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Peer Review File

Multi-Objective Wetland Design for Water Quality and Waterfowl Habitat

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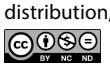

Associate Editor Summary (Anand Jayakaran, Washington State University)

Thank you for submitting a well-crafted manuscript that three peer reviewers reviewed. My apologies for the long review time. We initially struggled to find reviewers but eventually found three very qualified peers to review. All the reviewers thought the work was an excellent fit for *JEED* and addressed a topic important to ecological engineering practice. The reviewers have offered suggestions to improve the paper listed below - please pay attention to nomenclature suggestions from reviewer 3 and the suggested title change from reviewer 1. We hope to see a revised version of the manuscript soon. My decision for this paper is that a moderate revision is needed for *JEED* to consider this manuscript for publication.

Reviewer 1 (Anonymous):

Reviewer summary to be shared with the author and editors:
This research case study explores the potential for wetland management techniques in improving phosphorus retention and waterfowl habitat provisioning. The authors also developed a model and spreadsheet tool to quantify water quality and habitat responses to various management strategies. It is a quality submission that fits the theme of JEED quite well. The multi-objective theme needs to be addressed more in our field of Ecological Engineering. Therefore, I suggest the manuscript be accepted following the minor revisions noted below.

Detailed reviewer notes to be shared with the author and editors:
Title:
Perhaps; “Multi-objective wetland design for water quality improvement and waterfowl habitat provisioning” or not. This is a more specific paper than the title indicates.

Abstract:
Remove “one of the great Laurentian Great Lakes”
Don’t define acronyms you don’t use again in the abstract

Introduction:
Throughout introduction:
1. It is unclear whether the authors are referencing natural wetlands, restored wetlands, or constructed wetlands throughout the introduction. I suggest the use of more descriptive language to avoid confusion.

2. Suggest additional information and literature regarding P loading in freshwater ecosystems. While it is clear throughout the introduction that this article will focus on P and waterfowl habitat, the connection between the two could be stronger. Also, why are wetlands so important to waterfowl? Macroinvertebrates communities ought to be mentioned somewhere in the introduction.

Line 57: Define acronym for harmful algal bloom (HAB) where first used

Line 57: Revise to “household pets”

Line 58: Suggest providing examples of which aquatic waterbody uses are most threatened by HABs (e.g., drinking water reservoirs, recreation)

Line 66: Define acronym for total phosphorus (TP) where first used

Line 71: Singular objectives such as what?

Line 95: Good use of the descriptor “engineered” wetlands here. Suggest focusing more of the introduction on engineered wetlands after first introducing wetlands broadly in paragraph 1.

Case Study Site:
Line 102: Revise to “HABs within Lake Erie”

Line 105: Convert square miles to square kilometers

Line 106: How does row crop agriculture relate to nutrient practices and wetlands?

Lines 110 – 114: Common species names should not be capitalized

Line 111: Revise to “mallard duck”

Line 115: Suggest revise to include more context on why populations are declining

Methods:
Kadlec and Wallace is 2009, not 2008!

Line 123: What is the difference between waterfowl and waterbird? Can one term be used instead of both?

Line 125: Revise “generated hydroperiods on the monthly basis” to “generated monthly hydroperiods”

Line 129: Revise “sections” to “subsections”

Figure 2 caption: Include “and” before “waterfowl/bird”

Line 141: Suggest replacing “checked” with a different term

Line 143: Why were groundwater inputs and outputs ignored?

Line 145: Citation for Thornwaite method?

Line 147: Citation for SCS CN method?

Line 154: Suggest revise to "using the k-C model outlined in (Kadlec and Knight 1996)"

Lines 159 – 161: Suggest adding units in parentheses following a given parameter rather than using "in X"

Line 166: Replace decades ago with "in 1996"

Lines 168 – 169: Strange wordage, perhaps revise using "most appropriate for this application"

Line 180: Is this a comprehensive list of all of the waterfowl species which inhabit the study region? A relevant citation would be helpful.

Lines 188 – 192: This section reads a bit rudimentary. I suggest adding more details and expanding this section significantly, emphasizing why this spreadsheet tool is unique and useful for stakeholders which include …?

