

Human climatic and ecological drivers of Holocene fire regimes in North America

Corresponding Author: Dr Alexandra Tsalickis

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

This paper investigates Holocene-scale records of fire activity among North America's ecoregions, and relates patterns in fire activity to human, climate, and ecological factors which may have influenced burning. The authors use charcoal-derived fire records that have been z-scored and binned into 500 year intervals to interpret patterns over the full Holocene, and to compare fire activity with other proxies, including archaeological 14C dates, July mean temperature, solar insolation, Antarctic atmospheric CO₂ data, surface water elevation, surface air temperature, precipitation, burned area fraction, and net primary productivity. They found that July temperature, atmospheric CO₂ concentration, net primary productivity, and humans drove Holocene fire activity across North America. They also found that humans may have impacted fire activity earlier than previously thought – between 4,000 – 6,000 years ago – though records are too coarse to provide any more specific insights during this time.

This is a well-written paper and impressively brings together 140 Holocene-length fire records! I liked the use of ecoregions as a grouping classifier for the fire data and was left wishing that some of these regions had more records. I only had two primary concerns with the manuscript, which I outline below.

1) The use of z-scores is an excellent way to combine and standardize multiple records. The authors outline the process through which they transformed their data (following Power et al., 2010), but they do not present the base period they use. While the selection of base period may only shift the standardization of the data by a small amount, I feel that it is important to list and justify which base period was selected. I suggest that the authors explore how several base periods impact their standardization (e.g., full Holocene, leave out the last 200 years, etc) and include these as a figure in the Supplement. It is common to leave out the last 200 years such that post-industrial activity is not included in the longer-term mean. At the very least, please include what you selected as your base period in the methods.

2) The use of 14C dates as a proxy for human activity is common (summed probability density data) but needs to be corrected for taphonomic bias. The authors allude to the possibility that taphonomic bias may impact the preservation of archaeological records in the North American Desert ecoregion on line 347, but do not acknowledge that time also impacts taphonomy. In Figure 4F, this is demonstrated such that we see more 14C dates in younger bins (generally) and fewer in older bins. I suggest that the authors explore the possibility of taphonomically correcting this data following (Surovell et al., 2009) and (Bluhm and Surovell, 2019). This approach was taken in (Davies et al., 2022) and (Mosher et al., 2024) to explore how population density in southern Africa impacted fire activity. It would also be interesting to see how population data aligns with the ecoregions. Presumably this data is not evenly distributed across North America, and therefore fire in some ecoregions may be more likely to be impacted by humans than in others. While some of these analyses may be beyond the scope of this work (though would be excellent to incorporate if possible!), I would like to see further discussion about the geographic distribution of these dates, the challenges of taphonomic bias and attempts to correct for this, and what the archaeological literature has to say about how humans were using fire and impacting land use during the Holocene.

A few other comments:

Line 49: Define fire behavior, as this can refer to many things. In the context of this paper, do you mean fire activity?

Line 63: In addition to agriculture intensification, post-industrial landscape impacts may be worth noting.

Line 75: Fire intensity is hard to measure in paleofire records. Some methods, like FTIR (e.g., Minatre et al., 2024), are exploring this though it's not clear if you are referring to that here. The way this sentence is written, it seems that you are referring to the CHAR reconstructions, which arguably provide magnitude as our best (imperfect) estimate of intensity. This is a minor point but perhaps could be re-worded for clarity.

Lines 108-111: Please provide more clarity on the selection process for these records. All 140 records spanned the full Holocene? Why weren't any shorter records included? That would certainly weight the data towards the present, but if standardized, may improve interpretations during the mid-late Holocene. Some explanation as to why shorter records were not included would be helpful. Additionally, I didn't understand what was meant by "high quality data resolution consisting of more than three data points". A full Holocene record with 3+ data points is extremely low resolution!

Figure 2: Text on the axes and legend is a bit blurry. I also found it quite interesting that the Marine West Coast Forest and North American Desert showed such similar trends. Table 1 highlights that these trends are not significant, but they're still an unexpected (to me) similarity in geographically disparate regions.

Figure 3: I really like this figure! Could you re-orient it to make the panels wider? I wanted to dive into it more deeply but found the short x-axes to make it a little more challenging to interpret. It would also be great if you could add a similar figure (third panel) by ecoregion, since lat/long will include, in many areas, more than one ecoregion.

Line 286: Could you add the threshold for significance you are using (i.e., $p < 0.05$ or < 0.001)?

Table 2: Spearman's correlation is a great statistic to use for these direction comparisons. The use of asterix for significance works, but I think you could format this table to include both R values and p values for each ecoregion.

Line 290: What is the raw comparison data? Un-transformed charcoal influx?

Line 323: 6 cal yrs BP is 1944 CE? Is this a typo? I believe we see heavy human impacts in the last ~80 years, but since you refer to archaeological radiocarbon dates I am led to believe you may have meant something else here? If not, maybe include the CE date in parentheses to be clear. This might be supposed to be 6,000 cal yrs BP.

I would like to see more discussion on the onset of human impacts on fire between 6,000 and 4,000 years ago. Is this being driven by ecoregion? How do the archaeological dates align with the ecoregions? What does the archaeological literature have to say about regional or continental scale land use at this time, and why fire might be starting to be impacted by people during this period?

This is a very timely and interesting paper. The application of paleofire records to North American ecoregions is novel, and of interest to a broad community of paleoecologists both in North America and worldwide. I look forward to seeing this work published and to future work building off or inspired by this paper.

Bluhm, L.E., Surovell, T.A., 2019. Validation of a global model of taphonomic bias using geologic radiocarbon ages. *Quaternary Research* 91, 325-328.

Davies, B., Power, M.J., Braun, D.R., Douglass, M.J., Mosher, S.G., Quick, L.J., Esteban, I., Sealy, J., Parkington, J., Faith, J.T., 2022. Fire and human management of late Holocene ecosystems in southern Africa. *Quaternary Science Reviews* 289, 107600.

Minatre, K.L., Arienzo, M.M., Moosmüller, H., Maezumi, S.Y., 2024. Charcoal analysis for temperature reconstruction with infrared spectroscopy. *Frontiers in Earth Science* 12.

Mosher, S.G., Power, M.J., Quick, L.J., Haberzettl, T., Kasper, T., Kirsten, K.L., Braun, D.R., Faith, J.T., 2024. Examining the effects of climate change and human impacts on a high-resolution, late Holocene paleofire record from South Africa's winter rainfall zone. *Quaternary Science Advances* 14, 100194.

Surovell, T.A., Byrd Finley, J., Smith, G.M., Brantingham, P.J., Kelly, R., 2009. Correcting temporal frequency distributions for taphonomic bias. *Journal of Archaeological Science* 36, 1715-1724.

Reviewer #3

(Remarks to the Author)

This is an interesting paper that attempts to create a novel database of Holocene-length charcoal records from south of the Laurentide Ice Sheet to analyze fire activity at sub-continental scale over time. Specifically, the authors are interested in evaluating the impacts of human activity, climate, and vegetation productivity on fire over time within generalized ecoregions of North America.

I think this is a commendable effort. The results of this sort of study would be of broad interest to paleoecologists, paleofire scientists, and archaeologists. In its current form, it is not clear that the authors have a good grasp on the existing literature (I make some suggestions below, but this just scratches the surface) and does not seem to acknowledge all of the work done by the Global Paleofire Working Group over the last 15-20 years with similar efforts.

