**Second revision**

**JWM-22-0212 -**

**Editor comments:**

In addition, please revise the Supplemental Material and Figures. In Supplemental Material, delete the mile scale bar. The JWM does not use the Imperial system.

On Figure 1, delete the mile scale bar, only capitalize first words in the legend (e.g., NW MN marshbirds, MN DNR properities), and define all abbreviations in the figure in the title or spell them out on the figure. On Figures 2 and 3 only capitalize 1st words (e.g., Land cover types, Expected mean counts). Also, do not use scientific names in captions.

1. Make sure the title is 15 words or less.

Author response – the title has 10 words

2. Make sure the last paragraph of the Introduction is a clear statement of your objectives, hypotheses, and predictions only without introductory material or implications of the research. If the study is descriptive, say so.

Author response – the last paragraph of the Introduction includes our specific objectives and predictions. We are not sure what constitutes “introductory material” in this context.

3. The Study Area section must include the location (including country), size of area, time period of the study, elevation, topography, weather, climate, annual seasons, dominant fauna and flora, and land use.

Author response – the Study Area section includes the location of the study (northwestern Minnesota, USA and a map indicating the study are and geographical context), the period of the treatment (5 August–6 September 2015), the elevation of the study area (~285 –355 m amsl), weather and climate [Climate in northwestern Minnesota is classified as warm-summer humid continental with mean annual precipitation from 51–56 cm, with a small proportion of the total coming as snow (MNDNR 2015). Extreme minimum temperatures are -40 – -43⸰C and extreme maximum temperatures are near 35⸰C (MNDNR 2015)], and annual seasons (included in the description of climate as warm-summer humid continental). We believe that dominant flora are adequately described in the context of the wetlands we included in our study, and the general reference to the Ecoregion in which these wetlands were located. We do not believe that a description of the fauna of northwestern Minnesota is relevant to our study, except for the community of marshbirds that we studied. We have not provided a description of land use, except to note that our study sites were wildlife management areas managed primarily for wildlife by the Minnesota Department of Natural Resources.

4. Ensure that your Methods are clear and can be duplicated.

Author response – We believe that our methods are clear and can be duplicated. We have described some of our methods In more detail based on reviewer and AE comments on the previous two submissions of this manuscript.

5. Include an animal welfare statement in an ETHICS STATEMENT section above REFERENCES for all studies that involve vertebrates, including observational studies.

Author response – We have provided reference to our Animal Care and Use Committee protocol approval in the Methods and also repeat that information in the ETHICS STATEMENT section preceding the LITERATURE CITED section.

6. Provide a biological magnitude of effect for all variables deemed influential. Beta values provide magnitude, but it is difficult for the reader to quickly discern biological effect from these values. This requirement can be fulfilled in many ways, including examples in the text (e.g., predicted distance was 5 km for males and 15 km for females), odds ratio statements, or figures showing the relationship between variables.

Author response – Our response variable is change in counts of marshbirds by species post-treatment with changes in counts in reference areas as part of a BACI experimental design. We have provided estimates and associated errors for expected counts in Fig. 3.

7. Management Implications should include the implications of your research only. Do not repeat results, do not reference the results of others, and do not write about future research. This section can also be titled Research Implications or Conservation Implications.

Author response – we believe that the Management Implications section of our manuscript is concise and does not repeat results, nor does it speculate about future research.

8. Carefully examine the References for format and accuracy.

Author response – we have checked the reference to Literature Cited against the text, and conformed to the format in recent issues of the Journal of Wildlife Management.

9. All figure and table titles should include the animal being studied, topic, location (including country), and study period. Also, all abbreviations used in the table or figures must be identified. When maps are used, they should have a scale bar and north arrow above or adjacent to the scale bar. If you have panels in figures, identify them with an upper case letter within parentheses. Figures should not have horizontal and vertical lines within them, top or right hand rules (i.e., lines), or boxes around them, and only the first word of the axis or labels should be capitalized. All axes of figures should be identified even if they are obvious.

Author response – we are studying a suite of birds and not a single species, and refer to this suite of birds as “marshbirds” in table and figure legends and throughout the text. We indicate the location of the study as “Minnesota, USA” in tables and figures. Figs. 1 and 2 include maps and we have incorporated a scale bare and north arrow adjacent to the scale bar. Figure 3 summarizes model results with individual panels for each species of marshbird we included in our analyses, with acronyms identified in the figure legend. It is not clear to us that we would want to individually label these panels, which would unnecessarily make the figure more complex. Please provide guidance about whether it is necessary to identify panels with upper case letters in Fig. 3. Regarding Figs. 1 and 2, it seems to us that these require a boundary around them, as they are maps. Please provide guidance about whether these figures should have boxes. There are no axes in Figs. 1 and 2, and both axes are labelled in Fig. 3.

