nature portfolio

Peer Review File

Global warming amplifies wildfire health burden and reshapes inequality

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

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
See the attachment

Referee #2

(Remarks to the Author)
Summary of the key results

This paper uses several established modeling techniques to show that increasing wildfire activity could results in elevated mortality from fine particulate air pollution attributable to wildfires. The authors also show sensitivities regionally to different assumptions in the SSP estimates. Finally, their analyses show perhaps less convincingly that inequalities in wildfire-related deaths will decline.

Originality and significance

While using established methods and data products, the paper still provides an original analysis due to the numerous linked products it combines. Wildfires are becoming projected to become more frequent and severe because of climate change, increased development at the wildland-urban interface, and forest mismanagement (broadly falling into too much fire suppression in some regions and too much intentional burning for agricultural conversion in others). Given the potential for growing impacts at a global scale from wildfires the paper is significant and of potential interest to wider scientific and policy audiences.

Data and Methodology

Although generally acceptable, I have several comments here, some relating to presentation clarity, some to sensitivity analyses, and others on noted weaknesses.

The authors note that the ML method they developed agrees “well” with other predictions and trends. Can they please be more specific about how the model agrees well. This is vague and uninformative to the reader, and something more specific should be in the main body of the paper.

On the fire emissions, can the authors offer some assurance to the reader that the current GFED estimates will be accurate on what are likely to be very different vegetative regimes by the end of the century? This seems to be something readers might question.

The assumption that the peatland contributions will remain fixed seems unrealistic given the increased chance of drought –

could the authors offer some conjecture on what this exclusion would mean in the worst case for their estimates? Is there literature that the authors could cite on projected changes in peatland burning to put the importance of this assumption in context?

The authors assume equal toxicity in their health impact assessment (HIA) which is based on the undifferentiated PM2.5 functions found in the GEMM model, but they are suggesting in the introduction that the wildfires could be more toxic. If they are suggesting this, a recent paper by R. Connelly et al. confronts this issue by adjusting the chronic exposures from PM2.5 with acute wildfire estimates. https://www.science.org/doi/10.1126/sciadv.adl1252. Could the authors implement a similar approach to show the sensitivity of their estimates to an explicit treatment of greater toxicity – the Connelly paper does give a global estimate for all-cause mortality that could potentially be used in this analysis to show the sensitivity of the reported estimates to inclusion of a dose-response function that accounted for greater toxicity in wildfire PM2.5? This would give a first approximation of what a different dose-response function would mean for the HIA estimates, and it would probably show even greater impacts.

The authors explain the integration of the population data into the 2 * 2.5 degree grids, but they also need to give the scale of the GBD data and discuss how this was harmonized.

Results and Figures

Figure 1 has way too much information jammed into it, which makes it somewhat hard to follow for readers. Panel B seems to stick out and doesn't flow well. It is fine to make this point, just not on the same figure. When it says all other regions – does that mean in Africa. Sorry the plot confused me, and I am not sure what to recommend other than a re-thinking of what they are trying to say with this and to put more in the description on the figure.

Figure 4 – it was never explained as to why the lower emission scenarios resulted in large variation in the ratios presented. It was unclear why in Africa and the middle east the green bars showing the ratios of the lower emission scenarios tend to produce larger ratios and why in other places they produce much lower ratios than the red bars representing the higher ratios. Similarly, there are huge differences going in the other direction in wealthier places such as Europe and North America. Some further explanation is needed here. It also merits mentioning that the higher emission scenarios result in higher ratios in most places.

Clarity and Context of Abstract, Intro and Conclusions
The first line of the abstract didn't agree well with the content of the paper and I would suggest changing this to something closer to the first line of the introduction.

As noted above, wildfires are becoming projected to become more frequent and severe because of climate change, increased development at the wildland-urban interface, and forest mismanagement (broadly falling into too much fire suppression in some regions and too much intentional burning for agricultural conversion in others). The introduction only emphasizes the climate change and drought as driving factors, but the issues of WUI and forest mismanagement should be mentioned as well. These are covered in the conclusion, but the following line is somewhat inaccurate. The controlled fires to improve forest management are usually not intended to convert land to other uses, but they are meant to reduce fuel loads and stop larger fires from propagating into areas where they can grown rapidly. Land conversions in many instances such as in the Amazon are a root driving force behind higher wildfire risk because the land conversion from forests to agriculture often result in uncontrolled fires, so the authors need to be more accurate here.

"Wildfires can be partially mitigated through evidence-based fire management, as well as thoughtful planning and design of natural and urban landscapes32, including controlled fires initiated by humans for land conversion purposes."

The authors note that particles from wildfires tend to be more toxic. Here because there's still considerable uncertainty on the toxicity of wildfire particles compared to undifferentiated PM, authors should moderate this statement by noting can be more toxic or have been shown in some studies to be more toxic would be better (this raises another methodological issue which is discussed under the methodology section of this review).

Related to this point, the authors have assumed that the exposure to wildfires represents a long-term or chronic exposure, but there are no existing studies that directly address the long-term impacts of wildfire-specific PM2.5 on mortality at this time. So they are making a rather large assumption that the periodic nature of wildfire smoke also equals a chronic exposure. The Connelly article noted above addresses this issue in detail. The authors probably need to acknowledge this limitation in treating the exposure as chronic when there are no primary studies that address this.

References

The citation to Cohen et al. 2015 could be replaced or supplemented with the Burnett et al. 2018 report in PNAS because this latter study used arguably better methods to estimate the total burden of death from air pollution.

Referee #3

(Remarks to the Author)

This paper addresses future wildfire smoke and its impacts on health and radiative forcing of climate. Since wildfires have emerged in the past decade as an important unknown as climate changes, and because Earth System models (ESMs) are working to imbed fire occurrence internally but lack consistent approaches, this paper is timely. The authors use a novel method in which they use a historical (for recent years) fire emission inventory as well as climate parameters to train a machine learning model to predict fire occurrence and emissions in future scenarios to 2100 where climate is changing. Based on the introduction and references listed there, it seems that other studies have similarly used statistical models and machine learning models to explore relationships between climate variables and fire occurrence. But it seems that the present paper is the first to do this on a global scale and to apply the relationships found to future climate scenarios globally.

