nature portfolio

Peer Review File

The Initial Spread of Peaches across Eastern North America was Structured by Indigenous Communities and Ecologies

Reviewers' Comments:

Reviewer #1:
Remarks to the Author:
This article is an important archaeological study that is worthy of publication, pending consideration of some revisions, in Nature Communications. I think some revisions should be made or at least considered before the paper is accepted.

The major arguments in this paper are the following: (1) peaches were introduced to Indigenous cultural landscape of the Native American Southeast prior to permanent European settlement of what is now the interior US Southeast; (2) there was a significant amount of time between early Spanish contact and adoption of peaches; and (3) once peaches were introduced, they were widely and rapidly adopted. I think of point #1 as having been documented by Kristen J. Gremillion and Gregory A. Waselkov, but the geographic coverage here by Holland-Lulewicz et al. is much greater, and they bring together a valuable regional dataset here, and greater chronological precision than preceding contributions to the literature. I find point #2 convincing, although a major case study discussed in the article presents an exception to the pattern (which makes it highly interesting). Point #3 is something that scholars have thought about, but Holland-Lulewicz et al. can make the point more strongly with important and innovative Bayesian analyses of radiocarbon dates, which is a major contribution from this article. In my view, the single-most important idea and contribution from this paper is the idea of the "Indigenization" of peaches, and the role of Indigenous socialities and ecologies in shaping the spread of peaches across the Southeast. I recommend giving this latter point even greater emphasis than it does receive, including the highly interesting discussions (already in the article manuscript) about how the presence of clearings in the forest (around towns and farmsteads, for example) played a role in the spread of peaches. I think the "Indigenization" model outlined here is really interesting, and it sets this paper apart from other scholarly discussions of the introduction and adoption of peaches in the Native American Southeast, which, as these coauthors note, are somewhat scattered across the literature—another valuable byproduct of this paper is bringing many of these disparate sources together, although as you will see, I recommend adding some more references that I deem to be relevant.

The article considers data from many archaeological sites (N=28) across a wide area of the US Southeast, but one site that receives particular emphasis is the Lindsey site, in the Oconee Valley in Georgia. It strikes me as a good site to focus on; it is a relatively small and perhaps a single-household farmstead—presumably a very typical kind of settlement for that period in that region (as demonstrated through scholarship by Mark Williams and others)— and if peaches were there, they were probably also reaching settlements in the region where large earthen mounds and plazas (community centers, broadly speaking) were located. What is surprising (to me, but I am convinced by what they present here) are the relatively early dates, in the early-to-mid sixteenth century. These early dates suggest a date fairly soon after early Spanish contact in Georgia, beginning with the Hernando de Soto expedition (1539–1543), although the Narvaez expedition (1528–1529) traversed areas of northern Florida not too much farther south, and there were attempts at settlement in coastal South Carolina by Lucas Vazquez de Ayllon in 1521 and 1526. (One of the leading experts, and arguably the leading expert, on these Spanish entradas in the Southeast is John Worth, and he

is a coauthor of this paper—there is probably nothing I could say about these expeditions that he does not already know.) What I would say here, though, is that if the AMS dates from the Lindsey site reported here are accurate (and I take them to be, with good contextual associations), the peaches from the Lindsey site must have come not from a Spanish colonial town nor a Catholic mission, but from one of these or another Spanish entrada. (My own guess is that peaches reached the Oconee Valley from the Ayllon settlements in coastal South Carolina, which were a likely source of metal items that Soto and his men saw in the province of Cofitachequi in 1540, and while I cannot reject the possibility that the source of early peaches in Oconee Valley was the Soto expedition, it just seems more likely peaches were grown at an attempted plantation by Ayllon than carried on a military expedition by Soto.) What I would say further here is that the early dates from Lindsey are surprising to me, and they seem to either be a challenge to or an exception to the argument for a time lag between early Spanish contact and the arrival of peaches in this particular study area. This one example does not necessarily undermine the broader points advocated in the article, but as an article, its "main" case study seems more of an exception than the pattern.

Is it correct to say that peach pits from Lindsey are now the earliest known peaches from an archaeological site in the US Southeast? If that is the case—the dates seem robust to me—I think this is one of the "headlines" that may warrant some greater emphasis. I would go further to argue that this early date emphasizes the point about "Indigenization" of peaches, much of which happened in the context of the seventeenth-century Catholic mission period, but which in some instances was associated with the period of sixteenth-century Spanish entradas and exploration. Maybe "isolated" cases such as these were important precursors to the more widespread and rapid spread of peaches later on, and maybe the early dates in the 1500s from Lindsey help explain why peaches seem relatively common in the Oconee Valley during the 1600s.

I think it is important to cite and to engage with relevant discussions in Gabrielle C. Purcell's dissertation at UNC-Chapel Hill, "An Analysis of Cherokee Foodways during European Colonization" (2022). Purcell outlines a general model of how peaches spread across the Southeast and when, and while the authors of the article may have different ideas about these topics, I think Purcell's work contributes to the conversation, and I think it should be acknowledged and cited. Her dissertation focuses on sites that represent Cherokee towns, and the article in review focuses somewhat more on sites associated with Muscogee groups.

Other articles that may be relevant to consider include the following.

Hammett, Julia E.
1992 Ethnohistory of Aboriginal Landscapes in the Southeastern United States. Southern Indian Studies 41:1-50. https://www.rla.unc.edu/Publications/NCArch/SIS_41.pdf.

Peles, Ashley
2015 Exploring Household Foodways in the North Carolina Piedmont, 1450–1710. In Beyond the Walls: New Perspectives on the Archaeology of Historical Households, edited by Kevin R. Fogle, pp. 47–70. University Press of Florida, Gainesville. https://academic.oup.com/florida-scholarship-online/book/17150/chapter-abstract/174512696.

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VanDerwarker, Amber M., Jon B. Marcoux, and Kandace D. Hollenbach
2013 Farming and Foraging at the Crossroads: The Consequences of Cherokee and European Interaction through the Late Eighteenth Century. American Antiquity 78(1):68–88.
http://www.jstor.org/stable/23486385.

