nature portfolio

Peer Review File

A fire deficit persists across diverse North American forests despite recent increases in area burned

Corresponding Author: Dr Sean Parks

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
This study represents a timely and significant test of whether increased area burned in recent decades has exceeded the "fire deficit" of the post-settlement period in North America, or whether historical burning still exceeds that seen in the contemporary period. Importantly, the authors found evidence of a persistent fire deficit, with recent extreme fire years being close to the historical norm in many ecoregions (at least in terms of area burned), and years with no fire being much more prevalent than historically. The analytical approach appears valid, well-reasoned, and well-described, and the authors take advantage of a recently published database of tree-ring fire scar records (the North American Fire Scar Network, NAFSN). The analyses provide strong support for the paper's main conclusion, namely that, on average across the NAFSN, contemporary fires have affected less area than would have been expected to have burned based on the historical period, despite increasing fire activity and fire-conducive climate conditions in recent decades. The writing is generally clear, well-grounded in the literature, and provides sufficient context to communicate the nuance of how contemporary burning differs from that of the historical period in multiple dimensions, including in its ecosystem and societal impacts, beyond area burned alone. I have several minor comments to improve the paper and further clarify the approach:
• Even within the contemporary period, fire activity after 2000 generally exceeds that before 2000 (at least in western U.S. forests). While I don't expect that the main conclusions of the paper would change, it is probably worth testing the magnitude of the difference between the rate of burning prior to 1880 and that after 2000 to more closely reflect recent increases.
• The Discussion briefly touches on some of the caveats associated with using fire scar records, including the relatively low sample representation in boreal and non-forest biomes. However, the analysis is only done in reference to ecoregions. While ecoregions may largely map to forest type, there is also likely to be diversity in forest types within ecoregions, and widespread forest types that occur within multiple ecoregions. It would be valuable to, at minimum, report the representation of forest types within the NAFSN. It would also be interesting, if possible, to report some of the analyses at the level of forest type (vs. ecoregion) as a supplement.
• While I like the conceptual Figure 4, I feel that the terms "stabilizing/destabilizing feedback" need a more careful treatment in the text. At present, it is not fully clear whether the terminology of stabilizing/destabilizing feedbacks maps onto the concepts of negative/positive fire-vegetation feedbacks (e.g., see Tepley et al. 2018, doi:10.1111/1365-2745.12950). Alternatively, it seems that "destabilizing" in this case is meant to refer to fire as a catalyst or mechanism of state change (i.e., ecological transformation or vegetation type conversion). If so, the concept & terminology need to be explained more fully/ integrated more explicitly into the Discussion. I would also argue that there is uncertainty in whether fire would always operate as a destabilizing force in the context of a persistent fire deficit and changing climate, or whether it could function to realign fire-suppressed forests with historical conditions over time (particularly in conjunction with changes in management).
• Lastly, one question that came up while reading the paper was how spatial patterns of widespread fire years varied between the historical and contemporary periods. For example, was the extensive fire activity in years like 2020 more concentrated in particular regions (e.g., where fire weather exceeded suppression capacity) than would have been expected historically? In the historical record, were extensive fire years fairly distributed across the NAFSN? This is a point of curiosity that could be interesting as a supplemental analysis, but not critical to address in the main text.

Line comments:
[112-120] I like this contrast in management implications based on the two potential alternate results from this analysis. I would like to see these clearly revisited in the Discussion. In particular, the implications for fire suppression are worth discussing in more detail, to more clearly highlight the importance of this study specifically for informing fire/forest management.

