Mary O’Connor and Gary Bradfield were the most recent reviewers of the manuscript; each provided comments on version 3.3 in mid-November. I have summarized their critiques and my revisions by each manuscript section, below.

I reference line numbers in version 3.3, as annotated by each reviewer. Co-authors/reviewers are referenced by their initials as follows: GB – Gary Bradfield, MOC – Mary O’Connor, SLL – Stefanie Lane, MD – Madlen Denoth,TGM – Tara Martin, NAS – Nancy Shackelford.

In general, I accepted all of GB’s edits to v. 3.3. I tracked changes in v. 3.4, but ran into formatting issues with Word’s Reference feature. To get around the issues, I started v. 3.41 without tracking changes. If anyone wants to review the markup, I can send v. 3.4 (although it will not have all the finalized edits as shown in v. 3.4).

# General

GB pointed out ‘exotic’ may not be an appropriate term for species that have naturalized in our ecoregion, and the term is incongruous with ‘non-native’ used in supplemental tables. I’ve standardized all instances to ‘non-native’ for consistency.

MOC had several comments in Methods & Results about study design/analyses chosen, specifically about whether all individual species were recorded within the plot. I’ve tried to emphasize in the methods that all species were recorded, so measures using presence/absence and compositional abundance should be appropriate. No statistical models were used, and therefore I have not addressed model or statistical assumptions.

# Introduction

Line 3-5 (second sentence), MOC pointed out species dominance may shift by interspecific interactions such as facilitation (Bruno, 2000). I’ve broken down this statement to address concepts of interspecific interaction and community stability separately.

Line 44, GB correctly suggested I meant to indicate “within and **between**” assemblages, not “within and across” assemblages.

# Methods

Lines 68-82: MOC suggested recounting previous studies’ methods in Ladner Marsh reads more like literature review, and is not directly informative of this study’s methods. I’ve moved some content about the previous study’s goals to the Introduction where co-authors’ studies are introduced. I’ve condensed survey method details entirely within the context of how the 2019 sampling was conducted for this manuscript.

Line 95-96: MOC pointed out bias is introduced by placing sampling plots within patches subjectively assessed as dominated by one or two species. I agree method does introduce bias, however the method described was used in order to repeat the sampling as closely as possible to methods used in 1979, and to make comparisons between datasets. I have acknowledged the bias in line 70 of v. 3.4, and proposed that within the context of this bias meaningful comparisons can still be made about changes in floristic diversity and compositional abundance.

Lines 142-147: GB suggested omitting supplemental references to Bray-Curtis results; I’ve left them to indicate presence of further supplemental materials.

# Results

GB pointed out that data limits significant digits to one decimal place – corrected in all tables.

GB pointed out that because cover abundance was reported as cover classes, calculating percent change isn’t an accurate representation of change in cover abundance in the assemblage/community. Ex: if a species’ cover dropped from 4 to 2, I reported a 50% loss of mean cover. However, actual cover could have changed from 76% to 50%, which would not be a 50% loss. Therefore, I’ve amended language around loss of cover to reflect cover classes (not raw percentages). Same critique applies to Table 7; I’ve taken Gary’s advice to use +/- for gain/loss, blanks for species not recorded.

MOC’s comments that analyses may not be appropriate for data may be resolved by clarification of methods/analyses used in Methods section. That is, violations of ‘model’ assumptions don’t necessarily apply.

MOC suggested evidence for species loss was not strong, SLL suspects that in the context of turnover, loss of 1-2 species per sampling period is not enough to state (line 244) “our results present a compelling example of …native species biodiversity loss…. SLL removed ‘compelling,’ although SLL maintains that there is still an overall trend of species loss in the data.

# Discussion

GB had minor comments that were resolved while addressing MOC comments: characterizing Ladner Marsh as ‘pristine’ (line 328) inappropriate; restructured sentence to get main point across that there is a trend of biodiversity loss in conservation management area.

MOC had concerns for some overgeneralizations in the Discussion. I’ve rephrased these to not overextend inferences the data can provide:

* (lines 241-246): “We observed a decline of native species richness … may indicate a loss of community stability. … Our results present a compelling case example of … global trends of native species biodiversity loss, …” Concern was that evidence presented was insufficient to support (1) loss of richness directly indicates loss of stability, and (2) compelling native richness loss within Ladner Marsh
  + Amended to state: “We observed a decline of native species richness accompanied by an increased richness and abundance of non-native species. Of greater concern is our observation of the homogenization of cover abundance within assemblages, and overall loss of indicator species for the Sedge and Fescue assemblages. Increasing abundance of non-native species within each assemblage is likely driving the greater similarity within assemblages (homogenization) and greater dissimilarity between assemblages, as shown by cluster analysis (Figure 2). While addition of non-native species can contribute to greater biodiversity (Sagoff, 2005), the homogenization of plant communities leads to lower diversity overall (Houlahan & Findlay, 2004), which in turn may lead to lower functional redundancy and potential for reduced ecosystem stability (de Bello et al., 2021).”
* (lines 288-292): “More concerning is the net loss of (five) … species over the study period, as this represents a loss of functional redundancy.” Here, concern was this could be interpreted as total loss from Ladner Marsh (rather than just the survey plots).
  + Added: “… as this represents a loss of functional redundancy. This species loss from the observed datasets may not represent species loss from the entire Ladner Marsh Wildlife Management Area, however the net species loss from the dataset, along with the addition of three non-native species to the datasets, poses concern for potential of species loss from the habitat over time.”
* (lines 302-304) “…native species loss … result(s) in ecosystem instability through altered trophic cascades…”
  + Changed to: “…native species loss … potentially lead(s) to ecosystem instability through altered trophic cascades, especially when top-down trophic interactions are also lost from the ecosystem (Duffy, 2003, Ecol. Letters).”
* (lines 324-327) “…(mis)assumption that ‘undisturbed’ areas represent … appropriate reference states. Our findings support … sites [like Ladner Marsh] are not sufficient benchmarks …” Concern is that this implies Ladner Marsh is not a suitable control site for comparison.
  + Changed to: “……(mis)assumption that ‘undisturbed’ areas represent … appropriate reference states. Our findings illustrate how, in a heavily impacted landscape (Finn et al., 2021), compositional states have likely shifted from recent (< 100 years) historical reference, yet may still contribute value as an example of potential ecological benchmarks for restoration success (Shackelford, et al., 2021). We suggest that the plant community changes described here should alert land managers not only to what species diversity might be targeted in conservation practice, but also to how reference ideals may have changed during the span of 20-40 years.”