The group of coauthors for this article includes scholars from different disciplines and different institutions, and one coauthor is a member of an Indigenous community and the Historic and Cultural Preservation Department of the Muscogee (Creek) Nation based in Okmulgee, Oklahoma. This kind of interdisciplinary collaboration is important in archaeology today, as is the involvement (as is very well demonstrated here) of members of Indigenous communities and stakeholders. Collectively, the group of coauthors are an impressive group of scholars, and those who are archaeologists are highly regarded in our field. The point that Muscogee people carried peaches from ancestral homelands in Georgia and Alabama to Oklahoma, and that contemporary heirloom peaches grown in Oklahoma may be "descended" from those peaches and those trees, is profoundly interesting and important. I want to emphasize just how important these aspects of coauthorship and collaborative scholarship are in North American archaeology today, and I applaud this research team in these respects.

Some specific points in the manuscript where I recommend some changes are the following.

lines 54–56 – I guess many of us do make this or similar assumptions, but it does make sense that peaches came principally from seventeenth-century Catholic missions and less so from sixteenth-century Spanish explorations and military entradas. The dates posited by Purcell (2022:129) in her Figure 6.3 tend towards the 1600s and 1700s, and she suggests peaches moved inland via Indigenous trade routes.

lines 78–83 – Excellent points, but the "historical" and "ecological" dimensions are, in my view, more well developed and articulated in the paper than is the "sociopolitical" dimension—which I think could be developed, but just is not quite as much in its current form.

lines 122–125 – I am not sure it is essential, but I recommend mentioning the Ayllon, Narvaez, and Luna expeditions here, each of which might have been sources of peaches brought to the Interior Southeast; maybe even the Pardo entradas are relevant to note here, although they were situated further north than the Oconee Valley. One of the coauthors of the article, John Worth, probably

knows the relevant historical sources as well as anybody (and better than I do).

lines 141–142 – As far as I am aware, excavations at the Berry site in western North Carolina have yielded no evidence of peaches, so while there is some evidence of peaches at Santa Elena (South Carolina), there is no evidence of peaches at the principal outpost in the interior associated with the Pardo expeditions whose point of departure was Santa Elena.

lines 150–151 – The Alarka site is located in southwestern North Carolina, not central North Carolina.

lines 155–157 – I recommend citing John Lawson here, and/or Julia Hammett's (1992:18) summary point that Lawson witnessed peach trees growing in the North Carolina Piedmont in 1701, perhaps with minimal encouragement (conveying the idea that they just grew relatively well mostly on their own wherever they were grown).

Reviewer #2:
Remarks to the Author:
The article is well written, with arguments clearly laid out and previous studies and existing literature appropriately referenced. I personally liked the multidimensional approach adopted, and the effort made by the authors in including and giving back agency to Indigenous communities in the topic of foreign plant dispersal and uptake. I especially liked the discussion section, where each line of evidence is laid out and merged together in cohesive interpretations. The review of known peach data and this approach makes a very valuable contribution to the field.

There are some minor comments that if addressed would improve the article and the solidity of the arguments.
Main article text:
Line 62: some references to the archaeological publication that cite peach's abilities as mentioned should be added
Line 72: I suggest to rephrase the conclusion of the sentence, as currently it is unclear who is the "they" that diffuse on their own accord.
Line 136-137: check repetition moved the plant rapidly- adopting the plant rapidly

Supplementary Material:
Table S2: It isn't clearly understandable which individual AMS dates listed in the table refer to which site, I suggest you add indication of this in the table or group dates by sites, so that the reader can check which material was used for establishing each site chronology.
Although it is stated that AMS dating was undertaken only on short-lived plant material (line 97-98), table S2 then lists the charcoal used for AMS dating as unidentified. Therefore it is unclear how it can be stated that the plant it came from was short-lived, if not even the genus has been identified. This needs to be clarified. It should also be made clear that peach presence at Lindsey is attested by association, rather than direct peach pit dating. This is not presently clear by the way the data is

presented in both the text and the supplementary material, only by looking at the final Table S2 this becomes apparent. This does not take away nor undermines the interpretations and conclusions, but a more transparent presentation of the data is needed.

Overall, this is a great study and opens up the field to more inclusive approaches and interpretations.

Reviewer #3:
Remarks to the Author:
For this paper, I focus on Bayesian modelling, which is my specialty. Overall, although the paper is interesting and certainly a contribution, I am not certain that it merits publication in Nat Comms. I would recommend a different journal instead.

Here are my recommendations:

The SI notes that alternative models produced comparable results but there is no sensitivity testing. This is important and should be done using the Difference command.

In models where an Outlier_Model is applied (i.e., a prior probability is enforced), agreement indices should not be used as incompatible. Moreover, I recommend that the outlier analysis is shown in the plots (OxCal option). Otherwise, Outlier_Model results are not accessible/presented without modelling.

Some models have limited sample sizes, e.g., Model E with n=2. Although small sample size is unfortunately commonplace in archeology, it should be noted somewhere that this is the case and that, therefore, there is a degree of uncertainty regarding the modelled results and more data is needed to increase resolution.

Considering my last point and the fact that all estimates are distributions with uncertainty (where the cultural event is likely to occur at any point in a given temporal range at x probability) the statements in lines 122-132 are rather simplistic (subtracting known ages from one extreme of the 68% CI). I would advise a more conservative approach and reporting the 95% CI (e.g., use max and min instead, as in lines 182-184).

Response to Reviewer Comments

All noted page numbers and lines correspond to the Manuscript Word Document.

Reviewer #1 (Remarks to the Author):

This article is an important archaeological study that is worthy of publication, pending consideration of some revisions, in Nature Communications. I think some revisions should be made or at least considered before the paper is accepted.

- Noted.

The major arguments in this paper are the following: (1) peaches were introduced to Indigenous cultural landscape of the Native American Southeast prior to permanent European settlement of what is now the interior US Southeast; (2) there was a significant amount of time between early Spanish contact and adoption of peaches; and (3) once peaches were introduced, they were widely and rapidly adopted. I think of point #1 as having been documented by Kristen J. Gremillion and Gregory A. Waselkov, but the geographic coverage here by Holland-Lulewicz et al. is much greater, and they bring together a valuable regional dataset here, and greater chronological precision than preceding contributions to the literature. I find point #2 convincing, although a major case study discussed in the article presents an exception to the pattern (which makes it highly interesting). Point #3 is something that scholars have thought about, but Holland-Lulewicz et al. can make the point more strongly with important and innovative Bayesian analyses of radiocarbon dates, which is a major contribution from this article. In my view, the single-most important idea and contribution from this paper is the idea of the "Indigenization" of peaches, and the role of Indigenous socialities and ecologies in shaping the spread of peaches across the Southeast. I recommend giving this latter point even greater emphasis than it does receive, including the highly interesting discussions (already in the article manuscript) about how the presence of clearings in the forest (around towns and farmsteads, for example) played a role in the spread of peaches. I think the "Indigenization" model outlined here is really interesting, and it sets this paper apart from other scholarly discussions of the introduction and adoption of peaches in the Native American Southeast, which, as these coauthors note, are somewhat scattered across the literature—another valuable byproduct of this paper is bringing many of these disparate sources together, although as you will see, I recommend adding some more references that I deem to be relevant.