More importantly, I am concerned about some of the methodological choices that are not well justified. For example, the 500-year binning of the charcoal data creates a very coarse-grained record and it is not clear from the manuscript how the values of the bins were generated. Maximum in that window? Mean of samples in the window? Weighted mean by time coverage? Median? 500-year windows for 12,000 years yields only 24 data points for comparison between the variables and only 8 data points for the Middle Holocene. What variability is obscured by this? What artifacts are introduced into the variables by doing this? How sensitive are the results of the analysis to this coarse binning?

In terms of results and interpretation, the manuscript does very little to disentangle the positive correlations (and co-correlations) in some ecoregions and negative relationships in others. While the 500-year binned data sets seems to indicate stronger correlations between a proxy for human population and charcoal over the last 4,000-6,000 years, this relationship also correlates with climate and vegetation variables. Why? What would drive this? Agricultural land use is not a good explanation for the geography of these data points since agriculture was either never practiced or did not appear until the last 1,000 years for many of these locations. What explains this?

I think this is a very worthwhile effort but I feel that the authors need rethink parts of their analysis. They might also benefit from recruiting archaeologists from their well-represented regions to assist with interpretation.

Line 58 (and later): the language is sloppy here. The melting Laurentide ice sheet did not cause warming, it was melting because orbital forcing and greenhouse feedbacks were driving the warming. You use this same phrase elsewhere and should clean that up.

Line 63: these two references (Fowler and Konopik, and Lightfoot and Cuthrell) are not appropriate for the statement declaring increasing human control over natural processes such as agriculture. Native Californians were never agriculturalists (Lightfoot) and the Fowler paper is more about the history of fire use in the South with little reference to Indigenous agricultural history. You would be better off citing one of the major treatises, such as Doolittle 2000 (Cultivated Landscapes of Native North America) or even Barker 2006 (The Agricultural Revolution in Prehistory).

Line 65: similarly, these are not good citations here. Abatzoglou and Williams is a great paper but it is really about analyzing climate change on recent wildfires and Oswald et al. actually asserts that Indigenous people had NO influence on fire activity. There are lots of good alternatives here (e.g., Carter et al. 2021, Roos et al. 2023, Henne et al. 2026, Delcourt and Delcourt 1997, Delcourt and Delcourt 1998, Delcourt et al. 1998). This is where Lightfoot and Cuthrell would make sense.

Line 72: "most direct" should be "most commonly used." Charcoal is certainly not a "direct" proxy since so many variables factor into its production, distribution, and preservation (Scott 2010). But it IS commonly used.

Lines 85-87. I would encourage the authors to be more circumspect with the statements they make in this paragraph. For example, although it is not a charcoal study, Margolis et al. (Margolis et al. 2025) provide an outstanding continental scale analysis of these same drivers using tree-ring records (see also Margolis et al. 2022). So I would add the following qualification to the end of this sentence "...have not been examined through paleofire research at continental and Holocene scales." But even then, this overlooks Global Charcoal Database group's papers (Marlon et al. 2008, Marlon et al. 2013, Marlon et al. 2012, Power et al. 2008).

Line 105-108: rephrase this sentence for clarity so it reads as follows: "We chose to limit the northern spatial extent to south of 52 degrees N as the area north of this included the Laurentide Ice Sheet during the Early and Middle Holocene..."

Line 110: I hope I am misreading this, but it seems to suggest that "high quality data resolution" in the study involves 3 or more data points across 12,000 years. One data point per 4,000 years would not be very useful for comparisons across time or across records. Please explain and clarify.

Line 113: by "wrong temporal scale" do you mean insufficient coverage of the Holocene? Be more specific. And what about exclusion of records that have insufficient data resolution? That was indicated to be important above (even if it is unclear how you have defined it)?

Line 120: why did you bin the records into 500-year intervals? This seems to be very coarsely resolved. How sensitive are your analyses to this arbitrary bin width? And how did you assign values to each bin? Is it the maximum value within that window? The average? The median? A weighted mean (if variable numbers of data points)? This should be explicit.

Line 129-130: how exactly will averaging already heavily smoothed and binned data across ecoregions allow you to identify human influences that are spatially and temporally heterogeneous and at much finer spatial scales? Roos (2020) articulated serious methodological problems with spatial averaging for evaluating the influence of Indigenous cultural burning in charcoal and pollen records. How would you address this critique?

Line 147: I realize that you are binning the archaeological dates by 500 years to make the units of fire and humans comparable for analysis but what is the impact of de-resolving the human record this way? How sensitive is your analysis to this?

Line 149-151: I appreciate that you are trying to do this comparison systematically but I am very skeptical that you can get at causal inference here. Couldn't humans and fire both respond to climate in the same way creating a correlation without

causation (like the classic ice cream sales and drownings correlation).

Line 158-159: the analysis is similarly problematic here, even for Holocene/continental scales, especially with the unevenness of the geographic distribution of records (lines 192-194 and Figure 1).

Lines 213-224: I wonder how this compares with the observations of the Global Paleofire Charcoal papers. I have certainly observed similar patterns in the records I have worked on too.

Table 2 and the accompanying results section: Can you explain any of the geographic variabilities in correlations better? Why would Marine West Coast Forest have weak negative relationships to temperature, productivity, and humans? Why do so many regions have significant correlations to climate AND people?

Line 326: I think you mean the 2011 Bowman paper on human dimensions (Bowman et al. 2011).

Line 355-357: you should be much more circumspect here. Your analysis does not reveal that human presence began impacting fire activity, just that the proxy for population and fire correlate from that point onward. This correlation is interesting but lacks specific mechanisms because of the time/space averaging.

Lines 360-361: again, there are better citations for this. See my suggestions for line 65.

Line 431-432: the causality mixup with temperature and the ice sheet again.

Bowman, D. M. J. S., J. Balch, P. Artaxo, W. J. Bond, M. A. Cochrane, C. M. D'Antonio, R. DeFries, F. H. Johnston, J. E. Keeley, M. A. Krawchuk, C. A. Kull, M. Mack, M. A. Moritz, S. Pyne, C. I. Roos, A. C. Scott, N. S. Sodhi & T. W. Swetnam (2011) The human dimension of fire regimes on Earth. *Journal of Biogeography*, 38, 2223-2236.

Carter, V. A., A. Brunelle, M. J. Power, R. J. DeRose, M. F. Bekker, I. Hart, S. Brewer, J. Spangler, E. Robinson, M. Abbott, S. Y. Maezumi & B. F. Coddling (2021) Legacies of Indigenous land use shaped past wildfire regimes in the Basin-Plateau Region, USA. *Communications Earth & Environment*, 2, 72.

Delcourt, H. R. & P. A. Delcourt (1997) Pre-Columbian Native American Use of Fire on Southern Appalachian Landscapes. *Conservation Biology*, 11, 1010-1014.

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Henne, P. D., S. Stolze, N. M. Kehrwald, B. Brice & C. D. Allen (2026) Millennial-scale climatic and cultural impacts on vegetation and fire at the southern edge of the Rocky Mountains, USA. *Quaternary Science Reviews*, 376, 109821.