[146] Minor wording suggestion: “would be expected to” is more precise language than “should have”.
[158-159] Suggest adding a clause to further explain the reasoning for using hexels – it wasn’t fully clear before reading the methods that this was global analysis averaging across hexels (vs. analyzing locations individually). Additionally, the wording here was confusing on first read. Suggest changing to something like “a maximum of 5% sites burned in any individual year in the contemporary record (in 2011 and 2021), whereas a maximum of 17% burned in 1748 in the historical record…”
[165-166] The wording in this sentence is difficult to interpret. Please rephrase.
[237] Check citation format.
[334] I suspect that fire regimes were significantly altered at different times across the continent (e.g., earlier in the eastern than western U.S.), but the citation here is only for the inland Northwest. A clearer justification for using a single cutoff at 1880 is needed, with further support from prior research and/or an analysis using the NAFSN itself (e.g., to identify the timing of a significant departure in rate of burning over time within each ecoregion).
[Table 3] I suggest using “proportion of years” in place of “likelihood”, as it is clearer/ more precise. Correct me if I'm wrong, but my understanding is that likelihood is a population parameter which is being estimated here (e.g., there is some true underlying probability of no sites burning in a given year, and the data represent a single “run” of a particular set of years).
[Fig. 4] While this is effective as a conceptual figure, it is also distracting as a reader to wonder why the black line of average annual area burned is represented conceptually, instead of being quantified. It seems like it would be possible to put numbers to that y-axis using the NAFSN data, unless there is something about fire scar records during the 1880-1984 period that precludes such an analysis?

Reviewer #2

(Remarks to the Author)
The manuscript examines whether contemporary burned area in North-America forests and woodland deviates (deficit or surplus) from the pre-1880 period, i.e. before generalized fire suppression took place. The authors resort to an impressive dataset (an extensive network of sites containing fire-scarred trees) and acknowledge the limitations (stand-replacing fire regimes eliminating trees). I don’t think such limitations have a meaningful impact on the results and conclusions. I don’t detect problems in the analyses and find the ms. very well written and clear, perfectly fitting the characteristics of the journal. It is certainly worthy of publication and I have only a few minor concerns, see below.

L57-59. I see this paper as potentially being used by deniers of climate change impacts. Consider if possible some rephrasing here to put even more emphasis on impact rather than on burned area.

L104. The ecological problems/implications of the fire deficit are only vaguely suggested here, but the more casual reader will benefit from additional information. Please be more explicit, as in the Discussion, but it should be stated right from the start and be present in the Abstract.

L132. Individual fire perimeters, especially from older dates, can miss unburned patches within the fire area, thus overestimating the NAFSN sites burned %. Could this be an issue here?

L140. Only in the eastern US?

L167. The 2020 maximum was already mentioned before, I don’t think the repetition is needed.

L182. Not all fires scar trees. So, also maybe because the historical burning figures are likely to be underestimates?

L197. No references for such accumulated body of evidence?

L229-243. Eastern US is presented separately and with detail. Why not a more balanced treatment of North America geography?

L237. A reference that should be a number.

L248. Maybe add the recent fire in Jasper, Canada?

L267. I’m assuming it’s biomass decomposition, but might not be totally clear to some readers.

L342. Are the two Canadian sources consistent with each other?

L349. But maybe that was not the case before 1880 and so comparisons might be impacted?

Figure 4. Just a matter of taste but I would prefer a font not looking as hand-written. Quantification added to the annual area burned axis would more effectively communicate the results.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
The authors fully satisfied my comments in the revised manuscript and I believe those of the other reviewer as well. Look forward to see the work published.

Reviewer #1 feedback:
This study represents a timely and significant test of whether increased area burned in recent decades has exceeded the "fire deficit" of the post-settlement period in North America, or whether historical burning still exceeds that seen in the contemporary period. Importantly, the authors found evidence of a persistent fire deficit, with recent extreme fire years being close to the historical norm in many ecoregions (at least in terms of area burned), and years with no fire being much more prevalent than historically. The analytical approach appears valid, well-reasoned, and well-described, and the authors take advantage of a recently published database of tree-ring fire scar records (the North American Fire Scar Network, NAFSN). The analyses provide strong support for the paper's main conclusion, namely that, on average across the NAFSN, contemporary fires have affected less area than would have been expected to have burned based on the historical period, despite increasing fire activity and fire-conducive climate conditions in recent decades. The writing is generally clear, well-grounded in the literature, and provides sufficient context to communicate the nuance of how contemporary burning differs from that of the historical period in multiple dimensions, including in its ecosystem and societal impacts, beyond area burned alone.

