

Fire smoke in the U.S. disproportionately damages senior citizen, Native American, and Black communities

Corresponding Author: Dr Luke Dennin

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

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 Dr Dennin,

Your manuscript titled "Modeling wildland fire smoke damages in the U.S. and unpacking impact disparities by social vulnerability" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In particular for publication in Communications Earth & Environment, we ask the authors to

- (1) provide novel and firmly supported insight into the relationship between social vulnerability, wildfire and prescribed fire smoke exposure in the US.
- (2) Outline your methodology in detail, including explanation of the economic cost and cost of premature death, clarify per-capita weighting and possible over-valuation, and justify use of only one year of data for the analysis; and
- (3) Provide a sensitivity analysis and discussion about model assumption and uncertainty.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred.

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Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

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orcid.org/0000-0003-2809-0434

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Summary:

This paper combines public data on emissions from wildfires (WFS) and prescribed burns (PBS), census tract data on population and social vulnerability, and spatially explicit chemical modeling to estimate the damages resulting from human exposure to air pollution attributed to fires. Importantly, the modeling allows the authors to attribute both the sources of smoke and the ultimate recipients of smoke derived from controlled (PBS) and uncontrolled (WFS) burning, as well as to begin to untangle the relationships between social vulnerability, exposure to wildfire smoke, and economic damages.

This work is timely and critically important, given the increasing frequency and intensity of fire activity in the US. Results hint at regional differences and dimensions of social vulnerability that are important to consider when planning interventions both in fire management and in public health. Overall, I believe this paper is relevant to a broad audience and appropriate for publication in Communications Earth & Environment. That said, I would recommend several adjustments to clarify and better contextualize the results, which will aid understanding and accurate interpretation of the paper by non-expert readers. Please see my detailed comments below.

General Observations:

(1) Abbreviations: I appreciate the list of abbreviations early in the document, but it would be helpful to the average reader if you take care to spell out acronyms in the first instance that they are presented. I think the abstract and figure captions should stand alone, without searching for acronyms to enable interpretation. This includes the first mention of AP4T in the Introduction, which is eventually defined in the Methods section.

(2) Figure Captions: Please revise the figure captions to be consistent with the Communications Earth & Environment Style Guide. Specifically, consider rewriting the captions in sentence case rather than title case to make them easier to read. The bolded text (nominally the caption) reads more like figure titles, which seemed unnecessary and hard to read in some cases. I found the Notes to be quite informative and more useful than the captions, though they were sometimes repeated in the body of the text (Results and Methods). There is a lot of great information in there, but I think integrating Notes and Sources into a clear, concise caption will be extremely helpful for readers trying to navigate very complex figures.

(3) Sub-headings: I found the use of Title case in long headings within the text to be distracting, but I realize this is personal preference and did not see specific guidance on this in the style guide. If you opt to maintain Title case, I suggest NOT capitalizing minor words like “per”, “by”, etc. to make it easier to read (following APA style guide).

(4) Figures: In general, there is far too much information squeezed into each multi-panel figure, especially Figures 2-3. As I understand it, Articles are allowed up to 10 visual elements for 5000 words. Consider either simplifying or splitting up the multi-panel figures to help readers process them more readily. For example:

- Fig. 3a could stand on its own and eliminate the need for Fig. 2a, since it contains the same information.
- If you broke up the remaining panels in Figures 2 and 3 to create stand-alone figures, I believe you would have <10 visual elements. An advantage is that the figures could then be placed close to the relevant explanatory text in the Results section, which both helps with flow/interpretation and could eliminate the need for repetition in the Notes.
- Figures 1 and 4 are less overwhelming and could be effective as 2-panel figures.
- If you needed to eliminate a visual element for space, Table 2 may be unnecessary or could go to the Supplemental.

(5) Premature deaths: I'll preface this comment by saying that I am not an expert in public health nor estimation of economic damages from exposure to natural/climate hazards. Nevertheless, I failed to understand the logic behind the costing of premature deaths and it distracted me throughout the manuscript. My confusion is rooted in two issues that were not clearly explained in the text:

- First is the definition of “premature death”. From a public health perspective, I would think that a premature death is one that occurs earlier than the life expectancy of a given population or sub-population. So, if the average life expectancy of a US person was 78.6 years in 2017 (81.1 for females, 76.1 for males), it follows that a premature death would be one occurring earlier than that age. Yet your results indicate that White people over 85 account for 25% of total WFS damages. Why?
- Second, I would assume that the economic valuation of smoke damages must be connected to the cumulative “lost time” due to an early death. By that logic, premature deaths in younger people cause greater economic damage given the many years of foregone income, productive activities, and years of life. How is it, then, that the incurred smoke damages per capita

increase exponentially with every decade of life? It makes sense that it might increase in total, given that more elderly people are likely to succumb to smoke exposure. But the per capita weighting suggests that your model (over)values the lives of people that have already exceeded the life expectancy (>80 yo) relative to younger people (e.g., in their 30s to 70s).

Please clarify! If this approach is indeed the norm for this type of estimation, I would like to see a clear description of the logic and assumptions that support this.

Detailed Comments

ABSTRACT:

- * Line 4: Spell out AP4T or just avoid the acronym...
- * Line 9: Replace "fire smoke-polluted" with "smoke-polluted". You have already defined that you are talking about smoke from fires. The "fire smoke" wording strikes me as unnecessarily awkward here and throughout the manuscript. To avoid repeating it, you could state something like "fire smoke (hereafter referred to as smoke)" early on?
- * Line 10, Simpson's Paradox: Will the average reader know what this is? If you are defining it immediately after, perhaps include a colon instead of a semicolon to emphasize that?

INTRODUCTION:

- The Introduction could be focused a bit more to outline what we know and describe the gaps in our knowledge, including the key questions or research objectives addressed by this study. How does this study increase our understanding and what were your expectations or hypotheses going into it? It's there but could be emphasized more. And if you state it very clearly in the Introduction it will better frame your Results and contextualize your Discussion.
- * Line 24-26: For clarity, replace "are those from exposure to the resulting smoke" with "there are substantial costs associated with exposure to the resulting smoke."
- * Line 29-30: ["people of a lower socioeconomic status"]: Relative to what? Lower than what?
- * Line 44: Replace "and lesser among" with "compared with..."
- * Line 48: Suggest adding "In this study, we expand..." for clarity.
- * Line 48: Please spell out AP4T to define, since this is the first instance when it is mentioned. I think the first time you write it out is in Line 473 of the Methods. No need to elaborate, but it's helpful to know that it is a new version of the "Air Pollution Emissions Experiments and Policy model" early on since it is a major focus of the paper...
- * Line 96: "When controlling for age": Per my comments above, it is unclear how you control for age and why you don't control for age in all analyses.

RESULTS

- Figure 2: This is far too much information for one figure. It is really very useful, but poorly explained in the caption and hard to follow when all lumped into a single figure. I would break these up into multiple figures and/or send some panels to the Supplemental Information, as noted previously.
- Figures 2-3: Have you considered rotating these panel figures 90 degrees? Especially for the panels in Figure 3, it could make it easier to read the labels and interpret the directionality (left of line is negative, right of line is positive). Having the figure labels on top and oriented horizontally will also facilitate interpretation. Just a thought...
- * Lines 224-225: If California has such a disproportionate effect, perhaps it should be analyzed separately to enhance interpretability?
- * Line 228: The word "effected" should be "affected" here.
- * Line 244: You spelled out VSL earlier but have not defined it for the reader. I would specify, "since the VSL is treated as constant at \$XXX per capita" (REF) just to give readers a foothold for understanding your approach and Results before getting to the Methods. (Then again, if it varies with age perhaps this may confuse matters...).
- * Line 266-267: This seems like an opportunity to consolidate into one figure, with a nice clear caption, rather than repeating information in Fig. 2a and Fig 3a.
- * Line 297-298: How would you interpret this finding? Would it be worthwhile to explore in the Discussion?
- * Line 334: As noted above in the General Observations, this seems counter-intuitive. Shouldn't these costs be scaled somehow to account for the de facto life-years lost due to premature death? If a 40-yo with an 80-yr life expectancy dies prematurely shouldn't the cost be higher than a 79-yo with an 80-y life expectancy?
- * Lines 378-385: [Accounting for the influence of age...] As noted before, I am not an expert in public health and recognize that I may be missing something fundamental, but I don't understand how the costing of "premature deaths" due to fire is performed. Does it account for differences in life expectancy among groups? How is it, for example, that if the population life expectancy is <80 years we have the highest cost of exposure attributed to people >80? It would seem to me that there should be a way to quantify the lost years of life so the earlier deaths due to fire would be costlier than the later ones, but this seems to be precisely the opposite of the logic used.
- * Lines 386-394: This goes beyond Results and starts bleeding into the discussion, interpretation, and recommendations. Perhaps, it belongs in the Discussion section?

DISCUSSION:

- Overall, I think the Discussion needs to be strengthened. I appreciated the uncertainty modeling and comparison with other approaches, but I almost think that could go into the Methods, while several other elements of the Methods and Results would greatly enrich the Discussion, as noted in those sections.
- More to the point, I think the authors have missed an opportunity here for a more nuanced discussion of the policy implications of this work. They could also do more to explore the potential explanations underlying observed trends, which could motivate future work and help readers think about the complex information presented.

* Line 455-456: "policymakers should prioritize measures protecting older Americans".

I am not suggesting that older Americans should not be a priority, but I do question ranking this population first based on the economic valuation (for all the reasons mentioned above). If your method "over-values" the lives of senior citizens, of course you would conclude that they are the priority. The logic seems circular and, perhaps, is not the most interesting result of your work.

For example, I would be more interested in a careful exploration of the spatial differences in exposure and vulnerability of populations, particularly for prescribed burns which are intentional and reflect management decisions that may intersect with SVIs. Why or how are those decisions made? Is there any information to explain why PBS is more likely to occur in certain regions vs. others? What about WFS? I miss some analysis of these differences and discussion of their broader implications, although I leave it to the authors to identify the salient points.

* Line 462: Delete "also" to avoid repetition.

METHODS

* Line 482: Helpful and intuitive definition of receptors here, which I missed earlier in the paper. It took some time to get my head around the sources vs. receptors language. It would help to have it explained earlier in the Results or Introduction using an "exporter" vs. "importer" analogy or a "where damages are generated" vs. "where damages are experienced".

* Line 619: Change "by" to "but"

* Line 621-625: This is a critical point of Discussion and should not be buried in the Methods. It really needs to come out strongly in the Discussion and Results, so readers don't jump to the wrong conclusions. In general, I think much of the content in "Health Effects Modeling Limitations" should be considered directly in the Discussion rather than in Methods. It really is the crux of the paper/implications.

* Line 628 and 631: I think "infiltration" should be "filtration" in this context? I am assuming you mean the systems that can filter particulate matter in indoor HVAC systems?

* Line 634: Change "effect" to "affect"

* Line 669: Repetitive title. Perhaps "Social vulnerability variables characterizing..." would be clearer?

Thank you for the opportunity to review this ambitious and interesting paper. I learned a lot from reading it and hope my comments are useful.

Reviewer #2 (Remarks to the Author):

This manuscript makes an important contribution in delineating the inequitable impacts of wildland fire smoke in the contiguous US through modeling. It builds on previous work because it narrows the spatial scale of modeling capabilities to the census tract level, a refinement that is key to understanding more precisely the varying impacts of pollutants on different demographic populations. The study also sought to distinguish between smoke from wildfires and prescribed fires, which have very different causes (and consequences). The authors found that age is a primary determinant of wildland fire smoke damages, but after accounting for age, American Indian or Alaskan Natives and Black persons were disproportionately affected. These findings point to the importance of managing risks for senior citizens and demonstrate the nuance that is necessary in understanding racial and ethnic dimensions of social vulnerability. I commend the authors on a well-written manuscript overall; the narrative flowed well, and each time I thought to myself that a limitation or point of nuance should be discussed, the manuscript addressed that concern in the subsequent paragraph. I have only a few minor suggestions for revisions, as follows.

L6 and L400-401 The phrasing here is confusing, perhaps misleading. You emphasize that wildfire smoke is responsible for almost half of the damages, which is substantial, but then go on to say that prescribed burning is responsible for more than half, which is perhaps even more shocking. Are these the only two possible categories? Perhaps it would be better to put these in the context of other damages from particulate matter? If we are comparing only WFS and PBS, I would actually expect that damages are much higher from WFS. But perhaps it would depend on the year. Either way, I think the points of emphasis should be reworded or recontextualized here.

L13 insert "smoke" before the word 'damages'

L19 when you cite that wildfire emissions are increasing, what time period are you referring to? I assume recent decades, but not a period of more than a century. This context is important.

L27 intentionally "and legally"

L50 why was the analysis focused only on 2017? I can see how it would be much more robust to include some other years,

including more recent years that had even higher levels of smoke damages. I understand that the study must be bounded somewhere, but please speak to the decision to focus on 2017, and please explain any limitations of not including other years of data.

L68-69 increased morbidity and mortality risk compared to what? Other pollutants? Or just compared against clean air?

L73 replace 'exposures from' with 'exposures to'

L83 It is confusing to have this paragraph come in the introduction section. I suggest simply moving up the RESULTS heading above this paragraph.

L207-213 Aren't the people most affected by PBS also likely to be the people receiving the most benefit from prescribed burning (eg fire hazard reduction)? This should be explained here.

L346 Figure 4 a and b. I think this figure would be more interpretable if the colors were scaled along a gradient with age. Age is a continuous variable, after all. The categorical color scheme makes it difficult to interpret.

L421-422 suggest rewording. In the prescribed fire management world, smoke dispersal is considered a positive thing, because smoke lofting and becoming less concentrated means it will have a less detrimental effect on people (in the vicinity). Perhaps it would be better to say smoke from wildfires 'reaches farther' than from prescribed burns?

L466 Could elaborate on the actions that should be taken.

L499 Which MTBS data-- simply the location of fire perimeters? Or fire severity data? Or other?

Somewhere in your discussion, you might more fully discuss literature that clarifies differences in the quality/impact of prescribed fire smoke versus wildfire smoke. See for example Jaffe et al 2020 (who also focus on the US in 2017). I am not sure these nuances are captured in your modeling approach, which would be worth discussing.