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Margolis, E. Q., C. H. Guiterman, R. Chavardès, J. Coop, K. Copes-Gerbitz, D. Dawe, D. A. Falk, J. Johnston, E. R. Larson, H. Li, J. M. Marschall, C. Naficy, A. T. Naito, M.-A. Parisien, S. A. Parks, J. Portier, H. M. Poulos, K. M. Robertson, J. Speer, M. C. Stambaugh, T. W. Swetnam, A. J. Tepley, I. Thapa, C. D. Allen, Y. Bergeron, L. Daniels, P. Z. Fulé, D. Gervais, M. P. Girardin, G. L. Harley, J. Harvey, K. Hoffman, J. Huffman, M. D. Hurteau, L. B. Johnson, C. W. Lafon, M. Lopez, R. S. Maxwell, J. Meunier, M. North, M. T. Rother, M. Schmidt, R. Sherriff, L. A. Stachowiak, A. Taylor, E. J. Taylor, V. Trouet, M. Villarreal, L. L. Yocom, K. Arabas, A. Arizpe, D. Arseneault, A. Azpeleta Tarancón, C. H. Baisan, E. Bigio, F. Biondi, G. Cahalan, A. C. Caprio, J. Cerano-Paredes, B. M. Collins, D. C. Dey, I. Drobyshev, C. Farris, M. Fenwick, W. Flatley, M. L. Floyd, Z. e. Gedalof, A. Holz, L. Howard, D. W. Huffman, J. M. Iniguez, K. F. Kipfmueller, S. G. Kitchen, K. Lombardo, D. McKenzie, A. Merschel, K. Metlen, J. Minor, C. D. O'Connor, L. Platt, W. J. Platt, T. Saladyga, A. B. Stan, S. L. Stephens, C. Suthaimer, R. Touchan & P. J. Weisberg (2022) The North American Fire Scar Network. *Ecosphere*, 13, e4159.

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Marlon, J. R., P. J. Bartlein, A.-L. Daniau, S. P. Harrison, S. Y. Maezumi, M. J. Power, W. Tinner & B. Vannière (2013) Global biomass burning: a synthesis and review of Holocene paleofire records and their controls. *Quaternary Science Reviews*, 65, 5-25.

Marlon, J. R., P. J. Bartlein, D. G. Gavin, C. J. Long, R. S. Anderson, C. E. Briles, K. J. Brown, D. Colombaroli, D. J. Hallett, M. J. Power, E. A. Scharf & M. K. Walsh (2012) Long-term perspective on wildfires in the western USA. *Proceedings of the*

National Academy of Sciences, 109, E535-E543.

Power, M. J., J. Marlon, N. Ortiz, P. Bartlein, S. Harrison, F. Mayle, A. Ballouche, R. H. W. Bradshaw, C. Carcaillet & C. Cordova (2008) Changes in fire regimes since the Last Glacial Maximum: an assessment based on a global synthesis and analysis of charcoal data. *Climate Dynamics*, 30, 887-907.

Roos, C. I. (2020) Scale in the study of Indigenous burning. *Nature Sustainability*, 3, 898-899.

Roos, C. I., N. C. Laluk, W. Reitze & O. K. Davis (2023) Stratigraphic evidence for culturally variable Indigenous fire regimes in ponderosa pine forests of the Mogollon Rim area, east-central Arizona. *Quaternary Research*, 113, 69-86.

Scott, A. C. (2010) Charcoal recognition, taphonomy and uses in palaeoenvironmental analysis. *Palaeogeography, Palaeoclimatology, Palaeoecology*, 291, 11-39.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

Nature Communications Earth and Environment

Tsalickis et al. Human, climatic, and ecological drivers of Holocene fire regimes in North America Review #2

Thank you for thoughtfully addressing my comments and concerns outlined in my first review. I am pleased with the additions and edits that the authors have made to the manuscript and am in support of its publication. I have listed a few minor edits below:

Line 18: "reconstructed" is the wrong tense.

Line 86: Could this be written as "continental-to-global" and "regional-to-local" spatial scales?

Lines 87 and 90: Sub-continental is written once with a hyphen (line 87) and once without (line 90). There are some other inconsistencies throughout the text as well but the authors mostly use "subcontinental".

Line 139: Is this supposed to be Davies et al. (2022) or another Davies paper? In either case, a reference to Davies is missing in the references section!

Line 204: Missing a closing ")" for the i.e. list.

Line 242: I think you can just say "yrs" here rather than "kyr" because you already have the thousands of years listed.

Figure 2: What is the red line for?

Figure 3: Another red line? This might just be an artifact of the PDF but be sure to describe what the lines are for if they carry meaning.

Figure 3C. Y-axis needs a label.

469: Demonstrates?

498: Between 6,000 cal yr BP and(?) present instead of "to present"?

Figure 6 A-C: Double check that the scale bars are accurate. 6A ranges from 0-10 which 6B and 6C range from 0-1.

549: Extra comma after "Mutch".

Thanks again for an interesting paper and well-designed study.

Reviewer #3

(Remarks to the Author)

I would like to thank the author for considering and responding to many of my concerns.

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In revision, please reach the follow[ing] editorial thresholds:

We thank the editors for the opportunity to revise our manuscript. We have provided detailed point-by-point responses to the editor and reviewer comments below. Where possible, we have tried to summarize our changes in response to the editor comments, while providing more in-depth responses to the relevant review comments later on in the document.

1. Please justify or refine the 500-year binning and the inclusion of low-resolution records, providing a sensitivity analysis to demonstrate that the observed trends are not artefacts of data smoothing.

In our revision we have both justified and refined our choice of temporal binning interval. Namely, we have conducted a sensitivity analysis of varying bin sizes of 125, 250, and 500-year intervals to determine the extent that binned z-scores deviate from original unbinned z-scores. We found that only minor differences occurred among each binned interval which indicates that the dataset is relatively robust to these different temporal groupings. The 125-year bin retained slightly more variance in z-scores, but the improvement was minimal. We have included this additional sensitivity analysis as a supplementary figure (Supplemental Figure 1 and Supplementary Table 1) and have described this analysis in our revised manuscript. Other studies including Davies et al., 2023, Marlon et al., 2013, and Power et al., 2008 have also used bin sizes of 500 years or a combination of 500 and 250 years. As a result, in our revision, we have refined and better justified our selected 500-year bins, and clarified that we chose these bin sizes to highlight temporally broader fire trends while minimizing higher frequency noise.

2. The human population proxy should be corrected for taphonomic bias using established archaeological methods to ensure that the late-Holocene increase in human-fire correlation is not a result of preservation bias.

We have corrected the human population proxy for taphonomic bias following methods by Surovell et al., (2009) and Davies et al., (2022), as suggested by the reviewers. We have removed the previously included uncorrected data from the manuscript, updated figures accordingly, and included new text describing our methodology.

3. The discussion should be substantially expanded to move beyond simple correlation, specifically addressing how regional cultural practices or climate-human feedback explain the diverging trends observed across different ecoregions.

We have incorporated discussion of differences in climate-human feedbacks that may be responsible for the differences in fire activity that are observed across different ecoregions. We include explanation of how various ecoregions have different human land-use practices and cultural fire regimes which could be responsible for the variation in fire activity (Lines 388-400). However, we would also like to point out that we are

purposefully cautious to not overinterpret our data as it cannot completely resolve these relationships.

REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

This paper investigates Holocene-scale records of fire activity among North America's ecoregions, and relates patterns in fire activity to human, climate, and ecological factors which may have influenced burning. The authors use charcoal-derived fire records that have been z-scored and binned into 500 year intervals to interpret patterns over the full Holocene, and to compare fire activity with other proxies, including archaeological ^{14}C dates, July mean temperature, solar insolation, Antarctic atmospheric CO_2 data, surface water elevation, surface air temperature, precipitation, burned area fraction, and net primary productivity. They found that July temperature, atmospheric CO_2 concentration, net primary productivity, and humans drove Holocene fire activity across North America. They also found that humans may have impacted fire activity earlier than previously thought – between 4,000 – 6,000 years ago – though records are too coarse to provide any more specific insights during this time.

This is a well-written paper and impressively brings together 140 Holocene-length fire records! I liked the use of ecoregions as a grouping classifier for the fire data and was left wishing that some of these regions had more records. I only had two primary concerns with the manuscript, which I outline below.

Response: Thank you for this positive comment. We are happy to hear the Reviewer thinks this is a well-written paper. We have revised the paper to address the Reviewer's primary concerns.

1) The use of z-scores is an excellent way to combine and standardize multiple records. The authors outline the process through which they transformed their data (following Power et al., 2010), but they do not present the base period they use. While the selection of base period may only shift the standardization of the data by a small amount, I feel that it is important to list and justify which base period was selected. I suggest that the authors explore how several base periods impact their standardization (e.g., full Holocene, leave out the last 200 years, etc) and include these as a figure in the Supplement. It is common to leave out the last 200 years such that post-industrial activity is not included in the longer-term mean. At the very least, please include what you selected as your base period in the methods.

Response: Thank you for providing this suggestion. We agree that detailing what base period we used is important to include. We have added text explaining that that we used the full Holocene as our base period for z-score calculations and a rationale for including the modern era in this base period in our analyses (Lines 127-131).

2) The use of ¹⁴C dates as a proxy for human activity is common (summed probability density data) but needs to be corrected for taphonomic bias. The authors allude to the possibility that taphonomic bias may impact the preservation of archaeological records in the North American Desert ecoregion on line 347, but do not acknowledge that time also impacts taphonomy. In Figure 4F, this is demonstrated such that we see more ¹⁴C dates in younger bins (generally) and fewer in older bins. I suggest that the authors explore the possibility of taphonomically correcting this data following (Surovell et al., 2009) and (Bluhm and Surovell, 2019). This approach was taken in (Davies et al., 2022) and (Mosher et al., 2024) to explore how population density in southern Africa impacted fire activity. It would also be interesting to see how population data aligns with the ecoregions. Presumably this data is not evenly distributed across North America, and therefore fire in some ecoregions may be more likely to be impacted by humans than in others. While some of these analyses may be beyond the scope of this work (though would be excellent to incorporate if possible!), I would like to see further discussion about the geographic distribution of these dates, the challenges of taphonomic bias and attempts to correct for this, and what the archaeological literature has to say about how humans were using fire and impacting land use during the Holocene.

Response: We thank the Reviewer for suggesting to taphonomically correct the radiocarbon data and for providing Davies et al. (2022) as a reference to use their methods as a guide. After reviewing the methodology of Bluhm and Surovell (2018), we realized that taphonomic correction must be applied to open sites only. The radiocarbon dataset we are using consists of over 98,000 radiocarbon entries without enough information for each entry in the metadata to determine whether these radiocarbon dates are sourced from closed or open sites. As a result, we applied the taphonomic correction equation to all sites included in the archaeological database provided by Kelly et al. (2022) (See supplemental figure 3). We realize this is not the ideal method and we have included text in the methods section to acknowledge this (Lines 171-174). We also explored the geographic distribution of the radiocarbon data and incorporated this into the discussion (Lines 403-413) along with a new figure (Supplemental Figure 4). We have additionally acknowledged the role of taphonomic bias in the discussion (Lines 379-381). We have added text explaining how humans were using fire and impacting land use during the Holocene (Lines 393-400).

A few other comments:

Line 49: Define fire behavior, as this can refer to many things. In the context of this paper, do you mean fire activity?

Response: Thank you for the recommended specification. We changed the term to fire activity (Line 49).

Line 63: In addition to agriculture intensification, post-industrial landscape impacts may be worth noting.

Response: This is a great suggestion especially as we include the modern era in our base period of z-score calculations. We have included post-industrial landscape impacts to this sentence (Line 66).

Line 75: Fire intensity is hard to measure in paleofire records. Some methods, like FTIR (e.g., Minatre et al., 2024), are exploring this though it's not clear if you are referring to that here. The way this sentence is written, it seems that you are referring to the CHAR reconstructions, which arguably provide magnitude as our best (imperfect) estimate of intensity. This is a minor point but perhaps could be re-worded for clarity.

Response: Thank you for pointing this out. We have changed the wording to fire magnitude rather than fire intensity (Line 78).

Lines 108-111: Please provide more clarity on the selection process for these records. All 140 records spanned the full Holocene? Why weren't any shorter records included? That would certainly weight the data towards the present, but if standardized, may improve interpretations during the mid-late Holocene. Some explanation as to why shorter records were not included would be helpful. Additionally, I didn't understand what was meant by "high quality data resolution consisting of more than three data points". A full Holocene record with 3+ data points is extremely low resolution!

Response: We realize we explained our record selection process with a lack of clarity and have added details to fix this. We changed the first criteria to "being constrained within the Holocene period" as shorter records were included in the analysis. We have also clarified the second criterion by altering to "high quality data resolution consisting of seven or more points" as the record with the lowest resolution was comprised of seven data points (Lines 115-118) with a caveat of three data points in the southeastern region of the United States as previous paleofire compilation of the southeastern United States has verified that charcoal records in this region are poor quality (Tsallikis et al. 2025).

Reference:

Tsallikis A, Vachula RS, Karmalkar AV, Balascio NL and Waters MN (2025) Distinct latitudinal gradients define Holocene fire trends across the southeastern USA. *Quaternary Science Reviews* 350: 109161: doi:10.1016/j.quascirev.2024.109161.

Figure 2: Text on the axes and legend is a bit blurry. I also found it quite interesting that the Marine West Coast Forest and North American Desert showed such similar trends. Table 1 highlights that these trends are not significant, but they're still an unexpected (to me) similarity in geographically disparate regions.

Response: We have updated Figure 2 so the text is no longer blurry. We have added text in the discussion providing more explanation for varying correlations between fire activity among ecoregions and either human, climatic, or vegetative controls (Lines 338-347).

Figure 3: I really like this figure! Could you re-orient it to make the panels wider? I wanted to dive into it more deeply but found the short x-axes to make it a little more challenging to interpret. It would also be great if you could add a similar figure (third panel) by ecoregion, since lat/long will include, in many areas, more than one ecoregion.

Response: Thank you for this suggestion. We have re-oriented this figure to make the panels easier to read. We have also included the addition of a third panel by ecoregion (see Figure 3).

Line 286: Could you add the threshold for significance you are using (i.e., $p < 0.05$ or < 0.001)?

Response: Yes, thank you for catching this. We have added the significance threshold as $p < 0.05$ (Line 314-315).

Table 2: Spearman's correlation is a great statistic to use for these direction comparisons. The use of asterix for significance works, but I think you could format this table to include both R values and p values for each ecoregion.

Response: We have added p values for each ecoregion to this table (see Table 2).