> **Response:** Thank you for the positive feedback and constructive review.

Minor comments:
• Even within the contemporary period, fire activity after 2000 generally exceeds that before 2000 (at least in western U.S. forests). While I don't expect that the main conclusions of the paper would change, it is probably worth testing the magnitude of the difference between the rate of burning prior to 1880 and that after 2000 to more closely reflect recent increases.

> **Response:** This is an interesting idea, but as the reviewer says, the main results and conclusions of the paper would remain the same. For example, modern 'extreme' fire years such as 2020 would still be an 'average' year historically (1600-1880). As such, we feel that adding additional analyses would be distracting to our audience, as several figures and tables would need to be added to include this suggestion. We also think it is best to leverage as much data as possible by comparing historical records to the full range of modern data available.

• The Discussion briefly touches on some of the caveats associated with using fire scar records, including the relatively low sample representation in boreal and non-forest biomes. However, the analysis is only done in reference to ecoregions. While ecoregions may largely map to forest type, there is also likely to be diversity in forest types within ecoregions, and widespread forest types that occur within multiple ecoregions. It would be valuable to, at minimum, report the representation of forest types within the NAFSN. It would also be interesting, if possible, to report some of the analyses at the level of forest type (vs. ecoregion) as a supplement.

> **Response:** The reviewer is correct in that there may be multiple forest types within a given ecoregion, which we acknowledge [lines 137-139]. It would be quite difficult to conduct our analysis by forest type given that there is not a consistent forest type map spanning both Canada

and the US. Further, some forest type maps (e.g. Landfire, which is only un the US) contain too many forest classes to conduct such an analysis, and any effort to group them into more generic types often results in broad categories that would be analogous to using ecoregions. Instead, we provide a supplemental figure showing, for each hexel, the share of sampled trees belonging to some of the most common fire-scarred tree species (Fig. S2). This figure confirms that using ecoregions is a relatively good proxy for forest types, as the species-specific spatial patterns *generally* match with the delimitation of the ecoregions. We also added the following text to the Methods:

> "The ecoregions used in our study (Fig. 1) are intended to represent a diversity in forest types. However, we acknowledge that there is variation in forest types and dominant tree species within ecoregions and that some forest types may occur across multiple ecoregions. For example, in viewing the fire-scarred tree species across our study area, ponderosa pine (*Pinus ponderosa*) and Douglas fir (*Pseudotsuga menziesii*) are found in most or all western US ecoregions, post oak (*Quercus stellata*) and shortleaf pine (*Pinus echinata*) are found in the Eastern Temperate Forests Ecoregion, lodgepole pine (*Pinus contorta*) is present in the northwest portion of our study area, and red pine (*Pinus resinosa*) in the northeast portion of our study area (Fig. S2). Fire scars were recorded by over 70 species in this study." [lines 384-391]

• While I like the conceptual Figure 4, I feel that the terms "stabilizing/destabilizing feedback" need a more careful treatment in the text. At present, it is not fully clear whether the terminology of stabilizing/destabilizing feedbacks maps onto the concepts of negative/positive fire-vegetation feedbacks (e.g., see Tepley et al. 2018, doi:10.1111/1365-2745.12950). Alternatively, it seems that "destabilizing" in this case is meant to refer to fire as a catalyst or mechanism of state change (i.e., ecological transformation or vegetation type conversion). If so, the concept & terminology need to be explained more fully/ integrated more explicitly into the Discussion. I would also argue that there is uncertainty in whether fire would always operate as a destabilizing force in the context of a persistent fire deficit and changing climate, or whether it could function to realign fire-suppressed forests with historical conditions over time (particularly in conjunction with changes in management).