Jaffe, D. A., O'Neill, S. M., Larkin, N. K., Holder, A. L., Peterson, D. L., Halofsky, J. E., & Rappold, A. G. (2020). Wildfire and prescribed burning impacts on air quality in the United States. *Journal of the Air & Waste Management Association*, 70(6), 583–615. <https://doi.org/10.1080/10962247.2020.1749731>

Reviewer #3 (Remarks to the Author):

In this paper, Dennin et al. used an integrated approach (reduced complexity air quality model + concentration-response function + VSL) to calculate the impacts of wildfires and prescribed fires in 2017, on surface PM_{2.5} concentrations, premature mortality, and monetized damages in the US. They also calculated the correlation between different socio-demographic variables and the smoke damage.

Overall, I think this is an interesting paper that addresses an important and understudied question. While there is a growing literature on air pollution damages from wildfires and prescribed fires, less is known about the correlation between wildfire damages and socio-economic variables. Reduced complexity methods for wildfire smoke damage calculations are also desirable given their low computational burden. However, given the substantial uncertainty in modeling wildfire effects on air quality and health damages, I think the paper would benefit from extensive sensitivity analyses and discussions of the model assumptions and uncertainty behind these results. I hope my comments can help improve this paper.

Major:

1) Given the importance of different model assumptions in calculating wildfire damages, I suggest the authors move these assumptions more upfront in the introduction sections. Especially, two key assumptions need to be included given their importance for calculating health damages of smoke: what emissions inventory for wildfire/prescribed fires and what concentration-response functions (linking smoke PM_{2.5} and mortality) are used.

2) For the air pollution impacts, it is widely acknowledged that the estimated fire impacts on air quality are highly uncertain, due to uncertainty in underlying fire emission inventories. For example, prior studies have noted that estimated fire smoke PM_{2.5} can differ by several magnitudes across different fire emission inventories, even when using the same air quality model. Given this large uncertainty, I think it is important to further discuss the potential uncertainty of the emissions inventory used in this work. While a full comparison of the NEI emission inventory with all other leading fire emission inventories may not be necessary, perhaps the authors could also compare NEI emission estimates to at least one of the leading estimates such as GFED4s? Are there any papers that the authors can cite on this?

3) Given the uncertainty of fire emission inventory, I think the paper would benefit from analysis that may help benchmark the estimated PM_{2.5} effects. Perhaps the authors could directly compare their estimated annual PM_{2.5} effects to the data from Childs et al. and existing CTM work that also focused on 2017 (for example Xie, Y., Lin, M., & Horowitz, L. W. (2020). Summer PM_{2.5} pollution extremes caused by wildfires over the western United States during 2017–2018. *Geophysical*

Research Letters, 47(16), e2020GL089429.). The comparison between Childs et al in terms of health damages is very helpful, but a direct comparison in terms of annual PM2.5 concentrations would also be useful.

In Table 1, it is incorrect to say that Childs et al estimates only include wildfire effects. Their estimate is simply based on HMS plume and surface PM data, so as long as the prescribed fire is large enough to be captured by the HMS plume, their data will potentially capture that. Perhaps the authors could edit their footnote in Table 1 to be more accurate.

4) As mentioned above, I think Reduced complexity methods for wildfire smoke damage calculations are also desirable given their low computational burden. Given the lack of similar tools or the studies applying such tools to fire studies, I think this study can make potential contributions in discussing the advantages and uncertainties of applying existing RCMs to wildfires. Modeling wildfire effects on PM2.5 is particularly challenging, even for full-complexity chemical transport models. One of the challenges is on the fire injection heights, which the authors discussed on L536. What is the assumption of APT4 on the emission heights? I think it would be useful to include a standalone paragraph to discuss the benefits and challenges in terms of applying RCMs to fire impact studies in the main text, and the authors could elevate some information in the method section here given its importance to understanding the results.

5) The correlation results between SVI and smoke damage are very interesting, and I appreciate the analysis of the different themes which shed more light on this aggregate metric. However, I find it a bit challenging to reconcile the correlation results and the section below on age/race-specific baseline mortality rates. I am a bit confused as to what “vulnerability” means in the calculation. It seems like the “vulnerability” only influences the damage through the baseline mortality rate, is this correct? It appears that the authors are using county-specific mortality results so that perhaps capture some of the vulnerability in addition to age/race (e.g., low-income counties may have higher baseline mortality results). How much variations do age and race alone explain the aggregate correlation between SVI and smoke damage? Could the authors clarify this?

6) The results section is quite long and hard to follow as there are too many figures and each figure was discussed with great detail. Some of the discussions also feel repetitive (for example figure 2 vs figure 3). If I understand correctly, figure 3 results only differ from figure 2 as the damage is now normalized by population. If that’s the case, would it make sense to just include one of the two figures in the main text (and the other one in SI)?

7) The fire-specific results from L161-165 are very interesting. I think it would be helpful to illustrate more about the method behind these results. For example, how did the authors link the gridded NEI emission dataset to specific fire events? It would also be interesting if the authors had more results to show on this in addition to the Tubbs fire (on that note, I was curious why the authors chose the Tubbs fire). For example, it would be interesting to calculate the damages of the top 10 fires in 2017, and compare their health damages to the direct damages (like what the authors did for the Tubbs fire).

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Decision Letter:

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Dear Dr Dennin,

Your manuscript titled "Modeling wildland fire smoke damages in the U.S. and unpacking impact disparities by social vulnerability" has now been seen by our 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.

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Paula Ribeiro Prist, PhD
Editorial Board Member
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Martina Grecequet, PhD
Senior Editor,
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear Authors,

I appreciate your thoughtful and comprehensive responses to the reviewer comments. I learned a great deal from your explanations and found the revised manuscript to be much improved. For my part, I especially appreciated the addition of the VSLY analysis and found the revised figures to be attractive, informative, and much easier to interpret. Thank you for addressing my concerns and providing relevant information that I needed to better contextualize your results. I have no further comments at this time, and wish you all the best with publication of this timely and important analysis.

All the Best,
Reviewer 1

Reviewer #2 (Remarks to the Author):

Thank you for your very thorough revisions and detailed rebuttal

Reviewer #3 (Remarks to the Author):

I appreciate the detailed efforts from the authors to address my comments. I think the paper has substantially improved compared to the prior version. I have no further comments.

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Reviewer 1 Responses

R1.1) Abbreviations: I appreciate the list of abbreviations early in the document, but it would be helpful to the average reader if you take care to spell out acronyms in the first instance that they are presented. I think the abstract and figure captions should stand alone, without searching for acronyms to enable interpretation. This includes the first mention of AP4T in the Introduction, which is eventually defined in the Methods section.

We have updated the Abstract to avoid acronyms, except for U.S., which we contend is well recognized and understood as “the United States” among readers. Specifically:

- “AP4T” is now referred to as “an integrated assessment model” (L4).
- “AIAN” is now written out as “Native American” (L13).

We have updated the Figure Captions to avoid acronyms, again except for U.S. The other exception is AP4T, as it is the name of the model rather than an acronym. That said, we have moved the description of AP4T, originally in the *Methods* section, to the *Introduction* (L49-L51).

R1.2) Figure Captions: Please revise the figure captions to be consistent with the Communications Earth & Environment Style Guide. Specifically, consider rewriting the captions in sentence case rather than title case to make them easier to read. The bolded text (nominally the caption) reads more like figure titles, which seemed unnecessary and hard to read in some cases. I found the Notes to be quite informative and more useful than the captions, though they were sometimes repeated in the body of the text (Results and Methods). There is a lot of great information in there, but I think integrating Notes and Sources into a clear, concise caption will be extremely helpful for readers trying to navigate very complex figures.

We have revised the figure captions to align with the *Communications Earth & Environment* style guide. The captions are now in sentence case, with the notes and sources integrated into a clear, concise format. We have attempted to remove some unnecessary replication of information while also following the guidance that “Readers should be able to understand the Figure without reference to the main text” instructed by the style guide.¹ We believe these edits significantly enhance the readability and will help readers better understand the figures.

R1.3) Sub-headings: I found the use of Title case in long headings within the text to be distracting, but I realize this is personal preference and did not see specific guidance on this in the style guide. If you opt to maintain Title case, I suggest NOT capitalizing minor words like “per”, “by”, etc. to make it easier to read (following APA style guide).

We have updated the sub-headings to not be title case.

¹ <https://www.nature.com/commsenv/submit/submission-guidelines>

R1.4) Figures: In general, there is far too much information squeezed into each multi-panel figure, especially Figures 2-3. As I understand it, Articles are allowed up to 10 visual elements for 5000 words. Consider either simplifying or splitting up the multi-panel figures to help readers process them more readily. For example:

- a. Fig. 3a could stand on its own and eliminate the need for Fig. 2a, since it contains the same information.**

The information previously presented in Original Fig. 3a and 3b and Original Fig. 2a and 2b has been synthesized into the standalone New Fig. 2a and 2b. We appreciate the reviewer's insightful comment, as we believe this revision significantly enhances the clarity and overall quality of the paper.

- b. If you broke up the remaining panels in Figures 2 and 3 to create stand-alone figures, I believe you would have <10 visual elements. An advantage is that the figures could then be placed close to the relevant explanatory text in the Results section, which both helps with flow/interpretation and could eliminate the need for repetition in the Notes.**

We have made the following updates in response to this comment:

- Original Fig. 3a and 3b and Original Fig. 2a and 2b are now presented as New Fig. 2a and 2b.
- Original Fig. 2c and 2d are replaced with New Fig. 4.
- Original Fig. 3c and 3d are now presented separately as New Fig. 5a and 5b.
- We have added New Fig. 3a and 3b, which focus on smoke exposure and social vulnerability by census division. This addition was influenced by *Comment R1.8.c*, as further explained in our response to that comment.

We thank the reviewer for this valuable suggestion. We believe these figure updates significantly improve the paper, making the visuals clearer and easier to interpret.

- c. Figures 1 and 4 are less overwhelming and could be effective as 2-panel figures.**

We agree with the reviewer and have retained Fig. 1 and Original Fig. 4 (now New Fig. 6) largely as they appeared in the original manuscript. However, New Fig. 6 has been updated in response to *Comment R2.10*.

- d. If you needed to eliminate a visual element for space, Table 2 may be unnecessary or could go to the Supplemental.**

As noted by the reviewer, the style guide allows for ten display items per ~5,000 words.² With the changes discussed as part of this response (*Comment R1.4.a-c*), we count six figures. We also add two other figures to the *Methods* section—i.e., New Fig. 7 and New Fig. 8, which focuses on alternative health effects and valuation modeling approaches. Adding the two tables in the *Methods* section results in ten total display items. Moreover, we find that defining the variables comprising the Social Vulnerability Index in the manuscript is critical, given its importance to the study. That said, we appreciate the reviewer’s suggestion.

R1.5) Premature deaths: I’ll preface this comment by saying that I am not an expert in public health nor estimation of economic damages from exposure to natural/climate hazards. Nevertheless, I failed to understand the logic behind the costing of premature deaths and it distracted me throughout the manuscript. My confusion is rooted in two issues that were not clearly explained in the text:

- a. First is the definition of “premature death”. From a public health perspective, I would think that a premature death is one that occurs earlier than the life expectancy of a given population or sub-population. So, if the average life expectancy of a US person was 78.6 years in 2017 (81.1. for females, 76.1 for males), it follows that a premature death would be one occurring earlier than that age. Yet your results indicate that White people over 85 account for 25% of total WFS damages. Why?**

Premature death is a widely used concept in the literature exploring public health impacts from environmental pollution [18]. In this study, premature death refers to a death that occurs earlier than it would have absent exposure to air pollution. We have clarified the definition of “premature mortality” upon its first mention to ensure understanding (L24-L25).

This definition is based on relative mortality risk—i.e., the chances of all-cause mortality occurring in an exposed group versus the chances in a non-exposed group (L669-L671). This is a metric studied by the epidemiological studies employed herein [19], [20]. Equation 5 of the manuscript takes the relative risk from Krewski et al. (2009) to compute β , which is an input to the log-linear concentration-response function (Equation 4 of the manuscript). The relative risk in Krewski et al. (2009) is 1.06 per 10 $\mu\text{g}/\text{m}^3$ of long-term $\text{PM}_{2.5}$ exposure. We can compute the change in mortality risk (Δy) using this information, the change in $\text{PM}_{2.5}$ modeled by AP4T, and the baseline all-cause mortality rate. Let us consider an example where there is a 1 $\mu\text{g}/\text{m}^3$ increase (ΔPM_{AP4T}) and the baseline mortality rate (y_0) of the population of focus is 1 in 100, or 0.01:

$$\Delta y = y_0 \left[1 - \frac{1}{\exp(\beta \times \Delta PM_{AP4T})} \right]$$

² <https://www.nature.com/commsenv/submit/submission-guidelines>

$$\Delta y = 0.01 \left[1 - \frac{1}{\exp\left(\frac{\ln(1.06)}{10 \mu g/m^3} \times 1 \mu g/m^3\right)} \right]$$

$$\Delta y = 0.01 \left[1 - \frac{1}{\exp(0.00583)} \right] = 0.01 \times 0.00581 = 0.000581$$

A “premature death” is one that occurs because of Δy : the additional 5.81 in 10,000 chances of mortality for the population of focus in the example.

Critically, populations have unique baseline mortality rates (y_0), particularly those that differ by age. A “premature death” as defined herein is a death occurring because of the increase in mortality risk (Δy) from that unique baseline. As such, both a 30-year-old and an 85-year-old can experience premature mortality from increased PM_{2.5} exposure because their risk increases from their individual baseline levels, regardless of their age.

- b. Second, I would assume that the economic valuation of smoke damages must be connected to the cumulative “lost time” due to an early death. By that logic, premature deaths in younger people cause greater economic damage given the many years of foregone income, productive activities, and years of life. How is it, then, that the incurred smoke damages per capita increase exponentially with every decade of life? It makes sense that it might increase in total, given that more elderly people are likely to succumb to smoke exposure. But the per capita weighting suggests that your model (over)values the lives of people that have already exceeded the life expectancy (>80 yo) relative to younger people (e.g., in their 30s to 70s).**

The argument that monetizing mortality risk uniformly across different age groups, such as young adults and senior citizens, is flawed due to varying life expectancies is indeed valid for multiple reasons. These include variation in remaining life expectancy, differences in risk tolerance, and disposable income [12].³ Assigning value based on expected remaining years lost has been employed in prior research [23]. This clearly changes the monetary damages by age group. The value of a statistical life year (VSLY) approach does recognize differences in remaining life years, but it does not account for differences in risk tolerance or income,⁴ both of which are important drivers of an individual’s willingness to pay (WTP) for a reduction—or willingness to accept an increase—in mortality risk [8], [9], [10]. In a theoretical sense, it would be ideal to know each individual’s personal WTP. In the absence of this information, the most ethically-grounded approach is to use a uniform VSL, assuming that everyone’s WTP is equal regardless of age, health status, income level, or other factors.