I have several questions and concerns as described below. But I also expect that with satisfactory attention to these questions and concerns, and revisions to the paper where appropriate, that this can be an important addition to the current literature on the importance of climate change for future wildfire smoke and impacts.

Concerning the machine learning estimates of fire emissions:
1) The idea to analyze historical fire emissions for climate drivers using machine learning is a good one. However, the machine learning methods are a bit of a black box. Very little text in the main paper describes the inputs to the machine learning, and only one sentence on it appears under Methods, although there is more text and some results in the supplement. Can more be said about how the machine learning is conducted, and why these methods vs. others are used?
2) I am not clear about how observations of climate (represented in the ECMWF ERA5 renalaysis) versus the ESM historical simulations are used in training the machine learning. Supplement line 41 says that only the ESMs are used in training the machine learning, and not ECMWF, and supplement line 61 says that ECMWF is used to estimate historical burned area after the machine learning was conducted. The ESMs are commonly run with meteorology that "floats," meaning that it does not necessarily represent historical meteorology within a given year, and it would miss important natural variations. For that reason, I would think it best to use ECMWF meteorology parameters, and not the ESMs, to best capture historical meteorological parameters for the machine learning. Why were the ESMs used for training the machine learning, and not ECMWF? Were the ESMs run with meteorology nudged to observations or were they freely floating?
3) There would be much interest in understanding the historical climatic drivers of past wildfire burned area and emissions. The authors show the importance of different variables for future climate change in the two scenarios (Supplement Figures 6 and 7), but here they do not interpret the results. For example, what does it mean that "Coordinate" is the most important predictor variable? More importantly, a similar analysis of drivers is not given for the historical conditions analyzed, and I think this would be valuable. While it would be possible to have a follow-up paper focus on this analysis, adding it here can help the readers evaluate the results of the ML.
4) In addition to meteorological drivers, I understand that human land management and forestry can play an important role in determining fires (I'm not an expert in this area). The authors acknowledge opportunities to use fire management in the future to adapt to climate change, but the authors did not acknowledge that human actions may add uncertainty to their machine learning model of past fire occurrence, nor did they include any land management variable in the machine learning.
5) Looking at the novelty of the fire projections, the authors say in the introduction that ESMs disagree with one another, adding motivation for a machine learning estimate. But then in the results, the estimates here for the future agree well with the ESMs (although the ESMs themselves do not agree?).
6) In line 409, the authors describe the methods of using biomass from ESMs along with their machine learning estimates of fires to get the emissions. If the fire occurrence within the ESMs differs from that projected using these methods, would the available biomass be different, and would that then affect emissions? More broadly, are there reasons to think that errors in the methods used here might grow over the coming century, or are the authors confident that these methods can be used reasonably for projections that far into the future?

Concerning their estimates of DRF from fires:
1) I do not understand the results presented (Fig. 1c) because it is not clear what time period DRF is relative to. If the preindustrial, then how are preindustrial conditions modeled?
2) Wildfires emit both gases and particles. The authors estimate DRF only for aerosols (accounting for the secondary production of aerosols by emitted gases, I assume), but they leave out changes to greenhouse gases including methane and ozone through atmospheric chemistry. I thought that for fires, the effect on methane and ozone would be relevant for climate, but perhaps I am wrong. The authors should expand to include other forcings, or should justify showing results for only aerosol DRF.
3) Given that the authors present CO2 emissions from fires and the DRF from aerosols, it may be possible to present the net climate effects of the fires from all forcings, and as a climate feedback factor (additional forcing from fires per degree warming).
4) It's hard to know what to conclude from the DRF shown. The abstract, for example, doesn't give any conclusion about the DRF estimated. One option the authors might consider would be to focus this paper on PM2.5 and health, and leave the DRF climate consequences for follow-on papers - but I will leave that decision to the authors.

Comments on the health impact estimates:
1) The 2x2.5 degree resolution modeling is generally good for estimating fire occurrence, but is coarse for estimating mortality, as significant differences in concentration can occur within a grid cell – for example between urban and rural regions. The state of the art today would typically use finer resolution for health impacts, so this choice adds to the overall uncertainty.
2) l. 130 – The authors show that their present-day estimate of PM2.5-related deaths agrees well with the Global Burden of Disease study, but they are comparing with the GBD of several years ago. The most recent GBD is here: https://www.thelancet.com/journals/lancet/article/PIIS0140-6736(24)00933-4/fulltext. More importantly, the authors are using GEMM which has been estimated to give much larger numbers of PM2.5 deaths globally than the 4 million here and in the

GBD studies. That leaves the question of why the global estimate here is so much lower than other estimates using GEMM.

Other comments:
l. 30 – I think the number here is per year, not accumulated over the 5 years.
l. 37 – How does the diminishing disparity underscore the need to address … ?
l. 123 – On what basis do you conclude that your estimates are more reasonable?
l. 133 – Is it coincidence that both scenarios give the same future deaths from wildfire smoke? Wouldn't we expect the more severe climate change scenario to yield more deaths, since it had more fire and fire C emissions (Fig 1)?
l. 138 – Radiative forcing is defined for a change in state, often from one year to another. By convention radiative forcing is most commonly defined relative to preindustrial concentrations. It's not clear in this paragraph what years are being compared when giving radiative forcing outcomes. Is DRF in the future relative to the present? Or relative to preindustrial? How about the historical DRF presented?
l.154 – If I understand correctly, the "POP" scenarios do not require another simulation of GEOS-chem, but rather would use the same GEOS-chem simulation with different population inputs for the mortality calculation.
l. 159 – Again, if I understand correctly, the "POP" scenarios change both total population and age structure, from which it would not be possible to separate the influence of population growth from the age structure. But here the authors attribute the change to the age structure alone.
l. 160 – The numbers of deaths here and in Figure 3 can only be understood as contributors to the overall impact of fires on deaths, which is given earlier as 1.19 million. I suggest they be presented as such.
l. 179 – Here the authors clarify that different population assumptions are made in the two SSP scenarios. While it's true that the SSPs have different population, it might be more straightforward to keep population the same between the scenarios, to focus on the impacts of fires.
l. 183 – In the text it is not clear whether the authors are comparing future wildfire smoke deaths with a) present wildfire smoke deaths or b) all PM2.5-related deaths, when they reach the conclusion that disparities diminish. Figure 4a clarifies that it is compared to present wildfire smoke deaths. That should be clarified in the text, and the authors might consider comparing with all PM2.5 deaths since they also estimate that.
l.198 and Figure 4 – Consider presenting disparities in smoke relative to percents of population, rather than percent of global GDP. For an exposure and health issue, I'm not sure how GDP is relevant.
l. 206 – Might more be said here to put the fire smoke PM2.5 deaths into context of current or projected deaths from PM2.5 more generally?
l.409 – What is PRD? I don't see it defined.
l. 428 – "Species" is both singular and plural. "specie" isn't a word.
Figure 3 – The labels on the 2nd bar seem wrong as 346 looks bigger than 536.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
I appreciate the authors' great efforts to improve the manuscript. I have the following suggestions, which are mostly minor.