- The Summary/Abstract (Page 1 Lines17-28) has been rewritten to emphasize both the (1) earliest dates on peach bearing archaeological contexts in the United Sates and (2) the primary role of Indigenous structured ecologies and landscapes in the spread of peaches.
- We continue to include the full discussions of the role of Indigenous socialities and ecologies in the discussion (as Reviewer #1 mentions), but we also signpost these impactful interpretations and results much earlier in the manuscript, in the Introduction (Page 4 Lines 3-17). In this paragraph we also point to the different kinds of Indigenous land uses that would have allowed the successful and rapid spread of peach trees, which includes land-clearing practices associated with clearing field for agriculture, clearing space for towns and habitation, and the clearing and thinning of forests for fuelwood.
- We think that this addresses Reviewer #1's call for "more emphasis" and think it is an appropriate amount of discussion. We are likewise EXTREMELY fascinated by these results (although we are of course biased). That said, we agree with Reviewer #2 that "…this is a great study and opens up the field to more inclusive approaches and interpretations." In this regard, we agree 100% with Reviewer #1, and would argue that the current manuscript opens the door for more explicit, empirical evaluation of the exact character and scale of Indigenous landscape modifications.

- Right now, the ecological discussions that we include are a model to partly explain our dating results.
- To this end, we have also included some outstanding research questions at the end of the Ecological discussion that outlines specific research questions that have been raised by this study. We think that this will not only add more emphasis as Reviewer #1 suggests, but will also be a very fruitful addition for readers to cite in designing and undertaking further research in these topics (Page 17 Lines 14-22).

The article considers data from many archaeological sites (N=28) across a wide area of the US Southeast, but one site that receives particular emphasis is the Lindsey site, in the Oconee Valley in Georgia. It strikes me as a good site to focus on; it is a relatively small and perhaps a single-household farmstead—presumably a very typical kind of settlement for that period in that region (as demonstrated through scholarship by Mark Williams and others)— and if peaches were there, they were probably also reaching settlements in the region where large earthen mounds and plazas (community centers, broadly speaking) were located. What is surprising (to me, but I am convinced by what they present here) are the relatively early dates, in the early-to-mid sixteenth century. These early dates suggest a date fairly soon after early Spanish contact in Georgia, beginning with the Hernando de Soto expedition (1539–1543), although the Narvaez expedition (1528–1529) traversed areas of northern Florida not too much farther south, and there were attempts at settlement in coastal South Carolina by Lucas Vazquez de Ayllon in 1521 and 1526. (One of the leading experts, and arguably the leading expert, on these Spanish entradas in the Southeast is John Worth, and he is a coauthor of this paper—there is probably nothing I could say about these expeditions that he does not already know.) What I would say here, though, is that if the AMS dates from the Lindsey site reported here are accurate (and I take them to be, with good contextual associations), the peaches from the Lindsey site must have come not from a Spanish colonial town nor a Catholic mission, but from one of these or another Spanish entrada. (My own guess is that peaches reached the Oconee Valley from the Ayllon settlements in coastal South Carolina, which were a likely source of metal items that Soto and his men saw in the province of Cofitachequi in 1540, and while I cannot reject the possibility that the source of early peaches in Oconee Valley was the Soto expedition, it just seems more likely peaches were grown at an attempted plantation by Ayllon than carried on a military expedition by Soto.) What I would say further here is that the early dates from Lindsey are surprising to me, and they seem to either be a challenge to or an exception to the argument for a time lag between early Spanish contact and the arrival of peaches in this particular study area. This one example does not necessarily undermine the broader points advocated in the article, but as an article, its "main" case study seems more of an exception than the pattern.

Is it correct to say that peach pits from Lindsey are now the earliest known peaches from an archaeological site in the US Southeast? If that is the case—the dates seem robust to me—I think this is one of the "headlines" that may warrant some greater emphasis. I would go further to argue that this early date emphasizes the point about "Indigenization" of peaches, much of which happened in the context of the seventeenth-century Catholic mission period, but which in some instances was associated with the period of sixteenth-century Spanish entradas and exploration. Maybe "isolated" cases such as these were important precursors to the more widespread and rapid spread of peaches later on, and maybe the early dates in the 1500s from Lindsey help explain why peaches seem relatively common in the Oconee Valley during the 1600s.

- Absolutely, yes. We agree that there are two separate "headlines" that resulted from our study. And we agree that they are not in conflict with each other and that they may each be the result of different processes and historical circumstances. Generally, our models allow us to track the widespread adoption of peaches, this is Process #1. How/when/where did peaches rapidly spread across the southeast? Given the timing of this "boom" these peaches likely came from Spanish missions/towns established in the late 16th C. Lindsey, however, offers a glimpse into an

"exception" to this broader process. It seems that there were likely isolated instances in which peaches made their way into Indigenous communities BEFORE they were widespread. We argue that there may or may not be connections between this process (earliest presence) and the later process (widespread adoption). Reviewer #1 suggests that his early presence may have set the stage for the Oconee Valley's later role. This might certainly be the case. But it may also be the case that this isolate early presence is not connected to the later spread of peaches. In our view, this demonstrates more the central role of the Oconee Valley in historical contacts and interactions with the Spanish, from the earliest entradas to later missions. That said, we cannot discount the possibility that there may be other early (early to mid 16th C) indigenous sites in the American Southeast where peaches might be found, but have not yet been excavated. As we continue to date contact period sites with higher resolution that has previously been available, we may well find more evidence of early peach presence. But this is of course a future direction.

- We note our preferred explanation, the central, enduring role of Oconee Valley communities in interactions with Spanish colonizers, in our discussion of Indigenous sociopolitics, which also addresses Reviewer #1's comment about adding emphasis to the sociopolitical "domain" (Pages 14-15).
- The "earliest dates" results have been emphasized by putting them directly into the Abstract/Summary, in the intro, and in the discussion. Additionally, we note that not only, to our knowledge, are these the earliest dated peach contexts in the southeast, but in the United States more broadly.

