nature portfolio

Peer Review File

A multi-century perspective reveals that contemporary fires are less frequent but more severe in dry conifer forests of the southwestern United States

Corresponding Author: Ms Emma McClure

Attachments originally supplied by the reviewers can be found at the end of this file, in order by reviewer number and revision

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Ms McClure,

Your manuscript titled "A multi-century perspective on contemporary fire deficit and anomalous fire severity in the southwestern United States" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

In the following, we list our main editorial concerns.

• Provide firm and robust evidence to demonstrate that contemporary fires in the US are less frequent than those in the past but have higher tree mortality.

• Examine how representative of forested areas in the region are sites used for dendroecological analyses and discuss potential methodological limitations of historical reconstruction of fire severity.

• Provide pertinent methodological details on the i) quantification of different fire management strategies from remote sensing data (including statistical analyses considered); ii) assessment of tree mortality, how it was linked to a specific fire event and whether climate effects on mortality were accounted for.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

Link Redacted

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Rossella Guerrieri, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-5247-0432

Carolina Ortiz Guerrero, PhD
Associate Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements
(Download the link to your computer as a PDF.)

For Manuscripts that fall into the following fields:
• Behavioural and social science
• Ecological, evolutionary & environmental sciences
• Life sciences
An updated and completed version of our Reporting Summary must be uploaded with the revised manuscript
You can download the form here:
https://www.nature.com/documents/nr-reporting-summary.zip

For your information, you can find some guidance regarding format requirements summarized on the following checklist: (https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This is a review of the manuscript by McClure et al. The manuscript presents an overview of the changes in the frequency and severity of wildfires in the southwestern USA in recent times. The authors compare the results among different fire management strategies. The manuscript provides relevant results, and the methods seem adequate, although there is a significant need for improvement in the clarity of the methodological section.
My main concern about this study is that it exclusively targets fire-scar sites, and there is no information about how representative these sites are of the landscape (not only temporally). In other words, are similar forests in each of the geographic areas (beyond the historical fires sites sampled) affected by the same fire regime? This lack of information requires that the authors adjust the text accordingly because, with the provided information, the comparison of fire frequency and severity could be biased.
Another concern is how tree mortality was linked to a specific fire when multiple fires were recorded between 1985 and 2020 in the field surveyed sites. Additionally, whether increased mortality in fire-scarred trees could be linked to drought or other climate-related effects (with or without the passage of fire), as most fire-scarred trees are supposed to be very old for their species, but no information is available about this.
More comments below.
L 72. Utilized "century" instead of "C," as the latter is typically used for carbon.
L 120-121. This statement seems to imply that this is the first study to use fire scar records and satellite observations, which is not accurate. For example, multiple studies included in Margolis et al. 2022 have employed similar methodologies. Please clarify the novelty of this study, as "attributes of historical and contemporary" seems rather vague.
L 178. Were the trees sampled historically?
L 196. This sentence requires clarification. First and second-entry fires are referenced in the results, but there is no explanation about these fires in the methods.
L 202. Were alternative models used? From which specific models are these AIC values derived? This needs to be clearly explained in both the results and methods sections.
L 316-318. It appears that there is a sampling bias towards sites with a higher presence of historical fires, as the "other" sites

that were not sampled have a significantly lower percentage of tree-ring sites burned from 1985 to 2020 (see Table 1).
L 218-220. Discuss how the exclusive study of fire-scar sites affects this result. Indicate whether the fire occurrence in the regions could be overestimated, as only sites with a certain threshold of fire-scarred trees are included.
L 221 & L 38-39: What is the average lifespan of these tree species? How old were the fire-scarred trees from the study sites? Is it reasonable to assume that very old trees (several centuries old) with multiple fire scars injuries have a higher likelihood of natural death, with or without the occurrence of a fire passage?
L 227-228: However, the study exclusively uses fire-scar sites.
L 344: Please explain the gradient.
L 380-413: This section of the methods is somewhat confusing. Why is there a need to model mortality instead of directly utilizing the observed mortality rates per site? It seems that the model aims to determine a CBI threshold where mortality is more probable, but this is not clearly specified. Do the dbh thresholds of 12.7 come from a previous study? If not, how were they determined? This section requires extensive reorganization and clarification.
343-364: How was mortality linked to a specific fire when multiple fires were recorded for the field surveyed sites?
Figure 1 Legend: Inside the figure, "Fire history sites burned vs. unburned." Does "burned/unburned" refer to the period from 1985 to 2020? This needs clarification.
L 420-423: More details about the model are necessary. Explain the function used, the number of samples included, and present the results in a table (if possible) or clarify the statistics shown in the results.

Reviewer #2 (Remarks to the Author):

This is an exceptional work with a good starting hypothesis, a great review and description of the state of the art and a quantity of data that gives robustness to the results. Also highlight the potential use and interest for both researchers in the field and for managers of the natural environment and that it can be a basis for the creation of planning or territorial management policies.