1. Intentional burning for agricultural conversion is not necessarily considered "forest mismanagement." While I understand the authors' intent, some readers may misinterpret the text as implying that all intentional burning is mismanagement. The authors do acknowledge the benefits of controlled fires for forest management, but only in the Discussion section rather than the Introduction. I suggest either removing the parenthetical phrase or clarifying the distinction more explicitly in the Introduction.

2. The GDP presented in Figure 4 does not specify the time range. The total GDP amount and country-specific shares are unlikely to be the same between 2010-2014 and 2095-2099.

Referee #2

(Remarks to the Author)
The authors have responded well to the comments with many useful revisions that have improved the clarity and content of the paper. I have no further suggestions.

Referee #3

(Remarks to the Author)
Overall the authors were thorough in their responses to my earlier comments, with some rather minor remaining questions highlighted below. They also made some efforts to improve estimates, such as by better accounting for fine-scale PM2.5 population exposure. I also had new observations on the equity analysis below. I think this is an interesting study that is well-conducted overall, that uses some new ML methods to project future fire occurrence and its impacts.

Regarding the inequality analysis, two comments. First, I think the authors are estimating premature deaths by regions (Figure 4a) or countries, and that is the basis for determining inequity on the basis of GDP of those regions or countries. That is a simplification that misses differences among GDP within countries, in which rural populations may be exposed to greater fire smoke generally than urban populations. Of course some countries have extremely large population. It may be appropriate to clarify the methods used and the limitations they pose. Second, it may be worthwhile to compare inequality due to fire smoke with that due to total PM2.5, at present and in the two future scenarios, to better put the inequality into a broader context. Do changes in fire smoke PM2.5 make existing inequalities for PM2.5 worse or do they alleviate them? Perhaps 3 new lines could be added to Figure 4b.

In their response to my question #3, I don't see that the authors addressed the question of whether the ESMs were run with fixed (actual historical) meteorology or meteorology that floats. Also, are all 5 models (4 ESMs and ERA5) provided as input predictors of meteorological parameters, and then the ML decides how to weight the different predictors?

Regarding my question #5, the historical training period is short enough that perhaps no big changes in land use and fire management occurred. But I suspect that had the authors trained over a much longer historical period (acknowledging the BA data would not be available), land management factors and particularly fire suppression seems like it would be quite important – perhaps a caution on future projections. The authors' response is sufficient unless they want to strengthen the discussion to include this point.

Regarding #8, the change in text is helpful. I would suggest putting a description of what DRF is relative to in the figure caption, so it is clear what quantity is plotted. But that is the authors' choice.

Regarding #16, I don't see how your estimates are "more reasonable". It is good to point out that they are different, as you do. But I don't see that you have a basis for concluding that one is more reasonable than another.

Regarding #26, the authors are clear in the definition of PRD(..), but it seems strange to use the PRD name for a variable if it does not stand for anything.

Referee #4

(Remarks to the Author)
In their manuscript, Zhao et al. use a machine learning approach and chemical transport model to project future wildfire-induced PM2.5 pollution under two different climate scenarios, and then use the long-term exposure estimates generated by this approach to estimate changes in the number of deaths attributable to wildfire-induced PM2.5. They also investigate the drivers of these changes (e.g., exposure/climate change, population, etc.) and the impact of changing emissions on Earth's radiative balance.

I appreciate the authors' work to address the first round of reviewer comments. Overall, the writing and data visualizations are good and the methodology seems relatively sound. I would have no problem recommending this manuscript for publication in a disciplinary journal after addressing my comments and other comments from the other reviewers. To me, the question for a Nature submission becomes what new insight has been gained from this analysis. This is where the manuscript comes up short for me, especially since there are recent papers on this topic (e.g., https://iopscience.iop.org/article/10.1088/1748-9326/ad1b7d/meta). Perhaps this a question for the journal editors and also an opportunity for the authors to think about the novelty that their results represent and ways to discuss that novelty.

MAJOR COMMENTS:
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• While I do not doubt the authors' findings that global inequity with respect to fire-sourced PM2.5-attributable mortality is projected to decrease, I'm personally not sure whether this finding is a positive one as the authors have framed it (e.g., in the title "while narrowing global inequality disparities", "diminished global inequity", etc.). This narrowing is accomplished by increases virtually everywhere but that are uneven such that northern mid-latitude countries "catch up" to places like Africa. Therefore there are no winners - everyone will suffer the impacts of changing wildfire smoke and associated health impacts. Can you rephrase these occurrences to something more realistic about why it is that inequality is projected to narrow in the future?

• In the discussion (Line 314ff), the authors mention "Given the potential for even greater fire-induced greenhouse gas radiative effects"; however, I thought that the authors found that increased fire-induced aerosols enhance the cooling effect in much of the world (e.g., line 200ff, figure 2)? Please clarify and adjust text as needed.