Line 290: What is the raw comparison data? Un-transformed charcoal influx?

Response: We have altered the text to explain that by raw comparison data we meant the unbinned data (Line 320).

Line 323: 6 cal yrs BP is 1944 CE? Is this a typo? I believe we see heavy human impacts in the last ~80 years, but since you refer to archaeological radiocarbon dates I am led to believe you may have meant something else here? If not, maybe include the CE date in parentheses to be clear. This might be supposed to be 6,000 cal yrs BP.

Response: Yes, this is a typo. It should read 6,500 cal yr BP. We have made the correction (Line 368)

I would like to see more discussion on the onset of human impacts on fire between 6,000 and 4,000 years ago. Is this being driven by ecoregion? How do the archaeological dates align with the ecoregions? What does the archaeological literature have to say about regional or continental scale land use at this time, and why fire might be starting to be impacted by people during this period?

Response: We have added text in the discussion describing what humans were using fire for and other studies that have found increases in fire activity around 5,000 cal yr BP in the United States (Lines 430-439). We also included discussion of how the archaeological dates align with the ecoregions and potential explanations for the observed spatial bias (Lines 403-413).

This is a very timely and interesting paper. The application of paleofire records to North American ecoregions is novel, and of interest to a broad community of paleoecologists both in North America and worldwide. I look forward to seeing this work published and to future work building off or inspired by this paper.

Response: We are glad the Reviewer finds this study to be timely and interesting and a novel contribution that is of broad interest to paleoecologists in North America and worldwide. We hope that after incorporating the suggested revisions by both Reviewers, this paper will improve in quality and be considered for publication.

Bluhm, L.E., Surovell, T.A., 2019. Validation of a global model of taphonomic bias using geologic radiocarbon ages. *Quaternary Research* 91, 325-328.

Davies, B., Power, M.J., Braun, D.R., Douglass, M.J., Mosher, S.G., Quick, L.J., Esteban, I., Sealy, J., Parkington, J., Faith, J.T., 2022. Fire and human management of late Holocene ecosystems in southern Africa. *Quaternary Science Reviews* 289, 107600.

Minatre, K.L., Arienzo, M.M., Moosmüller, H., Maezumi, S.Y., 2024. Charcoal analysis for temperature reconstruction with infrared spectroscopy. *Frontiers in Earth Science* 12.

Mosher, S.G., Power, M.J., Quick, L.J., Haberzettl, T., Kasper, T., Kirsten, K.L., Braun, D.R., Faith, J.T., 2024. Examining the effects of climate change and human impacts on a high-resolution, late Holocene paleofire record from South Africa's winter rainfall zone. *Quaternary Science Advances* 14, 100194.

Surovell, T.A., Byrd Finley, J., Smith, G.M., Brantingham, P.J., Kelly, R., 2009. Correcting temporal frequency distributions for taphonomic bias. *Journal of Archaeological Science* 36, 1715-1724.

Reviewer #3 (Remarks to the Author):

This is an interesting paper that attempts to create a novel database of Holocene-length charcoal records from south of the Laurentide Ice Sheet to analyze fire activity at sub-continental scale over time. Specifically, the authors are interested in evaluating the impacts of human activity, climate, and vegetation productivity on fire over time within generalized ecoregions of North America.

Response: We are happy that the Reviewer finds this to be an interesting paper.

I think this is a commendable effort. The results of this sort of study would be of broad interest to paleoecologists, paleofire scientists, and archaeologists. In its current form, it is not clear that the authors have a good grasp on the existing literature (I make some suggestions below, but this just scratches the surface) and does not seem to acknowledge all of the work done by the Global Paleofire Working Group over the last 15-20 years with similar efforts.

Response: We thank the Reviewer for acknowledging that the work we have done is a commendable effort. We also agree that the results of this study would be of broad interest to a mixture of paleoecologists, paleofire scientists, and archaeologists. We agree that the first version of this paper required improvement. The Reviewer is correct, that we lacked acknowledgement of the work done by the Global Paleofire Working Group and as a result we have added references to these studies throughout the introduction (e.g., Lines 90-91), methods (e.g., Line 136-137), and discussion sections (Lines 336-338). We hope that incorporating all of the suggested revisions based on the Reviewer comments has greatly improved the quality of this study.

More importantly, I am concerned about some of the methodological choices that are not well justified. For example, the 500-year binning of the charcoal data creates a very coarse-grained record and it is not clear from the manuscript how the values of the bins were generated. Maximum in that window? Mean of samples in the window? Weighted mean by time coverage? Median? 500-year windows for 12,000 years yields only 24 data points for comparison between the variables and only 8 data points for the Middle Holocene. What variability is obscured by this? What artifacts are introduced into the variables by doing this? How sensitive are the results of the analysis to this coarse binning?

Response: We agree that a lack of clarity in our 500-year binning descriptions has caused confusion about how the values of these bins were generated. We have now clarified that we used the average z-score values of each 500-year bin (Lines 131-132). We have additionally justified our rationale for selecting 500-year bins in a detailed

comment below by employing a sensitivity analysis (see Supplementary Figure 1 and Supplementary Table 1). We also acknowledge that other studies conducting charcoal compilation research similarly used 500 year bins (Davies et al., 2023; Marlon et al., 2013; Power et al., 2008).

In terms of results and interpretation, the manuscript does very little to disentangle the positive correlations (and co-correlations) in some ecoregions and negative relationships in others. While the 500-year binned data sets seems to indicate stronger correlations between a proxy for human population and charcoal over the last 4,000-6,000 years, this relationship also correlates with climate and vegetation variables. Why? What would drive this? Agricultural land use is not a good explanation for the geography of these data points since agriculture was either never practiced or did not appear until the last 1,000 years for many of these locations. What explains this?

Response: We have added text to the discussion explaining the relationships between increased human population size from the late Pleistocene to the mid-Holocene (Lines 432-436), why humans used fire during this time and that human fire use varied by region depending on cultural fire regime practices (Lines 393-400), and the effect fire would have on ecosystems (Lines 432-439). There is a great body of literature supporting agricultural use was highly practiced in the mid Holocene across many regions of North America and existed much earlier than the last 1,000 years (see Delcourt and Delcourt 2004; Walsh et al., 2015; Richardson et al., 2001 as examples).

References:

Delcourt, P.A. and Delcourt, H.R., 2004. *Prehistoric Native Americans and ecological change: human ecosystems in eastern North America since the Pleistocene*. Cambridge University Press.

Walsh, M.K., Marlon, J.R., Goring, S.J., Brown, K.J. and Gavin, D.G., 2015. A regional perspective on Holocene fire–climate–human interactions in the Pacific Northwest of North America. *Annals of the Association of American Geographers*, 105(6), pp.1135-1157.

Richerson, P.J., Boyd, R. and Bettinger, R.L., 2001. Was agriculture impossible during the Pleistocene but mandatory during the Holocene? A climate change hypothesis. *American antiquity*, 66(3), pp.387-411.

I think this is a very worthwhile effort but I feel that the authors need rethink parts of their analysis. They might also benefit from recruiting archaeologists from their well-represented regions to assist with interpretation.