10. Follow the JWM guidelines (e.g., use the active voice, use common names throughout text, tables, and figures but define scientific name after first mention). Please examine the first 10 pages of the JWM guidelines for general instructions. The remainder of the guidelines can be used for more information.

Author response – we have used active voice and common species names throughout the text, tables and figures. We have provided the scientific name at first mention in the text.

11. The Wildlife Society and editors of JWM expect creating and sharing well-documented data and analysis code. This offers a straightforward way to increase the reproducibility of wildlife science. Please do so.

Author response – if the Journal of Wildlife Management accepts our manuscript for publication, we intend to provide data and R code in the Data Repository for the University of Minnesota (DRUM).

**Associate Editor comments:**

This paper examined the effects of herbicide applications (to reduce cattail) on marsh birds in the Prairie  
Pothole Region. The authors used a very rigorous design: a paired BACI design and examined the effects  
of herbicide for 3 years following the herbicide treatments at 9 pairs of sites. The paper is concise and  
well-written, and the revision did a good job of addressing many (but not all) of the reviewers’  
suggestions and concerns.

I did not read the original version, and the revision was my first exposure to the paper. Below are some  
comments to consider:

Line #:

82: diminished “quality” based on what species and in what way? I suggest you revise this sentence to  
merely say “...remaining wetlands have reduced plant diversity and structural heterogeneity....”. Let  
readers decide whether that is good, bad, or not important.

Author response – revised as suggested.

91: other citations are warranted here in addition to Bolenbaugh et al. See chapters in Tacha and Braun  
1994 Migratory Shore and Upland Game Bird Management in North America, and see species accounts  
in Birds of the World, among others. Also see Harrity et al. 2020 which discusses the positive  
relationship between hemi-marsh and rail habitat quality.

Author response – we appreciate the suggestions of other relevant literature and have added references to this description based on this suggestion and also suggestions from reviewers of this and the previous submission of our manuscript. However, in another recent submission to JWM, we were advised that it was not appropriate to include references to Birds of the World species account (we aren’t clear why that is, but perhaps there is a desire to focus on the primary literature?). We have reviewed Tacha and Braun (1994), and have the same concerns about citing those general summaries, but have included reference to some of that work in our revised manuscript. We appreciate the suggestion to include reference to Harrity et al. (2020), but are reluctant to draw from remotely derived rail-habitat relations for a species of rail that occurs in such a different landscape and that does not occur in the Prairie Pothole Region. Partly for the same reason, we have not included reference to a recent summary of marshbird-habitat relations in the Great Lakes watershed, where marshbird abundance was positively related to amount of cattail cover in wetlands (see our response to the Associate Editor and reviewer comments in our first revision). In Prairie Pothole Region wetlands, the conservation issue is generally monotypic stands of invasive cattail that negatively influence marshbird abundance, rather than a lack of vegetation structure provided by cattail.

97-101: also see Stevens and Conway 2020 which shows range-wide relationships between marsh bird  
use/occupancy and native emergent vegetation, unimpeded/undeveloped water regimes/hydrology.

Author response – we appreciate this reference, but have not included it in our revised manuscript, as the models developed therein are at a much broader spatial scale than seems comparable to our study.

288-292: One potential problem with any Before-After comparison can occur if the treatment affects  
detection probability and the methods for assessing abundance can not account for differences in  
detection caused by the treatments. One approach that would help is to conduct your analysis with only  
vocal detections. Opening up the cattails will undoubtedly increase visual detection probabilities, but it  
likely doesn’t increase vocal detection. If your results suggest similar effects of the treatments with only  
vocal detections, that would be evidence that the differences don’t merely reflect increased detection.  
This analysis should be pretty quick and easy to do, and you can merely state in one sentence of the  
paper that the results were the same (if indeed they are) when only vocal detections were used in the  
analysis. If the results end being different when only vocal detections are included, that suggests the  
problem above is real.

Author response – we have rerun models using only aural detections, and these models produced similar results as those from the models using both aural and visual detections. We have described this assessment in the Methods, included a summary statement in the Results, and modified the Discussion to address the potential effect of vegetation changes on visual detection probability. We have also modified Table 1 to indicate the number of visual and aural detections. We have not provided model results (following the AE suggestion), but could include these model results in the manuscript or supplemental material, upon request.

329==336: What if the herbicide caused other negative effects on marshbirds, and possibly even created  
an ecological trap (e.g., if marsh birds were attracted to recently treated marshes due to the preferred  
habitat structure but the herbicide decreased reproduction or food)? Consider mentioning this explicitly  
here.

Author response – we have added “or direction” to our presentation of the recognition that we do not know how herbicide application might potentially affect food availability. We are reluctant to speculate further than that, or suggest that we have any insight into the possibility that changes in vegetation composition and structure may result in an ecological trap. Our objective was to assess marshbird response to herbicide application to control invasive cattail, based on counts of marshbirds. Assessing the potential for the presence of an ecological trap in conjunction with a lagged increase in marshbird counts seems quite speculative to us.