Response: Thanks for pointing out that we did not give more careful treatment to stabilizing and destabilizing feedbacks in the Discussion. We added a new paragraph to the Discussion defining these feedbacks and citing Tepley et al. (2018) and Coppoletta et al. (2016). In this paragraph, we note that, as mentioned by the reviewer, not all contemporary fire serves as a destabilizing feedback. We also note, in a different paragraph, that there are examples where fire once again serves as a stabilizing feedback [line 328] (also see lines 334-335). The new paragraph in the Discussion reads as follows:

> **"**Overall, the relationship among fire, forests, and people has changed substantially between the historical (1600-1880) and contemporary (1985-2022) time periods across much of Canada and the United States. Historically, particularly at the sites represented by the fire-scar data used in this study, fire often served as a stabilizing (or negative) feedback [88,89] for forests and people (Fig. 4), in which frequent fire consumed live and dead fuel, thereby perpetuating a fire regime dominated by low- to moderate-severity fire and selecting for tree species that could tolerate frequent fire [66] (and therefore record fire scars). These fires, some of which were intentionally ignited by Indigenous peoples, often had beneficial outcomes for people and societies in that they were used to promote certain foods and reduce the potential for undesirable fire [81].

> Conversely, contemporary fires are more often acting as a destabilizing (positive) feedback [88,89] for forests and people. For example, contemporary fire is more frequently acting as a catalyst for ecological transformation or vegetation type conversion [62,90] and often causes great harm to society [87,91], as previously described. Not all contemporary fires have this destabilizing effect, but our relationship with fire has changed so that many if not most fires are destabilizing to forests and/or people (Fig. 4)." [lines 272-285]
>
> Coppoletta, M., Merriam, K. E. & Collins, B. M. Post-fire vegetation and fuel development influences fire severity patterns in reburns. *Ecological Applications* **26**, 686–699 (2016).
>
> Tepley, A. J. *et al.* Influences of fire–vegetation feedbacks and post-fire recovery rates on forest landscape vulnerability to altered fire regimes. *Journal of Ecology* **106**, 1925–1940 (2018).

• Lastly, one question that came up while reading the paper was how spatial patterns of widespread fire years varied between the historical and contemporary periods. For example, was the extensive fire activity in years like 2020 more concentrated in particular regions (e.g., where fire weather exceeded suppression capacity) than would have been expected historically? In the historical record, were extensive fire years fairly distributed across the NAFSN? This is a point of curiosity that could be interesting as a supplemental analysis, but not critical to address in the main text.

> **Response:** This is an excellent suggestion. In fact, our research group explicitly addresses the question of whether historical fire activity is concentrated in particular regions during particular time periods in a separate paper currently in review. Nevertheless, to address this comment, we added a supplemental figure to the paper showing the spatial distribution of fire in particularly active fire years in the historical and contemporary time periods (Fig. S4). We also added a sentence to the first paragraph in the Discussion to briefly touch on this, as follows:
>
> > "Historically, fires in particularly active fire years were spatially more widespread and ubiquitous compared to fires burning during active contemporary years (Fig. S4)." [lines 199-201]

<u>Line-by-line comments:</u>

[112-120] I like this contrast in management implications based on the two potential alternate results from this analysis. I would like to see these clearly revisited in the Discussion. In particular, the implications for fire suppression are worth discussing in more detail, to more clearly highlight the importance of this study specifically for informing fire/forest management.

> **Response:** The requested content is more or less already covered in the last paragraph of the Discussion. However, we added a sentence [lines 338-340] and made some other small changes to make these points more evident.

[146] Minor wording suggestion: "would be expected to" is more precise language than "should have".

> **Response:** We made the suggested edit. Thank you.

