**Associate Editor: This is an interesting paper, but some revision is needed before further consideration. Although the revisions are mostly editorial, I agree with Reviewer #2 that the authors need to be more careful in both presenting their methods and in discussing the results relative to the study design and the fact that the authors cannot control many aspects of the arthropod communities in their system. The implications of this relative to interpreting the data from this study need to be explicitly and carefully discussed.**

**Reviewer #1: This is a well-written manuscript investigating the effect of invasive plant species on trophic interactions. The study is well described and presented, addressing a topic relevant to Biological Invasions readers. I just did not understand why the exclusion addresses only birds. Aren't there gleaning bats in your study region?**

We thank the reviewer for their positive opinion of the manuscript. The only gleaning, forest interior bat species in our study region is the northern long-eared bat (*Myotis septentrionalis*), which is a federally endangered species and increasingly rare in the northeastern U.S. Acoustic surveys of bats at our study site have not confirmed the presence of northern long-eared bats. In the event northern long-eared bats do in fact occur at the site, they would occur in such low abundance as to have a negligible effect on our results relative to birds. Big brown bats (*Eptesicus fuscus*), which are abundant in our region, also forage by gleaning, but much less commonly than foraging in flight over open areas. Foliage gleaning by big brown bats also usually occurs in open habitats and along edges much more so than in cluttered forest interior. Acoustic recordings in the areas of our study site where the experiment occurred have shown little to no big brown bat activity (C.L.S., unpublished data). For these reasons, we do not expect bats to have had an effect on our results.

**Bellow I provide some suggestions and comments:**

**In the first paragraph (introduction) I feel some abrupt changes of subject that could be avoided. Note that you change from biodiversity decline to economic loss and then to the challenge of managing invasive plants in just three sentences.**

The economic component was moved to the conclusion and these sentences rewritten for better clarity.

Line 45 now reads:

“Invasive species are widely considered to be a leading cause of global biodiversity decline (Bellard et al. 2016). Invasive species management totals $120 billion spent annually (Pimentel et al. 2007). Invasive plants are a particularly challenging category of invasives to manage in terrestrial ecosystems, with the cost of plant removal efforts still being difficult to estimate accurately for the U.S. or globally (Rai et al. 2022).”

**L68-69 - In "removal of particularly aggressive invasive plant species can drive recovery of arthropod assemblages", can you provide some explanation for the mechanism involved? Why should aggressive invasive plant species have negative effect on arthropods from the beginning?**

Added mechanism.

Line 64 now reads:

“Removal of invasive plant species can drive recovery of arthropod assemblages by allowing higher food-quality native plants to reestablish, facilitating an increase in insect prey abundance for songbirds (Gratton and Denno, 2005, Hopfensperger et al. 2017).”

**L85 - What do you mean by "behavior of arthropod communities"? Can you provide one example in the text?**

Mechanism added to clarify this example from the text.

Line 79 now reads:

“Furthermore, the atypical architecture of invasive plants modifies the foraging behavior of arthropod communities, changing encounter rates between predatory arthropods and prey (Pearson 2009, Lind and Parker 2010, Landsman et al. 2021).”

**L130-131 - Did you standardize the branch size? Can you provide some information on that?**

Paired branches within tree species were deliberately chosen to be of similar apparent leaf area. To ensure we did not bias treatments by leaf area, we completed a diagnostic analysis. We did not see any significant difference in leaf counts in paired branch treatments in these analyses. For brevity, and since these were just diagnostic tests, we did not include this in the manuscript.

Line 130 now reads:

“Each of these branches was paired with a nearby (< 10 m away) unmanipulated control branch of the same species and similar apparent leaf area.”

**L131-133 - I am not sure if the nearby branches are in the same tree or in nearby trees. Most likely nearby trees (because the branches are "< 10 m away" and in line 124 you mention 240 individual host plants), but it would be nice to state it clear.**

The majority of branches in each pair are on different trees, but in the case that trees with large lower canopies were available, we assembled a control and removal pair on the same tree. (Similar methodology to Singer et al. 2012 in *The American Naturalist* and Clark et al. 2016 in *Ecology*)

Line 132 now reads:

“When trees with larger understory canopies were variable, control and removal pairs were erected on the same tree (following methodology from Singer et al. 2012 and Clark et al. 2016).”