Table 1: Convert inches to cm in "Preferred Depths" column

Section 3.2.1.: Is one precipitation gauge appropriate to determine wet, dry, and average precipitation years? Perhaps one from each state or one from a given spatial extent (i.e., 200 sq. km) would be more appropriate?

Lines 217 – 218: Convert hectares to square km

Lines 214 – 216: Rephrase. "Some have said" is not appropriate

Line 221: It is not clear what "Water depths acted cumulatively from month to month" means. Rephrase.

Line 223: Is constant water depth across the wetlands an appropriate assumption? How does this compare to other wetland nutrient retention models?

Lines 228 – 233: It seems that use of "Active 2 and 3" nomenclature for the two static management scenarios represent an oxymoron. Perhaps, you could instead have four management scenarios: Passive 1, Active 2, Static 3, and Static 4? Or name the management scenarios based on their water level control depth (e.g., 2 m, 0.1 - 1 m, 0.2 m, 0.1 m).

Line 244 and throughout: The term “wetland data points” seems rudimentary. I suggest listing the actual parameters or creating categories (e.g., physical or hydrological features).

Lines 245 – 247: Rephrase.

Line 275: Suggest replacing (equation 2) with (Eq. 2)

Line 278: Replace “lined up” with “aligned”

Line 279: Rephrase.

Line 280: Rephrase, “fell close” is not appropriate

Lines 282 – 284: Use of parentheses seems odd here

Results:
Throughout Results: remain consistent with percent reporting (e.g., 3% [line 359] vs. three percent [line 368]).

Line 413 & 434: “Lower water depths (< 0.5 m)”

Lines 474-475: Suggest revise “struck a balance”

Line 482: Suggest summarizing the results of Baschuk et al., 2012 on drawdown effects on waterfowl biodiversity

Lines 502-503: Included species richness values which double (e.g., X to Y)

Line 515: Replace “in many instances” with “in this region”

Line 518: Is it biased to claim that Ducks Unlimited is the “world leader in wetland conservation”? Seems like overly strong language

Throughout: It is not clear why waterfowl species richness was used instead of abundances or another diversity index. Suggest clarifying in Methods section.

Line 533: Consider listing one or two of the major shortcomings of k-C model here.

Line 556-557: Is it necessary to use DRP acronym when it is only used twice throughout the manuscript?

Line 572: Is it necessary to use NbS acronym when it is only used twice throughout the manuscript?

Line 584: Suggest a stronger closing sentence connecting wetland management tool and approaches to nutrients, HABs and wildlife conservation
In general, I suggest more descriptive figure captions.

Reviewer 2 (Dani Winter-Lay, Purdue University):

Reviewer summary to be shared with the author and editors:

This manuscript successfully places wetland design and management at the forefront, making it a strong fit for the mission and vision of JEED. The findings related to wetland design and management are quite clear to the reader. The authors found that management is a stronger driver than wetland area and that ideal wetlands for P retention and waterfowl habitat suitability are larger wetlands (at least 2-7% of the incoming watershed) with lower water depths and active, dynamic water management. The findings related to the impacts of water management of P retention in wetlands align with findings in previous literature, but the overlay of the tradeoff of waterfowl habitat provisioning is a new take and is highly relevant for wetland management in the region.

The findings of the study are generally clear to the reader based on the text, but, in some cases, it is unclear at first glance what values are represented in the figures. Some additional labels and information in captions will improve clarity for readers and help better link the text to the visualizations.

Hydrologic regime is the paper's focus, which makes sense for assessing tradeoffs in waterfowl habitat and P retention. It would be helpful to qualify uncertainty in the findings based on variation in other key drivers of P retention, such as certain soil properties and hydrogeomorphic setting, and to describe similarities and differences between wetland data points and wetland conditions in the Maumee River Watershed beyond land use in the drainage area.

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

Introduction:
Restored wetlands, particularly those previously used for agriculture, can also be a significant source of P upon restoration. Maintaining wetlands with ponded water for prolonged periods, like for waterfowl habitat, has been associated with a higher risk of releasing P, so this investigation of water management is especially critical for assessing downstream impacts of wetland restoration.