[158-159] Suggest adding a clause to further explain the reasoning for using hexels – it wasn't fully clear before reading the methods that this was global analysis averaging across hexels (vs. analyzing locations individually). Additionally, the wording here was confusing on first read. Suggest changing to something like "a maximum of 5% sites burned in any individual year in the contemporary record (in 2011 and 2021), whereas a maximum of 17% burned in 1748 in the historical record…"

Response: We see how this was not clear. We added more text describing the hexel-based analysis. The relevant text now reads:

> "To address the varying density of fire-scar sites across the study region, we conducted a parallel analysis summarizing historical and contemporary fire within hexagonal polygons (hereafter 'hexels'; Fig. 1), thereby giving equal weight to each hexel, regardless of how many NAFSN sites they contained. To do so, we calculated the percent of NAFSN sites that burned per year within each hexel, then averaged these values for each year across all hexels (n=120; Fig. 1) and produced frequency distributions for historical and contemporary time periods. These hexel-based results show that an average of 5% burned in 2011 and 2021, whereas 17% burned in 1748 (Fig. 2b); an average of 7% burned per year during the 1600-1880 historical period." [lines 159-166]

As for the suggested wording to use "maximum", we think that the changes we made to clarify how hexels were analyzed now makes it clear that "average" is indeed the appropriate term.

[165-166] The wording in this sentence is difficult to interpret. Please rephrase.

Response: We changed the order of the sentence, and along with the following sentence that exemplifies the results, we believe it is now easier to interpret. The relevant text now reads:

> **"**The most widespread contemporary fire year had significantly fewer sites burned ($p \leq 0.05$) than the historical prevalence of such occurrences across all NAFSN sites and the majority of ecoregions (Table 2). For example, while 6% of all NAFSN sites burned in 2020 (the maximum observed from 1984-2022), ≥ 6% of sites burned in 37 out of every 100 years, on average, under the historical reference period (Table 2)." [lines 172-175]

[237] Check citation format.

Response: Thanks for catching that. The format has been fixed.

[334] I suspect that fire regimes were significantly altered at different times across the continent (e.g., earlier in the eastern than western U.S.), but the citation here is only for the inland Northwest. A clearer justification for using a single cutoff at 1880 is needed, with further support from prior research and/or an analysis using the NAFSN itself (e.g., to identify the timing of a significant departure in rate of burning over time within each ecoregion).

Response: The reviewer notes that fire regimes were altered at different times across the continent. This is true, but the reviewer's assumption that fire exclusion started earlier than 1880 in the east is not entirely correct. In fact, fire exclusion did not become apparent until ~1930 for many areas in the east, as post-colonization traditions of burning were widely adopted in the eastern US until this time. We edited the statement and added more text to make the rationale more evident. We also added two additional citations (Flatley et al. 2013 and Lafon et al. 2022) to better represent regions beyond the western US. The relevant text now reads:

> "We used data from the years 1600 to 1880 as our historical period. Our starting period begins in 1600 because the sample density and geographic distribution of fire-scar data limits analysis prior to ~1600 [29]. For consistency across the study area, we used 1880 as the historical period end date even though the effective date of fire exclusion varies across the continent, starting around 1880 in the western portions of our study area [125] and even later in the eastern portions [126,127], as post-colonization traditions of burning persisted in many parts of the eastern United States (US) until the early 1900s [45]." [lines 361-367]
>
> Flatley, W. T., Lafon, C. W., Grissino-Mayer, H. D. & LaForest, L. B. Fire history, related to climate and land use in three southern Appalachian landscapes in the eastern United States. *Ecological Applications* **23**, 1250–1266 (2013).
>
> Lafon, C. W., DeWeese, G. G., Flatley, W. T., Aldrich, S. R. & Naito, A. T. Historical Fire Regimes and Stand Dynamics of Xerophytic Pine–Oak Stands in the Southern Appalachian Mountains, Virginia, USA. *Annals of the American Association of Geographers* **112**, 387–409 (2022).

[Table 3] I suggest using "proportion of years" in place of "likelihood", as it is clearer/ more precise. Correct me if I'm wrong, but my understanding is that likelihood is a population parameter which is being estimated here (e.g., there is some true underlying probability of no sites burning in a given year, and the data represent a single "run" of a particular set of years).