Both reviewers commented on the importance of including measures regarding changes in vegetation  
structure caused by the treatments. Why is it not possible to use remote sensing products to document  
the change in vegetation across the 4 years at these treatment sites and include that in the revision?  
NDVI is one such metric that is readily available which could document changes in vegetation across the  
4 years. Landsat imagery seems like another potential option.

Author response – we have assessed indices of broad-scale changes in vegetation (NDVI) and extent of open water (NDWI) and present those assessments in Fig. S1. As this was not a part of our original study design and a post hoc evaluation using indices derived from remotely sensed data, we have included this information in the Supplementary Material rather than as primary results in the text. We are happy to consider incorporating this information into the body of the manuscript at the request of the Associate Editor. As currently revised, we describe this assessment in the text in both the Methods and Results.

**Reviewer 3 comments:**

The researchers evaluated the response of marshbirds to herbicide application of cattail dominated wetlands in the PPR of Minnesota. The paper is well written and easy to follow. Given the lack of information available on this subject, the work is necessary and useful.

I have a few comments and small edits to consider, however, it feels like the paper would be much more impactful had information concerning changes in habitat features such as species composition, structure and water availability been monitored and included in the analyses. Based on table 2, it appears marshbird numbers were all over the place with respect to control versus treatment and while I acknowledge there were significant findings for a couple of the birds during year 3 the others remained highly insignificant indicating no change in numbers for 3 species. Which makes me question why? Did something specific happen with respect to habitat that are conducive to nesting Sora and Virginia Rails, but not the other birds studied? Is there anything about their nesting behaviors that would have made Rails react positively to the treatment? How about the lack of response from the other birds, could that be linked to specific traits of these species? This information may help explain your findings beyond just speculating about habitat conditions following treatment application. With all the other noise in the environment, can we be certain the changes observed are in fact the result of the treatment? Outside of a few photographs provided in the supplemental materials and visual observations, it's hard for the reader to know exactly how the habitat was influenced.

Author response- in response to Associate Editor comments, we have provided a post hoc assessment of changes in NVDI (i.e., greenness) and NWDI (i.e., water reflectance) to better understand the effects of herbicide application on these wetlands. We have also characterized land-cover-type composition surrounding survey locations to demonstrate the general similarity between treatment and control survey locations prior to herbicide application in the late summer and early fall of 2015. These assessments are based on rather large-scale measures of wetland condition and the landscapes in which they are embedded, and don’t provide information on micro-habitat characteristics that may be important and vary among marshbird species. Marshbird-habitat relations at the micro-habitat scale are not well documented, and unknown in the landscape we studied, so we are reluctant to speculate at that scale about why some marshbird species responded positively to herbicide control of invasive cattail and others didn’t. We have incorporated additional reference to published information on marshbird-habitat relations to place our results in context.

Line 109 - you didn't actually assess changes in wetland condition, so I would recommend rewriting this opening sentence.

Author response – we revised this sentence to make clear that we assessed how the application of herbicide to control invasive cattails influences marshbird populations.

Line 117-120 - you acknowledge that the result of the treatment may have a different impact on the different species you studied which is good, and you should expand on this more in the discussion because those associations are mentioned (not explained) here but not much elsewhere.

Author response – our study design did not allow us to assess mechanisms resulting in changes in marshbird populations associated with herbicide application to control invasive cattails. In response to other Associate Editor and reviewer comments, we have provided a post hoc assessment of changes in vegetation (NVDI) and water reflectance (NWDI) following herbicide application, and evaluation of land-cover-type composition surrounding treatment and control survey locations. However, we do not have information regarding habitat characteristics at the micro-habitat scale and how those may be associated with marshbird populations. We provide further reference to pertinent literature to put our results in better context, and suggest in the Discussion that differences in marshbird-habitat relations among marshbird species vary, and could help explain the variation we observed among species. We have added the following as the last sentence in the Discussion – “Furthermore, although the general patterns among populations of marshbird species we monitored were similar, the strength and magnitude of responses varied among species, suggesting that other aspects of marshbird-habitat relations also likely influenced marshbird populations in the systems we studied.”

Line 122 - I may have missed it, by why only study wetlands greater than 30 ha, are some of the species sensitive to wetland size?

Author response – we have revised the second sentence in this paragraph to read as follows – “This landscape has low relief and high water-holding capacity, resulting in large (>30 ha) pooled basins with slow overland water flow and peat bog conditions…” We suspect that there may be an influence of wetland size on marshbird abundance (e.g., pied-billed grebes generally inhabit larger wetlands with open water), but our point here is that the wetlands in this landscape are generally large, and we therefore focused our sampling on large (>30 ha) wetlands. The revised language should make that clearer.

