Reviewer comments

**Reviewer #1**

This is a very well-written manuscript that describes relevant information related to the continued EAB invasion of North America. Nearly any time I had a question or was confused about a point you were making, it was clarified in the following sentence(s). The information flows well and the conclusions are supported by the data presented, with important caveats appropriately described.

Thank you for the helpful suggestions, and we are glad you found the manuscript to be well-written.

One specific comment relates to lines 152-54 where the information regarding interspecific variation would benefit from references included in the text.

We have included new references for this section.

My only general comments relate to the figures and how some contain several panels that are presenting a large amount of content (specifically, figures 4-6). It may be worth simplifying these or breaking them into separate figures so the key messages can be better highlighted.

We have split Figures 4 and 5 into three separate figures, combining the scatterplots within each panel into a new figure (Figure 6 in the revised manuscript). We have retained Figure 7 (Figure 6 of original submission) as is because we are hoping to make it easy on readers to compare recruitment and mortality by “inventory year” (2007 vs. 2018) and/or invasion cohort. The results section has also been re-arranged to reflect the presentation order of figures.

**Reviewer #2**

The study is based upon analyses of FIA data on Fraxinus seedlings, saplings, and trees, gathered in counties where EAB has been present for varying lengths of time, for the purpose of gaining an early understanding of the demographic trajectory of ash populations following invasion by EAB. The study is timely because it addresses a question that is on the minds of many, and in general I think it is well done.

Thank you for your insightful review!

The authors' biggest difficulty is that they cannot tie stand inventory data to a specific date of EAB entry, and their work-around for this is hampered by the fact that the stands themselves are remeasured at 5- to 7-year intervals. Their solution was to create "invasion cohorts" of FIA permanent plots based upon the first-known occurrence of the insect in a county, binned in 5- to 6-year intervals, and to summarize inventory data for two 6-year periods that coincide (though not exactly) with the first and third of the invasion cohorts. I'm sure there is no better way to do it, but this approach has two weaknesses. First, the initial occurrence of EAB in the county may precede by years its appearance in an FIA inventory plot, so some plots may be only nominal members of the invasion cohort. Second, even if the events were simultaneous, the superimposition of an inventory interval over a cohort interval means that any given plot within an invasion cohort may have been measured as much as 4 years before (i.e., effectively outside the cohort) or 5 years after actual invasion. The consequence of all this is that they can do no better than present a rather blurry image of ash demographics in response to EAB. This is not exactly made clear by the authors, and I think the paper would be strengthened if they would make these difficulties explicit and discuss their implications.

We now discuss these limitations in the methods (Lines 138-147).

Some specific comments follow.

Lines 42-44 Introductory sentences like this always seem trivial and perfunctory as reasons for the study and inducement for the reader to continue. The second sentence of the paragraph would make for a much livelier start.

Changed as suggested

Line 47 Please don't capitalize chestnut. Only ornithologists capitalize common names, and for reasons only they know.

Changed as suggested

Lines 62-63To my knowledge, the most comprehensive experimental evidence for intraspecific genetic variation is Steiner et al. 2019. Genetic, spatial, and temporal aspects of decline and mortality in a Fraxinus provenance test following invasion by the emerald ash borer. Biological Invasions 21: 3439-3450.

Thank you for this suggestion – we have added this reference to a couple of relevant locations in the revised manuscript.

Line 107 I'm not familiar with FIA protocols, but surely "defined here" is not correct judging from the structure of the sentence. 12.7 cm appears to be an FIA-defined threshold for what is measured in one plot size vs. another and therefore beyond control by the authors. Alternatively, if the authors mean that trees >12.7 cm DBH are "defined here as overstory trees" then they should write it that way.

Reworded for clarity as follows:

“Data on trees ≥12.7 cm DBH, referred to here as overstory trees, are measured on subplots, whereas…”

Line 113 The obvious question here is why do these two intervals resemble - but not exactly coincide with - two of the invasion cohorts? The use of similar but not identical intervals for invasion cohorts and inventory periods is puzzling, and needs some explanation to assist reader understanding.

Apologies for the confusion – inventory periods and invasion cohort intervals were not meant to coincide perfectly, given that the former reflect sampling dates and the latter were used to assign counties to bins by year of invasion (i.e., a proxy for invasion history length as of 2018). Thus, invasion cohort intervals were constructed independent of inventory years and to create a spectrum of invasion history lengths and impacts. We have added this clarification to the text (Lines 116-121), and worry that if the intervals did coincide perfectly that readers might think that the earliest invasion cohort was only evaluated using FIA data from 2002-2007 (vs. at two different time periods).

Lines 113-115 I don't think this shorthand works well because it reads as though estimates are tied to the particular years of 2007 and 2018, and not the intervals 2002-2007 and 2013-2018. Even with the shorthand in mind, there is the lingering question each time about whether in this instance the authors actually mean that particular year.