Response: Good call. We changed to "proportion of years when zero sites burned"

[Fig. 4] While this is effective as a conceptual figure, it is also distracting as a reader to wonder why the black line of average annual area burned is represented conceptually, instead of being quantified. It seems like it would be possible to put numbers to that y-axis using the NAFSN data, unless there is something about fire scar records during the 1880-1984 period that precludes such an analysis?

Response: Thanks for this feedback. As this is a conceptual figure, we would rather avoid putting numbers on the y-axis, particularly when the analyses itself is focused on number of NAFSN sites burned (with strong inferences to area burned). Also, other members of our research group are reconstructing annual area burned, and we don't want to negate their efforts by putting numbers here.

Reviewer #2 feedback:

The manuscript examines whether contemporary burned area in North-America forests and woodland deviates (deficit or surplus) from the pre-1880 period, i.e. before generalized fire suppression took place. The authors resort to an impressive dataset (an extensive network of sites containing fire-scarred trees) and acknowledge the limitations (stand-replacing fire regimes eliminating trees). I don't think such limitations have a meaningful impact on the results and conclusions. I don't detect problems in the analyses and find the ms. very well written and clear, perfectly fitting the characteristics of the journal. It is certainly worthy of publication and I have only a few minor concerns, see below.

Response: Thank you for the kind words.

L57-59. I see this paper as potentially being used by deniers of climate change impacts. Consider if possible some rephrasing here to put even more emphasis on impact rather than on burned area.

Response: We share the concern that climate change deniers may misuse our findings. In fact, we spend a large portion of the Discussion to show that fire is indeed unprecedented in its effects on forests and society. Due to the abstract word limit, we are unfortunately not able to expand what we already have, which we think succinctly describes these points:

> **"**Although contemporary fire extent is not unprecedented across many North American forests, there is abundant evidence that unprecedented contemporary fire severity is driving forest loss in many ecosystems and adversely impacting human lives, infrastructure, and water supplies." [lines 56-59]

We also include the following paragraph in the Discussion that touches on climate change and contemporary impacts of fires:

> "Many studies have reported increases in area burned associated with a warming climate over the last few decades across much of North America [9,10,41,52–55]. Considering these studies, forest managers and the general public may be surprised to learn that a significant fire *deficit* persists in many forested ecosystems even as contemporary socio-economic fire impacts are increasing. Our evidence indicates that, even under a warming climate, the rate at which NAFSN sites burned in recent decades has been much lower than historical rates across most of the continent. We attribute this disparity to aggressive fire suppression, disruption of traditional burning, and forest loss and fragmentation from land development and other land uses (e.g., conversion of forests and woodlands to agriculture). Although the substantial reduction in contemporary fire activity compared to historical time periods may seem desirable, it has greatly altered forest composition, structure, and continuity, in many respects adversely. Largely due to these changes, and compounded by climate change, the inevitable wildfires that do occur are often burning with deleterious impacts on forest ecosystems, human communities, and human health (Fig. 4) [56–58]. " [lines 209-221]

L104. The ecological problems/implications of the fire deficit are only vaguely suggested here, but the more casual reader will benefit from additional information. Please be more explicit, as in the Discussion, but it should be stated right from the start and be present in the Abstract.

Response: We devote several paragraphs in the Discussion to the ecological and social problems/implications associated with the fire deficit. As these problems/implications are some of the key take home messages (and are a result of the fire deficit we report in this study), we feel they are best suited for the Discussion, noting however, that many are introduced in this paragraph [lines 107-123] and in the abstract [lines 57-59].

L132. Individual fire perimeters, especially from older dates, can miss unburned patches within the fire area, thus overestimating the NAFSN sites burned %. Could this be an issue here?