The only thing missing is the discussion (or hypothesis) for various types of habitats (or main tree species).

I recommend to accept it for publication

Reviewer #3 (Remarks to the Author):

General Comments

I think this is a significant paper about a topic that will be of interest to researchers and mangers interested in the fire regimes of the southwestern US. I recommend publication after revision.

This the paper could use some clearer language, especially given the breadth of the journal. For example, wildland fire jargon like “fire severity,” “managed fire vs. suppressed fire,” or “fire-ring fire-scare fire history site” require more clarity. It can be difficult to track weather we are talking about trees that were field sampled by this research vs. records from previous databases. Some of this is because the Methods section often contains the first clear explanations of what was done. I think some of that explanatory material should be moved forward in the text.

What are the major claims of the paper?

This paper can contribute some strong evidence into an ongoing discussion about whether the increases we are seeing in wildland fire in the western US are simply a return to fire pattens that the area experienced prior to European colonialization. It is well understood that fire was quite common in the dry forests of the American Southwest. Multiple tree-ring records on large numbers of trees also show that these frequent fires tended to burn with low intensity leaving much of the overstory intact. These records also indicate that these frequent fires seem to have essentially stopped around the beginning of the 20th century.

However, in the last few decades we have seen a large increase in the frequency, size, and severity of fires in the region. This paper sets out to evaluate whether these recent fires are burning in ways that are similar to the fires from the 17th-19th century. Their methods are to sample trees that are within the perimeter of recent fires to ascertain whether the patterns of severity in these recent fires is different than the record seen in the tree rings. Their results show that the recent increase in wildland fire is not particularly like the historical fire regime. Fires seem to be burning with higher severity and killing more trees than we think we see in the tree-ring record. This would indicate that managers should consider methods to get more low severity fire onto the landscape.

Are the claims novel? If not, please identify the major papers that compromise novelty.

This paper offers new evidence for a long-term debate about fire in the dry forests of the American Southwest. The broad story that the paper lays out is well known and understood. The contribution here is to better distinguish recent fire regimes from those in the region prior to the 20th century. Their results do indicate that we are now seeing more high severity fire. This isn’t particularly groundbreaking, but there is currently debate about this idea. The question is really, is the “new Normal” in the American southwest, just a return to the “old normal.” The answer here is no.

Will the paper be of interest to others in the field?

I believe so. Maybe even of more interest to folks outside the arena.

Will the paper influence thinking in the field?

I think this paper can offer a good bit of evidence to shift this debate a fair bit.
Are the claims convincing? If not, what further evidence is needed?
Yes, though I have discussed several things I would like to see added in the editing notes below.

Are there other experiments that would strengthen the paper further? How much would they improve it, and how difficult are they likely to be?

Not necessary

Are the claims appropriately discussed in the context of previous literature?

Mostly. I have a few notes below about some missing citations.

If the manuscript is unacceptable in its present form, does the study seem sufficiently promising that the authors should be encouraged to consider a resubmission in the future?
NA

Line edits and recommendations.

Line 39, “anonymously?” “Perhaps define “suppressed fires.”

Line 52, I am not sure “vital rates” is widely understood jargon.

Line 54, we are going to need some definitions of “severity,” perhaps separating it from “intensity” for this sort journal.

Lines 54-56, Awkward sentence.

Line 68, Not sure what “some proportion” means

Line 103, Citation needed.

Lines 105-113, This is a key point and requires more explanation and citation as we will get no record of high severity fires in the tree-ring record. Therefore, we would expect tree-rings to tell us a story about areas with low severity fires, while we would expect remote sensing to tell us a story of the extent of high severity burning.

Line 119, I believe there are many papers that have “brought together” tree-rings and remote sensing. Google Scholar shows over 9,000 including:

Correa-Díaz, A., et al. "Linking remote sensing and dendrochronology to quantify climate-induced shifts in high-elevation forests over space and time." Journal of Geophysical Research: Biogeosciences 124.1 (2019): 166-183.

Zhou, Yang, et al. "Applying dendrochronology and remote sensing to explore climate-drive in montane forests over space and time." Quaternary Science Reviews 237 (2020): 106292.

Correa-Díaz, A., et al. "From trees to ecosystems: Spatiotemporal scaling of climatic impacts on montane landscapes using dendrochronological, isotopic, and remotely sensed data." Global Biogeochemical Cycles 34.3 (2020): e2019GB006325.
Etc.

Line 129, The question that is set out in the previous lines is fantastic and very clear. However, things get a bit ambiguous after that. For example, what is a “tree-ring fire-scare fire history site? Is this somewhere you found a scare, or a tree recorded in a data set? I know the answer, but there is considerable information in the methods that should perhaps be moved forward in the text so a reader unfamiliar with dendrochronology can tell what exactly you have done here.