61-76: Literature on the impacts of hydroperiod and water depth on P retention in wetlands (e.g., Hydrologic regime controls soil phosphorus fluxes in restoration and undisturbed wetlands by Aldous et al., 2005), will further support this discussion of the potential controls and is especially relevant in considering hydrologic management for waterfowl.

Perspective on how practitioners approach sizing wetlands seems to be primarily informed by discussions by Ducks Unlimited. These discussions with Ducks Unlimited are highly relevant given the focus on waterfowl/waterbird management. In addition to these "rules of thumb", regulations for compensatory mitigation projects may also guide sizing for wetland restoration projects.

73-76: Examples of the types of spatial data typically used for habitat suitability assessments will be helpful for readers.
Consider adding clear definitions of active and passive management to the introduction, including examples that are relevant to the region.

Materials and Methods:
Groundwater inputs and outputs were not considered in the water balance. Are the wetlands included in the model dominated by surface inputs and outputs because of their design and hydrogeomorphic setting? Some discussion around this assumption would be helpful to understand this choice, especially for a watershed with such flat topography and an abundance of tile drainage.

162 – extra words: 'in found in'

221 – 'Water depths acted cumulatively from month to month.' Consider rewording this for improved clarity.

231 – Consider listing months designated as waterfowl hunting season for the simulations and providing a more specific description for 'gradual drawdown' to aid replication of this approach.

240-247 – Did these literature reviews report daily, monthly, or annualized P retention values? It will be helpful to clearly state the data types gathered from these studies and any adjustments and assumptions that had to be made to input this data into the models.

240-247 – The wetland dataset for this case study was built based on selecting studies that also had agricultural land use in the wetland drainage areas. Were data from similar watersheds in the agricultural Midwest available and included in this dataset? Do most of the included studies have similar crops and agricultural practices as those found in the Maumee River Watershed? Certain soil characteristics, such as texture, mineralogy, and existing soil P stocks upon restoration, are known drivers of P release in restored wetlands. How do the soil characteristics of these studies

compare to the soils found in wetlands in the MRW? I think that this section could be further strengthened by additional discussion of why these data points are representative of the Maumee River Watershed. Alternatively, these studies may have demonstrated that there is limited variation in performance among agricultural wetlands regardless of the other wetland characteristics. Readers could benefit from further discussion about why these studies are transferrable to a model for the Maumee River Watershed.

Results:
The most important findings are clear to the reader: active management at 0.10 m depth is ideal for both P retention efficiency and waterfowl habitat provisioning. Dynamic active management is best for waterfowl on an annualized basis but still has notable P retention benefits.

Some of the results on plant species/vegetation species richness seem to come up abruptly in the results with limited context from previous sections. Further information about the modeling of plant species richness in the methods section will be helpful to readers.

Figure 3 – Is the 'preferred operation range' based on the P retention efficiency, or is this the preferred operating range for waterfowl/waterbird management superimposed on the plot? It is unclear to me how the preferred operating range was set at <0.5 m here.

Figure 4 & 5 – Multiple trials of each scenario were run (Lines 274-275). Does each bar represent a summary statistic for these trials (i.e., mean or median)? Could an error bar be added to these results to represent the spread among the trials for each scenario?

Figure 6 – The caption states 'annual P retention and P output,' but all labels on the figure say 'monthly.' Is this for a particular month or a measure of central tendency?

Figure 7 – I think that this plot represents some measure of central tendency for data filtered to be within the interdecile range or interquartile range, but the plot is labeled as if it represents the size of these ranges themselves. I would consider more specific labels and changes to the caption to highlight what data are being displayed.

Discussion:
Findings are once again clear in the discussion. The authors do a great job of putting the results in perspective of what is common practice for wetland restoration and management efforts aimed toward waterfowl in the region.

426-435: The authors did a good job of comparing findings of P loadings rates to other studies and can apply this framework to put their findings in the context of other studies of the impacts of water levels and hydroperiod on P retention. This literature is referenced in Lines 492-494, but it could be helpful to expand a bit more beyond water level.

448-458: Dry periods also impact the crystallinity of minerals associated with P storage, potentially impacting P release upon rewetting.

497: Could this potentially be re-worded for improved clarity?

563-565: Is this the median for all scenarios or for the scenario that is best for both P retention and waterfowl habitat provisioning?

