicable model?

Response: We have revised the conclusions to provide more recommendations for future research (Lines 633-640).