³ This is true up until a certain point. To illustrate, wealth per capita throughout the U.S. nearly doubles from 40-54 to 55-69 but then falls slightly from 55-69 to 70+, considering 2017 data [21], [22].

⁴ See Hammitt (2007) for a summary of various studies that looked at how the VSL and VSLY vary with age [12].

That said, we reiterate the validity of the original argument. While the assessments of distributional impacts concerning premature mortality remain unchanged regardless of the valuation approach, those concerning economic damages will vary. We conduct a sensitivity analysis using the VSLY approach, as outlined by the Department of Health and Human Services (HHS) [13]:

- We derive expected remaining life years by age group using the Centers for Disease Control and Prevention’s (CDC’s) life table for the total population in 2017 [24]. Expected remaining life years are survivor-weighted averages of the ages bounding the age group—e.g., expected remaining life years for 30 to 34 is the survivor-weighted average for 30- and 35-year-olds. The exception is 85+, which is a survivor-weighted average of expected remaining life years for 85-, 90-, 95-, and 100-year-olds.
- We calculate the VSLY considering the present value of remaining life years for a 40-year-old-individual (the average age reported in VSL studies) using a 3% discount rate [13]. Discounting assumes every person’s next year (i.e., 2018) is more valuable than every person’s year in a decade or two (i.e., 2027 or 2037).
- In 2017, a 40-year-old was expected to live another 40.7 years, which equates to a present value of 23.3 years. Dividing the \$9.97 million VSL by 23.3 years yields a VSLY of \$428 thousand (2020 U.S. dollars).
- We compute the value of mortality risk by age group using the group’s expected remaining life years and the VSLY, again considering a discount rate of 3%.

The following equation (i.e., present value of an annuity) is used to compute the present value of remaining life years and the value of mortality risk.

$$PV = A \frac{\left[1 - \frac{1}{(1+r)^n}\right]}{r}$$

In the equation (added as Supplementary Equation 18), A is the annuity (either 1 year or the \$428 thousand VSLY), r is the discount rate of 3%, and n is the number of expected remaining life years. Supplementary Table 36 reports the expected remaining life years [24], the present value of expected remaining life years, the nominal value of mortality risk, and the present value of mortality risk by age group.

We have incorporated analyses using the VSLY approach into the *Methods* section of the manuscript (New Fig. 7 and New Fig. 8). We also refer to the VSLY approach in the *Discussion*, particularly where we discuss the nexus of age, race/ethnicity, and social vulnerability in relation to the influence of senior citizen damages on the distributional impacts (L489-L493). The details discussed herein have been added the Supplementary Information. We believe this additional component of the paper greatly enhances our quantitative analysis and qualitative discussion and thank the reviewer for encouraging us to further consider this alternative approach.

R1.6) Detailed Comments: Abstract

- a. **Line 4: Spell out AP4T or just avoid the acronym...**

AP4T is now referred to as “an integrated assessment model” (L4).

- b. **Line 9: Replace “fire smoke-polluted” with “smoke-polluted”. You have already defined that you are talking about smoke from fires. The “fire smoke” wording strikes me as unnecessarily awkward here and throughout the manuscript. To avoid repeating it, you could state something like “fire smoke (hereafter referred to as smoke)” early on?**

We have accepted the reviewer’s suggested edit (L9). While we appreciate the suggestion, we do not believe there is a need to specify the smoke as being from the fires. It is reasonably intuitive.

- c. **Line 10, Simpson’s Paradox: Will the average reader know what this is? If you are defining it immediately after, perhaps include a colon instead of a semicolon to emphasize that?**

We have removed the mention of Simpson’s Paradox from the *Abstract*. We have also provided a definition of Simpson’s Paradox where it is mentioned later on in the manuscript (L336-L339).

R1.7) Detailed Comments: Introduction

- a. **The Introduction could be focused a bit more to outline what we know and describe the gaps in our knowledge, including the key questions or research objectives addressed by this study. How does this study increase our understanding and what were your expectations or hypotheses going into it? It’s there but could be emphasized more. And if you state it very clearly in the Introduction it will better frame your Results and contextualize your Discussion.**

We have revised the *Introduction* to enhance focus and streamline our key points. This update more effectively highlights our study’s contribution to the field. Below is an overview of the revised structure and rationale for each paragraph:

- **Paragraph 1:** Introduces air pollution from wildland fires as an increasing threat to public health.
- **Paragraph 2:** Discusses the well-established consensus in the literature that air pollution in the U.S. disproportionately impacts vulnerable populations, particularly People of Color and those of a low socioeconomic status. We specifically reference studies on anthropogenic emission sources, with emphasis on Tessum et al. (2021), while reserving discussion of fire smoke for Paragraph 5. Additionally, we highlight that the health impacts of air pollution disproportionately affect vulnerable populations, including senior citizens. In this context, we signal our hypothesis: we expect health effects to disproportionately exacerbate damages among vulnerable groups, regardless of exposure levels.

- **Paragraph 3:** This paragraph introduces our research question and model. We integrate citations to Paoella et al. (2018) and Spiller et al. (2021), which informed key model design choices, directly within this paragraph rather than placing them, along with an associated discussion, in a separate section as was done in the original manuscript.
- **Paragraph 4:** Reviews the existing air quality modeling literature on fire smoke, highlighting the two main approaches commonly used. We highlight that our use of a reduced-complexity model (RCM) to study emissions from wildfire and prescribed burns is innovative. Specifically, the advantage of using an RCM over other models allows for direct linkages between granular emission sources (i.e., location and fire type) and pollution receptors across varied scenarios.
- **Paragraph 5:** Returns to the issue of air quality impacts from wildland fires, emphasizing the existing uncertainty about which populations are most exposed to smoke. Our contribution is highlighted by our discussion of calculating social costs for populations differentiated by various aspects of vulnerability, as well as our investigation into the drivers of distributional impacts. Lastly, we believe that the final two sentences of this paragraph effectively summarize the impact of our work in this space.
- **Paragraph 6:** Reports the primary findings, as instructed by the *Communications Earth & Environment* style guide. According to the guide, “The final paragraph should be a brief summary of the major results and conclusions.”⁵

Overall, this revision enhances the *Introduction*’s focus and clearly frames our key contributions. We also avoid cumbersome hypotheses—such as the idea that disparities in smoke damages will exacerbate any observed disparities in exposure—opting instead for a logical progression of what is known and what our paper contributes. We appreciate the reviewer’s suggestion to strengthen this section and believe that the revision successfully meets that goal.

- b. Line 24-26: For clarity, replace “are those from exposure to the resulting smoke” with “, there are substantial costs associated with exposure to the resulting smoke.”**

We have accepted the reviewer’s suggested edit (L28).

- c. Line 29-30: [“people of a lower socioeconomic status”]: Relative to what? Lower than what?**

We have updated “people of a lower socioeconomic status” to “people of low socioeconomic status” (L32-L33). We note that this is suggested by the American Psychological Association’s *Inclusive Language Guide*.⁶

- d. Line 44: Replace “and lesser among” with “, compared with...”**

⁵ <https://www.nature.com/commseenv/submit/submission-guidelines>

⁶ <https://www.apa.org/about/apa/equity-diversity-inclusion/language-guidelines>

We have revised the *Introduction* (mostly as a response to *Comment R1.7.a*), and this wording is no longer used. The related phrasing is now in a new location (L57-L59).

e. Line 48: Suggest adding “In this study, we expand...” for clarity.

We have revised the *Introduction* (mostly as a response to *Comment R1.7.a*), and this exact wording is no longer used. That said, we incorporate “In this study, ...” to begin discussing our work as part of this paper (L44).

f. Line 48: Please spell out AP4T to define, since this is the first instance when it is mentioned. I think the first time you write it out is in Line 473 of the Methods. No need to elaborate, but it’s helpful to know that it is a new version of the “Air Pollution Emissions Experiments and Policy model” early on since it is a major focus of the paper...

We have moved the description of AP4T (as it relates to its predecessors), originally in the *Methods* section, to the *Introduction* (L49-L51). AP4T is the name of the model, not an acronym. However, we contend that writing out the Air Pollution Emission Experiments and Policy model (APEEP) in this sentence will provide readers with the clarity requested by the reviewer.

g. Line 96: “When controlling for age”: Per my comments above, it is unclear how you control for age and why you don’t control for age in all analyses.

We have updated the text from “when controlling for age” to “within age groups” (L12-L13 & L104). We believe this change clarifies our intended meaning regarding the control of age groups and understand why the original phrasing may lead to some confusion.

R1.8) Detailed Comments: Results

a. Figure 2: This is far too much information for one figure. It is really very useful, but poorly explained in the caption and hard to follow when all lumped into a single figure. I would break these up into multiple figures and/or send some panels to the Supplemental Information, as noted previously.

As noted in our response to *Comment R1.4.b*, we have made the following updates in response to this comment:

- Original Fig. 3a and 3b and Original Fig. 2a and 2b are now presented as New Fig. 2a and 2b.
- Original Fig. 2c and 2d are replaced with New Fig. 4.

Again, we thank the reviewer for this valuable suggestion and believe these figure updates greatly improve the paper.

- b. Figures 2-3: Have you considered rotating these panel figures 90 degrees? Especially for the panels in Figure 3, it could make it easier to read the labels and interpret the directionality (left of line is negative, right of line is positive). Having the figure labels on top and oriented horizontally will also facilitate interpretation. Just a thought...**

We (really like and) have accepted the reviewer's suggested edit (New Fig. 2a and 2b).

- c. Lines 224-225: If California has such a disproportionate effect, perhaps it should be analyzed separately to enhance interpretability?**

We have added New Fig. 3a to enhance the interpretability of our findings in New Fig. 2a and 2b. New Fig. 3a examines smoke exposure and vulnerability by geographic region, specifically the nine census divisions, which are grouped into four census regions. This figure helps control for California's disproportionate effect on the relationship between wildfire smoke and social vulnerability, as it can only influence the Pacific census division. We observe that the relationship between smoke exposure and vulnerability is far less pronounced in the other census divisions. (Note: New Fig. 3a also provides the benefit of looking within the most prescribed burn smoke-polluted census divisions, some of which demonstrate clear positive correlations between exposure and vulnerability).

We also analyze California separately in several instances throughout the Supplementary Information:

- **Supplementary Fig. 20** shows tract-level smoke damages from the three most air pollution-damaging wildfire events in California in 2017 (see our response to *Comment R3.7*). Importantly, these "events" include multiple individual wildfires that ignited in close geographic proximity to one another at relatively similar times.
- **Supplementary Fig. 27** shows average tract-level percentile ranks (i.e., vulnerability) by social vulnerability variable in California.
- **Supplementary Fig. 28** shows exposure and deaths/damages per capita disparities by Social Vulnerability Index decile (1) in the base case, (2) without California, (3) without Oregon or Washington, (4) without states in the Mountain census division, and (5) using only states in the Pacific and Mountain census divisions.
- **Supplementary Fig. 32** shows wildfire smoke exposure versus social vulnerability overall and for each theme within states (including California).

We believe that these analyses sufficiently enhance the interpretability of our findings for readers who wish to explore the data further. We also cite a recent paper that looked at correlations between smoke and several social vulnerability variables within California [25] but hesitate to give too much further attention to the topic given that our work and key findings are driven by regional (not within state) trends.

- d. Line 228: The word "effected" should be "affected" here.**

We have revised the relevant section, and this wording is no longer used.

- e. Line 244: You spelled out VSL earlier but have not defined it for the reader. I would specify, “since the VSL is treated as constant at \$XXX per capita” (REF) just to give readers a foothold for understanding your approach and Results before getting to the Methods. (Then again, if it varies with age perhaps this may confuse matters...).**

We have added the numerical value upon first discussing the value of a statistical life (VSL) in the context of damage estimation as well as in the identified location to ensure clarity for the reader; both are in the *Results* section (L153 & L284). It is intentionally not specified in the *Introduction*, upon first mentioning the VSL, for brevity in that section. The VSL is \$9.97 million (2020 U.S. dollars) for 2017 (see Supplementary Table 11 and the associated discussion for more information).

- f. Line 266-267: This seems like an opportunity to consolidate into one figure, with a nice clear caption, rather than repeating information in Fig. 2a and Fig 3a.**

As noted in our response to *Comment R1.4.b* and *Comment R1.8.a*, we have made the following update in response to this comment:

- Original Fig. 3a and 3b and Original Fig. 2a and 2b are now presented as New Fig. 2a and 2b.

Once again, we thank the reviewer. We were glad to have the opportunity to make this change.

- g. Line 297-298: How would you interpret this finding? Would it be worthwhile to explore in the Discussion?**

We thank the reviewer for their interest in this observation. Prescribed burn smoke exposure exhibits negative relationships with the social vulnerability variables of Minority, Limited English, and Crowding because California and the West, which are minimally impacted by prescribed burn smoke, are home to a disproportionate portion of the most vulnerable populations by these criteria. This can be observed in Supplement Fig. 28 and our newly added Supplementary Table 25. Ultimately, the conclusions we can draw from this finding are part of the broader conclusions of the paper that it is the geographic distribution of social vulnerability and fire smoke that drive the observed trends. As such, and because our *Discussion* is already quite lengthy, we have not added any commentary on this matter to the discussion. That said, we have added related text in the *Results* section to concisely explain this observation (L308-L309).

- h. Line 334: As noted above in the General Observations, this seems counter-intuitive. Shouldn't these costs be scaled somehow to account for the de facto life-years lost due to premature death? If a 40-yo with an 80-yr life expectancy dies prematurely shouldn't the cost be higher than a 79-yo with an 80-y life expectancy?**

As addressed in our response to *Comment R1.5.a*, “premature death” in this study refers to a death that occurs earlier than it would have without exposure to fire smoke. This definition applies to individuals of any age. We have clarified the definition of “premature mortality” upon its first mention to ensure understanding (L24-L25).

Under the value of a statistical life (VSL) approach, each premature death is assigned the same value, regardless of age or other factors. The VSL is grounded in the willingness-to-pay or willingness-to-accept for changes in mortality risk [8], [9], [10], a metric that does not decrease linearly with age [12].