**L184 - My first understanding for "branch as a random effect" was that you were using the three sampling periods for each branch independently. But then, in line 188, you mention that the three samples were pooled together. From these my conclusion is that you are talking about the nearby branches. Please make sure to state it clearer. Maybe describe as "paired branches" or "branch pair" in the methods and use the term along the text.**

Branch # was used as a random effect because the biomass and abundance models had repeated measures (three samples of arthropods). In the later analyses, arthropods had to be pooled together to get enough biomass for nitrogen content. We removed the sentence about “avoiding psuedoreplication” because that was likely what was causing confusion.

Line 188 now reads:

“Nitrogen content models were fit with a normal distribution, but since all arthropod samples were pooled across sampling periods to gain enough biomass for the assay. In these analyses, host-plant species was used as a main effect (GLM).”

**L186 - Why did you use the invasive status instead of the plant species just in the abundance models?**

This was an accidental holdover from an older analysis. Apologies for the mistake. This line now reads:

“In abundance models, host-plant species with bird-exclusion treatment were fitted as fixed effects, and branch was included as a random effect.”

**L191-192 - Because you did not use the host-plant species as fixed factor in abundance models, the post-hoc tests should not apply to compare changes in abundance. Am I correct?**

The comment above addresses this mistake in the statistics methods. The text has been edited to the correct methods.

**L213 - Fig. 1D is inappropriately cited. Cite Figure S4 instead.**

Anyone

**Figure 1 and 2 - Most likely these figures are interchanged.**

Anyone

**L254 - Should mention Figure 3D instead of 3A.**

Anyone

**L295-296 - Your result does not "highlight that some invasive plants should be prioritized over others depending on the habitat in question". Such conclusion should emerge from observed differences between habitats.**

Updated text to be more specific to this study.

Line 295 now reads:

“While our study does not suggest invasive plants have no negative ecological consequences, it highlights that nearby native plants do not always yield significant differences in food availability to songbirds.”

**L309 - remove comma before "bird".**

Line 309 now reads:

“We found that common invasive plants in our study system are used as a foraging substrate by insectivorous songbirds just as intensively as natives.”

**L316-317 - This last sentence is not well connected to the paragraph.**

Change made. Line 317 now reads:

“These difference in architecture may explain why spider abundance was higher on low-lying Japanese barberry, matching other observations with invasive plants like Japanese stiltgrass (Landsman et al. 2020).”

**L333-338 - You well point that "the native plant community is a critical comparison point", but most likely the plant community in your study region has been already changed with reduced populations of at least two plant species known to provide high-quality food for forest insects. This certainly deserve further discussion. Should your results be different if you have a non-disturbed plant community as reference? You mention the deer over-browsing as a factor driving the decline of Prunus (cherries) and Quercus (oaks) populations. Can invasive species be a further driver?**

We agree that it is an interesting hypothetical to consider about the results of a similar study to ours at a site with a non-disturbed (or simply different) plant community and we believe that it is entirely possible that less-disturbed native plant communities would provide significantly more invertebrate resources to birds than non-native invaders. However, “non-disturbed” plant communities are extremely rare in the northeastern U.S. and are not representative of what is realistically achievable for managers of invaded systems. For example, a recent study in New Jersey by Kelly and Ray (2023, Forest Ecology & Mgt.) estimated that post-agriculture forests represent half of the state’s forest cover while primary forests account for as little as 5-14%. Comparing non-native plants to native plants in the same system rather than a rare, non-disturbed forest is of greater relevance to the vast majority of the landscape and the alternatives land managers face. Accordingly, we have decided not to include an extended discussion to these questions in the text and added the following sentence to lines 344 – 346 of the discussion to suggest this as a direction for future studies:

“Accordingly, future studies replicating our work in less-disturbed plant communities may provide interesting insights into variation in resources provided by native plant communities relative to invasives.”