• I feel that the section on projecting future fire emissions (lines 489ff) could use some more explanation, specifically regarding what drivers of future land-use changes and therefore emissions are accounted for in their machine learning approach.
The authors mention that biomass estimates from ESMs account for land-use changes (line 321), but it wasn't clear to me whether these are climate-change induced impacts on land use (e.g., changing precipitation patterns lead to less arable land) versus exogenous events (e.g., deforestation for increased agriculture). It would be great to understand the extent to which these factors are accounted for and whether or not their predictions at least roughly follow projections of land-use changes from the literature (e.g., https://www.sciencedirect.com/science/article/pii/S0959378016303399#fig0015;

https://www.nature.com/articles/s41598-024-61035-0). I'm also curious as to the ability of the approach used by the authors to estimate changing emissions and whether or not this approach and/or any assumptions of the machine learning algorithm (e.g., linearity, etc.) can capture potential non-linearities in land-use changes.

MINOR COMMENTS:
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• Lines 29-32: I would argue that the authors' results regarding the growing burden of disease is alone enough to underscore the need for coordinated efforts to address wildfire-related health threats. As it's currently written, it seems as if this call to action is only valid because health disparities will diminish. Suggest rephrasing.

• Lines 221-223: Since the number of attributable deaths (1.51 and 1.87 million for the two scenarios) are higher than the total number found using the non-Connolly RR approach (i.e., line 185ff), are these results found using the Connolly approach? Please clarify and make this distinction in the text.

• Lines 245ff: These phrases feel non-sequitur. The authors mention that fire emissions are expected to decline over Sub-Saharan Africa and then "these changes are associated with shifts in the age structure...". In this case A != B and there could be emissions changes without a change in the population structure. Please revise. Maybe "…decline in fire emissions over this region. At the same time, there are shifts in the age structure…"

• Line 329: "*A* previous study"

• Lines 535 and Equation 4: What does the superscript 245/585 represent?

• Lines 582-583: Calculated in real time sounds a little weird and more like something that would be used for a short-term air quality forecasting model. Maybe "were calculated online using MERRA-2 meteorological fields" would be better?

• Line 155: Was significance assessed formally?

Version 2:

Reviewer comments:

Referee #3

(Remarks to the Author)
The authors have responded adequately to my previous comments and concerns with one exception.

The authors now clarified that they used output of 4 ESMs run with floating (not historical) meteorology to train the AI, and use the meteorological reanalysis ERA5 to test the results. I remain confused on the motivation to do this. While the 4 ESMs likely have realistic representations of present-day meteorology, they will not capture year-to-year variability correctly. It seems to me that should limit the abilities of the ML to diagnose the relationships between meteorological variables and BA. For example, one would expect that fires would occur during hot and dry conditions. In a year when a fire occurred, the ESMs would be as likely as not to identify this as a drier than normal year. The variability would be random. How then can the ML diagnose the right relationships? I don't understand why 4 ESMs would be used for this purpose rather than ERA5, which would get the variability right. I'd appreciate if the authors would clarify the reasons for this choice, and whether such an approach is used commonly in the literature. I'll also comment that this is presented only briefly in the supplement, without discussion.

I will also comment that I read the points of Reviewer 4 on the novelty of the paper and the long response by the authors. Since I do not work in this immediate field, it is hard for me to judge the novelty. While the long response clarifies the novelty, I will just comment that very little of this is written in the paper itself, and so it seems that the paper is missing a discussion of why it is novel. The paper is a well-done study, apart from my confusion on the use of ESMs to train the ML, and it seems that it can be a solid addition to this field. But the decision of whether or not it is sufficiently novel for publication in Nature lies with the editor, based on the authors' discussion of novelty.

l. 31 – Clarify that the deaths are related to fire smoke.

Referee #4

(Remarks to the Author)
Thanks to the authors for addressing my earlier comments. As I read through their responses, the revised manuscript, and supplement, several additional comments arose. For those that do not directly stem from changes they introduced in their revisions, I apologize for not catching these earlier. However, I believe that they should be clarified before this paper can be considered for publication as future readers may have similar questions.

Major comments:

• The authors quote climate change only driven changes in fire emissions (23% under SSP2-4.5 and 53% under SSP5-8.5) in the abstract and main text (Lines 28-30, 108ff). However, I'm confused about whether these changes truly isolate climate change alone since, to the best of my understanding of their study, the biomass estimates are derived from ESMs that use a land use/land cover data that accounts for changes related to human activity (e.g., deforestation) and socioeconomics (Lines 497-499). Please clarify and adjust, if needed.

• The authors' ML BA dataset was trained on data from GFED, along with other datasets such as the ERA-5 reanalysis (Lines 85-89, SI Text 1-2). But then the authors say that they conducted a performance assessment by comparing their ML predictions of BA with satellite observations from GFED (SI Text 2). First, what do the authors mean by *satellite observations* from GFED? GFED is an emissions inventory and not a satellite instrument. Second, isn't there some problematic circular logic if GFED is used as an input/in training and then the performance of the newly-created dataset is assessed also using GFED and they conclude that the model successfully captured the observed global BA changes (Lines 89-90)? Please clarify.

• One of the authors' calls to action is "expanding regulatory scopes" (Lines 271-272). This is a pretty vague statement, so I'd encourage more specificity. In particular, wildland fire smoke is much more difficult to regulate than emissions coming out of, say, a tail pipe or flue, so I'm curious what regulations they have in mind.

Minor comments:

• There are many occasions in the paper where the text isn't clear or where there are typos that hinder an easy understanding of the text. It would enhance the readability and interpretability of this text if the authors could review their manuscript with this in mind. A few, but not all, that I jotted down include "Part regions" (line 167), "the coordinate space notation (coordinate)" in SI Table 2, "assumingthat" (line 283), "The future meteorologies" (line 557), and several places where the tense changes from past to present in an unexpected way.

• In Lines 289ff, the authors state that "while biomass estimates from ESMs account for land-use changes, variations in emission factors due to land-use changes were incorporated into our future fire emission calculations." Should there be a *not* in the second part of that sentence?

• The authors describe spin-up for the meteorological inputs that are then input to the GEOS-Chem model in Lines 559-561. By talking about 19 years of spin-up in the 2040s (which is a time period they didn't specifically analyze or model), I think there's some unnecessary confusion introduced since it was initially unclear that this spin-up was referring to the meteorological inputs. Personally, I'm not sure whether they even need to describe one of the inputs for GEOS-Chem with this level of detail if these inputs have a reference, and I would encourage them to rework this part of the text.