However, to address concerns like the one raised here, we have conducted sensitivity analyses using the value of a statistical life year (VSLY) approach, which considers the value of expected remaining life-years lost [13]. Please see our response to *Comment R1.5.b* for further information. We have incorporated analyses using the VSLY approach into the *Methods* section of the manuscript (New Fig. 7 and New Fig. 8). We also refer to the VSLY approach in the *Discussion*, particularly where we discuss the nexus of age, race/ethnicity, and social vulnerability in relation to the influence of senior citizen damages on the distributional impacts (L489-L493).

- i. Lines 378-385: [Accounting for the influence of age...] As noted before, I am not an expert in public health and recognize that I may be missing something fundamental, but I don’t understand how the costing of “premature deaths” due to fire is performed. Does it account for differences in life expectancy among groups? How is it, for example, that if the population life expectancy is <80 years we have the highest cost of exposure attributed to people >80? It would seem to me that there should be a way to quantify the lost years of life so the earlier deaths due to fire would be costlier than the later ones, but this seems to be precisely the opposite of the logic used.**

Please refer to our responses to *Comment R1.5* for a detailed explanation. Our responses to *Comment R1.7.g* and *Comment R1.8.h* are also relevant.

In summary, the term “premature death” refers to any death that occurs earlier than it would have without exposure to fire smoke, regardless of the person's age (L24-L25). The value of a statistical life (VSL) approach assigns a uniform value to each premature death, not varying by age, life expectancy, or other demographic factors.

An Individual’s true VSL will depend on remaining life-years, their risk tolerance, and income level. Both because of missing information (for example risk tolerance) and because of ethical concerns associated with tailoring the VSL to different populations based on income, we apply the VSL uniformly. This approach is widely used in academic research and policy analysis. As an alternative, we apply a value of a statistical life year (VSLY) approach which tabulates the value of mortality risk as the product of expected life years remaining and a constant VSLY. In this approach, the VSLY is derived from the VSL [13].

- j. Lines 386-394: This goes beyond Results and starts bleeding into the discussion, interpretation, and recommendations. Perhaps, it belongs in the Discussion section?**

We thank the reviewer for the suggestion and agree that this content is more appropriately suited for the *Discussion* section. The text has been removed from the *Results* section, and an adapted version is now included in the *Discussion* section (L477...).

R 1.9) Detailed Comments: Discussion

- a. Overall, I think the Discussion needs to be strengthened. I appreciated the uncertainty modeling and comparison with other approaches, but I almost think that could go into the Methods, while several other elements of the Methods and Results would greatly enrich the Discussion, as noted in those sections.**

We have revised the *Discussion* in a manner that strengthens the manuscript. While we have referenced modeling uncertainty in the *Discussion* (L488-L494), most of that content is now in the *Methods* section, as suggested. We have carefully crafted the new discussion to align with some of the most important comments made by reviewers as listed below:

- *Comment R2.9*: Benefits of prescribed burns (**Paragraph 3** and **Paragraph 4**).
- *Comment R1.9.c.ii*: Spatial differences in prescribed burning (**Paragraph 4**).
- *Comment R2.12*: Actions to be taken for public protection during smoke events (**Paragraph 5**).
- *Comment R1.9.b* & *Comment R1.10.c*: Explanations for underlying observed trends pertaining to race/ethnicity and age (**Paragraph 6**).
- *Comment E2*, *Comment E3*, *Comment R1.5.b*, *Comment R1.8.h*, *Comment R1.8.i*, & *Comment R1.9.c*: Alternative modeling inputs that significantly influence the distributional impacts of smoke exposure (**Paragraph 7**).

Otherwise, **Paragraph 1** and **Paragraph 2** are an overview of key results, and **Paragraph 8** finishes with a call for informed action based on our findings herein.

- b. More to the point, I think the authors have missed an opportunity here for a more nuanced discussion of the policy implications of this work. They could also do more to explore the potential explanations underlying observed trends, which could motivate future work and help readers think about the complex information presented.**

We thank the reviewer for their comment. We believe our enhanced *Discussion* now more fully explores the policy implications of this work across several critical topics. We interpret the reviewer's comment as primarily referring to the findings related to age and race/ethnicity, which we have addressed in **Paragraph 6** and **Paragraph 7** of the *Discussion* section. These paragraphs provide detailed commentary on the nuances of distributional impacts from air quality damages and suggest that alternative approaches—such as differentiated relative risk and the value of a statistical life year—may be especially pertinent when considering environmental justice. Furthermore, our discussion of the benefits, costs, and regional decision-making related to

prescribed burns (**Paragraph 3** and **Paragraph 4**), as well as the actionable solutions for vulnerable populations (**Paragraph 5**), adds substantial value to the paper. These insights not only motivate future research but also help readers contextualize the complex information in broader terms.

c. Line 455-456: “policymakers should prioritize measures protecting older Americans”.

i. I am not suggesting that older Americans should not be a priority, but I do question ranking this population first based on the economic valuation (for all the reasons mentioned above). If your method “over-values” the lives of senior citizens, of course you would conclude that they are the priority. The logic seems circular and, perhaps, is not the most interesting result of your work.

We contend that our methodology does not “over-value” the lives of senior citizens. As discussed in responses to several previous comments (*R1.5*, *R1.8.h*, and *R1.8.i*), the value of a statistical life (VSL) approach is based on an individual's willingness-to-pay (WTP) for reduced mortality risk [8], [9], [10]. In addition to remaining life-years, risk tolerance and income are critical drivers of the VSL. As is customary both in the academic literature and in applied policy analysis, we apply the VSL uniformly. In the context of public policy, we contend that this is the most ethically-grounded approach is to use a uniform VSL. That said, your concerns have encouraged us to evaluate a value of a statistical life year (VSLY) approach alternative [12], [13]. Please see our response to *Comment R1.5.b* for further information. We have incorporated analyses using the VSLY approach into the *Methods* section of the manuscript (New Fig. 7 and New Fig. 8). We also refer to the VSLY approach in the *Discussion*, particularly where we discuss the nexus of age, race/ethnicity, and social vulnerability in relation to the influence of senior citizen damages on the distributional impacts (L489-L493).

ii. For example, I would be more interested in a careful exploration of the spatial differences in exposure and vulnerability of populations, particularly for prescribed burns which are intentional and reflect management decisions that may intersect with SVIs. Why or how are those decisions made? Is there any information to explain why PBS is more likely to occur in certain regions vs. others? What about WFS? I miss some analysis of these differences and discussion of their broader implications, although I leave it to the authors to identify the salient points.

While we assert that our findings regarding senior citizens incurring the most damage (1) is informative for public policy decision-making and (2) facilitates a meaningful discussion on environmental justice pertaining to the nexus of age, race/ethnicity, and social vulnerability, we also find this suggestion to be particularly valuable. Hence, we have added commentary to the *Discussion* section that addresses the spatial differences in where prescribed burns are conducted in the U.S. and how that relates to social vulnerability (L454-L455). Related is the discussion of

benefits and costs associated with controlled fires (L440-L446 & L448-L449). We believe this addition to the manuscript provides valuable insight into regional approaches to wildland management and highlights the barriers to prescribed burning in the West, which greatly affects the sources of smoke in different parts of the country. Furthermore, it encourages further research to explore the broader benefits and costs of controlled fires, aiming to identify the most effective strategies to protect vulnerable populations.

d. Line 462: Delete “also” to avoid repetition.

We thank the reviewer for their suggestion. This part of the *Discussion* section has been revised, so the suggested edit is no longer applicable.

R1.10) Detailed Comments: Methods

- a. Line 482: Helpful and intuitive definition of receptors here, which I missed earlier in the paper. It took some time to get my head around the sources vs. receptors language. It would help to have it explained earlier in the Results or Introduction using an “exporter” vs. “importer” analogy or a “where damages are generated” vs. “where damages are experienced”.**

We have incorporated the reviewer’s suggestion by adding “where and from what emissions are generated” and “where pollution is experienced” in Fig. 1’s caption (L138-L139). This is an ideal spot because it is where damages by source and receptor are mapped. Definitions are also provided when referencing Fig. 1a (L117). However, we chose not to use the terms “exporters” and “importers” because a source is only an exporter when it impacts receptors other than itself, and similarly, a receptor is only an importer when it is impacted by sources other than itself.

To clarify, we provide the following example: When Census Tract/County **A** releases emissions (source) and damages Census Tracts/Counties **A, B, C, etc.** (receptors), it is only an exporter to **B, C, etc.** Conversely, when Census Tract/County **A** receives pollution (receptor) from Census Tracts/Counties **A, B, C, etc.** (sources), it is only an importer from **B, C, etc.** While the “exporter” and “importer” analogy is intuitive, we believe this explanation illustrates why these terms may not fully capture the dynamics of “sources” and “receptors,” and we therefore chose to avoid them for the sake of accuracy.

- b. Line 619: Change “by” to “but”**

We have accepted the reviewer’s suggested edit. This text is now in the *Discussion* section (L484).

- c. Line 621-625: This is a critical point of Discussion and should not be buried in the Methods. It really needs to come out strongly in the Discussion and Results, so readers don't jump to the wrong conclusions. In general, I think much of the content in “Health Effects Modeling Limitations” should be considered directly in the Discussion rather than in Methods. It really is the crux of the paper/implications.**

We thank the reviewer for this comment and have revised the discussion to reflect its suggestion. Please see **Paragraph 6** of the *Discussion* section for our change (L477-L487), which we believe meets the request of ensuring this point comes out strongly in the main sections of the paper (rather than in the *Methods* section). The other content in the now “Health effects and valuation modeling limitations” subsection, however, is kept there given space constraints in the *Discussion* section.

- d. Line 628 and 631: I think “infiltration” should be “filtration” in this context? I am assuming you mean the systems that can filter particulate matter in indoor HVAC systems?**

Where we use “infiltration” (L730), we are indeed referring to the process of smoke entering indoor spaces from the outdoor environment. This terminology is consistent with the studies cited in this paragraph, specifically Burke et al. (2022) [29] and Kramer et al. (2023) [25]. However, we use “filtration” as defined by the reviewer as well (L467 & L728).

- e. Line 634: Change “effect” to “affect”**

We have accepted the reviewer’s suggested edit (L732).

- f. Line 669: Repetitive title. Perhaps “Social vulnerability variables characterizing...” would be clearer?**

We have revised the title from “Social vulnerability index’s social vulnerability variables...” to “Social Vulnerability Index’s variables...” (L868). We chose to retain the emphasis on the index in the title to clearly indicate that the table contains variables related to the social vulnerability index. This revision addresses the repetition while ensuring that readers understand the specific focus of the table.

Reviewer 2 Responses

R2.1.) L6 and L400-401 The phrasing here is confusing, perhaps misleading. You emphasize that wildfire smoke is responsible for almost half of the damages, which is substantial, but then go on to say that prescribed burning is responsible for more than half, which is perhaps even more shocking. Are these the only two possible categories? perhaps it would be better to put these in the context of other damages from particulate matter? If we are comparing only WFS and PBS, I would actually expect that damages are much higher from WFS. But perhaps it would depend on the year. Either way, I think the points of emphasis should be reworded or recontextualized here.

We appreciate the reviewer's feedback and recognize that the original phrasing may have been confusing. To clarify, we have revised the sentence to read "Nearly half of this damage was from wildfires, predominantly in the western U.S., while the other half was from prescribed burns, distributed across the Southeast" to make it clear that we are referring to the \$200 billion mentioned in the previous sentence (L5-L7). Wildfire emissions and prescribed burn emissions are indeed the only two categories in wildland fire emissions. Our intent with the phrase "...while the other half was from prescribed burns..." is partially to succinctly inform readers that these two sources fully encompass the wildland fire emissions category evaluated in the study. We also believe that our mention of wildland fires in the first sentence and then wildfires and prescribed burns in the second leads readers in the right direction.

Ideally, we would include that wildland fire emissions accounted for 17% of U.S. damages, with wildfires contributing 8.3% and prescribed burns 8.4%. However, due to space constraints in the *Abstract*, the total wildland fire contribution is detailed in the last paragraph of the *Introduction* section focusing on primary findings (L89-L90) and the wildfire and prescribed burn contributions at the beginning of the *Results* section, where more details on sources and receptors are discussed (L113-L114). We also reference Supplementary Table 1 (L132) for readers interested in the broader context of wildland fire emissions relative to other sources.

Additionally, as shown in Supplementary Table 15, while wildfires emitted more pollution overall in 2017, prescribed burns caused more damage per ton due to their occurrence in more densely populated areas. Our modeling accounts for the subsequent transport, exposure, and response, which results in slightly higher damages from prescribed burn smoke. We've added a brief acknowledgment of this in the manuscript (L145-L147) to further highlight this point: "Importantly, there is heterogeneity in damages per ton, driven by discharge location, which explains how wildfires can emit more pollution but result in fewer damages than prescribed burns (Supplementary Table 15)."

R2.2) L13 insert "smoke" before the word 'damages'

We have accepted the reviewer's suggested edit (L13).

R2.3) L19 when you cite that wildfire emissions are increasing, what time period are you referring to? I assume recent decades, but not a period of more than a century. This context is important.

We have clarified the time period by specifying “Since the latter part of the 20th century...,” which is applicable for both the decline of anthropogenic emissions and the rise of wildfire emissions in the United States (L18-L20). Regarding anthropogenic emissions, Aldy (2022) [30] highlights the impact of the Clean Air Act of 1970 and its subsequent amendments as key drivers. For wildfire emissions, sources such as Burke et al. (2021) [29], McClure & Jaffe (2018) [31], and O’Dell et al. (2019) [32] provide evidence that wildfire activity has been increasing since the 1980s. To maintain brevity, we believe this provides sufficient context for the time period and supports our claims.

R2.4) L27 intentionally “and legally”

We have accepted the reviewer’s suggested edit (L30).

R2.5) L50 why was the analysis focused only on 2017? I can see how it would be much more robust to include some other years, including more recent years that had even higher levels of smoke damages. I understand that the study must be bounded somewhere, but please speak to the decision to focus on 2017, and please explain any limitations of not including other years of data.

We have added commentary to the manuscript that explains our choice to focus on 2017 (L513-L520). This decision is partially aimed at maintaining simplicity; our manuscript conducts substantial evaluations within that year. Introducing a temporal dimension would add complexity, requiring us to address this factor at potentially every stage of the analysis.