Response: We are glad that the Reviewer thinks that this is a worthwhile effort. While we did not recruit an archaeologist, we did incorporate several archaeology based papers to help interpret some of the observed differences in fire activity among regions as well as the increase in fire activity that occurred in the mid Holocene. We would also note that the paleofire data that we focus upon is distinct from archeological perspectives in terms of approach, data type, temporal resolution, etc. Though it may be tempting to draw connections between our data and archeology, we must also be forthright about the uncertainties related to paleofire proxies (e.g., spatial fidelity and scope, fire regime variables, inability to differentiate natural vs. human-caused ignitions). As such, we were hesitant to overstep the bounds of the data's ability to reconstruct fire histories.

Line 58 (and later): the language is sloppy here. The melting Laurentide ice sheet did not cause warming, it was melting because orbital forcing and greenhouse feedbacks were driving the warming. You use this same phrase elsewhere and should clean that up.

Response: We agree that the language could be improved here. We have included that changes in Earth's orbital forcings and greenhouse gas feedbacks drove deglaciation of the Laurentide ice sheet which resulted in sustained warming temperatures (Lines 59-60).

Line 63: these two references (Fowler and Konopik, and Lightfoot and Cuthrell) are not appropriate for the statement declaring increasing human control over natural processes such as agriculture. Native Californians were never agriculturalists (Lightfoot) and the Fowler paper is more about the history of fire use in the South with little reference to Indigenous agricultural history. You would be better off citing one of the major treatises, such as Doolittle 2000 (Cultivated Landscapes of Native North America) or even Barker 2006 (The Agricultural Revolution in Prehistory).

Response: Thank you for this suggestion. We have replaced the citations (Fowler and Konopik, and Lightfoot and Cuthrell) with the recommended Doolittle 2000 and Barker 2006 (Lines 65-66).

Line 65: similarly, these are not good citations here. Abatzoglou and Williams is a great paper but it is really about analyzing climate change on recent wildfires and Oswald et al. actually asserts that Indigenous people had NO influence on fire activity. There are lots of good alternatives here (e.g., Carter et al. 2021, Roos et al. 2023, Henne et al. 2026, Delcourt and Delcourt 1997, Delcourt and Delcourt 1998, Delcourt et al. 1998). This is where Lightfoot and Cuthrell would make sense.

Response: We have changed the citations to the suggested (Carter et al. 2021, Roos et al. 2023, Henne et al. 2026, Delcourt and Delcourt 1997, Delcourt and Delcourt 1998,

Lightfoot and Cuthrell 2015) (Lines 67-69).

Line 72: “most direct” should be “most commonly used.” Charcoal is certainly not a “direct” proxy since so many variables factor into its production, distribution, and preservation (Scott 2010). But it IS commonly used.

Response: We have made the corrections (Line 76).

Lines 85-87. I would encourage the authors to be more circumspect with the statements they make in this paragraph. For example, although it is not a charcoal study, Margolis et al. (Margolis et al. 2025) provide an outstanding continental scale analysis of these same drivers using tree-ring records (see also Margolis et al. 2022). So I would add the following qualification to the end of this sentence “...have not been examined through paleofire research at continental and Holocene scales.” But even then, this overlooks Global Charcoal Database group’s papers (Marlon et al. 2008, Marlon et al. 2013, Marlon et al. 2012, Power et al. 2008).

Response: We agree and have added the suggested text to the end of the sentence (Line 90). We somewhat disagree that our work overlooks the Global Charcoal Database group’s papers as we cite and acknowledge many of their papers in our introduction, methods, and discussion including Marlon et al., 2013; Power et al., 2008; Aleman et al., 2018; Marlon et al. 2008. However, we do include the recommended papers by the Global Charcoal Database group in our list of references after the inclusion of the suggested text (Lines 90-91).

Line 105-108: rephrase this sentence for clarity so it reads as follows: “We chose to limit the northern spatial extent to south of 52 degrees N as the area north of this included the Laurentide Ice Sheet during the Early and Middle Holocene...”

Response: Thank you for providing this suggestion. We have rephrased the sentence (Lines 109-111).

Line 110: I hope I am misreading this, but it seems to suggest that “high quality data resolution” in the study involves 3 or more data points across 12,000 years. One data point per 4,000 years would not be very useful for comparisons across time or across records. Please explain and clarify.

Response: We have addressed this point in response to Reviewer 2’s comment as well. We have clarified the second criterion by altering to “high quality data resolution consisting of seven or more points” as the record with the lowest resolution was comprised of seven data points (Lines 115-118) with a caveat of three data points in the southeastern region of the United states as previous paleofire compilation of the southeastern United States has verified that charcoal records in this region are of poor quality (Tsalickis et al. 2025).

Reference:

Tsalickis A, Vachula RS, Karmalkar AV, Balascio NL and Waters MN (2025) Distinct latitudinal gradients define Holocene fire trends across the southeastern USA. *Quaternary Science Reviews* 350: 109161: doi:10.1016/j.quascirev.2024.109161.

Line 113: by “wrong temporal scale” do you mean insufficient coverage of the Holocene? Be more specific. And what about exclusion of records that have insufficient data resolution? That was indicated to be important above (even if it is unclear how you have defined it)?

Response: We have changed the text to more specifically says “temporal scales that were not inclusive of the Holocene period” (Lines 120-121). We have also clarified our constraints of sufficient data resolution that was used in this study (Lines 114-121).

Line 120: why did you bin the records into 500-year intervals? This seems to be very coarsely resolved. How sensitive are your analyses to this arbitrary bin width? And how did you assign values to each bin? Is it the maximum value within that window? The average? The median? A weighted mean (if variable numbers of data points)? This should be explicit.

Response: We chose to use 500-year intervals for binning based on other paleofire compilation papers that also used 500-year bins such as Power et al. 2007 and Davies et al. 2023. However, we realize a quantitative comparison between different bin sizes is the best way to choose a bin interval. To address this, we have conducted a sensitivity analysis comparing bin sizes of 125-, 250-, and 500-year intervals (See Supplemental Figure 1). We used a root mean square error (RMSE) combined with R^2 values to determine how far binned z-scores deviate from original unbinned z-scores at the varying 125-, 250-, and 500-year intervals (Supplemental Table 1). Only minor differences occurred among each binned interval which indicates that the dataset is relatively robust to these different temporal groupings. The 125-year bin retained slightly more variance in z-scores, but the improvement was minimal. As a result, we selected 500-year bins to highlight temporally broader, multi-millennial fire trends while minimizing the high frequency noise (Lines 132-138). Additionally, we have added that the average of the z-score values was used to assign values to 500-year bins (Line 131-132).

References:

Davies, M.A., McLaughlin, J.W., Packalen, M.S. and Finkelstein, S.A., 2023. Using Holocene paleo-fire records to estimate carbon stock vulnerabilities in Hudson Bay Lowlands peatlands. *Facets*, 8(1), pp.1-26.

Power, M.J., Marlon, J., Ortiz, N., Bartlein, P.J., Harrison, S.P., Mayle, F.E., Ballouche, A., Bradshaw, R.H., Carcaillet, C., Cordova, C. and Mooney, S., 2008. Changes in fire regimes since the Last Glacial Maximum: an assessment based on a global synthesis and analysis of charcoal data. *Climate dynamics*, 30(7), pp.887-907.

Line 129-130: how exactly will averaging already heavily smoothed and binned data across ecoregions allow you to identify human influences that are spatially and temporally heterogeneous and at much finer spatial scales? Roos (2020) articulated serious methodological problems with spatial averaging for evaluating the influence of Indigenous cultural burning in charcoal and pollen records. How would you address this critique?