I think it is important to cite and to engage with relevant discussions in Gabrielle C. Purcell's dissertation at UNC-Chapel Hill, "An Analysis of Cherokee Foodways during European Colonization" (2022). Purcell outlines a general model of how peaches spread across the Southeast and when, and while the authors of the article may have different ideas about these topics, I think Purcell's work contributes to the conversation, and I think it should be acknowledged and cited. Her dissertation focuses on sites that represent Cherokee towns, and the article in review focuses somewhat more on sites associated with Muscogee groups.

- We add a brief discussion of Purcell's work (Page 3, lines 3-12). Purcell is an early career researcher, and we do not wish to negate her findings or to unfairly criticize her work. The work is fabulous, though our high-precision dating allows us to present an alternative model to Purcell's model. Purcell's work, for an unknown reason, does not include any of the known peach-bearing sites across most of Georgia, including the region where we found the earliest dated peaches and peach-bearing contexts. So, our model is a bit different than Purcell's. That said, we reference and acknowledge Purcell's study (which is her dissertation) without emphasizing the differences between her results and ours. Additionally, Purcell's work is most relevant for how peaches were integrated into later (18th century) Indigenous towns, as her dataset is primarily from these later periods and presents a fabulous contribution to these later histories.

Other articles that may be relevant to consider include the following.

Hammett, Julia E.
1992 Ethnohistory of Aboriginal Landscapes in the Southeastern United States. Southern Indian Studies 41:1-50. https://www.rla.unc.edu/Publications/NCArch/SIS_41.pdf.

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2013 Farming and Foraging at the Crossroads: The Consequences of Cherokee and European Interaction through the Late Eighteenth Century. American Antiquity 78(1):68–88. http://www.jstor.org/stable/23486385.

- Citations to Peles, Vanderwarker et al 2013, and Hammett have been added to the text and integrated into discussions as Ref #s 18 (Peles, Page 8 Line 4), 19 (VanDerwarker et al., Page 8 Line 4), and 20 (Hammett, Page 8, Line 6; Page 15 Line 1).
- Citations to VanDerwarker and Detwiler (2001, 2000) were not added, as they are slightly outside of the scope of the discussion. They are indeed about one of the sites cited, but their discussion is a bit more specific about the processes of integrated new foods into Indigenous households, which, while fascinating, is beyond the scope of the processes considered in this study about the timing of the spread of the foodstuffs themselves. Further, without precise chronologies in these papers, it is hard to tie the processes that these authors discuss to any specific time period. They are great studies, they just don't necessarily correspond directly with (or as examples of) any of our foci.

The group of coauthors for this article includes scholars from different disciplines and different institutions, and one coauthor is a member of an Indigenous community and the Historic and Cultural Preservation Department of the Muscogee (Creek) Nation based in Okmulgee, Oklahoma. This kind of interdisciplinary collaboration is important in archaeology today, as is the involvement (as is very well demonstrated here) of members of Indigenous communities and stakeholders. Collectively, the group of coauthors are an impressive group of scholars, and those who are archaeologists are highly regarded in our field. The point that Muscogee people carried peaches from ancestral homelands in Georgia and Alabama to Oklahoma, and that contemporary heirloom peaches grown in Oklahoma may be "descended" from those peaches and those trees, is profoundly interesting and important. I want to emphasize just how important these aspects of coauthorship and collaborative scholarship are in North American archaeology today, and I applaud this research team in these respects.

- Thanks! The composition of our team, including archaeologists (both academic and professional), an ethnohistorian, a peach biology specialist, and two citizens (and official representatives) of the Muscogee Nation, was very intentional and resulted in a robust, holistic, and impactful study that could not have been produced otherwise.

Some specific points in the manuscript where I recommend some changes are the following.

lines 54–56 – I guess many of us do make this or similar assumptions, but it does make sense that peaches came principally from seventeenth-century Catholic missions and less so from sixteenth-century Spanish explorations and military entradas. The dates posited by Purcell (2022:129) in her Figure 6.3 tend towards the 1600s and 1700s, and she suggests peaches moved inland via Indigenous trade routes.

- Our study would not necessarily suggest a movement via multiple, independent or distinct trade routes or even from multiple origins. Our dating suggests that the missions and towns of Spanish La Florida were the epicenter of the spread of peaches. The dates indicate that they then spread inland and outward like a fan, with clear temporal contours reverberating out from northern Florida and coastal Georgia into the interior. We envision the networks across which peaches spread like a dendritic tree, growing branches out from Spanish La Florida and the Oconee Valley (which may have served someone like a "trunk" from which branches spread). This is what we mean when we discuss the Oconee Valley communities as "brokers" to the broader interior southeast.

lines 78–83 – Excellent points, but the "historical" and "ecological" dimensions are, in my view, more well developed and articulated in the paper than is the "sociopolitical" dimension—which I think could be developed, but just is not quite as much in its current form.

- This is true, although we would concede that the historical and ecological dimensions are of interest to a much wider, global audience and (as Reviewer #1 mentions in a previous comment) are probably the most interesting and "headlining" aspects of the study. This is partly the reason why we place more emphasis on these two dimensions. The sociopolitical dimension, which is concerned primarily with the broad structure of Indigenous networks and their expanse (including the central Georgia communities as potential brokers) is much more parochial in scope. That said, we think that a main contribution of our study to the sociopolitical domain can be framed for a much broader discussion. Specifically, we would note that our study highlights the heterogeneity not only of Indigenous-colonizer interactions but ALSO of Indigenous-Indigenous interactions. We demonstrate how Indigenous networks, especially the historically conditioned heterogeneity of social and political relationships between Indigenous communities, plays just as much of a role in structuring processes of colonialism as the specific characteristics of Indigenous-Spanish interactions. That is, we must understand Indigenous sociopolitics (specifically in this case the very coarse resolution of wide social networks) if we are going to build models of the multi-faceted process of colonialism. Our study contributes to these goals by offering a very coarse resolution model for very broad Indigenous networks at the time of contact. We do this on Pages 14 and 15.

lines 122–125 – I am not sure it is essential, but I recommend mentioning the Ayllon, Narvaez, and Luna expeditions here, each of which might have been sources of peaches brought to the Interior Southeast; maybe even the Pardo entradas are relevant to note here, although they were situated further north than the Oconee Valley. One of the coauthors of the article, John Worth, probably knows the relevant historical sources as well as anybody (and better than I do).