Prior to the original submission, we debated how best to report these years/inventory periods. We elected to use this shorthand ultimately to maintain consistency with the rFIA package documentation (Stanke and Finley 2020; Stanke et al. 2020). However, to make it clear that we are reporting on data collected across an interval of time and to maintain consistency with the rFIA package and its associated publication, we now refer to inventory periods ending in years 2007 and 2018 as INV-2007 and INV-2018, respectively.

Lines 120-121 Strictly speaking, annual mortality ≠ ratio of newly dead trees to number of still-living trees.

Thank you for bringing this up – this description of annual mortality was accidentally retained from an earlier calculation. We have updated with the correct description of this estimate, paraphrased from the rFIA package documentation.

Line 125 Because the variables annual mortality and annual recruitment are critical to the study objectives, you need to explain how these are quantified in plots that are only measured at 5-7 year intervals and in which (I assume) trees are not individually tagged. My guess is that one must assume that all dead trees in the plot have died since the last measurement, and that all trees that have died since the last measurement are still present. If I understand correctly, any dead trees remaining from the previous inventory period would cause mortality and presumably recruitment to be overestimated, and any missing trees the opposite. Are harvested trees accounted for separately? What about trees that grow out of the diameter class? Details like this cannot be ignored as mere agency protocol. Most readers will not know those protocols, and they are critical to evaluating the quality of the data.

Trees are tracked individually as part of the FIA sampling protocol, which enables accurate plot-level counts of trees that grew beyond the 12.7 cm threshold (recruitment or ingrowth) and trees that died since the previous inventory. Harvested trees are also counted separately and recorded as removals. We were initially interested in their potential role in the patterns we present, but the number of removed trees was extremely small compared to ingrowth and mortality.

To address these points in the manuscript, we have added (i) text explaining that individual trees are tracked as part of the FIA sampling protocol (Lines 130-132) and (2) an explanation in the methods that removals were extremely low (e.g., <0.006 trees per ha across all inventory years × invasion cohort) in comparison to ingrowth and mortality (Lines 134-137).

Lines 129-130 Sentence is unnecessary. You might as well say sizes were extracted in inches and converted to cm, but you do not.

Removed as suggested.

Line 143 "Abundances" is an awkward word, a noun that people do not expect to see in plural form because it is a quality not a quantity - like "beauties of the various young contestants." Why not just use "densities"?

Changed throughout as suggested

Lines 187-188 ". . . and there was . . ." This seems too trivial to mention, and Fig. 3b by itself does not illustrate the statement.

We have removed this sentence.

Line 190 Lower than what? I suggest changing to "uniformly low."

Changed as suggested

Lines 220-221 This sentence and a similar one in the previous paragraph are confusing to me. Please clarify.

Edited for clarity – we present the model input (slope coefficients, standard errors, test statistics, etc) for each model in the appendix.

Lines 243-245 I don't think those changes within the smallest diameter class necessarily support the inference. For example, they might be more than balanced by harvest removals from the largest diameter classes.

That is a great point – thank you! We have removed that part of the sentence.

Lines 246-247 The antecedent to "these" appears to be trees recruited into the overstory, so "these seedlings and saplings" is confusing.

Edited for clarity as follows:

“However, as the invasion continues, many of these young ash trees appear to be succumbing to…”

Lines 251-252 Well, we already know that, don't we? All you have to do is look around the countryside. Or ask a sawmill owner. Others have addressed mortality rates of existing canopy trees after EAB arrival. The interesting question now is whether and how well the ingrowth of pole-sized trees (>5 inches) survives after the irruption is over and EAB populations have presumably declined.

We have edited the first two sentences of this paragraph to more effectively highlight the finding we found interesting: there were no apparent trends in mortality by diameter class.

Lines 260-266 There is more literature that bears on this topic. See the Discussion by Steiner et al. (2019), who present data showing that larger have lower mortality rates until the irruption is nearly over.

We have added text and a citation on this topic (Lines 269-279).

Lines 283-285 I believe there is also documentation that ash fruits heavily as it begins to decline due to EAB attack.

We were not able to find documentation that ashes fruit as an EAB infestation progresses. However, we did include text in the original submission (e.g., Line 314 of revision) highlighting that top-killed trees are able to produce sprouts that contribute to regeneration. We apologize if we have overlooked a reference, however.

References

Stanke H, Finley A (2020) rFIA: Space-Time Estimation of Forest Variables using the FIA Database. R package version 0.2.0. https://CRAN.R-project.org/package=rFIA

Stanke H, Finley AO, Weed AS, et al (2020) rFIA: An R package for estimation of forest attributes with the US Forest Inventory and Analysis database. Environ Model Softw 127:104664. doi: 10.1016/j.envsoft.2020.104664