As important, however, is that the AP4T model incorporates several 2017-specific inputs. Principal among these is the emissions inventory that AP4T and its predecessors are built upon. This initial iteration of AP4T was developed using the 2017 National Emissions Inventory (NEI) [7]. These models assess emissions on the margin, meaning they model how emissions drive impacts on top of an existing baseline. To construct this baseline, we require national emissions data from all sources of air pollution across the contiguous U.S., which the NEI provides (see Supplementary Table 1 for national totals of NH₃, NO_x, primary PM_{2.5}, SO₂, and VOCs by major sources contributing to AP4’s/AP4T’s baseline). Additionally, baseline ambient PM_{2.5} is not the only year-specific input that matters for AP4/AP4T; populations and mortality rates by county (and census tract) also characterize 2017.

The NEI is released every three years, which dictates which years can be modeled using APEEP (e.g., AP3 was capable of modeling 2008, 2011, 2014, and 2017) [33], [34]. Although the 2020 NEI was released last year [35], the development of AP4T took place well before that, and its build-out for 2020 capabilities is ongoing as part of other research projects by different authors. Ultimately, this work sought to evaluate a snapshot in time, given that incorporating temporal

trends would add another layer of complexity to an already detailed body of research, and we choose to maintain this approach. We appreciate the comment acknowledging the necessary bounds of a study.

That said, the question remains: does 2017 provide a reasonable representation of typical fire emissions in the U.S.? The answer to this question is critical to some of our major conclusions. Therefore, we compare emissions from the 2014 and 2020 National Emissions Inventories (NEIs) [35], [36] with those from the 2017 NEI. This comparison has been added to the Supplementary Information, as referenced in the manuscript (L516-L518). For some analyses, we focus on primary PM_{2.5} because it accounted for over 70% of the air pollution damages from fire emissions in 2017 (Supplementary Table 15). Our investigation must consider two key aspects: (1) emission magnitudes and (2) emission distribution. The first aspect affects total impacts, while the second influences the distribution of impacts, both of which are crucial for our study. Notably, while damages are influenced by emissions, they also depend on many other variables. Thus, we cannot assume that a 5% increase in emissions translates directly to a 5% increase in smoke damage; we would have to work through comprehensive analysis through every component of the integrated assessment model to verify that fact. Nevertheless, this analysis provides insight into how sensitive our assessment may be to differing emissions occurring in different years.

Supplementary Fig. 4 shows the total emissions of NH₃, NO_x, primary PM_{2.5}, SO₂, and VOCs from all wildfires and prescribed burns in the contiguous U.S. for 2014, 2017, and 2020. Colors represent census divisions, and “color families” represent census regions. Supplementary Table 2 supports this by presenting emission totals for 2014 and 2020, along with percentage differences compared to 2017. We observe that wildfire emissions in 2014 were significantly lower than in 2017 (47% less primary PM_{2.5}), but higher in 2020 compared to 2017 (19% more primary PM_{2.5}). Prescribed burn emissions were lower in both 2014 and 2020 compared to 2017, with reductions of 11% and 19%, respectively, for primary PM_{2.5}. Consequently, we can infer that smoke-related damages from both wildfires and prescribed burns were lower in 2014 than in 2017. Additionally, while wildfire smoke damages were likely higher in 2020 than in 2017, prescribed burn smoke damages were lower in 2020.

Supplementary Table 3 presents the percentage of primary PM_{2.5} from wildfires and prescribed burns across different geographic areas for each year. Both Supplementary Fig. 4 and Supplementary Table 3 indicate that the distribution of emissions remained relatively consistent across the three years at the regional level. Similar to 2017, most wildfire emissions originated from the West in 2014 and 2020, while most prescribed burn emissions came from the South in those same years. This consistency is particularly important for some of our paper’s main conclusions. Since between-state correlations of vulnerability and smoke drive the national correlation, the stability of emission distribution at the regional level suggests that our conclusions would hold for other years as well.

Supplementary Fig. 5 zooms in to the state level, displaying the percentage of wildfire emissions from each state in 2014 and 2020 versus 2017. For wildfires in the West, we see a greater share of emissions came from California and lesser shares of emissions came from Montana and

Idaho for both years. A smaller percentage of emissions came from Oregon and a greater percentage of emissions came from Washington in 2014, and the reverse was the case in 2020. For wildfires in the rest of the country, there was a lower percentage of emissions from Oklahoma for both years. Shares from Texas and Louisiana were higher in 2014, and shares from Colorado were higher in 2020. Overall, the biggest concern for our paper's distributional impacts assessments would be if there was a notable decrease in the percentage of emissions coming from California. As noted in the manuscript, California is a big driver of the observed trends, and we can assume this influence would increase for 2014 and 2020.

Supplementary Fig. 5 also zooms in to the state level, showing the percentage of prescribed burn emissions from each state in 2014 and 2020 compared to 2017. In the Southeast, we observe that a larger share of emissions in both years came from Florida, Georgia, and Alabama, while Oklahoma, Missouri, and Kansas contributed smaller shares. Louisiana's contribution was higher in 2014 but lower in 2020, and the reverse was the case for Texas. Arkansas maintained a relatively consistent share across the years. Interestingly, Oregon contributed a greater share of prescribed burn emissions in both 2014 and 2020 compared to 2017. Meanwhile, other western states contributed higher shares in 2014 and lower shares in 2020. Like with wildfires, nothing here suggest that we should expect vastly different outcomes to our paper's conclusions in the other years. The emission shares increase in generally more vulnerable states (e.g., Florida, Georgia, and Alabama) and decrease in generally less vulnerable states (e.g., Kansas and Missouri), so the positive correlation between prescribed burn smoke and vulnerability could become stronger.

R2.6) L68-69 increased morbidity and mortality risk compared to what? Other pollutants? Or just compared against clean air?

Fire smoke is associated with increased morbidity and mortality risk relative to clean air. To clarify, we have revised the text to make it clear that fire smoke, similar to other air pollutants, is associated with increased morbidity and mortality risk, rather than suggesting it is more or less harmful than other pollutants (L72).

That said, there is evidence that PM_{2.5} from wildfire smoke is more toxic than ambient PM_{2.5} from other, typical sources [37], which we note in the Methods section's "Health effects and valuation modeling limitations" (L710-711). The difference in health effects from exposure to WFS and PBS at equal concentrations represents a gap in the literature [38], [39] (L709-710), but we refer to several relevant studies to help inform a discussion on potential differences (L711-718). Our modeling does not differentiate concentration toxicity in anyway—i.e., we do not differentiate between concentration-responses associated with fire smoke PM_{2.5} and anthropogenic air pollution PM_{2.5} nor between wildfire smoke and prescribed burn smoke (L718-L721). This is also discussed in our response to *Comment R2.14*.

R2.7) L73 replace 'exposures from' with 'exposures to'

We have accepted the reviewer's suggested edit (L79).

R2.8) L83 It is confusing to have this paragraph come in the introduction section. I suggest simply moving up the RESULTS heading above this paragraph.

While we appreciate the reviewer's suggestion and certainly find it valid, we have chosen to forgo this change because of instructions from the *Communications Earth & Environment* style guide. According to the guide, "The final paragraph should be a brief summary of the major results and conclusions."⁷

R2.9) L207-213 Aren't the people most affected by PBS also likely to be the people receiving the most benefit from prescribed burning (e.g., fire hazard reduction)? This should be explained here.

We thank the reviewer for this particularly valuable comment. In response, we have added commentary to the *Discussion* section addressing the benefits and costs of prescribed burns, specifically noting that they "offer substantial benefits, particularly in reducing wildfire risks by decreasing their likelihood and intensity[38], [39], [40], which ultimately helps protect nearby communities despite the initial pollution they cause" (L442-L444). This comment, along with *Comment R1.9.c.ii*, has prompted us to further explore the spatial differences in decision-making regarding prescribed burning and its associated benefits and costs for vulnerable populations. We believe this broader framing enhances our findings and provides a strong call for further research in this area, significantly enriching our *Discussion* section.

R2.10) L346 Figure 4 a and b. I think this figure would be more interpretable if the colors were scaled along a gradient with age. Age is a continuous variable, after all. The categorical color scheme makes it difficult to interpret.

We have accepted the reviewer's suggested edit (New Fig. 6).

R2.11) L421-422 suggest rewording. In the prescribed fire management world, smoke dispersal is considered a positive thing, because smoke lofting and becoming less concentrated means it will have a less detrimental effect on people (in the vicinity). Perhaps it would be better to say smoke from wildfires 'reaches farther' than from prescribed burns?

This is a comment that facilitates a more in-depth discussion on the differences between the plumes from wildfires and prescribed burns and the resulting concentrations. However, we go into depth on this matter in a forthcoming response to *Comment R2.14*.

We have changed our approach here. We give a far more comprehensive overview of the different characteristics of these fires in the *Methods* sections' "Air quality modeling limitations" (L588-L604). We no longer explicitly say that wildfire smoke disperses more the prescribed burn

⁷ <https://www.nature.com/commsenv/submit/submission-guidelines>

smoke. We do, however, discuss how wildfires' intensity and injection heights affects plume behavior and drive "[...] further transport and dilution of smoke (compared to the concentration at the source) [41]" (L596-L597). We then discuss how the conditions under which prescribed burns are usually conducted "[...] result in plumes that remain within the mixing layer, staying more localized compared to wildfires [41]" (L602-L603). Still, the other content in the section provides broader context for these two fire types and enables a richer understanding of their differences. We now ensure to note that prescribed burns "are conducted under carefully selected, milder conditions [42] to ensure safety and control [43]" (L593-L594) and "achieving effective smoke dispersion is also a goal of prescribed burns [43]" (L601). We believe our revisions effectively address the comment by providing a more thorough discussion of the planning involved in prescribed burns to minimize their negative impacts, while still acknowledging that wildfire smoke disperses more extensively due to the unique characteristics of the burn [41].

R2.12) L466 Could elaborate on the actions that should be taken.

We appreciate this important comment and have added new commentary to the *Discussion* section that addresses actions to protect vulnerable populations during smoke events (L461-L476). The paragraph carefully highlights key considerations for policymakers in this context. For instance, it emphasizes the need to involve trusted community leaders, which is crucial for effectively communicating with historically marginalized groups. It also acknowledges that there is no one-size-fits-all solution and that understanding different circumstances, such as the inability of outdoor workers to move indoors during smoke events, is essential. We believe that the addition of this paragraph significantly enhances the public policy relevance of our paper and provides clearer guidance for decision-making, as intended.

R2.13) L499 Which MTBS data-- simply the location of fire perimeters? Or fire severity data? Or other?

The MTBS data used refers specifically to fire perimeters or burned area boundaries. We have clarified this in the manuscript (L553). The reference to Supplementary Fig. 7 and the associated discussion in the manuscript (L556-L557) provides a resource for readers wishing to understand the methods in greater detail.

The referenced discussion in the Supplementary Information acknowledges a limitation related to fire severity. We discuss how emissions can vary based on the type and amount of fuel burned, not just the burned area [39], [44]. For example, a larger burn area may release fewer emissions than a smaller one, depending on the fuel type (please see Supplementary Fig. 7 and its associated discussion). Our methodology focuses on burned area boundaries and does not account for fuel type variation *within counties*. That said, this variation is captured *between counties* by the National Emissions Inventory emission estimates [7].

R2.14) Somewhere in your discussion, you might more fully discuss literature that clarifies differences in the quality/impact of prescribed fire smoke versus wildfire smoke. See for example Jaffe et al 2020 (who also focus on the US in 2017). I am not sure these nuances are captured in your modeling approach, which would be worth discussing.

This is a valuable comment that encouraged us to provide a more comprehensive discussion on the differences between wildfires and prescribed burns, as well as the smoke resulting from each, throughout the manuscript. We have incorporated these edits in the referenced locations:

- “[wildfires] emit fewer pollutants per acre burned compared to wildfires [43], [45], [46] (L445-L446).
- “Wildfires typically occur under specific environmental conditions, such as drought, high temperatures, strong winds, and low humidity [47], which significantly increase the rate and extent of fuel consumption [41], [45], [46]. Consequently, wildfires tend to be far more intense than prescribed burns, which are conducted under carefully selected, milder conditions [42] to ensure safety and control [43]” (L590-L594).
- “[...] wildfires generate more heat, driving greater smoke plume buoyancy, higher injection heights, and consequently, further transport and dilution of smoke (compared to the concentration at the source) [41]” (L595-L597)
- “[...] wildfires release more pollution in a shorter time frame, causing downwind areas to experience ambient PM_{2.5} concentrations that are orders of magnitude higher than typical conditions [16], [17], [39], [48]” (L597-L599).
- “While achieving effective smoke dispersion is also a goal of prescribed burns[43], the conditions of prescribed burns result in plumes that remain within the mixing layer, staying more localized compared to wildfires [41]” (L601-L603).
- “[prescribed burns] tend to contribute to PM_{2.5} more like other sources of emissions than wildfires, resulting in less dramatic changes in ambient air quality [39]” (L603-L604).
- “[...] the more acute, high-concentration events caused by wildfires pose a significantly greater risk for short-term health effects—e.g., exacerbations of asthma[49], eye irritation[50], cardiovascular episodes[51], and premature mortality[52]—compared to other sources of pollution, including prescribed burns” (L703-L706).
- “[...] it is well understood that wildfires emit more than prescribed burns per acre burnt [45], [46] and that their ignition times [42], [47], plume behaviors [41], and temporal impacts on air quality in downwind communities are distinct [39]” (L707-L709)

Now, we turn to the chemical composition of fire smoke, what we perceive to be the intent of the reviewer's comment. While most epidemiological and toxicological studies focus on PM_{2.5} in fire smoke [39], biomass smoke also contains other harmful compounds, including gases, n-alkane compounds, polyaromatic compounds, methoxyphenols, levoglucosan, and various inorganic constituents [51]. Our study only addresses PM_{2.5} caused by emissions of criteria air pollution (i.e., NH₃, NO_x, primary PM_{2.5}, SO₂, and VOCs) and does not consider the potential health impacts from these additional pollutants. Notably, Wegesser et al. (2009) observed that PM_{2.5} from wildfire

smoke was more toxic than ambient PM_{2.5} from other, typical sources [37] (L710-L711). We can hence conclude that, assuming equal concentrations, air pollution associated with fire smoke is more damaging than that from other sources. Our modeling does not differentiate concentration toxicity in anyway (L718-721).