• The caption of Figure 4 (Lines 353-354) contains materials and methods, specifically the source and version of country-specific GDP data, and I don't see these data referenced in the actual materials and methods part of their paper. I would not recommend that the authors place critical study materials and methods in a figure caption.

• The authors mention that they assess PM2.5-attributable mortality for the "six leading causes of death" (Lines 595-596). This is not an accurate statement, as these six endpoints are not the leading causes of premature death per se but are those for which the GBD has deemed there is sufficient evidence to link to long-term PM2.5 exposure from their burden of proof methods. I encourage the authors to read some of the related papers from IHME/GBD authors on this topic and adjust the text accordingly.

• As someone who is an advocate for FAIR principles and open science, the authors' statement that "source codes for the analysis of this study are available from the corresponding author upon reasonable request" doesn't cut it for me. Who is the arbiter of what constitutes a "reasonable request", and how does this statement jive with Nature's guidelines?
https://www.nature.com/nature-portfolio/editorial-policies/reporting-standards#availability-of-data

Version 3:

Reviewer comments:

Referee #4

(Remarks to the Author)
The authors have sufficiently responded to my comments from the previous round of revisions and have appropriately revised the text.

Responses to reviewers' comments

We appreciate the reviewer's careful and thoughtful comments of our manuscript entitled "***Global warming escalates wildfire smoke health burden while narrowing global inequality disparities***" and for the many helpful suggestions to improve the article. We have carefully considered all feedback and made revisions accordingly. In response to the reviewers' suggestions, particularly regarding the health impact calculations, we have undertaken a comprehensive reevaluation and made the following updates: (1) downscaled $PM_{2.5}$ exposure estimates to 0.1° resolution based on the latest GBD 2021 exposure data, (2) reassessed calculations using the updated GBD 2021 country-level baseline mortality data, and (3) incorporated Type II diabetes-related attributable deaths as well as recalculated 95% confidence intervals for all results. The sentences are depicted in red in the manuscript text to highlight the new addition and used strikethrough for deletion. To be clear, all the responses are in the green background below.

Responses to Reviewer 1 comments:

1. The authors have developed a pipeline to evaluate global burned areas and wildfire emissions until the end of this century based on climatic factors and further quantify the premature deaths and direct radiative forcing associated with fire-induced particulate matter ($PM_{2.5}$) pollution during both the contemporary period (2010–2014) to century end (2095–2099). The project is very ambitious, combining multiple modeling strategies. The finding on narrowing global inequality disparities is new, but, to my understanding, it is subject to caveats as it solely focused on fire $PM_{2.5}$-related deaths.

   While the project is innovative in combining multiple modeling approaches, a key vulnerability lies in the accuracy of these models. If any model is inaccurate, the errors could compound as results are passed through a sequence of interconnected models (as one's inputs use the other's outputs), leading to significant uncertainty in the final projections. Additionally, the premature death is restricted to fire $PM_{2.5}$-related deaths, which may not fully capture the whole picture of the health burden of wildfire. A natural follow-up question is how global wildfire carbon emissions influence climate conditions, and how these climatic changes, in turn, affect human health on a larger scale.

   Answer:

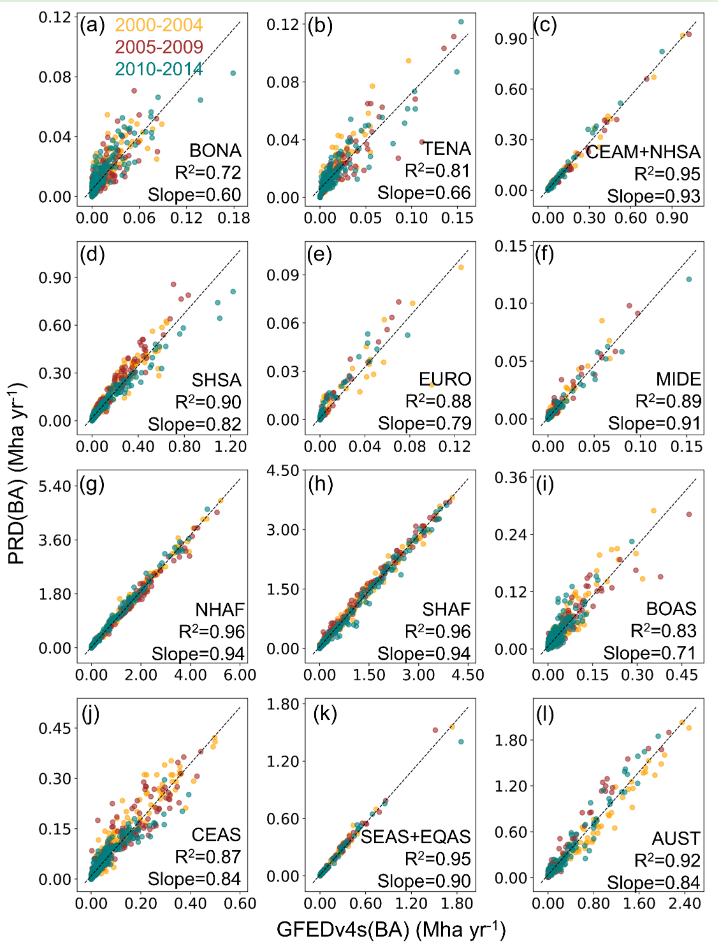
   Thank you so much for all your insightful comments and suggestions. We understand your concerns and have done our utmost to address and clarify the issues you raised. Please find our detailed responses below.

2. Given that a machine learning (ML)-based model was used, a key question is how accurate these models are. For example, what is the highest R^2 value the ML model achieved? Additionally, I am concerned that model performance may vary significantly across different regions.