Line 157-158 - please provide the rate of the herbicide used without the surfactant, I believe the 7.02 litters/ha is both the surfactant and herbicide combined.

Author response – we have reviewed and recalculated prescribed rates of herbicide application from the information provided to us by the MN DNR that specified in a third-party contract how herbicide was to be applied:

Apply the herbicide Rodeo or equivalent aquatic certified glyphosate (equal or greater rate of active ingredient as Rodeo -- 53.8% glyphosate) at a rate of 3 quarts/ac, including the non-ionic surfactant Activator 90 at a rate of 0.5% (4 pts/100 gallons) per solution volume and ammonium sulfate at a rate of dry product at 18 lbs/100 gallons solution (or equivalent concentration if using liquid AMS). ...We require at least 5 gallons of total solution be applied per acre.

Under these terms, the rate of application of active ingredient was between 3.77 liters/ha and a maximum of 7.02 liters/ha (if the contractor used 100% glyphosate in the solution). We have revised our description of herbicide application to include both the minimum (3.77 liters/ha) and theoretical maximum (7.02 liters/ha) rate if active ingredient application, and that the herbicide solution was specified to be applied via a minimum volume of ~47 liters/ha. We have revised our description of herbicide application in the Methods to indicate that these were target rates.

Line 160 - consider deleting (backpack and amphibious vehicles).

Author response – we have added “via” to the parenthetical reference to application method to be consistent with our prior description of application via aerial sprayers.

Line 167-168 - how did you evaluate initial composition, density and interspersion to determine sites were similar?

Author response – based on this and other reviewer comments, we have provided an assessment of land-cover-type composition around survey locations based on digital imagery from 2 years prior to the beginning of our study period (now presented as Fig. S5). We do not have information about vegetation density, composition, or interspersion at smaller special scales.

Line 187-189 - I am not sure the supplemental materials support the sentence other than to provide an example.

Author response – we have left reference to this supplemental material (photograph of one of our survey locations) as an example of the nature of vegetation at both treatment and control survey locations. We revised our description to indicate that all wetland areas adjacent to survey locations were dominated by cattail monoculture. In response to other comments, we have also provided an assessment of land-cover-type composition around treatment and control survey locations (Fig. S5).

Lines 202-206 - were birds detected during the initial observations included in the analyses or only those observed/heard during the 11 minute survey? Please clarify.

Author response – we have clarified this later in the Methods (line XXX), where we describe our analytical approach, but adding the phrase – “considering only marshbirds detected during the 11-mintue survey period”

Line 230-237 - were the analyses done in R or another program? Please report.

Author response – we conducted our statistical analyses in R, and have indicated that where we describe the models we used (lines XXX – XXX).

Line 256-267 - paragraph provides strong support for why data collection concerning the habitat would have greatly improved/strengthen the manuscript. This paragraph sets the stage for speculation throughout the discussion concerning the impact of herbicide treatment on marshbirds. Does this belong in the results section? Changes that can be expected post-herbicide treatment are described for years 1 and 2, but what was different during year 3? or are the birds responding in year 3 to changes described in year 2?

Author response – based on reviewer and AE comments, we have provided additional information about vegetation response (Fig. S4) and land-cover-type composition surrounding survey locations (Fig. S5). Our study design does not allow us to assess whether vegetation characteristics in a particular year were related to changes in marshbird counts. We acknowledge that more complete information about vegetation at smaller spatial scales may have allowed us to further assess relationships between habitat and marshbird counts, but that was not our objective nor possible with our study design. Using a BACI study design allowed us to assign cause and effect (e.g., herbicide application led to increases in counts of some species of marshbirds, with similar trends across all species we monitored), which was the objective of this study.

Almost the entire discussion is based on speculation concerning assumed changes in habitat conditions, could there be any other reasons for the findings?

Author response – we speculate that the most likely cause of changes in marshbird counts was a response to change in vegetation structure and composition following herbicide treatment of monocultures of invasive cattail in impounded wetlands in northwestern Minnesota, USA, based on the dramatic changes in vegetation that occurred following treatment (now presented in Fig. S4). We disagree that these are “presumed” changes in habitat conditions, and we have now presented additional information about vegetation changes at the spatial scale of the areas undergoing treatment (i.e., Fig. S4). We present other potential factors that could simultaneously influence marshbird counts (e.g., changes in detection probability for visual detections, which we have addressed via analyses only considering aural detections in response to reviewer comments) and attempt to assess their potential relative contribution. We did not assess changes in micro-habitat characteristics or potential changes in food availability, as these were beyond the scope of our study.

While I can appreciate pointing out the potential faults to the research perhaps save some of it for later in the discussion as both paragraph 2 and 3 of the discussion highlight short comings, consider moving these paragraphs to later in the discussion.