Line 134-136, This is the “big if” of the whole paper. I do buy the argument, but this needs a bit of development and perhaps citations because we will have very few tree-records in areas that experienced high severity fire. By definition, the tree-ring record underreports the history of high severity fire.
I do think the results are very significant and well worked out. However, there is very little discussion about the scale of the fire-scare dataset vs. the scale of the phenomenon in question. The problem here is with questions about scale. As with much dendrochronology published in more generalized journals, this study requires at least a brief discussion of the pitfalls of using opportunistic point-sampling to describe a large spatial extent.
For example, imagine finding a few houses on the coasts of Florida that have survived 6-7 old hurricanes in a few different towns. That does not indicate that hurricanes used to be less severe than recent ones that have destroyed 1000s of houses.

If those old houses were to then be destroyed in a new hurricane it could just be because they have aged. In the case of the sample trees in the data set, these trees are now over 100 years older than the last time they survived a fire. Perhaps this makes them far more susceptible.

Line 140, I am unconvinced you have enough acreage in prescribed fire treatments or even “managed fires,” whatever those are, to tell us much about these differences? Also, tell us somewhere how these differ from suppressed fires, and how much of the landscape each represent? For example, in the “contemporary fire record“ section you use MTBS to map the larger fires. However, it is very difficult to distinguish areas burned at night in backfires to reduce severity. Would these areas be considered managed fires?

Line 177, are the fire-scare sampled trees, ones you found, or from a dataset?

Line 187, “burning” also, we need to know what you mean by prescribed fire being most abundant? 1% of the landscape, or maybe 80%?

Line 220, What about the size of these fires? Does more frequent fire mean more landscape burned?

Line 343, How big was this “subset?”

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Ms McClure,

Your revised manuscript titled "A multi-century perspective reveals an extensive contemporary fire deficit but anomalous fire severity in dry conifer forests of the southwestern United States" has now been seen by two of the original three reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

Rossella Guerrieri, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-5247-0432

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed all the reviewers' comments, significantly improving the clarity and impact of the manuscript. I recommend it for publication as is.

Reviewer #3 (Remarks to the Author):

I find the paper greatly improved and recommend publication

General Comments

I think this is a significant paper about a topic that will be of interest to researchers and mangers interested in the fire regimes of the southwestern US. However, this the paper could use some clearer language, especially given the breadth of the journal. For example, wildland fire jargon like “fire severity,” “managed fire vs. suppressed fire,” or “fire-ring fire-scare fire history site” require more clarity. It can be difficult to track weather we are talking about trees that were field sampled by this research vs. records from previous databases.

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For example, imagine finding a few houses on the coasts of Florida that have survived 6-7 old hurricanes in a few different towns. That does not indicate that hurricanes used to be less severe than recent ones that have destroyed 1000s of houses. If those old houses were to then be destroyed in a new hurricane it could just be because they have aged. In the case of the sample trees in the data set, these trees are now over 100 years older than the last time they survived a fire. Perhaps this makes them far more susceptible.

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Line 343, How big was this “subset?”

Response to Reviewers

McClure et al. **A multi-century perspective reveals an extensive contemporary fire deficit but anomalous fire severity in dry conifer forests of the southwestern United States**

We thank the reviewers for the careful attention paid to this manuscript. In what follows, please find the Reviewer comments (italicized) followed by our responses (roman). We have considered the main comments from each reviewer first, then merged line by line comments from the two reviewers (#1 and #3) who provided them. Those from reviewer #3 are labeled with "Line", while those from reviewer #1 have the shorthand "L". The line numbers in reviewer comments refer to the original manuscript; we did not change them after making edits, but line numbers in our responses are updated. Note that in addition to the changes made in response to these reviews, we have made several minor edits to wording (visible in the tracked changes document) in response to a separate USGS review. Additionally, in response to the reviewer comments, we have made several changes to the references cited section.

Reviewer comments:

Reviewer #1 (Remarks to the Author):
This is a review of the manuscript by McClure et al. The manuscript presents an overview of the changes in the frequency and severity of wildfires in the southwestern USA in recent times. The authors compare the results among different fire management strategies. The manuscript provides relevant results, and the methods seem adequate, although there is a significant need for improvement in the clarity of the methodological section.

My main concern about this study is that it exclusively targets fire-scar sites, and there is no information about how representative these sites are of the landscape (not only temporally). In other words, are similar forests in each of the geographic areas (beyond the historical fires sites sampled) affected by the same fire regime? This lack of information requires that the authors adjust the text accordingly because, with the provided information, the comparison of fire frequency and severity could be biased.

> **Response**: We have made edits throughout the text to be clear that our study assesses changes occurring at fire history sites, but we also provide additional text and references to show that these hundreds of sites are broadly representative of historical fire regimes in dry conifer forests across the region. This includes research that shows that the fire scar sites occupy similar topographic and climate state space

as forests in the region (Margolis et al. 2022), that systematic and random sampling of fire-scars in dry conifer forests produces the same results as targeted sampling (e.g., Van Horne et al 2006, Farris et al. 2013) and that fire scars can reconstruct the landscape fire regime of known modern fires (e.g., Farris et al. 2010).