- This is a good point and is information worth adding for context. It places even more emphasis on the fact that even repeated contacts with the Spanish did not automatically or immediately intitate the widespread adoption of peaches. These other expeditions have been cited and discussed on Page 6 Lines 16-20.

lines 141–142 – As far as I am aware, excavations at the Berry site in western North Carolina have yielded no evidence of peaches, so while there is some evidence of peaches at Santa Elena (South Carolina), there is no evidence of peaches at the principal outpost in the interior associated with the Pardo expeditions whose point of departure was Santa Elena.

- This is really interesting, and I think speaks even more to the primary role of Indigenous network in spreading peaches, and not necessarily the movements of the Spanish.

lines 150–151 – The Alarka site is located in southwestern North Carolina, not central North Carolina.

- This has been edited on Page 7 Line 20.

lines 155–157 – I recommend citing John Lawson here, and/or Julia Hammett's (1992:18) summary point that Lawson witnessed peach trees growing in the North Carolina Piedmont in 1701, perhaps with minimal encouragement (conveying the idea that they just grew relatively well mostly on their own wherever they were grown).

- Citation to Hammett added and engaged with on Page 15 Line 1 as Ref # 20 (Hammett) while Lawson (as cited in Lefler, Ref #4) is already cited.

Reviewer #2 (Remarks to the Author):

The article is well written, with arguments clearly laid out and previous studies and existing literature appropriately referenced. I personally liked the multidimensional approach adopted, and the effort made by the authors in including and giving back agency to Indigenous communities in the topic of foreign plant dispersal and uptake. I especially liked the discussion section, where each line of evidence is laid out and merged together in cohesive interpretations. The review of known peach data and this approach makes a very valuable contribution to the field.

There are some minor comments that if addressed would improve the article and the solidity of the arguments.

Main article text:

Line 62: some references to the archaeological publication that cite peach's abilities as mentioned should be added

- Refs #s 3, 5, 8, and 9 have been added on Page 3 Line 14.

Line 72: I suggest to rephrase the conclusion of the sentence, as currently it is unclear who is the "they" that diffuse on their own accord.

- Reworded for clarity on Page 4 Lines 1 and 2.

Line 136-137: check repetition moved the plant rapidly- adopting the plant rapidly

- Repetitive word removed on Page 7 Line 19.

Supplementary Material:

Table S2: It isn't clearly understandable which individual AMS dates listed in the table refer to which site, I suggest you add indication of this in the table or group dates by sites, so that the reader can check which material was used for establishing each site chronology.

- The Site IDs included in Figure 1 and in Table S1 are now included in Table S2. This makes cross comparing samples with sites much easier.

Although it is stated that AMS dating was undertaken only on short-lived plant material (line 97-98), table S2 then lists the charcoal used for AMS dating as unidentified. Therefore it is unclear how it can be stated that the plant it came from was short-lived, if not even the genus has been identified. This needs to be clarified.

- This was a mistake. It has been changed to read "short-lived materials or individually wiggle-matched rings from charcoal samples". This change is on Page 3 n the Supp Material under the heading Chronometric Data.

It should also be made clear that peach presence at Lindsey is attested by association, rather than direct peach pit dating. This is not presently clear by the way the data is presented in both the text and the supplementary material, only by looking at the final Table S2 this becomes apparent. This does not take away nor undermines the interpretations and conclusions, but a more transparent presentation of the data is needed.

- This has been included in the main text on Page 9 Line 14-16.

Overall, this is a great study and opens up the field to more inclusive approaches and interpretations.

- Thanks!

Reviewer #3 (Remarks to the Author):

For this paper, I focus on Bayesian modelling, which is my specialty. Overall, although the paper is interesting and certainly a contribution, I am not certain that it merits publication in Nat Comms. I would recommend a different journal instead.

- We thank Reviewer #3 for their remarks, as they greatly improved the reporting of the chronological modeling. That said, we respectfully disagree about meriting publication in Nature Coms. The study tracks the spread of the FIRST Eurasian domesticate adopted by Indigenous peoples in what is today the United States and the FIRST to spread into the interior of what is today the United States. We believe hat the process of the adoption of newly introduced plants into new landscapes is a globally relevant process, processes which are often studied archaeologically and are often published in the world's top journals. Further, we think that our study's outcome of recentering the process of colonialism on the actions and decisions of Indigenous societies (rather than traditional approaches that center European actions) is also of global relevance as European colonialism of Indigenous peoples and places is equally a global phenomenon.

Here are my recommendations:

The SI notes that alternative models produced comparable results but there is no sensitivity testing. This is important and should be done using the Difference command.

- This sensitivity analysis has been added in the Supplemental Material on Page 6 under the heading Alternative Models and Sensitivity. The difference between the primary model and the alternative model (at the 68% interval) for the initial introduction of peaches is 0 years at the minimum (e.g., same modeled ages) or 10 years at the maximum difference. At the 95% interval, this range is slightly increased to a difference of 5-20 years. Neither difference alters the interpretations of the study. Code for this analysis has been added on Page 18.

In models where an Outlier_Model is applied (i.e., a prior probability is enforced), agreement indices should not be used as incompatible. Moreover, I recommend that the outlier analysis is shown in the plots (OxCal option). Otherwise, Outlier_Model results are not accessible/presented without modelling.

- Convergence indices have also been included in the model results presented in the Supplemental Materials. Where appropriate, all supplemental plot figures have been replaced with plots that also include Outlier results for each date (Figs S2.2-2.8).

Some models have limited sample sizes, e.g., Model E with n=2. Although small sample size is unfortunately commonplace in archeology, it should be noted somewhere that this is the case and that, therefore, there is a degree of uncertainty regarding the modelled results and more data is needed to increase resolution.

- A paragraph in the main text on Page 11 Lines 3-10 has been included discussing future directions, limited sample sizes, and increased resolution.

Considering my last point and the fact that all estimates are distributions with uncertainty (where the cultural event is likely to occur at any point in a given temporal range at x probability) the statements in lines 122-132 are rather simplistic (subtracting known ages from one extreme of the 68% CI). I would advise a more conservative approach and reporting the 95% CI (e.g., use max and min instead, as in lines 182-184).

- Clarification and precision has been added to the main text on Page 6 Lines 22-23 through Page 7 Lines 1-11. The 95% confidence interval is now included and the ranges adjusted by c. 5 years in accordance with the 95% interval (v. the 68% range).