Author response – based on a suggestion of the Associate Editor, we have indirectly addressed the potential confounding issue of changes in detection probability of marshbirds detected visually in paragraph 2 of the Discussion. Paragraph 3 addressed some of the issues raised by this and the other reviewer and Associate Editor—that we do not have detailed information about vegetation changes. As the treatment in this pseudo experiment was application of herbicide on vegetation, we think that it is reasonable to propose that changes in marshbird counts were mediated through changes in vegetation resulting from the treatment (that’s the strength of a BACI experimental design). We are happy to consider reorganizing the Discussion, but believe that describing the most likely mechanism for an effect of treatment is appropriate early in the Discussion, which is what is presented in paragraph 3.

Conclusions - reads as if all marshbirds increased as a result of herbicide application but table two suggests that is not the case.

Author response – we are not clear where this comment is directed. Table 2 presents a summary of models of estimated counts, indicating which estimated counts indicated a change in marshbird abundance in year 3 (i.e., soras and Virginia rails). In paragraph 1 of the Discussion, we point out that point estimates for estimated counts showed a similar pattern across species, and present those results graphically in Fig. 3. Our assessment is that based on a BACI study design, the fact that 2 marshbird species exhibited a statistically significant increase in estimated counts, and that point estimates of estimated counts similarly suggested that all 5 species we monitored likely increased in abundance of the 3-year period we monitored these sites following treatment, that we have provided evidence in support of our conclusion that marshbirds increase in abundance following herbicide application in our study area.

Table two - There is a very large range of wetland sizes that were used in this study, I wonder if wetland size had any notable influence on your results concerning marshbird use of wetlands?

Author response – we report the sizes of WMAs and areas treated with herbicide in Table 1, and describe the landscape in which the wetlands we studied were embedded in the Study Area section as follows:

We conducted surveys for breeding marshbirds at large (> 30 ha), impounded wetlands near the eastern edge of the PPR in northwestern Minnesota (Fig. 1), USA. This landscape has low relief and high water-holding capacity, resulting in large (>30 ha) pooled basins with slow overland water flow and peat bog conditions…

We have not estimated wetland size, nor have we presented size of the wetlands we included in our study, because delineating wetlands in this landscape is difficult. Wetlands are largely connected and consist if cattail-dominated impoundments, bogs, wet meadows, etc. We focused on impounded wetlands that were all >30 ha in size. The area within a wetland treated with herbicide varied considerably (Table 1), but those areas are not the same as estimates of wetland size. It is possible that variation in wetland attributes (such as size) could influence use of wetlands by marshbirds, but we don’t believe there is a way to address that issue in our system or study design.

Line 312-314 - did black terns also have a delay in increased numbers until year 3 or later or did they respond immediately? I know you mentioned there was little information concerning the impact of herbicide treatment on marshbird densities, but be sure to include all of them in your discussion as it is pretty thin with comparable studies as written.

Author response – we have clarified the timing of the response of black terns reported by Linz et al. (1994) and added a recent reference (Anderson et al. 2022) that evaluated marshbird response to herbicide application to monotypic cattail in east-central Minnesota, USA, albeit not in Prairie Pothole Region wetlands.

**Reviewer 4 comments:**

This manuscript evaluated the response of five secretive marsh bird species to invasive cattail management in prairie pothole wetlands in northern Minnesota. Marsh bird abundance was examined before and up to three years after cattail treatments and significant increases for sora and Virginia rail were observed after 3 years, with modest increases for American bittern and pied-billed grebe. The authors conclude that secretive marsh birds respond positively to herbicide control of invasive cattails and that there is a lag between treatments and increases in marsh bird abundance.

Understanding the impact of ongoing wetland management and restoration efforts on secretive marsh birds is crucial for marsh bird conservation. This study addresses this important topic and indicates that ongoing cattail management can have neutral-to-positive impacts on five secretive marsh birds, including relatively clear positive impacts for the two rail species included in the study. Studies of this nature are timely, important, and complement other recent studies on secretive marsh bird responses to similar wetland restoration (e.g., Anderson et al. 2019, Wetlands Ecology and Management). Evaluating impacts of restoration over multiple years is essential and the 3-year coverage in this study allows for at least initial conclusions to be drawn about the impacts of widescale cattail herbicide treatment. Additionally, the use of the BACI framework provides a powerful analysis tool for evaluating environmental impacts. The manuscript is generally clear and well-written.