We have also added the additional descriptor of "sites" in several places in the manuscript where additional clarity may be helpful (line 40 in the abstract, 197 in the results, and lines 230, 258, and 273 in the discussion). We have also modified text in the introduction to better describe the representativeness of fire scar sites as follows, beginning on line 113, "Fire-scarred trees are unequivocal evidence of low-severity fire at a particular place and time and can record dozens of fires over centuries. Aggregating these records from plots to forested landscapes to the southwestern US has been instrumental in establishing that frequent, low-severity fire was ubiquitous in dry conifer forests of the region for centuries prior to circa 1900. The current tree-ring fire-scar network in the southwestern US includes > 400 sites, thousands of fire-scarred trees, and tens of thousands of fire scars, and has been shown to be representative of the topographic and climatic state space of regional dry conifer forests." We have also added to and rewritten a portion of the discussion as follows, beginning on line 268, "While our samples are at the scale of fire history sites < 1-10 ha, patterns of historical burning recorded across networks of these sites has been shown to be broadly representative of large dry conifer forest landscapes >100-10,000 ha across the region, and many of the contemporary fires that burned over these sites exceeded 10,000 ha. Accordingly, our empirical assessment that contemporary severity is higher than historical norms at fire history sites is indicative of changes occurring regionally across dry conifer forest landscapes." Finally, we have added the description of "dry conifer forests" in multiple locations in the manuscript title, abstract, introduction, and discussion to further clarify to what extent our findings may be generalizable.

Another concern is how tree mortality was linked to a specific fire when multiple fires were recorded between 1985 and 2020 in the field surveyed sites. Additionally, whether increased mortality in fire-scarred trees could be linked to drought or other climate-related effects (with or without the passage of fire), as most fire-scarred trees are supposed to be very old for their species, but no information is available about this.

Response: Our model linking tree mortality to burn severity was based on sites that only burned once in the contemporary record, and where we could confidently assign mortality to that fire. We assessed mortality of all trees in our 10-m radius plot (not just old, fire-scarred trees, many of which were already dead (e.g. stumps) when they were originally sampled for fire scars in the decades prior to our study). We have added text to the results, described below, to clarify this point. While we believe that elevated mortality and fire severity is closely tied to altered fire behavior in contemporary wildfires (e.g., crown fire rather than surface fire), the suggestion that it may be linked to other stressors is interesting to consider. While beetle-kill is not an issue (yet) at our study sites, drought may elevate tree mortality during fires.

Accordingly, we have added the following text to the discussion, beginning on line 265, and reference to Hood et al. 2018. "Increased severity and tree mortality is expected to be associated with increased heat released by fires burning abundant fuels under warm and dry conditions but may also be imparted by pre-fire tree drought stress elevated in dense stands and a warmer, drier climate."

More comments below.

Reviewer #2 (Remarks to the Author):
This is an exceptional work with a good starting hypothesis, a great review and description of the state of the art and a quantity of data that gives robustness to the results. Also highlights the potential use and interest for both researchers in the field and for managers of the natural environment and that it can be a basis for the creation of planning or territorial management policies.
The only thing missing is the discussion (or hypothesis) for various types of habitats (or main tree species).
I recommend to accept it for publication

Response: We thank the reviewer for the highly positive assessment of our work. We have added text clarifying that our findings are limited to dry conifer forest types, and not other forest types.

Reviewer #3 (Remarks to the Author):
General Comments

I think this is a significant paper about a topic that will be of interest to researchers and mangers interested in the fire regimes of the southwestern US. I recommend publication after revision.
This the paper could use some clearer language, especially given the breadth of the journal. For example, wildland fire jargon like "fire severity," "managed fire vs. suppressed fire," or "fire-ring fire-scare fire history site" require more clarity. It can be difficult to track weather we are talking about trees that were field sampled by this research vs. records from previous databases. Some of this is because the Methods section often contains the first clear explanations of what was done. I think some of that explanatory material should be moved forward in the text.

Response: We thank the reviewer for the positive assessment and suggestions. We have provided additional clarification and definitions for the terms noted above (fire severity, managed fire, suppressed fire, and tree-ring fire-scar fire history sites) – each of these are described in the response to line-by-line comments below. Finally,

as per the reviewer's suggestion to move some further explanation forward, we have added extra clarifying description of methods to the results in two places. First, under Historical and contemporary fire frequency, line 157, we added "the full period of record (1700-2020; incorporating both tree-ring fire scars and fires mapped from satellite imagery)." Second, under Historical and contemporary fire severity, we have developed the first two sentences as follows, line 185, "Of the 406 fire history sites from the North American Fire-Scar Network (NAFSN)[40] used in this study, 206 had burned in the contemporary period, from which we extracted satellite-measured burn severity (given in modeled Composite Burn Index (CBI)). We conducted field surveys of fire effects in plots at 74 of these sites. Because much of the fire-scarred material from which the original fire histories was developed came from stumps and down wood, our goal was not to track the survival of individual trees sampled for fire history, but rather to quantify tree mortality at the plot scale. "

What are the major claims of the paper?