Conclusions
572 – NbS acronym is not defined and is only used once. I recommend just writing out nature-based solutions once more here.

581 – I think that 'potential effects of climate change' in this case is meant to refer to the impacts of climate change on waterfowl and P retention specifically. I recommend making it clear that this is not a reference to impacts on greenhouse gas production and emissions, as the presented results in the manuscript do not demonstrate that these specific design and management practices impact greenhouse gas production or emissions. Impacts on greenhouse gas fluxes could be discussed in the previous sections on potential future areas of research, as water levels and hydroperiod have strong impacts on methane and nitrous oxide production.

Reviewer 3 (Michael Rohde, Iowa State University):

Reviewer summary to be shared with the author and editors:

I found this work a timely and necessary addition to the literature and appreciated the opportunity to review it. My disciplinary expertise is in wildlife conservation and ecology, so I focused my review on the wildlife aspects and scientific merit in totality. I cannot speak as much to the details of the engineering components. There were three major areas I suggest the authors focus on in their revision.

1. The use of "habitat" throughout the manuscript is inconsistent and imprecise. This is common throughout the literature. I suggest the authors review Darracq & Tandy (2019) and Kirk et al. (2018) for more insight into the proper use of "habitat" and reconsider each mention of the term in the text considering their recommendations. In brief, "habitat" needs to be specific to species. Look into the citations I provided to clarify meaning throughout. Every time the term habitat is used in the manuscript, the authors should consider if the definition is correct. Consider changing the title with this in consideration as well.

2. Use of "waterfowl" and "waterbirds" is inconsistent. I suggest using just "waterbirds" throughout the article because it's more generic and inclusive of all the birds you studied,

whereas "waterfowl" is a specific order of birds (ducks, geese, and swans). I have pointed out a couple occasions of this below in my line-wise comments.

3. Finally, this work should work towards being more well placed within the context of primary literature, especially in reference to the wildlife. The amount of personal communication used in this manuscript is a slight cause for concern. Personal communication used sparingly can be acceptable and even meaningful. This manuscript uses them eight times throughout, seemingly as the foundation to many of the assertions for waterbirds. I suggest either including the communicator as an author or seeking out their expertise to place assertions within the context of primary literature. In this vein, I think you could bolster your rationale for using birds to indicate co-benefits. Birds are well studied, diverse and able to respond quickly to changes in landscape conditions. That's a strong rationale for focusing on them, even when other wetland taxa may be more critically imperiled. These features lend themselves to the modeling exercise quite well. In general, there are a handful of places that need to be bolstered with more support. I have attempted to point them out accordingly.

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

Line 57-58: HABs toxic sentence should have appropriate citation(s).

Line 61-77: This feels like it could be two paragraphs. One using the original topic sentence. The other about wetlands importance to waterbirds. Perhaps bolstering your argument for reasoning behind using waterbirds as a metric.

Line 83-85: See (Hall et al., 1997).

Line 88: Maybe environmental dynamics is more appropriate here rather than habitat.

Line 89-97: Objectives clearly stated great job.

Line 91: Consider establishing what an intermediate complexity model is. It may be common knowledge within engineering, but the authors provides no definition or citation. Defining this model type may be useful if attempting to reach a broader audience.

Line 109: Habitat used here almost works because you are referring directly to species. Would be correct if pluralized to habitats.

Line 111: Waterfowl are generally considered to be from the Order Anseriformes or often more specifically from the family Anatidae. Grebes are not included in either. This is part of the reasoning for using waterbirds as a more broad and accurate terminology throughout the manuscript.

Line 114-115: In text citation (Beillke et al. 2021) is not included in the references list.

Line 115: "wetland habitat suitability" should be reconsidered. See Darracq & Tandy (2019).

Line 130: I think this format of the model workflow is easily followed. Good work.

Line 145: Similarly, as with the intermediate complexity model, the Thornthwaite method may be commonplace in engineering, but some definition or citation should be used.

Line 177-185: The title of this section is Waterfowl/waterbird habitat suitability sub-model. The first sentence refers to waterfowl. It seems the authors are using this sentence as the support for the following paragraph, but no indication of waterbirds preferences is mentioned. As suggested above, using waterbirds throughout will add clarity to this manuscript.

