Rob notes to co-authors: **Bolded comments mean I have addressed them** because they are reasonable suggestions. Non-bolded means they are yet to be addressed because they wont improve the paper. *Italics are notes on my changes.*

01-May-2023

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> Dear Dr. Clark:

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> Thank you very much for submitting your manuscript "Are native plants always better for wildlife than invasives? Insights from a community-level bird-exclusion experiment" # EAP23-0003 to Ecological Applications. The reviewers appreciated the work you have accomplished, but they raised substantial concerns about the paper. In particular, both reviewers questioned the degree to which the methods were adequate to generate reliable conclusions, and about the degree to which you can make inference to "natives" versus "non-natives". I would add that when tests yield non-significant results, it is important not to conclude "no effect" without careful consideration of what might constitute a biological effect and that your confidence intervals exclude such an effect. I encourage you to read Steidl's classic paper on this topic: <https://nam12.safelinks.protection.outlook.com/?url=https%3A%2F%2Furldefense.com%2Fv3%2F__https%3A%2F%2Fwww.jstor.org%2Fstable%2F3802582I__%3B!!JmPEgBY0HMszNaDT!uMYy8CDILtwtp75Y3cwAvSIIWDI14Rd9xkvYR-CxYrJ3HxsdXW95wpsw7jNubA2zFl-FF0zPIF5MaGAG3zZvvkndBsD2HUgg%24&data=05%7C01%7Crobert.e.clark%40wsu.edu%7C0162673f79e848f58bf708db6cf6bcff%7Cb52be471f7f147b4a8790c799bb53db5%7C0%7C0%7C638223579206905820%7CUnknown%7CTWFpbGZsb3d8eyJWIjoiMC4wLjAwMDAiLCJQIjoiV2luMzIiLCJBTiI6Ik1haWwiLCJXVCI6Mn0%3D%7C3000%7C%7C%7C&sdata=ftMffCfuVERtLI5yv5Lyt34%2FjH0YIsqhp1ZM4Nbzf%2FU%3D&reserved=0> . Nevertheless, I commend you on your efforts to undertake a controlled experiment.

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> Based on the reviews, we will not be able to accept this manuscript for publication in the journal. Revising to address some of the proposed methodological flaws identified by the reviewers will help you in your preparation of this manuscript for another journal.

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> Reviewer comments are copied below, and we hope they will help you should you decide to revise the manuscript for submission elsewhere. Thank you again for thinking of the Journals of the Ecological Society. We will look forward to further contributions from you and your colleagues.

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> Sincerely,

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> Dr. Matthew Betts

> Subject-matter Editor, Ecological Applications

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> Reviewer Comments to Author:

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> Reviewer: 1

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> Comments to the Author

> Lines 37-40 - **the specific term tri-trophic relationship isn't mentioned in the text, so its odd to have new language introduced in the abstract.**

*Tri-trophic interactions removed from abstract*

> Lines 40-42 - **although though technically true, it misses the fact that the variability among plants was important so should have some caveat that most of the results were driven by single plants.**

*Edited out.*

> Line 114, Line 118 - **Sometimes in the text there is reference to simply non-native plants vs. NNI so sometimes it feels like these ideas are getting conflated.**

*Given new journal venue and the references found for CT invasive species management, all mention of NNI or non-native plants just edited to be simply ‘invasive’*

> Line 120 - **prediction is based on non-native vs. native and not NNI but previous section with the statement of the hypothesis (lines 109-111) indicates the study is focused on NNI. The same problem continues in the paragraph on predictions (lines 112-126). Writing should be clarified to ensure the reader knows which is the correct focus.**

*Removed NNI and just put in invasive. Federal law assumes invasives are non-native, so I'm going with that to avoid confusing myself and the reader. https://www.invasivespeciesinfo.gov/executive-orders-invasive-species*

> Line 134-135 - **who made the NNI designation for these plants? Is there a standard definition and if so, please provide definition and citations.**

*Added state designations from state of Connecticut*

> Lines 142-144 - This statement about NNI plant management decisions seems much more overreaching than the study being done. Likely more correct to say this research will be contributing to these management decisions.