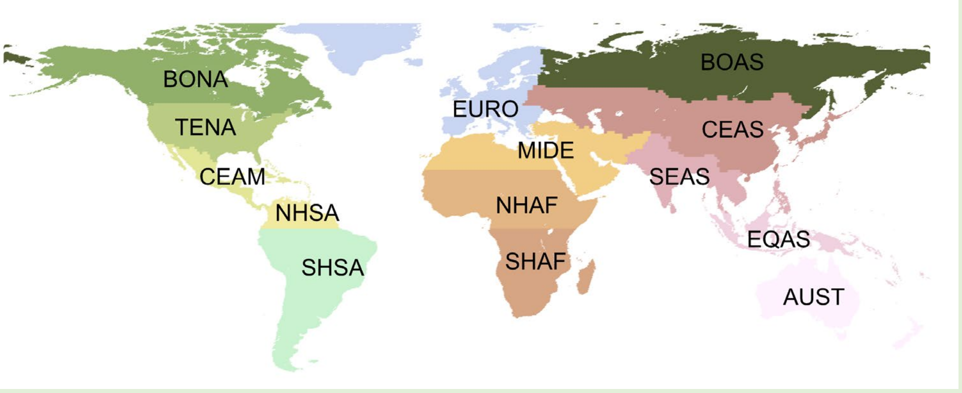
   Answer:

   As suggested, we use the 14 global wildfire emission partitions[1] from GFED to evaluate the ML predictions for the historical period (2000–2014), with the results included in **Supplementary Text 2** as follows:

"*The mean annual PRD (BA) of 466.6 (453.8 to 477.0) Mha yr^{-1} was slightly lower than the mean value of GFEDv4.1s (BA) (475.7 Mha yr^{-1}) for the same period (2000-2014). This discrepancy likely stems from the model's underestimation in fire-prone regions of Africa and Australia (Supplementary Fig. 2 (b,d)). To evaluate model performance across regions, we compared our results with the GFEDv4.1s using GFED's global wildfire emission partitions*[1] *(Supplementary Fig. 9). The comparison (Supplementary Fig. 3) shows strong agreement, with Pearson's R^2 values ranging from 0.72 to 0.96. However, overall predictions slightly underestimate, with slopes generally ranging from 0.8 to 0.9. Notably, North America and Boreal Asia exhibit slopes around 0.6–0.7, where underprediction is more pronounced. In contrast, the Africa region—where global wildfire activity is most concentrated—shows particularly strong performance, with both slope and R^2 values aligning well.*



Supplementary Fig. 3 Evaluation of ML-predicted burned area across different regions. *Panels (a) to (l) show evaluation results for global GFED regions, with the following abbreviations: Boreal North America (BONA), Temperate North America (TENA), Central America (CEAM), Northern Hemisphere South America (NHSA), Southern Hemisphere South America (SHSA), Europe (EURO), Middle East (MIDE), Northern Hemisphere Africa (NHAF), Southern Hemisphere Africa (SHAF), Boreal Asia (BOAS), Central Asia (CEAS), Southeast Asia (SEAS), Equatorial Asia (EQAS), and Australia (AUST). The scatter plots represent 5-year average results for the periods 2000–2004, 2006–2009, and 2010–2014, as indicated in panel (a). The vertical and horizontal axis lengths vary across panels. R^2 and slope represent Pearson's correlation coefficient and the regression line slope, respectively.*



Supplementary Fig. 9 Map of the 14 global wildfire emission partitions used in GFED framework[1]."

The evaluation results across different regions show high R^2 values, although some variation exists. However, given that the primary goal of the ML model in this study is to capture the long-term BA trends at the global and continental scales and to accurately represent their magnitudes, it compensates for the uncertainties introduced by the parameterization schemes in ESMs, thereby enabling more accurate future trend predictions. As shown in Supplementary Figs. 1 and 2, the model successfully captures both temporal and spatial BA trends. When compared with satellite-based fire emission estimates, the model exhibits significantly reduced bias in spatial predictions relative to ESM simulations. Therefore, we believe that the regional performance differences will not significantly affect the overall results.

3. I am unclear on which outcome variable was used to train the ML model. I assume the outcome is burned area (BA), but where exactly does the training data come from? The authors reference "satellite observation-based BA" as the outcome but isn't that data from GFEDv4.1s (BA)? If so, what is the rationale behind comparing PRD (BA) to GFEDv4.1s (BA) when the latter was already used for training? **This is a major concern**.

Answer:

To evaluate the ML model's predictive performance, we performed annual predictions for historical data (2000–2014). For instance, to predict BA for 2014, we trained the model using data from 2000–2013, and for 2013 BA, data from 2000–2012 and 2014 were used, and so on. The evaluation results are presented in Supplementary Text 2. Additionally, recognizing the lack of detailed explanations on the ML framework, input data, and evaluation process, we have expanded Supplementary Text 1 to include the following information:

Supplementary Text 1: ML framework for global burned area estimates

We trained the XGBoost[2] (machine learning algorithm under the Gradient Boosting framework) model to capture the nonlinear relationships between burned area (BA) and multiple climate factors that strongly affect fire occurrence[3,4], and further estimate the global BA. Hyperparameter optimization is a crucial step in improving ML model performance. In this study, the GridSearchCV function, a function in Scikit-learn (https://scikitlearn.org/stable/modules/generated/sklearn.model_selection.GridSearchCV.html) was used for hyperparameter tuning, which includes both grid search and cross-validation. In the grid search, all parameter combinations within a specified range were explored by adjusting the parameters with a specified step size to train the learning algorithm. Cross-validation is a statistical analysis method used to verify the performance and generalization ability of algorithm classifiers. Finally, we chose the set of hyperparameters that yielded the highest average coefficient of determination (i.e., the R^2 regression score function) across all validation sets.