Line 178: Consider reviewing (Kaminski & Elmberg, 2014) for more information towards avenues of support for this section with primary literature, especially waterfowl.

Line 196: Consider using metric instead of imperial. The citation Fredrickson and Reid 1986 uses metric.

Line 264: In reference to "…though this does not have seasonality incorporated into it": What are the limitations of your work? What if the wetland were perfect foraging depth, but it were also perfect depth for colonization of invasive plants like cattail or Phragmites? Could that constrain your method? I suggest the authors more transparently reflect the limitations of this design.

Line 265: the term "typical" is applied here with no support that this is typical marsh for any region other than Michigan due to the source. Great Lakes marshes may (likely) be very different from inland Great Black Swamp marshes. I encourage the authors to provide support for why the water depth ranges that were used here are "typical".

Line 281-284: This sentence was difficult to work through. Consider rewording or maneuvering around for more clarity. Perhaps removing parenthetical remark or making it into its own sentence.

Line 292: Figure 3 is a great visualization of what the authors are trying to convey. Really well done.

Line 518: Citing DU as "the world leader in wetland conservation" seems unconventional. Recent research from the US prairies shows that small wetlands and large wetlands have functionally the same probability of being used by breeding ducks, which may be worth reviewing and citing. (Mitchell et al., 2023).

Line 425: consider removing or rewording robustly.

Line 572: NbS, assuming it is an acronym for nature based system, is used here but not defined anywhere for what it means. Consider using a parenthetical (e.g. nature-based system (NbS)).

Line 572 – 575: I think this really hits the nail on the head as far as the main driving force behind this work.

References

Darracq, A. K., & Tandy, J. (2019). Misuse of Habitat Terminology by Wildlife Educators, Scientists, and Organizations. Journal of Wildlife Management, 83(4), 782–789. https://doi.org/10.1002/jwmg.21660

Hall, L. S., Krausman, P. R., Morrison, M. L., Hall, L. S., Krausman, P. R., & Morrison, M. L. (1997). The Habitat Concept and a Plea for Standard Terminology Linked references are available on JSTOR for this article : The habitat concept and a plea for standard terminology. Wildlife Society Bulletin (1973-2006), 25(1), 173–182.

Kaminski, R. M., & Elmberg, J. (2014). An introduction to habitat use and selection by waterfowl in the northern hemisphere. Wildfowl, 4, 9–16.

Kirk, D. A., Park, A. C., Smith, A. C., Howes, B. J., Prouse, B. K., Kyssa, N. G., Fairhurst, E. N., & Prior, K. A. (2018). Our use, misuse, and abandonment of a concept: Whither habitat? Ecology and Evolution, 8(8), 4197–4208. https://doi.org/10.1002/ece3.3812

Mitchell, B. J., Terry, C. V., Ringelman, K. M., Kemink, K. M., Anteau, M. J., & Janke, A. K. (2023). Wetland occupancy by duck broods in cropland-dominated landscapes of the United States Prairie Pothole Region. Journal of Wildlife Management, 87(2), 1–26. https://doi.org/10.1002/jwmg.22347

Response to Reviewers
"Multi-Objective Wetland Design for Water Quality and Habitat" (former title)
Maddie Carpenter and Brian Bledsoe
December 16, 2024

We would like to express our sincere gratitude to the reviewers for the time and effort they put into providing thoughtful and constructive feedback. We have accepted and incorporated all reviewer comments and recommendations with only a few exceptions as detailed in the

responses below. Line-by-line comments were all individually accepted and addressed, and a tracked changes version reflecting these changes is attached.

Reviewer 1:

Title: *Perhaps; "Perhaps; "Multi-objective wetland design for water quality improvement and waterfowl habitat provisioning" or not. This is a more specific paper than the title indicates.*

We have considered the change in title and have agreed that specificity in adding "waterfowl habitat" is appropriate. Because wetland design for water quality is always going to be for its improvement, we would prefer to avoid words like "improvement" and "provisioning" for conciseness.

Introduction: 2. Suggest additional information and literature regarding P loading in freshwater ecosystems. While it is clear throughout the introduction that this article will focus on P and waterfowl habitat, the connection between the two could be stronger. Also, why are wetlands so important to waterfowl? Macroinvertebrates communities ought to be mentioned somewhere in the introduction.