Response: It is an important point to bring up averaging binned data, however the spatial scales and research goals for this study and the study in Roos (2020) are very different. In Roos (2020), the commentary is about characterizing indigenous fire management, which is not the goal of our study. Additionally, Roos (2020) is referring to a smaller regional spatial area as opposed to the data set used in this project, which is at a spatial scale that is too large to characterize specific indigenous fire use and regimes. Instead, we are attempting to determine if increased human presence coincides with periods of increased fire activity. As a result, compared to Roos (2020), the research question is much broader in the context of this study.

Line 147: I realize that you are binning the archaeological dates by 500 years to make the units of fire and humans comparable for analysis but what is the impact of de-resolving the human record this way? How sensitive is your analysis to this?

Response: Other studies have also binned their temporal frequency distribution datasets by 500-year intervals such as Ugan and Byers, (2007) and Surovell et al., (2009). We have added text in the methods stating we used 500-year bin sizes following these studies (Lines 165-166).

References:

Surovell, T.A., Finley, J.B., Smith, G.M., Brantingham, P.J. and Kelly, R., 2009. Correcting temporal frequency distributions for taphonomic bias. *Journal of archaeological Science*, 36(8), pp.1715-1724.

Ugan, A. and Byers, D., 2007. Geographic and temporal trends in proboscidean and human radiocarbon histories during the late Pleistocene. *Quaternary Science Reviews*, 26(25-28), pp.3058-3080.

Line 149-151: I appreciate that you are trying to do this comparison systematically but I am very skeptical that you can get at causal inference here. Couldn't humans and fire

both respond to climate in the same way creating a correlation without causation (like the classic ice cream sales and drownings correlation).

Response: We completely agree that our analysis cannot establish causation, and we do not intend to claim causal inference from the correlations presented. Our goal is to evaluate whether the archaeological proxy for human presence covaries with fire activity, not to argue that humans were the singular or deterministic driver of those changes. To address your concern, we added a small paragraph to address this limitation at the end of the anthropogenic fire signals section of the discussion (Lines 440-446).

Line 158-159: the analysis is similarly problematic here, even for Holocene/continental scales, especially with the unevenness of the geographic distribution of records (lines 192-194 and Figure 1).

Response: We agree and we do address the unevenness of the geographic distribution of charcoal records in the first paragraph of our discussion (Lines 348-359). We also added the following text at the end of the first section of the discussion to acknowledge this caveat: "Because charcoal records are unevenly distributed across North America (Figure 1), subcontinental trends should be interpreted cautiously, as some regions are under-represented and may weaken spatial completeness of our analysis." (Lines 363-365).

Lines 213-224: I wonder how this compares with the observations of the Global Paleofire Charcoal papers. I have certainly observed similar patterns in the records I have worked on too.

Response: We have added comparison of the spatiotemporal variances of fire activity observed from our study to observations from the Global Paleofire Working Group (Lines 336-338). Mainly, we find that the observations are similar to both Eastern and Western North America based on results from (Marlon et al., 2012, 2013, and Power et al., 2008).

Table 2 and the accompanying results section: Can you explain any of the geographic variabilities in correlations better? Why would Marine West Coast Forest have weak negative relationships to temperature, productivity, and humans? Why do so many regions have significant correlations to climate AND people?

Response: We touch on the results from Table 2 in the discussion but do agree that it can be explained further. We have added text in the discussion explaining these variations (Lines 338-347 and Lines 393-400). We posit that the same climatic and vegetative controls that define ecosystems cause the variations in ecosystems to have correlations with climate and people versus negative relationships with temperature, productivity, and humans.

Line 326: I think you mean the 2011 Bowman paper on human dimensions (Bowman et al. 2011).

Response: We have made the correction (Line 371).

Line 355-357: you should be much more circumspect here. Your analysis does not reveal that human presence began impacting fire activity, just that the proxy for population and fire correlate from that point onward. This correlation is interesting but lacks specific mechanisms because of the time/space averaging.

Response: We have added discussion of human populations increasing from the late Pleistocene into the mid-Holocene based on archaeological evidence which shifted human populations impacts from community level to landscape level that effected entire ecosystems. Other studies additionally provide evidence of increased fire activity around 5,000 years ago in other regions of the United States such as the Pacific Northwest (Lines 430-439). We use this as evidence to support how increased human populations could cause a subsequent increase in fire activity during the mid-Holocene. However, we do agree that the observed correlations do not establish causal relationships and have included a paragraph describing this caveat (Lines 440-446).

Lines 360-361: again, there are better citations for this. See my suggestions for line 65.

Response: Thank you for pointing this out. We have changed the citations to the ones suggested earlier by the Reviewer including (Carter et al., 2021; Delcourt and Delcourt, 1997, 1998; Henne et al., 2026; Lightfoot and Cuthrell, 2015; Roos et al., 2023) (Lines 425-426).

Line 431-432: the causality mixup with temperature and the ice sheet again.

Response: Thank you for finding this error. We have now attributed increasing warming temperatures to changes in Earth's orbital forcings and greenhouse gas feedbacks (Lines 516-517).

Bowman, D. M. J. S., J. Balch, P. Artaxo, W. J. Bond, M. A. Cochrane, C. M. D'Antonio, R. DeFries, F. H. Johnston, J. E. Keeley, M. A. Krawchuk, C. A. Kull, M. Mack, M. A. Moritz, S. Pyne, C. I. Roos, A. C. Scott, N. S. Sodhi & T. W. Swetnam (2011) The human dimension of fire regimes on Earth. *Journal of Biogeography*, 38, 2223-2236.

Carter, V. A., A. Brunelle, M. J. Power, R. J. DeRose, M. F. Bekker, I. Hart, S. Brewer, J. Spangler, E. Robinson, M. Abbott, S. Y. Maezumi & B. F. Coddling (2021) Legacies of Indigenous land use shaped past wildfire regimes in the Basin-Plateau Region, USA. *Communications Earth & Environment*, 2, 72.

Delcourt, H. R. & P. A. Delcourt (1997) Pre-Columbian Native American Use of Fire on Southern Appalachian Landscapes. *Conservation Biology*, 11, 1010-1014.

Delcourt, P. A. & H. R. Delcourt (1998) The Influence of Prehistoric Human-Set Fires on Oak-Chestnut Forests in the Southern Appalachians. *Castanea*, 63, 337-345.

Delcourt, P. A., H. R. Delcourt, C. R. Ison, W. E. Sharp & K. J. Gremillion (1998) Prehistoric Human Use of Fire, the Eastern Agricultural Complex, and Appalachian Oak-Chestnut Forests: Paleoecology of Cliff Palace Pond, Kentucky. *American Antiquity*, 63, 263-278.

Henne, P. D., S. Stolze, N. M. Kehrwald, B. Brice & C. D. Allen (2026) Millennial-scale climatic and cultural impacts on vegetation and fire at the southern edge of the Rocky Mountains, USA. *Quaternary Science Reviews*, 376, 109821.

Margolis, E., A. Wion, J. Abatzoglou, L. Daniels, D. Falk, C. Guiterman, J. Johnston, K. Kipfmüller, C. Lafon, R. Loehman, M. Lonergan, C. Naficy, M.-A. Parisien, S. Parks, J. Portier, M. Stambaugh, E. Whitman, A. P. Williams & L. Yocom (2025) Spatiotemporal Synchrony of Climate and Fire Occurrence Across North American Forests (1750–1880). *Global Ecology and Biogeography*, 34, e13937.