Reviewers' Comments:

Reviewer #1:
Remarks to the Author:
I liked the original submitted version of this article, and I think the revised version is even better. I recommend accepting it as is for publication, and I think it is an important paper that is appropriately placed in Nature Communications. The argument is well-crafted, the text is well-written, the graphics are excellent, and the tables and supplementary materials are clear and thorough. The revisions that the coauthors have made, and the responses to comments from reviewers (including me), are good, and I appreciate the thoughtfulness and consideration with which they revised (or chose not to revise, in some cases) and responded. Thanks for the opportunity to comment, and I look forward to seeing this paper in its published form.

I find the article of greatest interest for its ideas about Indigenous ecologies and modes of sociality through which peaches became Indigenized, but it is valuable too, more generally, in its demonstration of how new analytical methods (high-precision dating, for example, in this case) can move research in the field forward.

Coauthors indicate in the article and in the "response to reviewers letter" that the Oconee Valley in Georgia was an important "trunk" in a dendritic tree of a sort across which peaches spread along several "branches" to other areas of the Native American South. I like this idea very much, and I think it "fits" well the data that are currently known and available; the next thought that comes to my mind is if that was the case, why was the Oconee Valley that trunk, and not somewhere else? Importantly, I do not think this question has to be resolved in this particular paper, but I mention it as one kind of "future consideration" question that this article will introduce into scholarly conversations, although admittedly such a question is of interest mostly to regional specialists in the archaeology of the US Southeast. My own first thoughts about answering my question would be that the chronology of mound centers and other sites in the Oconee Valley (several coauthors of the article have made key contributions to this topic) indicates there were communities and polities present in this region at the point when peaches were first introduced, and maybe something about the regional settlement pattern (political centers at sites with mounds, and relatively dispersed settlement of farmsteads in surrounding areas, and shifts in settlement foci from one area to another) was particularly favorable for the growth and spread of peaches, maybe the soils in the Oconee Valley were particularly well-suited to growing peach trees—or all of the above. If these are questions about why the Oconee Valley was such an important epicenter for Indigenizing peaches are worth asking, the first person I would think to ask about it is Mark Williams, who is a leading authority (and arguably the foremost authority) on archaeology of the Oconee Valley, and who is one of the coauthors of this paper.

This paper will be of great interest to archaeologists and ethnohistorians specializing in the study of the culture and history of Indigenous peoples of the US Southeast, but it will also be of interest to archaeologists interested in the study of colonialism in global perspective, and archaeologists interested in human-environment interactions in different world areas.

Reviewer #2:
Remarks to the Author:
The authors have addressed all previously raised issues, adding clarity to overall methodology, analysis, and results.
The revisions made make the paper an excellent contribution to the field, by providing new data and derived interpretation which will be a useful framework for future work in the topic of fruit dispersal and other closely related topics.

This kind of interdisciplinary work and inclusive approach is highly commendable.
I have no further comments.

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The authors' implemented most of my earlier recommendations regarding the Bayesian modelling, but not all. There are still some Bayesian modelling and radiocarbon issues that still need to be addressed (points no. 3 and 7 below are particularly important), which have implications on the interpretation of the data.

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On the sensitivity testing:
2. The authors now note that sensitivity testing has taken place to compare Model A and Model B for the Oconee Valley. Although the authors are right to comment that the difference is not likely to be significant (because both models represent a single, unordered Phase), the results of the analysis are not properly reported; the distribution for the Difference function has not been plotted. I tried running the code that was provided but, as it is written, it will not work. I am, therefore, unsure as to how the authors tested whether the distributions were comparable and obtained the results that are reported. On this (the reporting, in SI lines 230-239), if the results of the Difference function include zero at 95% CI, then the distributions are comparable. A lot of the text in this paragraph is, therefore, unnecessary. The results plot would be the best way of explaining/demonstrating this.
3. On the wiggle matching: It is understood that the authors would find wiggle matching useful given that the calibration curve for this period is non-monotonic (thus offering poor precision). The number of tree rings incorporated into the wiggle matching analysis is quite small (generally n=2), however, and at times not particularly helpful ('PM58 Charcoal' has two dates that are both the same). Moreover, the wiggle matching analysis, as performed in this study, does not address the issue of inbuilt age (see point 7) or date the cultural event of interest (e.g., the time of felling/burning, which would represent a cultural event and not the age of tree-ring formation). Regarding the latter point, the authors could use OxCal to estimate the age of the last event in an

effort to get closer to a felling date (place a Date function as last event and use this distribution/prior file in the site model instead).
4. Define AMS when first mentioned
5. Pg. 6 (bottom): Consider adding a 'likely' in the sentence starting with 'Results from Bayesian…', since these are estimates, i.e., 'peaches were likely present, and even widespread'. Also, rather than 'by' cal AD 1625-1640, I would note 'starting at cal AD' (given that a start estimate is being quoted).
6. Analysis in 'The eventual adoption of peaches was widespread and rapid' section: Beware; the authors are not comparing like with like. The cal AD 1625-1640 range is an estimated start for peach cultivation/harvest in Oconee Valley. The ages quoted for the different regions, I assume, are not modelled estimates for the start of peach cultivation/harvest, but directly dated events. This should be kept in mind as cal AD 1625-1640 for the Oconee Valley is necessarily earlier.
7. Pg. 9 (lines 10-20): Given that wood charcoal, was inbuilt age considered in the analysis? Did anthracological analysis take place to mitigate, i.e., were species identified to be short-lived or was bark found? There is a gap of 8 years in the 'Prov 2 LN 31 Charcoal' sample, which might be enough rings (n=10) for a species ID. Table S2 notes unidentified species and oak wood (although not for Lindsey site), which has a considerable life span. There is no mention of inbuilt age for wood charcoal in either main text or SI. This is particularly important because the site is noted to represent the earliest dated archaeological context containing peaches in the US (and the period the paper deals with is relatively short/young, requiring great precision/accuracy). If there is inbuilt age in the wood, however, the estimate could be erroneously old and the antiquity misrepresented. This is important to address during revision. If inbuilt age cannot be ruled out, then the assertion should not be made with such confidence and the interpretation must remain conservative in its approach. On this subject, Model G in the SI (which seems to correspond to the site discussed) mentions that Charcoal Outlier analysis (this makes the model assume that the charcoal age is erroneously too old, denoting inbuilt age) was applied to non-wiggle-matched dates, but this is not the case. The authors, therefore, seem to consider inbuilt age a certainty in the modelling (or, at least, in the modelling report), but do not mention this in the main text. This is all a bit confusing but the main concern (inbuilt age) remains. Continuing from this, I recommend the site name is added to Table S2. It would have helped when finding the charcoal samples from the Lindsey site there.
8. Pg. 9 (lines around 20): The end and start here likely overlap and are comparable. I suggest testing coevality using a Difference command and report.
9. In the SI, briefly describe the Outlier_Model (or models) that were used and why (or under what circumstance).
10. In the SI, model C description (also applies to other models): The authors note that no convergence values fall below 99.6, but not sure why this comment is necessary. The models presented are not necessarily complicated and so good convergence should be reached (the model will continue MCMC sampling until convergence is >95%).