The current introduction concisely sets the stage for readers without delving into the specificity of all concepts involved in the research. We feel the revised introduction, which mentions macroinvertebrates and highlights the importance of wetlands across all life stages, does an adequate job introducing the concepts of P loading in freshwater ecosystems and connections between P and waterfowl habitat. Both concepts are further expounded upon in subsequent sections of the manuscript.

Lines 110 – 114: Common species names should not be capitalized

The majority of scientific avian publications capitalize common names for birds because they have been given names by the International Ornithological Congress that distinguish descriptions from taxonomic species.

Line 111: Revise to "mallard duck"

The U.S. Fish and Wildlife Service and numerous other wildlife organizations note the common name for *Anas platyrhynchos* is Mallard.

Line 123: What is the difference between waterfowl and waterbird? Can one term be used instead of both?

As you know, waterfowl are a specific order of birds (Anseriformes). We would like to keep the separate distinction between waterfowl and waterbirds, as waterfowl are the primary focus of this study and tend to draw a broader audience than simply "waterbirds".

Line 143: Why were groundwater inputs and outputs ignored?

The system is dominated by surface water and data on groundwater fluxes were simply not available to include the water balance. This has been noted in the text.

Line 180: Is this a comprehensive list of all of the waterfowl species which inhabit the study region? A relevant citation would be helpful.

The citation is given in the Table 1 caption and we added a sentence on why those birds were chosen.

Lines 188 – 192: This section reads a bit rudimentary. I suggest adding more details and expanding this section significantly, emphasizing why this spreadsheet tool is unique and useful for stakeholders which include …?

We appreciate your suggestion to include the stakeholders for whom this tool will be unique and useful. We removed the assertion of uniqueness and added that the tool is applicable to wetland designers and managers. Details emphasizing why the spreadsheet tool is useful is outlined in a previous section (i.e., Section 3.1).

Line 223: Is constant water depth across the wetlands an appropriate assumption? How does this compare to other wetland nutrient retention models?

Microtopography is an important aspect of the abiotic template. We were hindered from incorporating it by a lack of accurate LiDAR, but we also are striving for a parsimonious model. Incorporating a high degree of spatial heterogeneity in the retention modeling would could push standard retention models beyond their limits

Lines 228 – 233: It seems that use of "Active 2 and 3" nomenclature for the two static management scenarios represent an oxymoron. Perhaps, you could instead have four management scenarios: Passive 1, Active 2, Static 3, and Static 4? Or name the management scenarios based on their water level control depth (e.g., 2 m, 0.1 - 1 m, 0.2 m, 0.1 m).

We agree that the Active terminology is less than ideal for describing management approaches where the water level is set at a particular target depth. However, we respectfully request that the current terminology be maintained given that the management approach is not entirely passive.

Line 244 and throughout: The term "wetland data points" seems rudimentary. I suggest listing the actual parameters or creating categories (e.g., physical or hydrological features).

We changed "wetland data points" to observations of wetland fluxes. Details are specifically listed in the Supplementary materials.

Line 275: Suggest replacing (equation 2) with (Eq. 2)

Done.

Line 482: Suggest summarizing the results of Baschuk et al., 2012 on drawdown effects on waterfowl biodiversity

The results of the study were concisely summarized with the study concluding that utilizing a drawdown series is the best management strategy for biodiversity.

Throughout: It is not clear why waterfowl species richness was used instead of abundances or another diversity index. Suggest clarifying in Methods section.

Richness was selected at the recommendation of Ducks Unlimited scientists.

Line 533: Consider listing one or two of the major shortcomings of k-C model here.

Done. We listed plug flow assumptions and simplification of soil-water-plant interactions.

Line 584: Suggest a stronger closing sentence connecting wetland management tool and approaches to nutrients, HABs and wildlife conservation

Done.

Reviewer 2:

Hydrologic regime is the paper's focus, which makes sense for assessing tradeoffs in waterfowl habitat and P retention. It would be helpful to qualify uncertainty in the findings based on variation in other key drivers of P retention, such as certain soil properties and hydrogeomorphic setting, and to describe similarities and differences between wetland data points and wetland conditions in the Maumee River Watershed beyond land use in the drainage area.