While the management strategy is clearly articulated and marsh bird community sampled appropriately, the study lacks a rigorous habitat/environmental component. Similar recent studies on secretive marsh bird response to wetland restoration typically include habitat characteristics that can be used as predictors in models and/or to control for confounding effects of environmental factors (e.g., water depth, percent emergent vegetation cover, etc.). The qualitative descriptions in the text and imagery in the supplemental figures is convincing that the herbicide treatments had a substantial impact on cattail cover and subsequently open water. Nonetheless, a quantitative evaluation of the environmental conditions at each site would provide much stronger support to the conclusions that the treatments were the primary driver of the marsh bird responses observed. If additional aerial imagery exists, such as that included in the supplementary materials, this could be provided for all sites/wetlands as additional support or possibly evaluated with spatial software to quantify changes in vegetation cover before-after treatment.

Author response – based on these and other reviewer and Associate Editor comments, we have included summaries of remotely sensed data related to the effects of herbicide treatment on wetland vegetation (Fig. S4), and the land-cover type composition around treatment and control survey locations prior to the study (Fig. S5). Collecting information on vegetation characteristics at the micro-habitat scale was not part of our study design, and precluded in part due to the logistics of making meaningful measurements of characteristics of monocultures of invasive cattail in these systems. The addition of remotely sensed data (trends in NVDI and NWDI) suggest a strong effect of herbicide application and the addition of land-cover-type composition information prior to treatment indicates similarity between treatment and control sites prior to herbicide application. We believe that these additions strengthen our manuscript and conclusions, and appreciate these helpful suggestions. Regarding developing predictive models of marshbird counts, that was not part of our experimental design, and we are not aware of any data that might be used to develop such models.

The lack of environmental variables does allow for the BACI design, which focuses on the impact (i.e., treatment) alone as the factor distinguishing responses in control and treatment sites. I appreciate the use of this design; however, more explanation should be provided for this analysis in the text, including some of the assumptions, and importantly, the manner in which an impact is deemed significant (the significance of the interaction). See Smokorowski and Randall (2017) and Christie et al. (2019) for some recent papers addressing BACI design in ecology which may be useful.

Author response – We agree that the BACI design that we used allows for strong inference about causal relationships between the treatment and our response variable (estimated marshbird counts). We have provided references to the statistical approach we used, and could add further explanation if requested. However, we are not convinced that it is necessary in our manuscript to further describe and discuss the merits of a BACI design and the analytical approach used to evaluate whether there was an effect of treatment (i.e., the significance of the interaction), as the objective of our manuscript is to assess whether we observed a response. We are willing to provide additional detail at the direction of the Associate Editor.

Finally, while the findings are interesting, the Discussion does not highlight these findings or compare them to other recent literature on restoration adequately. For example, the decrease in abundance of all species the year following treatment, while not significant, is consistent across species, and could be discussed in more depth. Similarly, the time lag in a marsh bird response is not given much text, but could be compared to that observed in other similar studies. Finally, while other studies of marsh birds in the PPR or in similar restoration contexts may by sparse (though see citations in the line-by-line comments below), there is still a wealth of recent literature on general responses of marsh bird to restoration that could be drawn upon for comparison.

Author response – We have added discussion of some of the patterns in our analyses (initial decrease, lag in observed increase), and added recent literature references based on this comment and comments of Reviewer 3. We appreciate the references suggested by this review—our manuscript has been being developed and now in revision for some time, and there are several publications that have become available along that way that we had missed incorporating into our last revision.

I provide specifics on these issues and other minor issues in line-by-line comments below.

Christie, A. P., T. Amano, P. A. Martin, G. E. Shackelford, B. I. Simmons, and W. J. Sutherland. 2019. Simple study designs in ecology produce inaccurate estimates of biodiversity responses. Journal of Applied Ecology 56:2742–2754.

Smokorowski, K. E., and R. G. Randall. 2017. Cautions on using the Before-After-Control-Impact design in environmental effects monitoring programs. Facets 2:212–232.

Author response – we appreciate these references to publications addressing concerns about BACI designs and further explaining the study design we used. However, as indicated previously, we do not believe these references are necessary in our manuscript, as we have provided an example manuscript that uses the same design (this manuscript was suggested to us in the review of the first submission of our manuscript). We are willing to add these or similar references at the request of the Associate Editor, if they are deemed necessary.

Line by line comments:

ABSTRACT

The abstract on the cover page differs substantially from that in the text. Perhaps this was not updated from a previous version.

Author response – we are not sure why the most recent version of the abstract was not included in the manuscript version that you reviewed. We have provided the most recent version of our abstract with this and the previous submission.

INTRODUCTION

L94: An important recent study that evaluated similar cattail treatments is Anderson et al. (2019). Given the similarity of treatments and proximity of study areas, this study should be mentioned.

Anderson, S. L., D. A. McGranahan, T. J. Hovick, and A. R. Hewitt. 2019. Passerine and secretive marsh bird responses to cattail management in temperate wetlands. Wetlands Ecology and Management 27:283–293.

L105: See also Orr et al. (2020), for a more recent study.