The input data for the ML model consist of BA from Global Fire Emissions Database, Version 4.1 (GFED4.1s) [5], and climate data, including 2 m air temperature (T2), relative humidity (RH), total precipitation (TP), 10 wind speed (W10), leaf area index (LAI), evaporation from the top of canopy (Evaporation). These climate data are sourced from the monthly estimates of four Earth system models (ESMs) and the European Centre for Medium-Range Weather Forecasts Reanalysis version 5 (ERA-5) dataset[6]. Furthermore, to account for seasonal climate variations and the spatiotemporal heterogeneity of climate factors on fire occurrences, we incorporated the sampling times (Time) and geographic coordinates (Coordinate) of the input data into the ML framework. To address data discontinuity, these datasets were transformed into periodic functions, following methods recommended in previous studies[7-9]:

$$\begin{bmatrix} \mathrm{M1} \\ \mathrm{M1} \end{bmatrix} = \begin{bmatrix} \cos\left(\mathrm{month}\dfrac{2\pi}{12}\right) \\ \hline \sin\left(\mathrm{month}\dfrac{2\pi}{12}\right) \end{bmatrix} \tag{1}$$

$$\begin{bmatrix} \mathrm{L1} \\ \mathrm{L2} \\ \mathrm{L3} \end{bmatrix} = \begin{bmatrix} \sin\left(\mathrm{lat}\dfrac{\pi}{180}\right) \\ \sin\left(\mathrm{lat}\dfrac{\pi}{180}\right)\cos(lat\dfrac{\pi}{180}) \\ -\cos\left(\mathrm{lon}\dfrac{\pi}{180}\right)\cos(lat\dfrac{\pi}{180}) \end{bmatrix} \tag{2}$$

The terms month, lat, and lon refer to the sampling data's month, latitude, and longitude, respectively, while M1-M2 and L1-L3 denote the resulting calculations of periodic functions.

Due to large fluctuations in the input data, we applied log transformation and normalization to reduce variability. The dynamic range of predictors was constrained to the [−1,1] interval using min–max normalization, as recommended in prior research[10]:

$$\mathrm{Y_{norm}} = \left[\frac{\mathrm{Y\text{-}Y_{min}}}{\mathrm{Y_{max}\text{-}Y_{min}}}\right] \tag{3}$$

where Y_{min} and Y_{max} are the minimum and maximum values in the data Y, respectively.

The transformed input data were matched to the BA data, which were aggregated into *global monthly data on a 2°×2.5° grid. To evaluate the predictive performance of the ML model, we conducted annual predictions for historical data (2000-2014). For example, to predict 2014 BA, we used data from 2000-2013 for training, and for 2013 BA, data from 2000-2012 and 2014 were used, and so on. Detailed evaluation results are provided in Supplementary Text 2. Last for future projections (2015-2099), we trained the ML model on historical data and used ESM-estimated future climate factors for prediction.*"

4. The authors mention using an "interpretable" ML framework but did not clarify what they mean by "interpretable" in this context.

Answer:

Thank you for pointing out the need for clarification. To address this, we have now clarified the term "interpretable" in the second paragraph of the ***Global wildfire carbon emissions under warming future*** section**:**

" *We used the SHapley Additive exPlanation method to assess the importance of each predictor in our ML model and quantify their contributions to BA predictions, enabling us to evaluate the relative impact of variables such as 2-meter temperature, which was found to be the primary driver of spatial and temporal variations in BA, outweighing other factors such as precipitation, evaporation, relative humidity, etc.—particularly under the SSP5-8.5 scenario, where its influence is projected to intensify in the future. The trend in drought-related SHAP values also suggests that the ML model successfully captures the expanded extent of wildfires under a future climate of global warming and increasing drought. (Supplement Text 4).* "

5. The fire behavior triangle consists of weather, fuel, and topography. Were topographic variables, like elevation, included in the ML model?

Answer:

In the current study, the model parimarily focused on climate-related factors (e.g., temperature, humidity, precipitation) and vegetation-related variables (e.g., leaf area index) that are known to significantly influence wildfire occurrence and behavior[4]. To account for the potential impact of seasonal and latitudinal variations in climate on fire occurrences, we incorporated the sampling times and geographic coordinates of the input data into the ML framework. To address data discontinuity, these datasets were transformed into periodic functions, following methods recommended in previous studies[7-9].

For the topographic variables that do not vary substantially with time, we assumed that by incorporating the spatiotemporal heterogeneity of climate factors and BA, the model indirectly accounts for topographic influences. This is because, across different grid locations, climate variables vary systematically with elevation and terrain features, influencing both the magnitude and frequency of BA. Additional details are provided in ***Supplementary Text 1***:

"*To account for seasonal climate variations and the spatiotemporal heterogeneity of climate factors on fire occurrences, we incorporated the sampling times (Time) and geographic coordinates (Coordinate) of the input data into the ML framework. To address data discontinuity, these datasets were transformed into periodic functions, following methods recommended in previous studies*[7-9].

$$\begin{bmatrix} \text{M1} \\ \hline \text{M1} \end{bmatrix} = \begin{bmatrix} \cos\left(\text{month}\dfrac{2\pi}{12}\right) \\ \hline \sin\left(\text{month}\dfrac{2\pi}{12}\right) \end{bmatrix} \tag{1}$$

$$\begin{bmatrix} \text{L1} \\ \text{L2} \\ \text{L3} \end{bmatrix} = \begin{bmatrix} \sin\left(\text{lat}\dfrac{\pi}{180}\right) \\ \sin\left(\text{lat}\dfrac{\pi}{180}\right)\cos(lat\dfrac{\pi}{180}) \\ -\cos\left(\text{lon}\dfrac{\pi}{180}\right)\cos(lat\dfrac{\pi}{180}) \end{bmatrix} \tag{2}$$

The terms month, lat, and lon refer to the sampling data's month, latitude, and longitude, respectively, while M1-M2 and L1-L3 denote the resulting calculations of periodic functions.".

6. I concern that the uncertainty of the results is hard to quantify, e.g., Projections indicate a surge in premature deaths attributed to wildfire smoke $PM_{2.5}$ exposure, reaching 1.19 million between 2095 and 2099. But how likely the premature death is 1.1 or 1.5 million?

Are 95% confidence intervals plausible?