> Line 183 - **feeding not feeing**

*Fixed*

> Line 203 - **again with the non-native vs. NNI**

*Another reason to move to invasives.*

Line 204 - **methods said 3 sample periods but here is says two - please fix in the appropriate place to reflect the correct number**

*Changed to three.*

Line 218 - citations not in chronological order

Lines 214-224 - **sometimes LLR ratio written out and sometime abbreviated. Please be consistent and write out and identify the abbreviation the first time.**

*Section edited to address LLR definition.*

> Lines 120-231 - did you look at any statistics on the influence of honeysuckle on the overall comparison? In looking at Fig 1, a single plant, honeysuckle, is driving the lack of difference between native and not.

> Lines 231-232 - **statistically or numerically higher?**

*This sentence was simply removed since it did not add much to the story.*

> Lines 240-242 - **Test states that abundance is higher on non**

*Fixed.*

Lines 255-257 - Although this statement is true, it is clear from looking at figure 4 that this result was driven by honey suckle and doesn't represent the grouping as a whole.

*This issue is addressed by no longer pooling invasives into one contrast.*

> Lines 268-270 - I'm not sure how to interpret this statement - please provide more information so that a reader who hasn't read that paper has context.

> Lines 274-276 - This statement starts to generalize and undermines the results found in this study that the category of the plant is not as important as the plant itself.

> Lines 278-282 - would be helpful to acknowledge here that what was measured in this study is one piece of a more complex puzzle.

> 284-286 - Statement implies that all non-native plants are disruptive and according to the author results, that is not true and based on some of the metrics measured, honeysuckle could have positive impacts.

> 298-302 - these mechanisms are stated as though they are universal facts - is that true? How do the plants that were tested fit (or don't fit) well into these mechanisms?

> Figures

> All - it would be helpful to use NNI and native instead of planned contrast groups to identify the different groups in the figures. Particularly with the 2-letter plan name abbreviations, it requires a bit of work to interpret the figures and it would be best if someone could walk away with information without reading the entire paper.

*The species names and codes are in the figure captions. The symbols clearly delineate natives and invasives.*

> Figure 3 - do not connect with and without birds with lines - it implies that there is a relationship between them and that is not the case here.

*There is a relationship since they are paired controls and treatments.*

> Figure 3 - it would be helpful to show when the native and not-natives were difference, as that is the premise of the paper and it's not on the figure.

*Figure 3 is moved to supplemental with new changes, I doubt this issue would come up again.*

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> Reviewer: 2

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> Comments to the Author

> Review for manuscript EAP23-0003

> Authors studied differences in arthropod community biomass and composition, specifically in terms of nutrient contents, between native and non-native plants at a watershed in northeastern US(Connecticut), with an predator exclusion experiment.

Authors claim that they found no difference in biomass of arthropod, predation effect on arthropods and nitrogen content, thus native birds foraged intensively on both native and non-native species without strong difference. And from their work they suggest reconsideration of bob-native plant management.

> Unfortunately, the authors' claims are not supported by their design of the study, nor the results. There are several parts of the manuscript, especially in the manuscript where authors fail to describe their methods correctly and do not provide enough information for replication, nor address strong potential biases from the sampling scheme. Also, the modeling approach is not clearly described enough in the manuscript. The general design of the experiment can only identify pairwise difference in abundance and biomass of arthropods in exclusion treatment and control branches of same species, not between species or in native-non-native species groups.

*Not worth addressing.*

> Authors observations cannot be generalized to all native and non-native species, but still is a valuable information on species-level variation in predation effects and arthropod community composition, biomass for each species. In my humble opinion, **I suggest the authors focus on species level variation, rather than non-native versus native framework when preparing the work for publication.**

*This is actually a good suggestion given editor and reviewer 1 comments, so I’m running with it.*

> Because I believe introduction and discussions require complete reframing, I only kept my detailed comments for the methods and results section.

*Don’t have time to rewrite discussion and introduction, nor is it necessary with our changes, so it’s not happening.*

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> Methods.

> Did the key comparison groups - native and non-native plants were sampled in spatial pairs? **What was the spatial distribution of these samples in the study area?** How do you address the potential bias from sampling locations and surrounding plants and systems?

*They are in pairs, so we included a map in the revisions since this also helps illustrate the experimental design.*

> Why are these species selected? Most of the native plants are shade-tolerant forest understory species, while non-native plants are early-seral species that can not do well under canopy shades. I don't believe these species selections are appropriate for making a general claim of native versus non-native plant species first place. At least ecological/phylogenetic counterpart plants (Lonicera canadensis, Berberis canadensis, etc.) should have been selected for such example.