**L341-348 - Please note that your final conclusions are not supported by the study results. Which result suggests that the removal of invasive plants must be paired with restoration of higher-quality native plants? I agree that the body of knowledge accumulated after studies like yours will support more nuanced management strategies in the future. But this is different than saying that your results suggest a more nuanced management strategy.**

We reached this final conclusion by combining the limited differences observed between native and non-native plants with the lack of regeneration of known high quality native plants. Removing the non-native plants without actively restoring native plants of equal or greater value will simply result in an empty understory and net reduction in resources to insects and birds until the non-native plants inevitably return in a few years’ time. To make this clearer, we have revised lines 347 – 353 of the discussion as follows:

“…and thus we chose for comparison the native trees and shrubs that are dominant or becoming dominant in our region’s forests and would therefore replace invasives in the absence of efforts to actively restore Prunus, Quercus, and similarly high-quality native plants. The lack of distinction between invasives and the present native-plant community in our study suggests that in many northeastern forests the removal of invasive plants must be paired with restoration of these higher-quality native plants…”

**Reviewer #2:**

**This study compares arthropods collected on native and non-native shrubs when birds are allowed access or are excluded. The premise is that although native plants in general produce more arthropod prey for birds than non-natives, all non-natives are not equally unproductive when compared to low value natives. I understand the hypothesis being tested here, but I have concerns about how the data were analyzed and interpreted. Simply put, when one compares the productivity of two groups of plants, one is comparing the herbivores, pollenivores, or nectivores produced by both groups. I fear the authors here have fallen into the trap of including non-target herbivores - - those generated by aquatic systems, by detritus from other plants, or simply incidental passersby, as well as the predators of those non-targets (primarily spiders) that are not directly produced by the plants in question in their data set. In my view, this makes accurately interpreting how the natives and non-natives studied here actually compare with each other impossible.**

We thank the reviewer for their constructive feedback. From this comment, it seems the objective of our study was slightly misunderstood and we have therefore revised the manuscript (provide lines) to make it clearer. Specifically, our goal was not to evaluate the quality of invasive plants as hosts and food sources for herbivorous insects. That is also an important topic that is in need of much more research, but our goal was to assess the amount of insect prey available to birds on invasive plants compared to natives. Birds prey on a wide variety of arthropods besides herbivores, and plants provide resources to arthropods other than leaves on which to feed. Plants provide shelter, hunting substrates for predators, suitable microclimates and thermal benefits, (add others), and such resources are likely to vary among plant species based on their structure, shape, position in forest strata, microclimates they create, and so on. As such, it is likely that some plant species are of greater value than others to non-herbivores in addition to herbivores, and therefore better food sources for insectivorous birds. Because our goal was to understand how the total amount of arthropod prey available to birds compares between invasive and native plants, we included all arthropod taxa in our analyses, not only herbivores. And since birds are known to be capable of distinguishing plant species and show preferences for foraging on certain plants because of the full assortment of arthropod prey that can be found on them, the bird exclusion portion of our experiment was designed to allow the birds to tell us how they value the invasive vs. native plants. Our finding that the predation effect of birds on arthropod biomass was similar between natives and non-natives shows that birds recognize the non-natives as a quality food source and exploit them just as often as the native plants. The foraging behavior of the birds and the arthropod biomass and protein content data all point in the same direction and collectively indicate the invasive plants we studied are of no lesser value to birds as a source of arthropod prey than the native plants. We therefore respectfully and fully disagree with the reviewer that it is “impossible” to know from our study how the natives and non-natives compare to each other as food sources for insectivorous birds.