This paper can contribute some strong evidence into an ongoing discussion about whether the increases we are seeing in wildland fire in the western US are simply a return to fire pattens that the area experienced prior to European colonialization. It is well understood that fire was quite common in the dry forests of the American Southwest. Multiple tree-ring records on large numbers of trees also show that these frequent fires tended to burn with low intensity leaving much of the overstory intact. These records also indicate that these frequent fires seem to have essentially stopped around the beginning of the 20th century.

However, in the last few decades we have seen a large increase in the frequency, size, and severity of fires in the region. This paper sets out to evaluate whether these recent fires are burning in ways that are similar to the fires from the 17th-19th century. Their methods are to sample trees that are within the perimeter of recent fires to ascertain whether the patterns of severity in these recent fires is different than the record seen in the tree rings. Their results show that the recent increase in wildland fire is not particularly like the historical fire regime. Fires seem to be burning with higher severity and killing more trees than we think we see in the tree-ring record. This would indicate that managers should consider methods to get more low severity fire onto the landscape.

Are the claims novel? If not, please identify the major papers that compromise novelty.

This paper offers new evidence for a long-term debate about fire in the dry forests of the American Southwest. The broad story that the paper lays out is well known and understood. The contribution here is to better distinguish recent fire regimes from those in the region prior to the 20th century. Their results do indicate that we are now seeing

more high severity fire. This isn't particularly groundbreaking, but there is currently debate about this idea. The question is really, is the "new Normal" in the American southwest, just a return to the "old normal." The answer here is no.

Will the paper be of interest to others in the field?

I believe so. Maybe even of more interest to folks outside the arena.

Will the paper influence thinking in the field?

I think this paper can offer a good bit of evidence to shift this debate a fair bit.

Are the claims convincing? If not, what further evidence is needed?

Yes, though I have discussed several things I would like to see added in the editing notes below.

Are there other experiments that would strengthen the paper further? How much would they improve it, and how difficult are they likely to be?

Not necessary

Are the claims appropriately discussed in the context of previous literature?

Mostly. I have a few notes below about some missing citations.

If the manuscript is unacceptable in its present form, does the study seem sufficiently promising that the authors should be encouraged to consider a resubmission in the future?

NA

Response: We thank the reviewer for the encouragement and detailed feedback.

Line-by-line responses

Line 39, "anonymously?" "Perhaps define "suppressed fires."

Response: Changed to "anomalous", line 41. Given abstract word limits we were not able to include a definition for "suppressed fires" here, however, this definition has been added to the introduction.

Line 52, I am not sure "vital rates" is widely understood jargon.

Response: Replaced with "dynamics."

Line 54, we are going to need some definitions of "severity," perhaps separating it from "intensity" for this sort of journal.

Response: Elaborated on severity following introductory sentence. Added sentence, beginning on line 58, which reads: "Severity in this context encompasses both immediate

and delayed effects to vegetation as measurable typically one year post-fire through satellite- or field-based metrics."

Lines 54-56, Awkward sentence.

Response: Reworked and shortened – the sentence now reads: "Increasing fire ***severity*** can drive anomalous tree mortality, even for the most fire-tolerant tree species (e.g., giant sequoia [*Sequoiadendron giganteum*]), thereby reducing propagule availability in high-severity patches and impeding recovery."

Line 68, Not sure what "some proportion" means

Response: Removed. Sentence, beginning on line 72, now reads "A large body of evidence demonstrates that many of these forests were historically characterized by frequent, low- to moderate-severity fires associated with prolonged dry seasons, profuse grassy fuels, and abundant ignitions from lightning and Indigenous land stewardship."

L 72. Utilized "century" instead of "C," as the latter is typically used for carbon.

Response: Changed to century throughout manuscript.

Line 103, Citation needed.

Response: Added citation (Coop et al., 2020).

Lines 105-113, This is a key point and requires more explanation and citation as we will get no record of high severity fires in the tree-ring record. Therefore, we would expect tree-rings to tell us a story about areas with low severity fires, while we would expect remote sensing to tell us a story of the extent of high severity burning.

Response: While the tree-ring fire-scar network can provide insight only into the frequency of low-severity fire, the modern satellite record can provide insight into BOTH low- and high-severity fire. If contemporary fire regimes are unchanged from historical fire regimes, the satellite record is expected to show frequent, low-severity fire at these tree-ring fire-scar sites. We replaced the final sentences of this paragraph, beginning on line 127, to now read "If contemporary fire regimes are not different from those that occurred historically, the satellite record would be expected to show similarly frequent, low-severity fire. However, if dry conifer forests across the region as represented by fire history sites are currently burning less frequently or at high severity, that would provide direct evidence of altered fire regimes."