*We mention why these species are selected, so I am ignoring this comment. Some reframing in the introduction should help alleviate this issue moving forward.*

> Also, model section does not describe use of interaction effects clearly although it has been used (line 249)

*We never mentioned model selection, so I am ignoring this comment.*

Line 145: how did sample locations and bagging time were distributed within the watershed? Nearly three weeks of deployment periods are long enough for quite a few arthropods, especially lepidopteran larvae to develop and move to pupation stage. What was the stage of leaf phenology? How did you address this phenological variation among branches?

*The reviewer is looking for things to pick on and I can’t address any of these comments so no action is being taken.*

> Line 147: mesh size? How large is a mesh bag and what is the size of the branch samples? Did your mesh size allow arthropods to move in and out to the branch but prevented birds from foraging? What if bagging had a negative effect on arthropod abundance as well, resulting in no difference between treatments and controls?

*Not getting into this in the manuscript. We cite Singer et al. in AmNat and move on.*

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> Line 161: when these repetitions happened? In what interval and how were they distributed across the samples (especially treatments/controls, and native plants/non-native plants)?

**“After a 2-wk waiting period, we then sampled foliage-foraging arthropods with a branch-beating technique (Wagner 2005) every other week, for a total of three repeated samples per branch.”**

> Line 187-188: Any evidence for "would be for the first bird foraging on a given branch" ? All 4 main taxonomic groups found from branch beating are key diet for breeding birds, **and it is yet unclear whether birds will select certain prey types**. Please give clear citation for this argument.

*This is a typo or simply not written well on our part. Should just say that is what we did (bags only) and leave it as that in the methods.*

> Rather it should be unaffected arthropod community (from foraging). No need to imply birds selecting high nutritional prey at a branch level.

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> Line 150: of the same species and individual? Or of different individuals? Sounds like these were different plants from line 151.

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> Line 204-205 - branch id? Is this to account for repeated visits the same branch (3)? Please clarify. It is confusing with the following statement on two sampling periods and pseudoreplication. It is also unclear if biomass was pooled across three repeated samples like N content (and why?). Like N content, biomass could have been pooled into single value per branch from three repeated removal sampling.

**> In the earlier section, authors mention three repeated samples per branch and I don't think I can find information on two sampling periods in this manuscript.**

*This is fixed to three across the board.*

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> Line 211: Which were unplanned contrasts? From my understanding of line 203, both non-native status and exclusion status are planned experimental components. Scheffe's method for multiple comparisons will produce more conservative (lower precision, hence less likely to be 'significant'). I

*We have a new contrast method so no changes needed in response to this.*

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> Results

> Line 229: higher biomass -> higher arthropod biomass Consider revising the following use of biomass as well. Authors did not investigate plant biomass in this article. Each statement should explain what results are by themselves.

*In edits well make sure it is always clear it’s arthropod biomass being discussed. Not sure where plant biomass came from.*

> Line 229: mean biomass and range? Reporting effect size or mean values in numeric would be valuable, than just reporting P, t and d.f.. This should be applied to the following result descriptions as well.

*No.*

> If you investigate figure 1, the overall mean and range of biomass for native plants are clearly higher than non-native plants, though the confidence interval may overlap and test p is near 0.1. I would not argue that this is evidence of no difference, but rather say a difference with very weak support (also note that lack of evidence is not equal to lack of difference). Given the magnitude of biomass differences are very small in foliage arthropods, this margin of error could have occurred from measurement error (sampling, sample storage etc.). Also, the mean biomass for each species indicates that more species of native plants (3/6) have higher biomass of arthropod than non-native plants (1/4). Although it is difficult to make direct comparison as species level difference of plants comes in place.

*Ignoring this comment because I can’t change how people interpret results based on what they feel in their gut.*

> Line 235: Furthermore, bird predation LLR was not significantly lower on NNI species than native species in a grouped planned contrast (P = 0.364, t ratio = 0.954, d.f. = 106).

*This will be edited out with new contrasts.*

> **What would be the grouped difference test results if you remove the outlier species, Carpinus?** In figure 2 results, Carpinus is pulling down then average value for LRR the whole native plant group. As you state, Musclewood were somehow biased by having more aquatic insects.

*This will be edited out with new contrasts probably, but we can run a quick model with this excluded to be sure and make a note in the appendices.*

> Line 242: yes, significant, but seems like the predation effects were stronger in native plants, not supporting the alternative claim of the authors that the native plant and non-native plants are indifferent to birds as foraging materials. This also applies to figure 3C, and line 246 on Lepidoptera.

*Not going with “seems like” as a suggestion for how to do statistics.*