Margolis, E. Q., C. H. Guiterman, R. Chavardès, J. Coop, K. Copes-Gerbitz, D. Dawe, D. A. Falk, J. Johnston, E. R. Larson, H. Li, J. M. Marschall, C. Naficy, A. T. Naito, M.-A. Parisien, S. A. Parks, J. Portier, H. M. Poulos, K. M. Robertson, J. Speer, M. C. Stambaugh, T. W. Swetnam, A. J. Tepley, I. Thapa, C. D. Allen, Y. Bergeron, L. Daniels, P. Z. Fulé, D. Gervais, M. P. Girardin, G. L. Harley, J. Harvey, K. Hoffman, J. Huffman, M. D. Hurteau, L. B. Johnson, C. W. Lafon, M. Lopez, R. S. Maxwell, J. Meunier, M. North, M. T. Rother, M. Schmidt, R. Sherriff, L. A. Stachowiak, A. Taylor, E. J. Taylor, V. Trouet, M. Villarreal, L. L. Yocom, K. Arabas, A. Arizpe, D. Arseneault, A. Azpeleta Tarancón, C. H. Baisan, E. Bigio, F. Biondi, G. Cahalan, A. C. Caprio, J. Cerano-Paredes, B. M. Collins, D. C. Dey, I. Drobyshev, C. Farris, M. Fenwick, W. Flatley, M. L. Floyd, Z. e. Gedalof, A. Holz, L. Howard, D. W. Huffman, J. M. Iniguez, K. F. Kipfmüller, S. G. Kitchen, K. Lombardo, D. McKenzie, A. Merschel, K. Metlen, J. Minor, C. D. O'Connor, L. Platt, W. J. Platt, T. Saladyga, A. B. Stan, S. L. Stephens, C. Sutheimer, R. Touchan & P. J. Weisberg (2022) The North American Fire Scar Network. *Ecosphere*, 13, e4159.

Marlon, J. R., P. J. Bartlein, C. Carcaillet, D. G. Gavin, S. P. Harrison, P. E. Higuera, F. Joos, M. J. Power & I. C. Prentice (2008) Climate and human influences on global biomass burning over the past two millennia. *Nature Geoscience*, 1, 697-702.

Marlon, J. R., P. J. Bartlein, A.-L. Daniau, S. P. Harrison, S. Y. Maezumi, M. J. Power, W. Tinner & B. Vannière (2013) Global biomass burning: a synthesis and review of Holocene paleofire records and their controls. *Quaternary Science Reviews*, 65, 5-25.

Marlon, J. R., P. J. Bartlein, D. G. Gavin, C. J. Long, R. S. Anderson, C. E. Briles, K. J. Brown, D. Colombaroli, D. J. Hallett, M. J. Power, E. A. Scharf & M. K. Walsh (2012) Long-term perspective on wildfires in the western USA. *Proceedings of the National Academy of Sciences*, 109, E535-E543.

Power, M. J., J. Marlon, N. Ortiz, P. Bartlein, S. Harrison, F. Mayle, A. Ballouche, R. H. W. Bradshaw, C. Carcaillet & C. Cordova (2008) Changes in fire regimes since the Last Glacial Maximum: an assessment based on a global synthesis and analysis of charcoal data. *Climate Dynamics*, 30, 887-907.

Roos, C. I. (2020) Scale in the study of Indigenous burning. *Nature Sustainability*, 3, 898-899.

Roos, C. I., N. C. Laluk, W. Reitze & O. K. Davis (2023) Stratigraphic evidence for culturally variable Indigenous fire regimes in ponderosa pine forests of the Mogollon Rim area, east-central Arizona. *Quaternary Research*, 113, 69-86.

Scott, A. C. (2010) Charcoal recognition, taphonomy and uses in palaeoenvironmental analysis. *Palaeogeography, Palaeoclimatology, Palaeoecology*, 291, 11-39.

Reviewers' comments:

Reviewer #2 (Remarks to the Author):

Nature Communications Earth and Environment

Tsalickis et al. Human, climatic, and ecological drivers of Holocene fire regimes in North America Review #2

Thank you for thoughtfully addressing my comments and concerns outlined in my first review. I am pleased with the additions and edits that the authors have made to the manuscript and am in support of its publication. I have listed a few minor edits below:

Response: We are happy that the Reviewer is pleased with the additions and edits that we have made to the manuscript and that the Reviewer is in support of this manuscript's publication. We will incorporate the minor edits that the Reviewer has suggested into this latest revision which will further improve the quality of the manuscript.

Line 18: "reconstructed" is the wrong tense.

Response: In this case reconstructed would be the correct tense because we did not reconstruct the climate parameters, global CO₂ emissions, ecological productivity, or human impacts ourselves. Rather, we illustrated and compared data collected from prior research that had already reconstructed these data sets.

Line 86: Could this be written as "continental-to-global" and "regional-to-local" spatial scales?

Response: Yes, we have made the correction (Line 83).

Lines 87 and 90: Sub-continental is written once with a hyphen (line 87) and once without (line 90). There are some other inconsistencies throughout the text as well but the authors mostly use "subcontinental".

Response: Thank you for catching these inconsistencies. We have gone through the manuscript to ensure subcontinental is consistently written without a hyphen.

Line 139: Is this supposed to be Davies et al. (2022) or another Davies paper? In either case, a reference to Davies is missing in the references section!

Response: Yes, this is supposed to be Davies et al. (2022), and we have made the correction (Line 134). We have also added the reference to Davies in the reference section.

Line 204: Missing a closing ")" for the i.e. list.

Response: Thank you for catching this mistake. We have added the closing ")" (Line 200).

Line 242: I think you can just say "yrs" here rather than "kyr" because you already have the thousands of years listed.

Response: We have made the correction to "yrs" (Line 238).

Figure 2: What is the red line for?

Response: In the “clean” version of the manuscript without tracked changes, there is no red line. We think this is an artifact of the tracked changes when we added the updated figure.

Figure 3: Another red line? This might just be an artifact of the PDF but be sure to describe what the lines are for if they carry meaning.

Response: The same issue is occurring here where the red line is from adding the updated figure and signifying a tracked change was made.

Figure 3C. Y-axis needs a label.

Response: Thank you for catching this error. We have added a label to Figure 3C y-axis.

469: Demonstrates?

Response: We have made the correction (Line 463).

498: Between 6,000 cal yr BP and(?) present instead of “to present”?

Response: We have made the correction (Line 492).

Figure 6 A-C: Double check that the scale bars are accurate. 6A ranges from 0-10 which 6B and 6C range from 0-1.

Response: This is correct. The fastest increase in temperature occurred in the early Holocene which coincided with the Laurentide deglaciation. The temperature increases during the mid- and late-Holocene were much smaller in comparison.

549: Extra comma after “Mutch”.

Response: We removed the extra comma (Line 543).

Thanks again for an interesting paper and well-designed study.

Response: We thank the Reviewer for taking the time to review this manuscript.

Reviewer #3 (Remarks to the Author):

I would like to thank the author for considering and responding to many of my concerns.

Response: We would like to thank the Reviewer for providing comments and suggestions for this manuscript which have greatly improved the quality of the manuscript.