**Line 55: you say there is no consensus about whether removing invasives benefits wildlife, but in the next three paragraphs, you cite several studies that show the benefits are clear. I guess the word consensus bothers me here. There is no consensus in the true sense of the word on whether climate change is real or human caused; there are still a few lone voices claiming it is not. But the overwhelming bulk of the evidence says it is real. Is that the same case for removing invasives? I can site studies that have concluded non-natives are equally as productive as natives because they either made the non-target error discussed above or chose low value natives for their comparisons. No one has ever claimed that all native plant species are more productive than all non-native plants.**

We agree that a majority studies likely do show benefits to the removal of invasives, but we would also argue that overreliance on majoritarian consensuses often leads to disregard for important exceptions, particularly among lay audiences. Our intention here was to highlight the potential need for nuance in management strategies and have revised this line to communicate that more clearly. It now reads:

“However, despite dramatic efforts to remove invasive plants, there are still doubts about how reliably these interventions benefit wildlife communities (Robichaud et al. 2021, Traylor et al. 2022).”

**Methods: How did you sample bagged branches? Did you remove the bag to use the beat sheet and then replace it? How did you select the plants for sampling? It is well known that plants in the sun host more insect herbivores than plants (or branches) in the shade. Was sun exposure controlled? Did the netting prevent arthropod access to the plant branches? Many moths have a wingspan much larger than the mesh of exclusion nets, so we need to know what the mesh size was in this study. Similarly, how did orthoptera (katydids) get into the bags? Also important is the question of plant biomass enclosed within the netting? For results to be meaningful, leaf biomass sampled has to be the same for all treatments, of arthropods sampled has to be expressed as a function of leaf biomass sampled. Finally, were the nets close enough to the leaves they enclosed that birds could actually forage on the leaves without actually entering the bags? Again, mesh size is important here.**

We appreciate the careful attention paid to the validity of our methods. As a general response, we would highlight our citations on lines 128 and 134-35 of Singer et al. (2012) and Clark et al. (2016), the sources of our bird exclusion technique, which provide additional details and demonstrate that this is an accepted method for studying bird predation on invertebrate communities, including caterpillars/moths. As for specific questions, we sampled plants opportunistically along access trails (with minimum distances from trails and other sampled plants) and all of the selected plants for sampling were located in the understory layer below a uniformly closed canopy. Thus, although we did not directly control for sun exposure, there was minimal variation among branches. The mesh size of exclusion bags was ½ inch, which both allowed access to all but the largest invertebrates (katydids did not reach large enough sizes for exclusion during our late-spring/early-summer study period) and also allowed us to sample invertebrates without removing the bags. Finally, although it is conceivable that birds removed some invertebrates through the mesh, we believe this is unlikely because 1) this mesh is designed for and successfully used in agricultural orchards to prevent access by a majority of birds, 2) the mesh would be unfamiliar to birds and likely a strong visual deterrent to landing, and 3) we expect that we would have seen more evidence of entangled birds if they were routinely active around them. We have added the detail “1/2-inch” to line 129 of the manuscript and rephrased lines 135 – 137 of the methods to add more details of our experimental setup. Those lines now read:

“We set up 12 treatment pairs for each of the 10 focal plant species (240 total individual host plants), which were located at least 10 m from actively used trails and 50 m from any conspecific pair.”

**I am trying to figure out why spider nitrogen content would differ on different plants if the species of spiders sampled were the same on each plant? Did spider community composition differ substantially among plant species?**

We are somewhat confused by this question as we are unaware of anywhere in our manuscript where we claimed that the species of spiders were the same on each plant species. (Similar to how we did not make the claim that herbivores were consistent across plant species). Thus, the most likely explanation is indeed that differences in species composition (e.g., more uloboridae on Barberry than any other host plant) were responsible for the variation in nitrogen content of spiders. Differences in nitrogen content of various species could then be achieved through genetic regulation of composition or through species-specific prey preferences and corresponding links to host plants.