Response to Reviewer Comments

All noted page numbers and lines correspond to the Manuscript Word Document.

Yellow highlighting indicates changes made during the first round of revisions. Pink highlighting indicates changes made during this current round of revisions.

Reviewer #1 (Remarks to the Author):

I liked the original submitted version of this article, and I think the revised version is even better. I recommend accepting it as is for publication, and I think it is an important paper that is appropriately placed in Nature Communications. The argument is well-crafted, the text is well-written, the graphics are excellent, and the tables and supplementary materials are clear and thorough. The revisions that the coauthors have made, and the responses to comments from reviewers (including me), are good, and I appreciate the thoughtfulness and consideration with which they revised (or chose not to revise, in some cases) and responded. Thanks for the opportunity to comment, and I look forward to seeing this paper in its published form.

I find the article of greatest interest for its ideas about Indigenous ecologies and modes of sociality through which peaches became Indigenized, but it is valuable too, more generally, in its demonstration of how new analytical methods (high-precision dating, for example, in this case) can move research in the field forward.

Coauthors indicate in the article and in the "response to reviewers letter" that the Oconee Valley in Georgia was an important "trunk" in a dendritic tree of a sort across which peaches spread along several "branches" to other areas of the Native American South. I like this idea very much, and I think it "fits" well the data that are currently known and available; the next thought that comes to my mind is if that was the case, why was the Oconee Valley that trunk, and not somewhere else? Importantly, I do not think this question has to be resolved in this particular paper, but I mention it as one kind of "future consideration" question that this article will introduce into scholarly conversations, although admittedly such a question is of interest mostly to regional specialists in the archaeology of the US Southeast. My own first thoughts about answering my question would be that the chronology of mound centers and other sites in the Oconee Valley (several coauthors of the article have made key contributions to this topic) indicates there were communities and polities present in this region at the point when peaches were first introduced, and maybe something about the regional settlement pattern (political centers at sites with mounds, and relatively dispersed settlement of farmsteads in surrounding areas, and shifts in settlement foci from one area to another) was particularly favorable for the growth and spread of peaches, maybe the soils in the Oconee Valley were particularly well-suited to growing peach trees—or all of the above. If these are questions about why the Oconee Valley was such an important epicenter for Indigenizing peaches are worth asking, the first person I would think to ask about it is Mark Williams, who is a leading authority (and arguably the foremost authority) on archaeology of the Oconee Valley, and who is one of the coauthors of this paper.

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- Thank you for the kind words and invaluable comments!!

Reviewer #2 (Remarks to the Author):

The authors have addressed all previously raised issues, adding clarity to overall methodology, analysis, and results.

The revisions made make the paper an excellent contribution to the field, by providing new data and derived interpretation which will be a useful framework for future work in the topic of fruit dispersal and other closely related topics.

This kind of interdisciplinary work and inclusive approach is highly commendable.

I have no further comments.

- Thanks so much for your previous comments and for the kind remarks!

Reviewer #3 (Remarks to the Author):

- Before addressing comments, we would like to thank Reviewer #3 for their suggestions, which have certainly strengthened our models and have forced as to be more cognizant of addressing all potential assumptions, parameters, and limitations. These comments have greatly contributed to the methodological robustness of the study.

The authors' implemented most of my earlier recommendations regarding the Bayesian modelling, but not all. There are still some Bayesian modelling and radiocarbon issues that still need to be addressed (points no. 3 and 7 below are particularly important), which have implications on the interpretation of the data.

1. Agreement indices are still being reported (e.g., model B description in SI) even though incompatible with Outlier_Model analysis. The authors' response on this point is null and not relating to the point (I did not mention the need to report convergence values).

   - All agreement indices have been removed.
   - Convergence values were reported as keeping in line with what (some practitioners) argue to be standard when running outlier models. Indeed, I have been lambasted before for NOT reporting convergence values for my outlier models for other papers. Lots of varying approaches and perspectives, I suppose, ha! But, in this case, they have all been removed in accordance with Reviewer #3's suggestion.

On the sensitivity testing:

2. The authors now note that sensitivity testing has taken place to compare Model A and Model B for the Oconee Valley. Although the authors are right to comment that the difference is not likely to be significant (because both models represent a single, unordered Phase), the results of the analysis are not properly reported; the distribution for the Difference function has not been plotted.

   - Plot has been included as Figure S2.1.

I tried running the code that was provided but, as it is written, it will not work. I am, therefore, unsure as to how the authors tested whether the distributions were comparable and obtained the results that are reported. On this (the reporting, in SI lines 230-239), if the results of the Difference function include zero at 95% CI, then the distributions are comparable. A lot of the text in this paragraph is, therefore, unnecessary. The results plot would be the best way of explaining/demonstrating this.

- Plot has been included as Figure S2.1.
- I tried to copy and paste the code again for Model I (the model used for the sensitivity analysis with the Difference command) directly from the supplemental document and it ran with no trouble. I sent it to others and they were also able to run it. I don't quite know how to proceed on this point. Please let the editors know if it still will not run for you and I will look into getting you the code some other way.

3. On the wiggle matching: It is understood that the authors would find wiggle matching useful given that the calibration curve for this period is non-monotonic (thus offering poor precision). The number of tree rings incorporated into the wiggle matching analysis is quite small (generally n=2), however, and at times not particularly helpful ('PM58 Charcoal' has two dates that are both the same). Moreover, the wiggle matching analysis, as performed in this study, does not address the issue of inbuilt age (see point 7) or date the cultural event of interest (e.g., the time of felling/burning, which would represent a cultural event and not the age of tree-ring formation). Regarding the latter point, the authors could use OxCal to estimate the age of the last event in an effort to get closer to a felling date (place a Date function as last event and use this distribution/prior file in the site model instead).

   - See all comments under comment #7.

4. Define AMS when first mentioned

   - AMS has been defined when first mentioned.