The differences in health impacts between wildfire smoke and prescribed burn smoke at equal concentrations represent a gap in the literature [38], [39] (L709-L710). While one might assume that both wildfires and prescribed burns, which consume similar fuels (i.e., wildland forestry), produce similar chemical compositions, Chen et al. (2021) observed that wood smoke generated in a controlled laboratory setting differs chemically from wildfire smoke in its natural environment [51]. This discrepancy is partly due to variations in vegetation characteristics, which might imply fewer chemical differences between prescribed burn smoke and lab-generated smoke compared to wildfire smoke. However, other key factors—such as combustion conditions, weather patterns, and the geographical areas burned—also contribute to significant distinctions between prescribed burns and wildfires [51] (L711-L715).

Several factors could influence the chemical composition of wildfire versus prescribed burn smoke. For instance, Fabian et al. (2010) examined particle diameters from various residential construction and furnishing materials upon burning [53]. All tested materials produced particles averaging less than 2.5 microns in diameter, classifying them as ambient PM_{2.5}. However, many of the particles were smaller than 0.1 microns, classifying them as ultrafine PM_{0.1}, which is generally considered more harmful [54]. Synthetic materials produced higher proportions of PM_{0.1} compared to wood-based materials [53] (L715-L717). Nonetheless, structures constitute only a small fraction of the material burned during a wildfire (L717-L718), and Fabian et al. (2010) did not compare residential materials with wildland materials, which would be a more relevant comparison.

A relevant study, Prunicki et al. (2019), investigated the health effects of smoke exposure from both wildfires and prescribed burns on children in Fresno, California, and found that wildfire smoke had more severe effects on children's cardiovascular and immune systems [55]. However, the study did not control for differences in emissions or concentrations between the two fire types, focusing instead on real-world scenarios. This limitation prevents us from concluding that wildfire smoke is inherently more damaging than prescribed burn smoke at equal concentrations. In summary, observed differences could potentially be due to distinct concentration-response relationships for wildfire and prescribed burn smoke. However, these differences could also be entirely driven by variations in the magnitude and distribution of exposure levels.

To our knowledge, the known differences between wildfire and prescribed burn smoke are more related to emissions, transport, and exposure rather than how the quality of the smoke impacts concentration-response. Notably, previous work has utilized concentration-response data from wildfire smoke exposure to study the health impacts of prescribed burns [56]. We echo the calls of Hill et al. (2022) [38] and Jaffe et al. (2020) [39] for further research into the differences in health impacts between these fire types. For our work, data on the relative mortality risks associated with long-term ambient PM_{2.5} concentrations from both wildfire and prescribed burn

smoke would be particularly valuable. Additionally, more information on the differing effects of short-term exposure episodes would also significantly advance the field.

Reviewer 3 Responses

R3.1) Given the importance of different model assumptions in calculating wildfire damages, I suggest the authors move these assumptions more upfront in the introduction sections. Especially, two key assumptions need to be included given their importance for calculating health damages of smoke: what emissions inventory for wildfire/prescribed fires and what concentration-response functions (linking smoke PM_{2.5} and mortality) are used.

We have revised the introduction to incorporate the reviewer's suggestion (L51-L54). Directly following AP4T's introduction, we state: "AP4T uses emissions data from the National Emissions Inventory (NEI) [7], a long-term mortality risk dose-response function from the American Cancer Society cohort study [19], and the value of a statistical life (VSL) suggested by U.S. Environmental Protection Agency (EPA) [18]."

While this summary does not cover all of AP4T's inputs—such as population [57] or mortality rate [58] data—it emphasizes key assumptions that could significantly affect our results, as further discussed in the *Methods* section.

R3.2) For the air pollution impacts, it is widely acknowledged that the estimated fire impacts on air quality are highly uncertain, due to uncertainty in underlying fire emission inventories. For example, prior studies have noted that estimated fire smoke PM_{2.5} can differ by several magnitudes across different fire emission inventories, even when using the same air quality model. Given this large uncertainty, I think it is important to further discuss the potential uncertainty of the emissions inventory used in this work. While a full comparison of the NEI emission inventory with all other leading fire emission inventories may not be necessary, perhaps the authors could also compare NEI emission estimates to at least one of the leading estimates such as GFED4s? Are there any papers that the authors can cite on this?

We have added commentary to the manuscript that acknowledges this uncertainty and refers to a comparison to the Global Fire Emissions Database (GFED) [14], [15] in the Supplementary Information (L521-L524). We note that our assessment of our smoke PM_{2.5} estimates to those from Xie et al. (2020) [16], requested by *Comment R3.3*, also provides an indirect comparison because the study used GFED emissions data (L615-L655). However, Xie et al. (2020) adjusted their GFED emissions inputs (i.e., multiplied them by five in the western U.S. and southwestern Canada), and GFED has been updated since the study's publication.

We extract emissions estimates from GFED [59], [60], [61], [62], [63], [64], [65]⁸ to compare to those from the National Emissions Inventory (NEI), as suggested by the reviewer. This comparison has been added to the Supplementary Information, as referenced in the manuscript (L523-L524). We download the most recent, GFED5, gridded files. We note that these data are part of a Beta version and that the data may change until the associated paper is published, but, for

⁸ <https://www.globalfiredata.org/data.html>

our purposes, we find that benefits of using the newest data outweigh the downside of potential changes.

We begin by comparing our primary PM_{2.5} emission totals (Supplementary Table 1) to the 2017 total from the GFED. The NEI reports 1.22 million metric tons for wildfires and another 787 thousand for prescribed burns, for a total of 2.01 million metric tons of primary PM_{2.5} emissions for wildland fires overall. These totals can be verified via the Technical Support Document associated with the NEI [7]. The GFED reports 1.39 million metric tons of primary PM_{2.5} emissions across Temperate North America (TENA). TENA is effectively the contiguous U.S. Hence, the NEI reports emissions that are 45% higher than those from the GFED.

GFED provides data at a global scale in raster file format. The spatial resolution is 0.25 degrees, and the estimates are provided at daily and monthly temporal resolutions. For our purposes, the key variables of the dataset are grid area (m²) and emissions factors (g per m²). We compute primary PM_{2.5} emissions for all of 2017, analogous to our NEI data, by extracting monthly data to the county level and then summing across all 12 months.

Supplementary Fig. 1 shows state-level totals of primary PM_{2.5} emissions from fires as reported by the NEI (y-axis) versus the GFED (x-axis). States are colored by census division, with “color families” representing census regions. We can see that the NEI overestimates primary PM_{2.5} emissions as compared to the GFED for California (+9%), Oregon (+15%), Montana (+27%), Idaho (+132%), and Washington (+64%), the particularly wildfire-smoke damaging western states in our analysis. The NEI also overestimates emissions for Oklahoma (+403%), Kansas (+143%), Missouri (+450%), Arkansas (+112%), Texas (+43%), and Louisiana (+119%), some of the particularly prescribed burn-smoke damaging southern states in our analysis. While the NEI underestimates emissions for seven states, the two most important are Florida (-47%) and Georgia (-54%).

Supplementary Fig. 2 shows county-level differences in primary PM_{2.5} emissions from fires as reported by the NEI versus the GFED. Counties are colored red where the NEI reports higher emissions and blue where the NEI reports fewer emissions. We see substantial differences in several counties throughout the U.S. Some of the greatest differences are in:

- Idaho County, ID: +52.7 thousand metric tons (140% higher)
- Siskiyou County, CA: +48.2 thousand metric tons (81% higher)
- Tulare County, CA: +34.3 thousand metric tons (224% higher)
- Curry County, OR: -25.7 thousand metric tons (34% fewer)
- Ventura County, CA: -24.9 thousand metric tons (56% fewer)
- Powell County, MT: +24.7 thousand metric tons (121% higher)
- Ware County, GA: -20.4 thousand metric tons (98% fewer)
- Kittitas County, WA: +20.2 thousand metric tons (223% higher)
- Yakima County, WA: +19.2 thousand metric tons (163% higher)
- Hood River County, OR: +17.8 thousand metric tons (164% higher)

Supplementary Fig. 3 shows county-level fractional biases in primary PM_{2.5} emissions from fires as reported by the NEI versus the GFED. The fractional bias is used to highlight relative differences between the two estimates, acknowledging that neither serves as the “absolute truth.” The equation for fractional bias in this context is:

$$\text{Fractional Bias} = \frac{E_{NEI} - E_{GFED}}{\left(\frac{E_{NEI} + E_{GFED}}{2}\right)}$$

E_{NEI} are county-level emissions from the NEI, and E_{GFED} are county-level emissions from GFED. Counties are colored red where the NEI reports higher emissions and blue where the NEI reports fewer emissions. Significant relative differences are observed across many counties in the U.S. Unlike in Supplementary Fig. 2, where large absolute differences typically occur in areas with high emissions, a county does not need to have high emissions to exhibit notable relative differences.

In the Supplementary Information (Supplementary Fig. 1 and this comparison to GFED), we discuss the NEI’s procedure for estimating fire emissions. While many of the NEI’s point sources, like heavy manufacturing or fossil fuel-fired power plants, are directly monitored, most ground-level sources (including wildland fires) require a more complicated procedure to estimate emissions. The NEI estimates wildland fire emissions using fire activity data collected from state, local, and tribal (S/L/T) agencies as well as from national agencies and organizations. These raw burned area datasets are compiled and then reconciled using other data sources—e.g., satellite data from the National Oceanic and Atmospheric Administration’s (NOAA’s) Hazard Mapping System (HMS)—via a geographic information system-based algorithm, SMARTFIRE [66]. Then, it uses a fuel (here, wildland vegetation) consumption-to-emissions model to derive emission factors, BlueSky [67]. Using SMARTFIRE and BlueSky together yields emissions by pollutant type at the county level, reported by the NEI [7]. For more information, see the Technical Support Document (TSD) for the NEI (note: Figure 7-4 of the TSD is particularly informative) [7]. One of the NEI’s most significant advantages is its focus on the U.S. and use of S/L/T data. GFED focuses on all regions throughout the world and, like many other inventories, relies on satellite information [39]. Notably, satellites tend to better detect larger, more intense fires (like wildfires) than smaller fires (like prescribed burns) [39], suggesting that GFED may miss some of these emissions in the Southeastern states where the NEI overestimates in comparison. More information on GFED’s burned area, fuel consumption, and emission factors procedures can be found in Chen et al. (2023) [59], Van Wees et al. (2022) [62], and Vernooij et al. (2023) and Shahid et al. (in review) [63], [64], respectively.

While we can hypothesize various reasons for the discrepancies between these emissions inventories (e.g., difficulties satellites may face in accurately identifying prescribed burn activity), GFED’s estimates still support the validity of our findings regarding exposures and vulnerability. We emphasize that the positive relationship between smoke and the Social Vulnerability Index is primarily driven by regional trends (i.e., between regions rather than within regions). The NEI

reports higher emissions than GFED for certain key states (e.g., California and Oklahoma) within our focus regions (i.e., the West and the South). However, GFED reports higher emissions from these regions compared to others (e.g., the Northeast and the Midwest). We encourage future research to further explore the differences between these emissions inventories and potentially model GFED's data for the U.S. (for 2017 or other years). Nonetheless, we assert that our analysis thoughtfully addresses the uncertainties associated with the NEI's estimates, and our major conclusions remain robust and defensible.

R3.3) Given the uncertainty of fire emission inventory, I think the paper would benefit from analysis that may help benchmark the estimated PM_{2.5} effects. Perhaps the authors could directly compare their estimated annual PM_{2.5} effects to the data from Childs et al. and existing CTM work that also focused on 2017 (for example Xie, Y., Lin, M., & Horowitz, L. W. (2020). Summer PM_{2.5} pollution extremes caused by wildfires over the western United States during 2017–2018. *Geophysical Research Letters*, 47(16), e2020GL089429.). The comparison between Childs et al in terms of health damages is very helpful, but a direct comparison in terms of annual PM_{2.5} concentrations would also be useful.

We thank the reviewer for their insightful suggestion. We would like to highlight that the original manuscript's approach of comparing health damages estimated by our study to those modeled using smoke estimates from Childs et al. (2022) effectively assessed ambient PM_{2.5} differences, as the population exposure, concentration-response, and valuation modules were identical across both studies. Hence, New Table 1 still contains the comparison in terms of health damages. However, we agree that a direct comparison of annual PM_{2.5} concentrations is indeed beneficial.

We have added an assessment of our smoke PM_{2.5} estimates to those from Xie et al. (2020) [16] and Childs et al. (2022) [17] (L615-L655). More information can be found in the Supplementary Information, as referenced by the manuscript (L622-L623). Xie et al. (2020) employed an Earth Systems Model (ESM) [68] and the Global Fire Emissions Database (GFED) [14], [15] (discussed in our preceding response to *Comment R3.2*) and provided monthly smoke PM_{2.5} mixing ratios, along with surface-level temperature and pressure, over North America [16]. ESMs are comparable to Chemical Transport Models (CTMs) but offer a more comprehensive assessment of environmental systems. Notably, Xie et al. (2020) found that using GFED resulted in smoke PM_{2.5} levels significantly lower than those observed at monitoring stations, prompting them to increase emissions from the western U.S. and southwestern Canada by five times. This study also modeled smoke PM_{2.5} without the influence of Canadian fires, which is particularly relevant for our work, as we do not model emissions from outside the contiguous U.S. Childs et al. (2022) utilized a machine learning model that integrated ground, satellite, and reanalysis data to estimate daily smoke PM_{2.5} concentrations across the contiguous U.S. from 2006 to 2020 [17], as discussed further in our forthcoming response to *Comment R3.4*.

Because the data provided by Xie et al. (2020) are mixing ratios (*MMR*), we must convert them to $\mu\text{g}/\text{m}^3$ to compare them to our estimates. This requires multiplying the mixing ratio by the

density of dry air (ρ_{air}). The density of dry air is a function of pressure (P) and temperature (T) as well as the universal gas constant (R). The computation is depicted in the equation below:

$$Concentration \left(\frac{\mu g}{m^3} \right) = MMR \left(\frac{kg}{kg} \right) \times \rho_{air} \left(\frac{kg}{m^3} \right) \times 1e^9 \left(\frac{\mu g}{kg} \right)$$

$$\rho_{air} \left(\frac{kg}{m^3} \right) = \frac{PM}{RT} = \frac{94,800 (Pa) \times 0.029 \left(\frac{kg}{mol} \right)}{8.314 \left(\frac{J}{(mol \times K)} \right) \times T(K)}$$

Importantly, Xie et al. (2020)'s ambient PM_{2.5} estimates incorporate emissions from all sources. Hence, we use their no fires experiment to subtract non-smoke PM_{2.5} from the GFEDx5 experiment and the GFEDx5 without Canadian fire emissions experiment.