Orr, J. T., C. A. Duquette, T. J. Hovick, B. A. Geaumont, and T. M. Harms. 2020. Secretive Marsh Bird Densities and Habitat Associations in the Prairie Pothole Region. Wetlands 40:1529–1538.

Author response – thank you for providing these references. We first drafted this manuscript prior to publication of both of these publications, and had not incorporated these recent publications into our first revision. We have included references to both of these publications in our current revision.

L119: Because habitat variables were not quantified explicitly in this study, I suggest removing the text “…based on different marsh bird-habitat associations.” While there may indeed be different responses among species, the study is limited in it’s ability to determine specific responses based on habitat features.

Author response – we have removed this clause, as suggested.

STUDY AREA

L149: Was any active water-level manipulation being done on the wetlands in this study? The combination of varying water levels and cattail treatment could have impacted the results seen for each marsh bird species. I understand that water levels were not measured during the study, but if there is auxiliary information available, that would be important to provide. And if not, water levels should be acknowledged as potentially impacting the findings.

Author response – we do not have access to historical information on water level manipulation on our study sites. We do agree that both recent and historical water-level manipulation (including the time during the season that water-level manipulation occurs) could influence marshbird use of wetlands. In our study design, any recent variation in water-level manipulation among study sites would potentially weaken our ability to find an effect of treatment. It may also be possible that historical patterns of water-level manipulation at individual study sites could influence current marshbird communities, although again, this would negatively influence our ability to detect a response. So, although we are not sure how water levels might affect marshbird communities in the wetlands we studied—any potential unkown variation in water-level management among our study sites would result in error that would reduce our ability to detect a treatment effect.

METHODS

L175: I really appreciate Figure 2 to demonstrate the survey design.

Author response – thank you for this feedback. In the review of the original submission of this manuscript, 1 of the reviewers asked us to delete this figure because it provided no useful information!

L188: I appreciate the inclusion of the photograph in the supplementary materials. While there may not have been on-the-ground measures of habitat, does aerial imagery and/or historic vegetation data exist for these wetlands that could at least be used to determine their similarity pre-treatment more rigorously? This could even be accomplished in a course manner by determining the percent of the land cover types displayed in Fig. 2 for each of the survey locations (i.e., the buffers displayed in Fig 2), and then comparing proportions among treatment and control locations prior to treatment. Similar conditions of treatment and control locations underlies the usefulness of the BACI analysis design. If nothing else, perhaps a set of supplementary photos, one from each sampling location would be helpful, if they exist.

Author response – based on this and comments from other reviewers and the Associate Editor, we have included an assessment of the effects of treatment on wetlands using NVDI and NWDI (Fig. S4) and of land-cover-type compositions around survey locations prior to the initiation of our study (Fib. S5). We appreciate this suggestion and think that these assessments strengthen our manuscript.

L193-195: (and L173-177) It is confusing what is meant by “site”. From Table 1 and Fig 2, it’s clear that there are 9 WMAs and within each, several point locations were sampled such that 1) control points were placed as close as possible to treatment points and 2) there were a similar number of both control and treatment points in each WMA (though not always). “Sites” seem to be groups of paired treatment and control locations, but this convention seems clunkly when a site can mean different numbers of paired locations. I suggest adding clarity about what a site is, or simply calling each point location a site and explaining that these locations were grouped spatially as well as possible to have equal number of control and treatment locations near each other.

The use of the term “site” is more problematic for 2017 and 2018 when not all sites were sampled. The actual number of point locations sampled each year should be included here in the text, or in Table 1or 2.

Author response – we have revised our terminology around “study sites” to define a study site as a paired treatment and control groups of survey locations along the edges of wetlands dominated by cattail monoculture. Treatment survey locations are associated with areas treated with herbicide, and control survey locations are associated with areas not treated with herbicides. The numbers of treatment and control survey locations are indicated in Table 1, and in the text we describe which study sites were surveyed in which years.

L230-237: See general comments above about additional explanation of BACI design and analysis. More generally, BACI designs may be less familiar to wildlife and marsh bird researchers in particular, so a brief explanation of the pros/cons would be helpful.

Author response – we have reference literature that more fully describes our statistical analyses, and don’t agree that it is necessary in this manuscript, focused on the potential effects of herbicide application on marshbird abundance, to provide additional detail about the statistical analyses. However, if the Associate Editor believes that further detail is necessary, we are prepared to add that detail.

L236: I think the use of a 90% CI (and subsequently P < 0.10 significance cutoff) is fine here, but a brief explanation could help clarify why this value was believed to be more appropriate than the typical 95% CI (and P < 0.05) for the study. The significant p-values observed for sora and Virginia rail, would still be significant at P = 0.05, so on its surface the less strict significance cutoff seems unnecessary. However, if this was used for other reasons (small sample size, etc.), then that should be stated.