5. Pg. 6 (bottom): Consider adding a 'likely' in the sentence starting with 'Results from Bayesian…', since these are estimates, i.e., 'peaches were likely present, and even widespread'. Also, rather than 'by' cal AD 1625-1640, I would note 'starting at cal AD' (given that a start estimate is being quoted).

   - These edits have been made.

6. Analysis in 'The eventual adoption of peaches was widespread and rapid' section: Beware; the authors are not comparing like with like. The cal AD 1625-1640 range is an estimated start for peach cultivation/harvest in Oconee Valley. The ages quoted for the different regions, I assume, are not modelled estimates for the start of peach cultivation/harvest, but directly dated events. This should be kept in mind as cal AD 1625-1640 for the Oconee Valley is necessarily earlier.

   - Yes, absolutely. We changed a little bit of text here to tie that date range more explicitly to the Oconee Valley (v. the entire Southeast). Otherwise, we hope we are clear that our datasets are variable and include (1) modeled chronologies for regions, (2) modeled chronologies for single sites, (3) singular dates, (4) archaeological materials, and (5) ethnohistoric accounts. The data used for each site and/or region is included in Supplemental Table S1.

7. Pg. 9 (lines 10-20): Given that wood charcoal, was inbuilt age considered in the analysis? Did anthracological analysis take place to mitigate, i.e., were species identified to be short-lived or was bark found? There is a gap of 8 years in the 'Prov 2 LN 31 Charcoal' sample, which might be enough

rings (n=10) for a species ID. Table S2 notes unidentified species and oak wood (although not for Lindsey site), which has a considerable life span. There is no mention of inbuilt age for wood charcoal in either main text or SI. This is particularly important because the site is noted to represent the earliest dated archaeological context containing peaches in the US (and the period the paper deals with is relatively short/young, requiring great precision/accuracy). If there is inbuilt age in the wood, however, the estimate could be erroneously old and the antiquity misrepresented. This is important to address during revision. If inbuilt age cannot be ruled out, then the assertion should not be made with such confidence and the interpretation must remain conservative in its approach. On this subject, Model G in the SI (which seems to correspond to the site discussed) mentions that Charcoal Outlier analysis (this makes the model assume that the charcoal age is erroneously too old, denoting inbuilt age) was applied to non-wiggle-matched dates, but this is not the case. The authors, therefore, seem to consider inbuilt age a certainty in the modelling (or, at least, in the modelling report), but do not mention this in the main text. This is all a bit confusing but the main concern (inbuilt age) remains. Continuing from this, I recommend the site name is added to Table S2. It would have helped when finding the charcoal samples from the Lindsey site there.

- We have made alterations for the Lindsey site model (Model G) following Reviewer 3's comments. Notes on this below:
    - As for wood species, the UID charcoal from Lindsey was indeed ID's in the 90s as *Pinus sp.*. While I have confidence in the analyst, I did not have confidence that the ID's were robust given the small samples of charcoal. These IDs were also never published or subject to any secondary review or confirmation. So, to be cautious, I was more comfortable treating them as UID species.
    - SSimple was originally used on wiggle-matched charcoal dates because, in the case of d_sequences, the event to be dated is meant to be the life of the individual ring. Because the date of a single ring is the event to be dated, we didn't apply a charcoal outlier model to wiggle-matched dates because there is no reason to believe that the ring is older than the event we are trying to date (e.g., the ring itself). All other samples in the model were either maize, hickory, or walnut. There are no non-wiggle matched charcoal dates in the model, so no charcoal outliers were originally used. Though, see below for change to this.
    - Reviewer 3 is correct, though, that we then have no way to account for potential "old wood" problems so to speak, especially without knowing if we have dated an outer ring. We deal with this in two ways:
        - Following Reviewer #3's suggestions, we ran each of the three charcoal samples as three separate, independent models (resulting in 3 independent d_sequences). The "date" command was included as the last event in these sequences. These dates were then incorporated into the overall Lindsey site model as priors extracted from the individual d_sequence models. Once brought into the overall site model, a charcoal outlier was then applied to these three dates (priors) to account for them being UID charcoal.
        - The second benefit of our model is that only 3 of the 7 dates included in the model are on wood charcoal, the other 4 are on short lived species (2 on maize, 1 on hickory, and 1 on walnut). We would certainly be more worried and skeptical, as Reviewer #3 rightly points out, if our entire model was based solely on UID wood charcoal.
    - Results from the new Model G differ by c. 5-10 years from our original model. Date ranges have been updated to reflect this alteration.

8. Pg. 9 (lines around 20): The end and start here likely overlap and are comparable. I suggest testing coevality using a Difference command and report.

- This has been reported in the supplemental material under Model G.

9. In the SI, briefly describe the Outlier_Model (or models) that were used and why (or under what circumstance).

   - Descriptions of outlier models used has been included on page 6 of the supplemental document.

10. In the SI, model C description (also applies to other models): The authors note that no convergence values fall below 99.6, but not sure why this comment is necessary. The models presented are not necessarily complicated and so good convergence should be reached (the model will continue MCMC sampling until convergence is >95%).

    - All convergence values have been removed.

Reviewers' Comments:

Reviewer #3:
Remarks to the Author:
The authors have mostly incorporated my recommendations concerning the chronology and Bayesian modeling. The methodology is more robust now and implications drawn are also better discussed. Regarding their Model G in the SI, inbuilt age for charcoal samples, which has the potential to yield erroneously old ages, needs to be mentioned explicitly. The authors currently seem to dance around this by noting '...to account for some of the uncertainty in not having the outer ring'.

Response to Reviewers

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- With guidance from the editors, wording has been updated on lines 20, 129, and 223 (now 228). We have included wording about charcoal samples in a few new sentences on lines 220-25. Though we still point out that only 3 samples are on charcoal, the rest (4 samples) are on short-lived nuts and seeds. These are better samples to date than charcoal, even charcoal with an outer ring. Our claim of “earliest known dated” does not rest solely (or even primarily) on the charcoal samples that Reviewer #3 is focused on. These charcoal samples are also included in a model alongside these short-lived, high-precision dates, which means that their fit to the model (and to the other samples in the model) are evaluated. In this evaluation, they are shown to be a good fit with the other short-lived species. Another line of evidence that these charcoal samples are not likely to be erroneously old (though the caveat has been added as requested, lines 220-25). The way we have presented our charcoal dates, dealt with them in our models, and interpreted the resulting models are all in line with best/standard practices in Bayesian chronological modeling studies.