Supplementary Table 13 shows smoke PM_{2.5} estimates from AP4T, Childs et al. (2022), and Xie et al. (2020) summarized by geographic area. Supplementary Fig. 15 maps smoke PM_{2.5} estimates at the county level. Estimates are population-weighted averages. Supplementary Fig. 16 shows county-level differences in smoke PM_{2.5} between AP4T's wildfire smoke estimates (left) or total fire smoke estimates (right) and the three comparison estimates. Supplementary Fig. 17 shows county-level relative differences (via fractional biases) of the same. The fractional bias assesses relative differences between estimates, acknowledging that neither serves as the "absolute truth." The equation for fractional bias in this context is:

$$Fractional\ Bias = \frac{Smoke_{AP4T} - Smoke_{Comparison}}{\left(\frac{Smoke_{AP4T} + Smoke_{Comparison}}{2} \right)}$$

For both Supplementary Fig. 16 and Supplementary Fig. 17, counties are colored red where AP4T estimates higher smoke PM_{2.5} and blue where AP4T estimates lower smoke PM_{2.5}. Supplementary Table 14 reports comparison statistics for tract-level estimates. The statistics are mean fractional error (MFE), mean fraction bias (MFB), Pearson's correlation coefficient (r), mean absolute error (MAE), and mean error (ME). The statistics are depicted below, and the MFE and MFB are Supplementary Equations 7 and 8:

$$MFE = \frac{1}{n} \sum_{i=1}^n \frac{|Smoke_{AP4T,i} - Smoke_{Comparison,i}|}{\left(\frac{Smoke_{AP4T,i} + Smoke_{Comparison,i}}{2} \right)}$$

$$MFB = \frac{1}{n} \sum_{i=1}^n \frac{Smoke_{AP4T,i} - Smoke_{Comparison,i}}{\left(\frac{Smoke_{AP4T,i} + Smoke_{Comparison,i}}{2} \right)}$$

$$r = \frac{\sum_{i=1}^n (\text{Smoke}_{AP4T,i} - \overline{\text{Smoke}_{AP4T}})(\text{Smoke}_{\text{Comparison},i} - \overline{\text{Smoke}_{\text{Comparison}}})}{\sqrt{\sum_{i=1}^n (\text{Smoke}_{AP4T,i} - \overline{\text{Smoke}_{AP4T}})^2} \sqrt{\sum_{i=1}^n (\text{Smoke}_{\text{Comparison},i} - \overline{\text{Smoke}_{\text{Comparison}}})^2}}$$

$$MAE = \frac{1}{n} \sum_{i=1}^n |\text{Smoke}_{AP4T,i} - \text{Smoke}_{\text{Comparison},i}|$$

$$ME = \frac{1}{n} \sum_{i=1}^n \text{Smoke}_{AP4T,i} - \text{Smoke}_{\text{Comparison},i}$$

As discussed in the manuscript (L623-L655), we generally underestimate concentrations in the Midwest along the Canada-U.S. border and in the Northeast. We hypothesize that Canadian fires may drive these differences, which Xie et al. (2020)’s experiment excluding these fires helps to verify. We only model impacts from fires burning within contiguous U.S. borders (i.e., emissions tracked by the NEI). Hence, pollution coming from north of the Canada-U.S. border is excluded. Notably, British Columbia, just north of Washington, Idaho, and western Montana, had a substantial wildfire season in 2017, resulting in nearly three million acres burned [69]. Importantly, AP4T’s calibration uses ground-level data from monitors that Canadian wildfire smoke may influence. This is of increasing relevancy for AP4T, especially given the unprecedented WFS events in the northeastern and midwestern U.S. originating from Quebec and other Canadian provinces in June of 2023 [70], [71], [72].

That said, this underestimation is likely also related to injection heights associated with wildfire smoke, which would explain how fires in the western U.S. (and Canada) could drive substantial PM_{2.5} along the northern U.S. border further east in ways AP4T does not capture. The injection height—i.e., the vertical point at which transport downwind begins—for wildfires is high [41]. Wildfires’ intensity can result in their smoke breaking through atmospheric interfaces such that the pollution more greatly disperses. As explained by Williamson et al. (2016), there is an association between injection height and intensity because the greater intensity drives a greater rate of biomass consumption and heat release [41]. The heat drives greater buoyancy of the plume and, in turn, a greater injection height. Although not a perfect comparison, wildfires may function more like high-polluting facilities with smokestacks, such as electric generating units, which are designed to disperse pollution away from local communities [73].

Conversely, we tend to overestimate concentrations in the West and the Southeast. When comparing to Xie et al. (2020), one explanation is differences in emissions inventories, as the NEI reports much greater emissions from key states such as California and Oklahoma (see Supplementary Fig. 1). However, Xie et al. (2020) found that using GFED resulted in smoke PM_{2.5} levels significantly lower than those observed at monitoring stations, prompting them to increase emissions from the western U.S. and southwestern Canada by five times [16], and GFED has been updated since the study’s publication. See our response to *Comment R3.2* for a comparison of the NEI to GFED.

For the West, injection heights could, again, be the culprit, as wildfire behavior is likely to carry pollution further away from local ambient air systems than we model. However, overestimations also could be explained by the temporal resolution of AP4T. AP4T models pollutant transport considering a representation of annually averaged atmospheric conditions (see Supplementary Equation 4 and the associated description). Many of the fires in California could have occurred when the specific meteorological conditions blew the smoke out of contiguous U.S. borders. Supplementary Fig. 18, showing PM_{2.5} mixing ratios by month, provides support for this theory, especially for October and December of 2017. Like its predecessors, AP4T is an annually calibrated model. This is a limitation of using AP4T for more episodic, rather than steadier, sources of air pollution. One of AP4T's related benefits, however, is that it enables the perspective of probabilistic expectation of PM_{2.5} in receptor locations if modeled emissions are released all at once. This could be particularly useful for policymakers and planners working toward future damage mitigation efforts, providing expected effects given unknowable ignition times and corresponding weather patterns.

For the Southeast, we can hypothesize that the cause of differences is mostly related to prescribed burn smoke. Notably, the comparison studies are not limited to wildfire smoke, but they are also heavily dependent on satellite imagery (note: GFED relies on this information), which tends to better detect larger, more intense fires (like wildfires) than smaller fires (like prescribed burns) [39]. This provides justification for comparing both wildfire smoke and total fire smoke estimates from AP4T to the comparison studies. We can assume these studies capture some but not all prescribed burn smoke in their estimates (see our response to *Comment R3.4* for more information pertaining to Childs et al. on this matter). As such, it is not unreasonable that our wildfire smoke estimates tend to be approximately equal to or lower than the comparison estimates, while our total fire smoke estimates tend to be approximately the same or higher.

This comparison provides substantial insights into the advantages and disadvantages of various modeling approaches, along with several actionable recommendations for future research in this area. It is evident that less satellite-dependent methods, such as the NEI compared to GFED, along with modeling techniques that can effectively differentiate by source (for example, reduced complexity models and CTMs versus receptor-dependent models), offer significant advantages for modeling both wildfire and prescribed burn smoke. However, further development of the AP4T model is necessary to accurately distinguish pollutant transport from wildfires (and potentially prescribed burns) from other ground-level sources. Treating fire emissions similarly to smokestack emissions could serve as a straightforward first step. However, it may be warranted to incorporate a new bin in the AP4/AP4T framework specifically for wildfires.

One important area for future research with the AP4/AP4T models is to address the calibration process, which currently relies on ground-level monitors influenced by Canadian fire smoke and other non-contiguous U.S. emission sources. A potential first step toward enhancing the model's accuracy could involve synthesizing data from the Air Quality System (AQS) monitors used for calibration [74] with the PM_{2.5} estimates from Xie et al. (2020). This synthesis could include comparisons of estimates both with and without Canadian fire emissions. By integrating

these datasets, we could better understand the impact of external smoke sources and refine the calibration of AP4/AP4T for improved performance.

R3.4) In Table 1, it is incorrect to say that Childs et al estimates only include wildfire effects. Their estimate is simply based on HMS plume and surface PM data, so as long as the prescribed fire is large enough to be captured by the HMS plume, their data will potentially capture that. Perhaps the authors could edit their footnote in Table 1 to be more accurate. As mentioned above, I think Reduced complexity methods for wildfire smoke damage calculations are also desirable given their low computational burden. Given the lack of similar tools or the studies applying such tools to fire studies, I think this study can make potential contributions in discussing the advantages and uncertainties of applying existing RCMs to wildfires. Modeling wildfire effects on PM_{2.5} is particularly challenging, even for full-complexity chemical transport models. One of the challenges is on the fire injection heights, which the authors discussed on L536. What is the assumption of APT4 on the emission heights? I think it would be useful to include a standalone paragraph to discuss the benefits and challenges in terms of applying RCMs to fire impact studies in the main text, and the authors could elevate some information in the method section here given its importance to understanding the results.

We thank the reviewer for pointing out the error in our description of the Childs et al. (2022) smoke PM_{2.5} estimates, and we appreciate the opportunity to correct it. We have updated the manuscript to reflect these corrections (L649-L651). Much of the discussion below has been added to the Supplementary Information, near the comparison to Xie et al. (2020) [16] and Childs et al. (2022) [17] (Supplementary Tables 13 and 14 and Supplementary Figs. 15 to 17).

Childs et al. (2022) used a machine learning model that integrated ground, satellite, and reanalysis data to estimate smoke PM_{2.5} levels across a 10 km grid covering the contiguous U.S. [17]. This approach involved three main steps:

1. **Identifying Smoke Days:** For each cell, “smoke days” were identified using the National Oceanic and Atmospheric Administration’s (NOAA’s) Hazard Mapping System (HMS) plume data on clear days. On cloudy days, HMS identified fire points, and the Hybrid Single-Particle Lagrangian Integrated Trajectory (HYSPLIT) model, coupled with aerosol optical depth (AOD) data, was used to fill gaps.
2. **Estimating Ground-Level Smoke PM_{2.5} at Monitored Sites:** On “smoke days,” ambient PM_{2.5} was compared to the median for that location and month, derived from a three-year dataset. Smoke PM_{2.5} was calculated by subtracting the median from total PM_{2.5}, with chemical composition monitors confirming smoke contributions.
3. **Predicting PM_{2.5} at Non-Monitored Locations:** A machine learning model was trained on monitored data and used to predict PM_{2.5} for all cells on all days.

Our initial error stemmed from implying that the study focused exclusively on wildfires. While the study did prioritize wildfires, it did not explicitly separate out prescribed burns. As the reviewer

correctly points out, if a prescribed burn produces a large enough plume, it could be captured by HMS and contribute to the PM_{2.5} estimates. Any deviation from the three-year median on a “smoke day” would be attributed to smoke PM_{2.5}, regardless of the fire type.

A key issue is HMS's ability to detect prescribed burn smoke. Jaffe et al. (2020) note that satellites tend to detect larger, more intense fires (like wildfires) while smaller fires (like prescribed burns) may go undetected [39] (L649-L651). Wildfires, being uncontrolled and often intense, generate more detectable plumes compared to the carefully managed prescribed burns [43]. This suggests that HMS may exclude some—though not all—prescribed burn smoke due to their generally smaller size and intensity (L765-L767).

Another important consideration is how PM_{2.5} from both wildfires and prescribed burns may mix on the same “smoke day.” If a wildfire triggers a “smoke day” and a nearby prescribed burn also contributes to the PM_{2.5} levels, this would be reflected in the estimates. However, the likelihood of this scenario is underexplored in the literature and remains a research gap.

At the ground level, wildfire events tend to cause sharp spikes in PM_{2.5} concentrations downwind (L597-L599), while prescribed burns lead to more moderate increases, akin to other sources of air pollution (L603-L604) [43]. This suggests that ground-level monitoring is more likely to detect wildfire-driven changes than those from prescribed burns. However, Childs et al. (2022)’s methodology of examining deviations from the median PM_{2.5} helps mitigate this limitation. There is, however, a related point to consider. If frequent prescribed burns influence a location’s median PM_{2.5}, the deviations would represent changes not from a smoke-free day but rather from a wildfire-smoke-free day. Ultimately, this effect would depend on the frequency of prescribed burns and the prevalence of their smoke across the distribution of days throughout the year.

This review highlights the benefits of using “source- or emissions-informed” models like reduced-complexity models (RCMs) in estimating smoke PM_{2.5}. While there are uncertainties in all fire smoke models, including chemical transport models (CTMs), RCMs and CTMs allow us to differentiate between wildfires and prescribed burns (as well as between specific fires—see our response to *Comment R3.7*), a key advantage over “receptor- or concentration-informed” models such as Childs et al. (2022). Importantly, AP4T’s benefit of differentiating prescribed burns from wildfires is an RCM benefit when compared Childs et al. (2022) but a National Emissions Inventory (NEI) benefit compared to Xie et al. (2020) [16], which used the Global Fire Emissions Database (GFED) that relies on satellite data [14], [15] (L618 & L650).

However, RCMs, particularly the current set up of AP4T, also come with limitations. One significant issue is the assumed emission height. As Williamson et al. (2016) noted, wildfires often inject smoke into higher atmospheric layers due to their intensity, leading to wider pollution dispersion, while prescribed burns, being controlled, generally stay within the atmospheric mixing layer [41] (L595-L597 & L601-L603). Both fire types, however, are modeled as ground-level sources in AP4T with emissions releasing from the surface (L583-L585), which may not fully reflect the reality of plume behavior. Although not a perfect comparison, wildfires may function

more like high-polluting facilities with smokestacks, such as electric generating units (EGUs), which are designed to disperse pollution away from local communities [73].

Furthermore, the temporal modeling of emissions presents another limitation (L605-L610). AP4T assumes emissions are released uniformly throughout the year [75], which works well for continuous sources like traffic or baseload EGUs. However, both wildfires and prescribed burns are episodic events. Wildfires can burn for days or weeks [76] at unpredictable times, while prescribed burns are conducted under specific meteorological conditions to ensure safety and effectiveness [42], with the goal of minimizing impacts on nearby communities [43]. This suggests that the plumes from these fires may differ either randomly or systematically from typical pollutant transport patterns, complicating AP4T's ability to accurately model the episodic nature of fire smoke. However, this limitation of AP4T for episodic, rather than steadier, sources of air pollution enables a beneficial perspective for decision-makers and planners working toward future wildfire smoke damage mitigation efforts, providing expected effects given the unknowable ignition times and corresponding weather patterns (L610-L614).