Response: We effectively assume a 100% recording rate during the contemporary period, while we might in fact, as the reviewer points out, overestimate the percent of sites burned due to undetected unburned patches. This, coupled with the historical recording rate being <100%, might bias our results toward a slight *underestimation* of the fire deficit, which we prefer to the opposite. The following text in the Discussion make this clear:

> "We then measured the observed number of times burned by counting NAFSN sites that annually intersected contemporary fire perimeters from 1984-2022 [cf. 124]. In doing so, we effectively assume a 100% fire recording rate in the contemporary time period, even though we might be overestimating the number of sites burned because of undetected and unmapped unburned patches. Coupled with the recording rate in the historical time period being < 100%, we are likely *underestimating* any observed fire deficits or other metrics analyzed in this study." [lines 402-407]

L140. Only in the eastern US?

Response: There is no evidence in the literature or the tree-ring fire-scar record that the widespread adoption of traditional burning by European American's occurred elsewhere in our study area (as was the case in the eastern US). The Methods were slightly updated to make this clearer, as follows:

> "For consistency across the study area, we used 1880 as the historical period end date even though the effective date of fire exclusion varies across the continent, starting around 1880 in the western portions of our study area [125] and even later in the eastern portions [126,127], as post-colonization traditions of burning persisted in many parts of the eastern United States (US) until the early 1900s [45]." [lines 363-367]

L167. The 2020 maximum was already mentioned before, I don't think the repetition is needed.

Response: "2020" has been removed, as suggested.

L182. Not all fires scar trees. So, also maybe because the historical burning figures are likely to be underestimates?

Response: Yes, this is correct. In fact, we have text in the Discussion describing this phenomenon, as follows:

> "We then measured the observed number of times burned by counting NAFSN sites that annually intersected contemporary fire perimeters from 1984-2022 [cf. 124]. In doing so, we effectively assume a 100% fire recording rate in the contemporary time period, even though we might be overestimating the number of sites burned because of undetected and unmapped unburned patches. Coupled with the recording rate in the historical time period being < 100%, we are likely *underestimating* any observed fire deficits or other metrics analyzed in this study." [lines 402-407]

L197. No references for such accumulated body of evidence?

Response: Thanks for pointing this out. We now include five citations to support this statement.

L229-243. Eastern US is presented separately and with detail. Why not a more balanced treatment of North America geography?

Response: The reviewer is correct that we provide detailed effects of fire exclusion in the eastern US. However, we also provide detailed effects of fire exclusion in western North America in the preceding paragraph [lines 223-238]. As such, we believe there is balanced treatment between the two regions representing the vast majority of the fire-scarred sites. The two paragraphs encompass the range of fire-suppression effects: increasing fuel loading with greater fire severity versus mesophication with reduced flammability.

L237. A reference that should be a number.

Response: Thanks for catching that. The format has been fixed.

L248. Maybe add the recent fire in Jasper, Canada?

Response: We added Jasper, as suggested.

L267. I'm assuming it's biomass decomposition, but might not be totally clear to some readers.

Response: We changed this to "decomposition of fire-scarred stumps, dead trees, and downed logs".

L342. Are the two Canadian sources consistent with each other?

Response: Although there are slight differences between the Canadian National Fire Atlas and the National Burned Area Composite at finer scales, the broader-scale fire patterns are nearly identical, in that there is substantial overlap in mapped perimeters for the two datasets where

their time periods overlap. According to our Canadian co-authors who are familiar with these datasets, the differences between the two datasets will have no measurable impact on our results.

L349. But maybe that was not the case before 1880 and so comparisons might be impacted?

Response: Please note that the historical fire scar record would include small fires as long as they met the criteria (at least two scarred trees per site or 10% of trees recording a fire in a given year [lines 354-358]). As such, we believe our statement here is valid given that the vast majority of contemporary area burned results from large fires.

Figure 4. Just a matter of taste but I would prefer a font not looking as hand-written. Quantification added to the annual area burned axis would more effectively communicate the results.

Response Thanks for this feedback. As for the font, we prefer the hand-written look. Also, as this is a conceptual figure, we would rather avoid putting numbers on the y-axis, particularly when the analyses itself is focused on number of NAFSN sites burned (with strong inferences to area burned). Also, other members of our research group are reconstructing annual area burned, and we don't want to negate their efforts by putting numbers here.