Line 119, I believe there are many papers that have "brought together" tree-rings and remote sensing. Google Scholar shows over 9,000 including:

- *Correa-Díaz, A., et al. "Linking remote sensing and dendrochronology to quantify climate-induced shifts in high-elevation forests over space and time." Journal of Geophysical Research: Biogeosciences 124.1 (2019): 166-183.*

- *Zhou, Yang, et al. "Applying dendrochronology and remote sensing to explore climate-drive in montane forests over space and time." Quaternary Science Reviews 237 (2020): 106292.*
- *Correa-Díaz, A., et al. "From trees to ecosystems: Spatiotemporal scaling of climatic impacts on montane landscapes using dendrochronological, isotopic, and remotely sensed data." Global Biogeochemical Cycles 34.3 (2020): e2019GB006325.*
- *Etc.*

  Response: While these papers and others like them have brought together tree rings and remote sensing, our study is the first that we know of that explicitly contrasts historical with contemporary fire regimes using both data types. However, we have changed the sentence as follows, line 124: "These two types of data—tree-ring fire-scar records and satellite observations—can be brought together to compare historical and contemporary fire frequency and severity."

L 120-121. This statement seems to imply that this is the first study to use fire scar records and satellite observations, which is not accurate. For example, multiple studies included in Margolis et al. 2022 have employed similar methodologies. Please clarify the novelty of this study, as "attributes of historical and contemporary" seems rather vague.

> **Response**: Again, we are unaware of other studies that have contrasted historical and contemporary fire regimes using the approaches developed herein. The sentence has been modified to further clarify the novelty of this study, see previous response.

Line 129, The question that is set out in the previous lines is fantastic and very clear. However, things get a bit ambiguous after that. For example, what is a "tree-ring fire-scare fire history site? Is this somewhere you found a scare, or a tree recorded in a data set? I know the answer, but there is considerable information in the methods that should perhaps be moved forward in the text so a reader unfamiliar with dendrochronology can tell what exactly you have done here.

> **Response**: Thank you. Clause was simplified and expanded to read, now beginning on line 135, "406 fire history sites, comprising thousands of individual trees, sampled prior to contemporary fires."

Line 134-136, This is the "big if" of the whole paper. I do buy the argument, but this needs a bit of development and perhaps citations because we will have very few tree-records in areas that experienced high severity fire. By definition, the tree-ring record underreports the history of high severity fire.

I do think the results are very significant and well worked out. However, there is very little discussion about the scale of the fire-scare dataset vs. the scale of the phenomenon in question. The problem here is with questions about scale. As with much dendrochronology

published in more generalized journals, this study requires at least a brief discussion of the pitfalls of using opportunistic point-sampling to describe a large spatial extent.

> **Response**: We appreciate this comment. Throughout, we have endeavored to be clear that our focus is on changes occurring at the scale of individual fire history sites, as described elsewhere in our responses. We have, however, added text that describes how the inferences from fire-scar sites may be applicable to larger, dry conifer forest landscapes, see for example text beginning on lines 113 and 268.

For example, imagine finding a few houses on the coasts of Florida that have survived 6-7 old hurricanes in a few different towns. That does not indicate that hurricanes used to be less severe than recent ones that have destroyed 1000s of houses. If those old houses were to then be destroyed in a new hurricane it could just be because they have aged. In the case of the sample trees in the data set, these trees are now over 100 years older than the last time they survived a fire. Perhaps this makes them far more susceptible.

> **Response**: As described above, the individual trees sampled in the original fire history studies were generally not the same as the trees occurring at these sites during recent wildfires. Because many of the original samples came from stumps and down logs, we could not quantify recent fire effects to those trees. Instead, we sampled burn severity and tree mortality across all trees within our 10-m radius sample plots. Additional text to clarify this point has been added to the results section, described below.

Line 140, I am unconvinced you have enough acreage in prescribed fire treatments or even "managed fires," whatever those are, to tell us much about these differences? Also, tell us somewhere how these differ from suppressed fires, and how much of the landscape each represent? For example, in the "contemporary fire record" section you use MTBS to map the larger fires. However, it is very difficult to distinguish areas burned at night in backfires to reduce severity. Would these areas be considered managed fires?

> **Response**: For classifying each fire by type, we drew the category directly from National Interagency Fire Center (NIFC) data. This allows for only one category per fire. The details of management decisions and actions like burnout operations are not well-tracked within individual fires, but undoubtedly influence subsequent patterns of burn severity in each of these fire types. However, such an assessment is beyond the scope of our analysis. Instead, we tested for differences across these coarse fire types, which we believe reflect strong differences in management strategy. We also tested for statistically significant differences between these categories using linear mixed-effects models. We have added more description about how these categories vary and provided additional detail on the analysis in the methods section, beginning on line 461, which now reads: "To assess associations