In summary, RCMs provide an efficient and accessible method for modeling smoke from wildfires and prescribed burns, enabling a clear distinction of pollution impacts based on their sources. However, these models have limitations in accurately capturing the physical dynamics of fires, as well as the temporal and spatial characteristics of their emissions and smoke plumes. While we appreciate the reviewer's suggestion to include a dedicated paragraph discussing the benefits and challenges of applying RCMs to fire smoke, we believe that our current approach—highlighting the main benefits in the *Introduction* (L68-L71) and discussing the more detailed advantages and limitations in the *Methods* section—maintains a better flow for the paper. Additionally, much of the commentary regarding the comparison of AP4T, Xie et al. (2020), and Childs et al. (2022) is specific to those studies and not directly related to an RCM versus a CTM or “receptor- or concentration-informed” approach.

R3.5) The correlation results between SVI and smoke damage are very interesting, and I appreciate the analysis of the different themes which shed more light on this aggregate metric. However, I find it a bit challenging to reconcile the correlation results and the section below on age/race-specific baseline mortality rates. I am a bit confused as to what “vulnerability” means in the calculation. It seems like the “vulnerability” only influences the damage through the baseline mortality rate, is this correct? It appears that the authors are using county-specific mortality results so that perhaps capture some of the vulnerability in addition to age/race (e.g., low-income counties may have higher baseline mortality results). How much variations do age and race alone explain the aggregate correlation between SVI and smoke damage? Could the authors clarify this?

We thank the reviewer for their thoughtful question and appreciate the opportunity to clarify our approach to vulnerability. Vulnerability in our analysis is considered both in terms of smoke exposure and how it influences smoke damages.

First, regarding smoke exposure, vulnerability is captured geographically using the Social Vulnerability Index (SVI), which ranks census tracts based on their vulnerability compared to others [26], [27]. Importantly, vulnerability is assigned at the tract level, so the most vulnerable person and the least vulnerable person in a single tract are treated the same for our analysis—they are assigned vulnerability based on their tract’s average characteristics (for reference, there are roughly 4,400 people per tract, see Supplementary Table 12). In this context, smoke exposure is linked to vulnerable populations via the tract-level SVI and tract-level PM_{2.5} concentration modeling with AP4T. When using population-weighted averages (e.g., New Fig. 2a and 2b, New Fig. 3a, and New Fig. 4a and 4b), each person’s (rather than tract’s) concentration exposure contributes equally to the average.

In terms of smoke damages, vulnerability influences damages beyond the geographic distribution of vulnerability via the SVI. Damages are a function of exposures, concentration-response, and valuation. Since the value of a statistical life (VSL) remains constant across populations, the main differentiator for damages, beyond exposure levels, is susceptibility to PM_{2.5}.⁹ This susceptibility is influenced by both relative and baseline mortality risks, which are incorporated into the dose-response function [19], [20] (see Equation 4 of the manuscript and our response to *Comment R1.5a*). While relative risk is not differentiated by default in our model, we provide sensitivity analyses using race/ethnicity-specific relative risks [11] (see New Fig. 7 and New Fig. 8). The key differentiator for health effects here is baseline mortality risk, which varies by geography, race/ethnicity, and age.

Baseline mortality rates increase with age (see Supplementary Table 9) and also differ by race/ethnicity (see Supplementary Table 10). Geography captures additional factors (e.g., income disparities between counties) that influence baseline mortality. We use county-level data [58] and estimate tract-level mortality rates for each age group by accounting for each tract’s population breakdown by race/ethnicity [57] (see Equation 6). For example, if a tract’s 85 and older population is 50% Black or African American and 50% Non-Hispanic or Latino White, its mortality rate for that age group is informed proportionally by the county-level rates for those races.

To clarify the role of vulnerability, while baseline mortality risk does not directly account for vulnerability as defined by the SVI, it is influenced by factors (e.g., race, age, geography) that are associated with vulnerability. This alignment ensures that tracts identified as vulnerable via the SVI reflect the corresponding influences on baseline mortality risk and damages per unit of smoke exposure. We have added a concise summary of this information to the manuscript in the *Methods* section focused on vulnerability and the SVI (L851-L858).

⁹ The alternative value of a statistical life year approach, used in New Fig. 7 and New Fig. 8, varies mortality risk valuation by age. Please see our response to *Comment R1.5.b* for more information.

R3.6) The results section is quite long and hard to follow as there are too many figures and each figure was discussed with great detail. Some of the discussions also feel repetitive (for example figure 2 vs figure 3). If I understand correctly, figure 3 results only differ from figure 2 as the damage is now normalized by population. If that's the case, would it make sense to just include one of the two figures in the main text (and the other one in SI)?

We have addressed this comment, along with others, by simplifying our figures and refining the results section. Please see our response to *Comment R1.4* for further details. The key point for this response is that Original Fig. 3a and 3b and Original Fig. 2a and 2b are now presented as New Fig. 2a and 2b. This revision clarifies the analysis and reduces redundancy while maintaining the integrity of our original narrative. We appreciate the reviewers' guidance on this matter.

To fully respond to the reviewer's concern, we point out that Original Fig. 2a and 2b of the presented population-weighted average smoke exposure by Social Vulnerability Index (SVI) decile versus that across the entire population, effectively normalizing by population since each individual contributes equally to the average for their respective SVI decile. Original Fig. 3a and 3b, on the other hand, depicted damages per capita by SVI decile versus that across the entire population, which also corresponds to deaths per capita given that the value of a statistical life (VSL) is constant (\$9.97 million per person). The overlap with Original Fig. 2a and 2b occurred when we included the population-weighted exposure data (as bars behind the points) for reference in Original Fig. 3a and 3b. We now recognize that this addition caused confusion.

The intent behind including both figures was to demonstrate that the positive correlation between smoke exposure and vulnerability does not hold when considering health effects—a finding contrary to our original hypothesis, as we expected that introducing health effects would exacerbate the disparities due to the higher susceptibility of vulnerable populations to pollution (as supported by citations in the manuscript). In summary, the goal was to separate findings focused on smoke exposures (Original Fig. 2) from those for damages per capita (Original Fig. 3), but a substantial finding of the latter is its change from the former. Because damages (or deaths) per capita are a function of smoke exposures and concentration-response, the differences between these figures are driven by health effects.

R3.7) The fire-specific results from L161-165 are very interesting. I think it would be helpful to illustrate more about the method behind these results. For example, how did the authors link the gridded NEI emission dataset to specific fire events? It would also be interesting if the authors had more results to show on this in addition to the Tubbs fire (on that note, I was curious why the authors chose the Tubbs fire). For example, it would be interesting to calculate the damages of the top 10 fires in 2017, and compare their health damages to the direct damages (like what the authors did for the Tubbs fire).

We thank the reviewer for their thoughtful comment and interest in this part of our work. Supplementary Fig. 20 and Supplementary Table 21, which were within the original Supplementary Information, present the damages from specific fires in 2017, including the following (with ignition dates in parentheses where available):

- **Tulare County, California:** Pier (8/29), Lion (9/25), Schaeffer (6/24), Indian (8/3), and Roadrunner (7/30) wildfires
- **Sonoma, Napa, and Solano Counties, California:** Tubbs (10/9), Nuns (10/9), Atlas (10/9), Pocket (10/9), and Thirty Seven (10/9) wildfires
- **Ventura and Santa Barbara Counties, California:** Thomas (12/4) wildfire
- **Collier County, Florida:** Prescribed burns
- **Cameron and Vermilion Counties, Louisiana, and Jefferson and Chambers Counties, Texas:** Prescribed burns

These fires were selected because they represent localized “hot spots” where high emissions from relatively small geographic areas resulted in significant damage. Emissions data from the National Emissions Inventory (NEI) [7] are distributed from the county level to the tract level using fire perimeters or burned area boundaries from the Monitoring Trends in Burn Severity (MTBS) database [77]. This methodology is briefly noted in the manuscript (L553) and described in detail in the text associated with Supplementary Fig. 7. Damages from these emissions are then modeled using AP4T. The fires were identified by locating census tracts and counties with the highest damages by source, focusing on these “hot spots” (typically clusters of adjacent or nearby census tracts in one or more counties), and cross-referencing MTBS to match damages with specific fires.

For example, Tulare County, California, experienced the greatest smoke damages of any county in 2017. Further investigation revealed that these damages were primarily concentrated in two census tracts, which, according to the MTBS, were the areas affected by the *Pier*, *Lion*, *Schaeffer*, *Indian*, and *Roadrunner* wildfires between June and August of 2017 [77]. By isolating the damages that originate from these census tracts, we are able to attribute them directly to these specific fires.

Another critical resource for our *Tubbs* wildfire analysis in the manuscript is the California Department of Forestry and Fire Protection’s 2017 Wildfire Activity Statistics report [76]. These annual reports provide detailed information on each fire, including ignition and containment dates, acreage burned, structures destroyed, structures damaged, and fatalities. Supplementary Table 22 includes this data for the specific fires evaluated, providing interested readers with a comprehensive reference.

It is important to note that the fires assessed are not strictly single, discrete events; many involve clusters of fire events. An exception is the *Tubbs* wildfire. As detailed in Supplementary Table 22, the *Tubbs* fire ignited on October 8, 2017, burned 37 thousand acres, destroyed 5.6 thousand structures, and resulted in 22 fatalities [76]. The *Tubbs* fire was selected for inclusion in the manuscript because it is the second most destructive wildfire in California’s history and the second deadliest of the 21st century, surpassed only by the *Camp Fire* in 2018 [78], [79]. By comparing the damages from *Tubbs*’ flames to those from its smoke (248 premature deaths, as derived from Supplementary Table 21 and using the value of a statistical life at \$9.97 million), we illustrate the significant impact of smoke-related damage.

The *Tubbs* fire was one of several fires in the North Bay Area that ignited in early October under severe fire weather conditions. Alongside *Tubbs*, the *Nuns*, *Atlas*, *Pocket*, and *Thirty Seven*

wildfires burned a combined total of 115 thousand acres, destroyed over 2.1 thousand structures, and resulted in 9 fatalities [76]. In total, these fires caused 549 premature smoke deaths (797 including *Tubbs*). Physical damages from these fires amounted to more than \$6 billion [76].¹⁰ Smoke damages from these fires amounted to nearly \$8 billion. We added the following in the manuscript to capitalize on the intended point of this assessment: “[*Tubbs*] was one of several that ignited in the North Bay Area in early October under severe fire weather conditions, collectively causing over \$6 billion in physical damages [76]. The smoke from these fires caused nearly \$8 billion in damages (797 premature deaths), highlighting that the social costs from fire emissions can rival the physical costs of even the most devastating wildfires” (L175-L179).

The Tulare County fires present a different scenario as they did not all ignite simultaneously. They are considered together here because, again, they occurred within just two census tracts, effectively making them a single source for the sake of our modeling. These wildfires were less destructive and deadly compared to those in the North Bay Area in early October 2017. Collectively, the *Pier*, *Lion*, *Schaeffer*, *Indian*, and *Roadrunner* fires burned 76 thousand acres, with *Pier* destroying 2 structures [76]. Physical damages from these fires amounted to just \$24 thousand [76].¹¹ However, these fires caused 824 premature deaths due to air pollution, surpassing the number of deaths attributed to the smoke from the *Tubbs*, *Nuns*, *Atlas*, *Pocket*, and *Thirty Seven* fires. Smoke damages from these fires amounted to more than \$8 billion.

Like the *Tubbs* wildfire, the *Thomas* wildfire is another distinct fire event. It ignited on December 4, 2017, and burned 282 thousand acres, making it the largest wildfire in California for that year [76]. The *Thomas* fire destroyed over 1 thousand structures and resulted in 2 fatalities. Notably, the *Thomas* fire continued burning into the new year, finally being contained on January 12, 2018. As a result, the total emissions from this fire event are only partially reflected in the NEI for 2017 [7]. Consequently, the count of 790 premature deaths attributed to its smoke represents only 27 of its 39 days (69% of its duration). The California Department of Forestry and Fire Protection does not report physical damages for Ventura or Santa Barbara County [76].

The prescribed burns in Florida, Louisiana, and Texas differ from the wildfire events discussed here because they are not specific fires but rather all prescribed burns in counties with exceptionally high smoke damages. Collier County, Florida, and Cameron County, Louisiana, are the only counties exceeding \$1 billion in smoke damages. The next highest is Vermilion County, Louisiana, which is Cameron County’s neighbor along the Gulf Coast, with roughly \$800 million in damages. Further analysis reveals that prescribed burns in Jefferson County (\$410 million in smoke damages) and Chambers County (\$230 million in smoke damages) in Texas, which are adjacent to the Louisiana counties along the Gulf Coast, are also significant. As prescribed burns, these events likely did not cause any physical damage and are therefore excluded from Supplementary Table 22.

While we appreciate the reviewer’s suggestion to examine the top ten fires of 2017 and compare their physical and smoke damages, as we did for the *Tubbs* fire and other specific

¹⁰ These are summed damages from Sonoma, Napa, and Solano counties and are, presumably, in 2017 U.S. dollars.

¹¹ These are reported damages from Tulare County and are, presumably, in 2017 U.S. dollars.

incidents, this approach is feasible for California wildfires due to the state's extensive documentation of fire activity and physical impacts [76]. It is possible that the top ten most damaging fires of 2017 are indeed California wildfires. However, focusing solely on these would shift our study's emphasis away from our primary goal of analyzing wildfire and prescribed burn smoke impacts nationwide. By highlighting the *Tubbs* fire as the most physically damaging wildfire in a state with significant fire activity and comparing its physical damages to smoke damages, we effectively contextualize these costs, as intended. Supplementary Fig. 20, and Supplementary Table 21 and now, Supplementary Table 22 and the associated discussion (some of which is taken from this response) provide additional context for interested readers. We consider the reviewer's suggestion intriguing but believe our existing analysis, combined with the additional information provided, sufficiently meets our research objectives while staying true to our main focus.

*****We want to sincerely thank the reviewers for their comments. We strongly believe that the feedback has pushed us to greatly improve the paper in numerous ways. Your engagement is highly appreciated!**

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