We do not have the data available for this type of analysis. Hydrogeomorphic setting is available to some extent, but would not be comparable against the types we studied. It is important to keep in mind that we are not comparing this data to one specific wetland in the MRW. We are generalizing agricultural wetland behavior so as to make it more broadly applicable, with this region as a case study for how general agricultural wetlands may respond to various hydrologic regimes.

61-76: Literature on the impacts of hydroperiod and water depth on P retention in wetlands (e.g., Hydrologic regime controls soil phosphorus fluxes in restoration and undisturbed wetlands by Aldous et al., 2005), will further support this discussion of the potential controls and is especially relevant in considering hydrologic management for waterfowl.

Thanks, this citation was added.

Perspective on how practitioners approach sizing wetlands seems to be primarily informed by discussions by Ducks Unlimited. These discussions with Ducks Unlimited are highly relevant given the focus on waterfowl/waterbird management. In addition to these "rules of thumb", regulations for compensatory mitigation projects may also guide sizing for wetland restoration projects.

We provide an example of a rule of thumb that DU utilizes. These projects are not compensatory mitigation projects and such projects have specific regulations pertaining to planning and design.

Consider adding clear definitions of active and passive management to the introduction, including examples that are relevant to the region.

We added text describing active as involving intentional water level management.

Groundwater inputs and outputs were not considered in the water balance. Are the wetlands included in the model dominated by surface inputs and outputs because of their design and hydrogeomorphic setting? Some discussion around this assumption would be helpful to understand this choice, especially for a watershed with such flat topography and an abundance of tile drainage.

See response above regarding lack of data on groundwater fluxes and dominance of surface runoff processes in this context.

231 – Consider listing months designated as waterfowl hunting season for the simulations and providing a more specific description for 'gradual drawdown' to aid replication of this approach.

Specifics are provided in the Supplementary materials that would enable replication.

240-247 – The wetland dataset for this case study was built based on selecting studies that also had agricultural land use in the wetland drainage areas. Were data from similar watersheds in the agricultural Midwest available and included in this dataset? Do most of the included studies have similar crops and agricultural practices as those found in the Maumee River Watershed? Certain soil characteristics, such as texture, mineralogy, and existing soil P stocks upon restoration, are known drivers of P release in restored wetlands. How do the soil characteristics of these studies compare to the soils found in wetlands in the MRW? I think that this section could be further strengthened by additional discussion of why these data points are representative of the Maumee River Watershed. Alternatively, these studies may have demonstrated that there is limited variation in performance among agricultural wetlands regardless of the other wetland characteristics. Readers could benefit from further discussion about why these studies are transferrable to a model for the Maumee River Watershed.

The dataset used can be found in supplementary materials. Some of the data points did come from this general region (quite a few from Indiana and Ohio, most notably Jiang 2020 being in this region). However, because we wanted to look at general "agricultural wetlands" performance, we selected wetlands that had a broad range of conditions and performances, since otherwise data would be too limited for analysis. We do not know how the soil characteristics of these studies compare to the soils found in wetlands in the MRW. Soil characteristics were not a focus for this study.

Some of the results on plant species/vegetation species richness seem to come up abruptly in the results with limited context from previous sections. Further information about the modeling of plant species richness in the methods section will be helpful to readers.

This has been reworded for clarity and additional text describing modeling has been added.

Figure 3 – Is the 'preferred operation range' based on the P retention efficiency, or is this the preferred operating range for waterfowl/waterbird management superimposed on the plot? It is unclear to me how the preferred operating range was set at <0.5 m here.

We added text to the caption relating this range to both waterfowl and HLR.

Figure 4 & 5 – Multiple trials of each scenario were run (Lines 274-275). Does each bar represent a summary statistic for these trials (i.e., mean or median)? Could an error bar be added to these results to represent the spread among the trials for each scenario?

Figures 4 includes a legend that states that each bar represents the average or mean P retention efficiency and average number of waterfowl species. The y-axis on Figure 5 states that each bar represents the total number of species. We did not include error bars for Figures 4 and 5 due to avoid confusing readers as different data types are represented.