between contemporary burn severity and management strategy, we classified fires by management type (suppressed wildfire, wildland fire use, and prescribed burn). These categories are determined at the fire level and reflect critical differences in management strategy. Suppressed fires are managed to limit fire size and effects through effective containment and extinguishment. Prescribed fires are the product of deliberate ignitions intended to achieve a wide range of objectives but are generally managed to burn at low severity. For the wildland fire use category, we included wildfires listed as "resource benefit," "prescribed natural fire," or "managed for multiple uses," depending on source and date as these terms were variously applied over the study period to refer to the same general type of fire. Each of these terms reflects that the management intent of the fire was to allow it to burn to achieve ecological objectives, like a prescribed fire, though originating from an unplanned ignition. We recognize that wildland fire use is an outdated term, however in the fire management community today, there is not a clear successor to it, so we used it for simplicity. Due to smaller sample sizes, we grouped the prescribed burns and wildland fire use fires together for analysis purposes. We tested for differences in burn severity (modeled CBI) of the first contemporary fire between sites burned in suppressed wildfire (160 sites, 34 fires) vs. wildland fire use (32 sites, 12 fires) and prescribed burn (12 sites, 8 fires) using a linear mixed-effects model with fire management strategy as a fixed effect and fire identity as a random effect." We made a note in the introduction referring to this expanded explanation, beginning on line 146, "we were also interested in assessing the extent to which varying contemporary fire management strategies (prescribed burning, managed wildfire, and full suppression, described fully in the Methods) might be associated with different fire outcomes."

Line 177, are the fire-scare sampled trees, ones you found, or from a dataset?

Response: Though spelled out in the methods, we agree with the reviewers that additional clarity would be useful here in the results. Accordingly, we have added some further description beginning on line 185, as follows: "Of the 406 fire history sites from the North American Fire-Scar Network (NAFSN) used in this study, 206 had burned in the contemporary period, from which we extracted satellite-measured burn severity (given in modeled Composite Burn Index (CBI)). We conducted field surveys of fire effects in plots at 74 of these sites. Because much of the fire-scarred material from which the original fire histories was developed came from stumps and down wood, our goal was not to track the survival of individual trees sampled for fire history, but rather to quantify tree mortality at the plot scale. We were, however, able to locate at least one of the original fire-scar sampled trees, stumps, or logs at 25 sites (33.8%; see Supplementary Figure 2 for images of fire-scarred stumps and trees)."

L 178. Were the trees sampled historically?

Response: See previous response (same confusion from both reviewers, we hope it is clearer now).

Line 187, "burning" also, we need to know what you mean by prescribed fire being most abundant? 1% of the landscape, or maybe 80%?

Response: Fixed grammar and removed "abundant" because the idea here was to speak to the frequency, not extent. Clause, on line 199, now reads "...particularly on the Kaibab Plateau and in the Rincon Mountains, where prescribed burning has been most consistently utilized through time."

L 196. This sentence requires clarification. First and second-entry fires are referenced in the results, but there is no explanation about these fires in the methods.

Response: Added sentence here in results section, line 211, to clarify: "First-entry fire refers to the first known occurrence of any type of fire following a period of fire exclusion." Also added a follow up in methods section, line 364, under Contemporary Fire Records: "For any sites which experienced more than one fire in the contemporary timeframe, we considered only the first-entry fire."

L 202. Were alternative models used? From which specific models are these AIC values derived? This needs to be clearly explained in both the results and methods sections.

Response: This was a typo, corrected to "CBI". Thanks for catching this.

L 218-220. Discuss how the exclusive study of fire-scar sites affects this result. Indicate whether the fire occurrence in the regions could be overestimated, as only sites with a certain threshold of fire-scarred trees are included.

Response: As described above, we have focused the manuscript more narrowly on these fire history sites but provided additional text justifying their representation of dry conifer forest landscapes.

Line 220, What about the size of these fires? Does more frequent fire mean more landscape burned?

Response: Our study focuses on changes in fire frequency and severity at fire history sites; to gain an understanding of changes in fire size, or relationships between fire frequency and size, would require a fundamentally different study. However, to answer the question: prior work (e.g., Farris et al. 2010) has demonstrated that the frequency of fires recorded in fire scar samples can be related to the spatial extent of fires – larger fires scar more trees.

L 221 & L 38-39: What is the average lifespan of these tree species? How old were the fire-scarred trees from the study sites? Is it reasonable to assume that very old trees (several centuries old) with multiple fire scars injuries have a higher likelihood of natural death, with or without the occurrence of a fire passage?

Response: We have adjusted language to emphasize that our inferences apply to tree mortality at sites, rather than individual trees. For example, the abstract now reads "At sites where trees historically survived many fires over centuries, 42% of recent fires burned with anomalous tree-killing severity." Because the original fire history sampling was often conducted on stumps and down logs, rather than live trees, here we are testing for a change in fire effects on plots containing a broad range of age classes, **not** on individual tracked trees. The assumption above is reasonable (there is literature

support for a U-shaped distribution of mortality as a function of size / age), yet this effect does not influence our findings.

L 227-228: However, the study exclusively uses fire-scar sites.