**Line 281: Your language here is explicit: you are saying that you are examining the contribution of a plant to local biodiversity. That means you have to measure the biodiversity that plant produces: not the biodiversity produced elsewhere and just happens to sitting on the plant when you sample it or the biodiversity that is using that plant as a structure to hunt aerial insects. The only biodiversity a plant directly produces are members of the second trophic level: herbivores. Your study should be restricted to them.**

Although we respect the reviewer’s attention to detail, we feel that they miss the mark with this comment as it is a statement about the current state of the field (specifically the underemphasis of potential contributions by invasive plants to biodiversity) rather than a statement of our particular objectives for this study. Thus, we are not making any claims that we might be beholden to later. Perhaps more importantly, though, we disagree with the reviewer’s assertion that evaluating contributions to biodiversity is limited to a consideration of species with direct trophic links to a particular plant species. In our view this is an artificial restriction that unduly neglects the important non-trophic relationships between plants and arthropods, possibly including shelter from elements, refugia from predators, hunting substrates, and thermal benefits. Finally, the ultimate focus of our study was the utility of plants as foraging resources for avian predators, which are not concerned with *how* the arthropods got to a plant, just whether they are present. Limiting our analysis to direct herbivores of our focal plant species would undermine this goal and overlook a diversity of other invertebrate taxa that are also important to insectivorous birds.

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**Line 285: No, you have not shown that invasives are producing equal amounts of resources for foraging predators.**

We agree with the reviewer that it would have been a mistake to assert strongly on this line that invasives in our study produced equal amounts of invertebrate resources via direct trophic interactions (although this was supported by our taxon-specific analyses of arthropod abundance). Fortunately, we made no such claim, instead writing on this line that our evidence suggested invasives were “comparable to the dominant native plants in their value as foraging resources for insectivorous birds.” Given that insectivorous birds have limited reasons to distinguish between the arthropods with trophic links to their host plants and the arthropods simply present on them, we believe that our measurements of a broader set of invertebrates are both reasonable and support this claim.

**Line 311: Why were you surprised to find similar foraging activity on natives and non-natives if the predators were foraging for aquatic insects and spiders not produced by those plants? Plants defenses have no impact on those arthropods. I could build a matrix of wooden scaffolding in the woods and collect the same number of resting arthropods on it that you did on the non-natives in your study, but I would be wrong to conclude that scaffolding is just as productive as native plants.**

With respect to the reviewer, it appears that they glossed over several key parts of the results that were essential to this part of the discussion. For one, contrary to their presumption that insects with aquatic larval stages constituted substantial portions of arthropods on all plant species, we clearly state on lines 221–222 (lines 220–221 of the original submission) of the results that aquatic insect orders were particularly associated with musclewood. As musclewood is a native plant, the inclusion of those orders would therefore only serve to increase the estimated productivity of native species, in fact making our finding of no difference between plant groups conservative. Additionally, from lines 241 to 251 (240–250 of the original submission) we present results concerning bird effects on the abundance of herbivorous taxa. These results are both precisely the kind of evidence the reviewer has requested and also once again support our conclusion that there was little evidence of substantial differences between native and non-native plants in either herbivore abundance or predation pressure.

In the context of these results, we believe that our claim that similar predation pressures were unexpected is reasonably well supported: we failed to find higher predation pressure on native plants despite 1) arthropod productivity being tilted in their favor by the presence of aquatic insects on musclewood, 2) anticipated differences in arthropod productivity due to variation in leaf tissue quality (largely unsupported in taxon-specific analyses), and 3) anticipated differences in arthropod productivity due to plant architecture (somewhat supported in spiders). Accordingly, we believe that the current text of this line is justified.

**Line 314: You say "First, leaf tissue is of lower quality or more highly defended than on native plants, reducing biomass of arthropods on invasive plants." What lower quality leaf tissue really produces is fewer insect herbivores. Not aquatics and spiders.**

We agree with the reviewer that variation in leaf tissue quality impacts arthropod communities most prominently and directly through trophic interactions, with lower quality resulting in fewer/smaller herbivores and largely unchanged numbers/biomass of aquatic species (spiders could be affected by indirect trophic interactions). Our reason for considering leaf tissue quality relevant to this discussion is that in our taxon-specific analyses only one out of three herbivorous orders (lepidoptera) had lower abundances on invasive plants than native plants. This finding provides evidence against our (and the reviewer’s) expectations and is completely unaffected by the inclusion of aquatics or spiders in other analyses. We have added the following sentence to lines 316–318 of the discussion in order to highlight the connection with this result:

“Our finding of comparable numbers of herbivorous hemipterans and orthopterans on invasive and native plants (Figure S6) suggests that this is not universally true.”