426-435: The authors did a good job of comparing findings of P loadings rates to other studies and can apply this framework to put their findings in the context of other studies of the impacts of water levels and hydroperiod on P retention. This literature is referenced in Lines 492-494, but it could be helpful to expand a bit more beyond water level.

We also reference hydraulic loading rate as a master variable in several instances.

448-458: Dry periods also impact the crystallinity of minerals associated with P storage, potentially impacting P release upon rewetting.

This P release mechanism was added to the discussion.

Reviewer 3:

1. The use of "habitat" throughout the manuscript is inconsistent and imprecise. This is common throughout the literature. I suggest the authors review Darracq & Tandy (2019) and Kirk et al. (2018) for more insight into the proper use of "habitat" and reconsider each mention of the term in the text considering their recommendations. In brief, "habitat" needs to be specific to species. Look into the citations I provided to clarify meaning throughout. Every time the term habitat is used in the manuscript, the authors should consider if the definition is correct. Consider changing the title with this in consideration as well.

After careful consideration, we propose to maintain our use of the word "habitat." While we acknowledge that the precise definition of "habitat" is species-specific, we believe our usage aligns with how the term is commonly applied in the broader literature and enhances communication to a broader audience of the topic and scope, given that most deem habitat to refer to the general environment that an animal or plant would occupy.

2. Use of "waterfowl" and "waterbirds" is inconsistent. I suggest using just "waterbirds" throughout the article because it's more generic and inclusive of all the birds you studied, whereas "waterfowl" is a specific order of birds (ducks, geese, and swans). I have pointed out a couple occasions of this below in my line-wise comments.

We would like to keep the separate distinction between waterfowl and waterbirds, as waterfowl are the primary focus of this study and tend to draw a broader audience than simply

"waterbirds". I have made corrections to help clarify consistencies between our use of waterfowl vs. waterbirds as mentioned in the line-by-line comments provided.

3. Finally, this work should work towards being more well placed within the context of primary literature, especially in reference to the wildlife. The amount of personal communication used in this manuscript is a slight cause for concern. Personal communication used sparingly can be acceptable and even meaningful. This manuscript uses them eight times throughout, seemingly as the foundation to many of the assertions for waterbirds. I suggest either including the communicator as an author or seeking out their expertise to place assertions within the context of primary literature. In this vein, I think you could bolster your rationale for using birds to indicate co-benefits. Birds are well studied, diverse and able to respond quickly to changes in landscape conditions. That's a strong rationale for focusing on them, even when other wetland taxa may be more critically imperiled. These features lend themselves to the modeling exercise quite well. In general, there are a handful of places that need to be bolstered with more support. I have attempted to point them out accordingly.

We understand your concern regarding the amount of personal communication and agree that it should be used only when necessary. We have added citations from the primary literature as described above and have attempted to bolster our rationale for using waterbirds for this study. Attributing the statements to a single individual from Ducks Unlimited and making them an author would not be appropriate, as the insights we included reflect a synthesis of input from a variety of professionals within Ducks Unlimited.

Line 91: Consider establishing what an intermediate complexity model is. It may be common knowledge within engineering, but the authors provides no definition or citation. Defining this model type may be useful if attempting to reach a broader audience.

We changed this to "parsimonious" model for improved clarity.

Line 178: Consider reviewing (Kaminski & Elmberg, 2014) for more information towards avenues of support for this section with primary literature, especially waterfowl.

Citation added.

Line 264: In reference to "…though this does not have seasonality incorporated into it": What are the limitations of your work? What if the wetland were perfect foraging depth, but it were also perfect depth for colonization of invasive plants like cattail or Phragmites? Could that constrain your method? I suggest the authors more transparently reflect the limitations of this design.

Text added: Plant species respond to growing season hydrologic regimes and fine scale elevational gradients. Accordingly, designs must be tempered with careful consideration of relationships between plant assemblages (including invasive species) and seasonal hydroperiod.

Line 265: the term "typical" is applied here with no support that this is typical marsh for any region other than Michigan due to the source. Great Lakes marshes may (likely) be very different from inland Great Black Swamp marshes. I encourage the authors to provide support for why the water depth ranges that were used here are "typical".

Removed the word "typical" to avoid misunderstanding.