Response: We assess changes at fire-scar sites by contrasting the historical records of fire at those sites with those recorded in the modern satellite record. As noted elsewhere in this response, we have added qualifying statements to make this clearer. We appreciate the comments highlighting that need.

L 316-318. It appears that there is a sampling bias towards sites with a higher presence of historical fires, as the "other" sites that were not sampled have a significantly lower percentage of tree-ring sites burned from 1985 to 2020 (see Table 1).

Response: This division into geographic areas of interest did not factor into primary analysis but rather was established for field sampling. We targeted areas of high sampling density from the NAFSN database, so this bias was desirable from a logistical standpoint. The goal was to revisit sites which burned since being sampled. This is described beginning on line 370.

Line 343, How big was this "subset?"

Response: Added number of pre-existing fire history sites re-visited in 2021 (n=74).

L 344: Please explain the gradient.

Response: Added the following by way of explanation, line 371: "For example, proportion of fires in the full suppression category ranged from 100% in the Pinaleño and Santa Catalina Mountains to 14% on the Kaibab Plateau. Similarly, the proportion of sites which burned with high probability of tree mortality (CBI > 1.61) ranged from 73% in the Santa Catalina Mountains to 0% on the Kaibab."

L 380-413: This section of the methods is somewhat confusing. Why is there a need to model mortality instead of directly utilizing the observed mortality rates per site? It seems that the model aims to determine a CBI threshold where mortality is more probable, but this is not clearly specified. Do the dbh thresholds of 12.7 come from a previous study? If not, how were they determined? This section requires extensive reorganization and clarification.

Response: Our objective was to assess how severely all these sites had burned in the contemporary period using satellite-derived burn severity maps. The purpose of the field sampling was to link the satellite imagery with field measures of tree mortality. Had we relied solely on the satellite imagery, we would not have been able to accurately quantify the extent to which trees were actually being killed by fire. Had we relied on just the field data, we would have had a much smaller sample size and would not have been able to generalize impacts to sites across the full study area. We have reorganized this section of the methods to increase the clarity of our

objectives and approaches, beginning on line 415: "Our implicit assumption is that historically, for living trees to have recorded multiple short-interval fires without being killed, those fires must have predominantly burned at a severity unlikely to result in extensive overstory mortality. However, fire severity above this level—with likely overstory mortality—would be inconsistent with historical norms across dry conifer forests of the southwestern US."

We describe the creation of our likely mortality threshold beginning on line 431: "To develop the tree mortality classification, we generated a logistic regression model relating CBI to field-measured tree mortality, and used it to identify the CBI threshold above which probability of overstory tree mortality exceeds 0.5—in other words, overstory trees are more likely than not to be killed. Using this 0.5 probability of mortality threshold avoids biasing our contemporary severity classification toward either a more conservative or liberal interpretation of severity."

The choice of our dbh threshold is explained as follows: "Our use of trees ≥ 12.7-cm dbh to model the relationship between burn severity and tree survival is consistent with prior studies, but also generally supported by the historical ages at which trees recorded fire scars (and thus demonstrate survival through historical fires), as follows. The median number of years between the pith ring (center) of the tree and the first fire scar across our data (n = 2215 trees with pith) was 47 years, which corresponds well with 35 years reported by Brown et al. and 52 years reported by Yocom and Fulé. Given that most fire scar samples are collected at <30 cm above the ground, the years between pith and first fire scar may underestimate tree age by up to 5 years; accordingly, we estimate the median tree age at first fire scar in our study to range between 47 and 52 years. Established age-size relationships for ponderosa pine in Arizona (age = 10.4* dbh(cm)$^{0.66}$), the most frequently sampled tree species, yield size estimates of 10-12 cm dbh for 47-52-year old ponderosa pine trees. Thus, we expect that historical fires were generally not lethal for trees ≥ 12.7-cm dbh for these trees to have recorded multiple fires over 5-10 decades beginning at ages between 47 and 52 years."

L 343-364: How was mortality linked to a specific fire when multiple fires were recorded for the field surveyed sites?

Response: This required some difficult interpretation at field sites where multiple fires were recorded, so we restricted analysis to sites where only one fire occurred between 1985-2020, and where no fires were known to have occurred for several decades prior to that contemporary period. See verbiage on first-entry fires for more detail and notes on revisions made.

L 420-423: More details about the model are necessary. Explain the function used, the number of samples included, and present the results in a table (if possible) or clarify the statistics shown in the results.

Response: We have added the number of sites and fires to the text and noted our use of the package glmmTMB (Brooks et al. 2017).

Figure 1 Legend: Inside the figure, "Fire history sites burned vs. unburned." Does "burned/unburned" refer to the period from 1985 to 2020? This needs clarification.

Response: Added clarification, caption now reads: "**Figure 1.** Study area map showing locations of 406 tree-ring fire history sites in the southwestern United States analyzed in this study and contemporary fire history (1985-2020). Burned and unburned designations refer only to defined contemporary period."