**Line 316: You say "Spider abundance was higher on low-lying Japanese barberry, similar to other observations with invasive plants like Japanese stiltgrass." Yes, but the prey those spiders are hunting are not produced by the plants in question. They are typically flying adults of detritivores (Tipulids etc), incidental aquatic adults like mayflies, dobsonflies, stoneflies, and caddisflies, as well as carrion flies, various beetles, etc. Again, you are implying that barberry and stiltgrass actually produce more prey than native plants. They do not.**

Considering the reviewer’s careful attention to potential trophic links throughout their feedback, we are rather surprised by their casual assertion that the spiders collected in our study (whether in general or specifically on barberry) were not depredating herbivores consuming those host plants. We would like to emphasize that we counted *all* spiders and we strongly suspect that at least *some* of them consumed the numerous herbivores that we did collect on our host plants. At a minimum, we are surprised that the reviewer was intimately familiar with our study site and all 213 invertebrate families we identified as to make this strong claim. More to the point, though, is that our purpose in writing this sentence was to highlight the potential impact of plant architecture on patterns of occupation by invertebrates that could subsequently become food for birds. Whereas the reviewer appears to be primarily concerned with trophic links between plants and arthropods, we would argue that such a narrow focus inevitably leaves out important factors that shape natural communities. We have revised this sentence (lines 321–323) to emphasize the connection with plant architecture. It now reads:

“These differences in architecture may explain why spider abundance was higher on low-lying Japanese barberry, matching other observations with invasive plants like Japanese stiltgrass (Landsman et al. 2020).”

**To reiterate, web-building spiders should be considered separately since they are using plants (and the ceiling corners in my house) just as structural support for their nets. Their prey are flying insects that are not associated with the plants in question. I have the same concern about aquatic insects. They are simply using the plants as resting sites. Because they are not produced by plants, they do not belong in a comparison of the insect productivity of native vs non-native plants. Their load on a plant is much more a function of that plant's proximity to a wetland rather than its evolutionary origin. The only arthropods predicted to be affected by nativity of plants are insect herbivores. The data gathered here should be re-analyzed excluding web-building spiders (free hunting spiders can be included as an index of prey load) and aquatic insects that were not produced by the plants sampled.**

Although we admire the reviewer’s devotion to maintaining a focus solely on trophic relationships between plants and invertebrate herbivores, we would like to respectfully remind them of several key aspects of our study that address their concerns about inappropriate analyses. First, web-building spiders were only a subset of the spider specimens that we collected and were only pooled with other taxa for analyses of biomass, both of which thoroughly limit the impact of their inclusion on the findings of our study. Moreover, it is an extreme and baseless assertion that their prey were only (or even predominantly) flying insects that were not associated with the host plants in question. Any insect that approached our study plants by air would be susceptible to their predation. Second, as we described above and in the results section of our manuscript, aquatic insects were only a large contributor to the arthropod community occupying musclewood, a native plant species that did not impact our estimates of productivity for invasive species. Additionally, like web-building spiders, these insects were only included in analyses of biomass and therefore had a highly limited impact on the overall conclusions of this study. Third, a substantial portion of our results was devoted to taxon-specific analyses. These analyses included several herbivorous taxa, exactly as the reviewer desired, and produced results that were congruent with the results of analyses of both total biomass and arthropod protein content. The complimentary nature of these datasets increases our confidence that analyses of biomass were not unduly impacted by the inclusion of web-building spiders or insects with aquatic larval stages. Lastly, and most importantly, our objective was never to narrowly compare the insect productivity of native and non-native plants, despite the reviewer’s interpretation as such. In fact, neither the word “produce” nor the word “productivity” appear once in the text of our manuscript. Instead, our stated goal on lines 89–92 of the introduction was to establish whether target invasive species offer lower quality food resources to insectivorous birds. The food resources on a plant available to birds potentially include all arthropods present, not just those “produced” by the plant and especially not just those “produced” in the very narrow sense of having direct trophic connections to the plant. We hope that these points help clarify why we chose this arrangement for our analyses.