RESULTS

L245 - Figure 3: I really like this figure, and the bottom panel is great for understanding the net differences in counts between treatment and control locations. While generally obvious, for clarity in the figure caption, it should be stated which panel refers to which response (e.g., “bottom panel” versus “top panel”).

Author response – we have revised this as suggested.

L260-264: Was new growth observed during marsh bird surveys, or were the sites visited later in the season to examine vegetation? Additionally, it is unclear whether this decay and breakup of floating mats was actually observed in the study, or just assumed to have occurred based on previous research.

Author response – in response to other reviewer and Associate Editor comments, we have provided additional information (e.g., Fig. S4) in support of our description of the general process by which treated cattail stands disintegrate through time following herbicide application, and believe that this comment has been addressed through those additions.

DISCUSSION

L276: “for most or all” – I believe this is too strong of a statement based on the results. For sora and Virginia rail, the positive trends appear clear, but there is only an indication of a positive trend for American bittern and pied-billed grebe, and clearly no positive trend for least bittern. With that said, I think an equally interesting finding is that the treatments clearly did not negatively impact marsh birds (except slightly in the first-year post treatment).

Author response – we have removed “or all” from this assertion. We also appreciate the observation that there did not appear to be any lasting negative effect of herbicide application, and included that observation in the Discussion.

L288-292: This is an important caveat to mention and warrants some more discussion. Including references to studies that either support or refute that detection is impacted by vegetation differences/changes would be helpful.

Author response – in response to a suggestion by the Associate Editor, we have provide some support for this assertion by comparing our full models to those that incorporate only aural detections. See response to previous comment.

L302: See comment in Methods—was this actually observed in this study, or simply assumed to have occurred?

Author response – in response to other reviewer and Associate Editor comments, we now provide additional support for changes in vegetation resulting from herbicide application. These descriptions have been revised throughout the manuscript, so we believe that it is now clear what is being described and a general pattern (with appropriate reference) and what evidence we have to support that a similar process happened in the treated wetlands we studied.

L326: I would argue that documenting these vegetation changes is vital to understanding the marsh bird response to habitat changes, and not necessarily beyond the scope of the study.

Author response – it was certainly beyond the scope of our study to assess micro-habitat characteristics and relate those to marshbird abundance. Ours was a rather straightforward assessment of the relationship between herbicide application and changes in marshbird abundance (indexed using counts) for 3 springs following herbicide application. At the suggestion of reviewers and 2 Associate Editors, we have provided post hoc assessment of changes in wetland conditions related to herbicide application, which we think addresses this comment, at least in part.

L329: While not conducted in the PPR, the recent study by Anderson et al. (2019), referenced above, evaluated similar restoration practices in Minnesota and found similar results (no/modest changes in marsh bird response to treatment). This would be an important study with which to compare. Other studies on marsh bird response to restoration, such as Hapner et al. (2011), Glisson et al. (2015), and the recently published Vanausdall et al. (2022) provide comparisons for post-restoration timing of marsh bird use of restored wetlands.

Hapner, J. A., J. A. Reinartz, G. G. Fredlund, K. G. Leithoff, N. J. Cutright, and W. P. Mueller. 2011. Avian succession in small created and restored wetlands. Wetlands 31:1089–1102.

Glisson, W. J., R. S. Brady, A. T. Paulios, S. K. Jacobi, and D. J. Larkin. 2015. Sensitivity of secretive marsh birds to vegetation condition in natural and restored wetlands in Wisconsin. The Journal of Wildlife Management 79:1101–1116.

Vanausdall, R. A., T. M. Harms, and S. J. Dinsmore. 2022. Marsh bird response to restored shallow lakes: Implications for future management. Wildlife Society Bulletin 46:e1296.

Author response – thank you for providing these references. We have incorporated several of these (and several others) into our revision. Please see our response to a previous comment about our rationale for including some of these and not others in our revised manuscript.

L335: The positive impact of cattail management on sora and Virginia rail appears to only begin at 3-years post treatment, rather than being evident for 3 years post treatment. Based on other literature, including that mentioned above, the positive impacts are likely just being realized, and perhaps may be increasing for species like American bittern and pied-billed grebe (up to a point several years in the future).

Author response – thank you for this comment. We have revised this sentence to indicate that we observed a response 3 years following treatment, and that the effect may continue longer than that.

L348-350: See comments above. I believe there is evidence from other literature that could provide some rough estimation of this timing, and this could be discussed further in the Discussion.

Author response – we have revised this statement to restrict our inference to the system we studied. As indicated in response to several previous comments, we are reluctant to draw conclusions from beyond the Prairie Pothole Region, as, for example, the relationship between cattail cover and rail habitat quality in wetlands dominated by cattail monoculture is different that in wetlands were cattails are a part of the vegetation structure and where rails are generally thought to respond to increases in cattail cover.