Answer:

We conducted uncertainty analysis on the premature death estimates using the methodology employed in previous studies[11-14], and updated the results throughout the manuscript. A description of the uncertainty analysis method has been added to the ***Methods*** section as follows:

" *Mortality refers to premature deaths attributable to $PM_{2.5}$ exposure in each country (c), calculated by multiplying the population (pop) of each age group (m) by the baseline mortality rate (y) and the attributable fraction, given by (RR-1)/RR for each specific disease (z), age group, and country. We employed the global exposure mortality model (GEMM)[15] to estimate the relative risk (RR) associated with $PM_{2.5}$ exposures:*

$$\mathrm{RR}_{z,m,c} = \exp\left\{ \frac{\theta_{z,m} \times \ln\left(\frac{Z_c}{\alpha_{z,m}} + 1 \right)}{1 + \exp\left\{ \frac{Z_c - \mu_{z,m}}{\nu_{z,m}} \right\}} \right\} \tag{10}$$

Here, Z is a function of $PM_{2.5}$ estimates and the Theoretical Minimum Risk Exposure Level (TMREL). Following previous studies[11,14], we assumed a uniform distribution for TMREL, ranging from 2.4 to 5.9 $\mu g\ m^{-3}$, and calculated the 95% Confidence Interval based on 2,000 randomly selected values. The parameters θ, α, μ, and ν characterize the shape of the concentration-response relationships (Table S2 in Burnett et al.[15]). "

In calculating the 95% confidence intervals and addressing feedback from other reviewers, we conducted a comprehensive reassessment of the health impact results. This includes: downscaling $PM_{2.5}$ exposure data, adding Type II diabetes-related attributable deaths, utilizing high-resolution (0.1°×0.1°) exposure data, and recalculating results for both historical and future scenarios based on the latest GBD2021 country-level baseline mortality data. Consequently, the relevant data and figures have been updated throughout the manuscript.

7. The emissions considered include pollutants like NO_x, CO, SO_2, NH_3, and VOCs, which are not necessarily $PM_{2.5}$. These pollutants can also have direct health impacts. I wonder why these health impacts were not accounted into the premature death calculations.

Answer:

Yes, pollutants such as NO_x, CO, SO_2, NH_3, and VOCs, while not directly part of $PM_{2.5}$, are important precursors that can form $PM_{2.5}$ through atmospheric chemical reactions with slightly direct health impacts[16]. In our study, we focused on $PM_{2.5}$ due to its well-established association with premature mortality[13] and its comprehensive inclusion in existing health impact models[15,17], which are widely recognized as critical for assessing health-related premature deaths. $PM_{2.5}$ has extensive epidemiological data supporting its role as a major contributor to health outcomes. This focus also aligns with the approach used in the Global Burden of Disease (GBD) Studies[18]， which consistently describe the burden of diseases and injuries from multiple risk factors, including exposure to air pollution. Even in the most recent GBD 2021 report[19] $PM_{2.5}$ pollution exposure was responsible for approximately 58% of global premature deaths, with ambient air pollution accounting for the majority of these deaths.

The direct health impacts of other pollutants like NO_x, CO, SO_2, NH_3, and VOCs[20] are less consistently linked to specific health outcomes like $PM_{2.5}$. Risk assessment models for these pollutants tend to be region-specific, complicating their integration into our analysis[21]. Additionally, $PM_{2.5}$, with its longer atmospheric lifetime, does not exhibit strong spatial gradients as it is well-mixed during formation, while short-lived pollutants such as NO_2 (with lifetimes on the order of hours) show strong spatial gradients that require high-resolution atmospheric concentration data, presenting another challenge for atmospheric modeling. We look forward to exploring this in future research by incorporating multiple pollutants will enhance the robustness of the health impact assessment and provide a more holistic understanding of the full range of health outcomes associated with air pollution exposure.

8. Does the cross-county/cross-region travel of $PM_{2.5}$ impacts the results, given $PM_{2.5}$ can travel 2000 km and did not respect the geographic boundaries? How is this accounted in

the regional premature deaths attributed to $PM_{2.5}$ exposure?

Answer:

Thank you for your questions. $PM_{2.5}$ can travel long distances, often crossing geographic boundaries, which is a critical factor in understanding its health impacts on regional populations. In our study, we recognize that the transboundary movement of $PM_{2.5}$ can affect local and regional concentrations, influencing premature death estimates. To account for this, we utilized atmospheric chemical transport models that reproduce the long-range movement of $PM_{2.5}$ across regions as well as the chemical reactions occurring during transport. These models provide estimates of $PM_{2.5}$ concentrations accounting for both local emissions and the contribution of cross-boundary transport. The regional premature deaths attributed to $PM_{2.5}$ exposure were derived from these model-based concentrations, which consider not only local emissions but also the contribution of pollutants transported from other regions or countries.

9. The colors of Figure 2(a)(c)(e) seem to be directly related to the population size of each country, since the colors are directly related to mortality. Mortality rate may be more intuitive.

Answer:

Yes, the wildfire-related mortality in Figure 2 is presented as absolute values, which are influenced by the population size of each country. This is calculated using the equation (5) in Method section, where pop_m refers to the population of different age groups in each country. However, considering the primary aim of this study is to highlight the changes in absolute mortality under future climate warming scenarios due to increased wildfire emissions, as well as the spatial and inequality shifts, we believe representing the mortality rate might not effectively convey the key message. With future demographic changes in different countries, expressing mortality as a rate could obscure the focus on the magnitude of mortality changes driven by wildfire emissions. Additionally, in our review of recent literature on this topic, we found that most studies report $PM_{2.5}$-related mortality in absolute terms[13,14,22-25], rather than as rates. Therefore, we would prefer to maintain the current expression in the figure for clarity.

10. “fire-induced mortality on the y-axis of Figure 3 implies a much broader meaning compared to “fire-induced $PM_{2.5}$-related premature deaths.”

Answer:

Thank you for pointing out the need for clarification. We have revised the figures based on recalculated results, and the corresponding axis titles have been updated to reflect "fire-induced $PM_{2.5}$-related premature deaths."

Responses to Reviewer 2 comments:

1. Summary of the key results

This paper uses several established modeling techniques to show that increasing wildfire activity could results in elevated mortality from fine particulate air pollution attributable to wildfires. The authors also show sensitivities regionally to different assumptions in the SSP estimates. Finally, their analyses show perhaps less convincingly that inequalities in wildfire-related deaths will decline.

Originality and significance

While using established methods and data products, the paper still provides an original analysis due to the numerous linked products it combines. Wildfires are becoming projected to become more frequent and severe because of climate change, increased development at the wildland-urban interface, and forest mismanagement (broadly falling into too much fire suppression in some regions and too much intentional burning for agricultural conversion in others). Given the potential for growing impacts at a global scale from wildfires the paper is significant and of potential interest to wider scientific and policy audiences.

Data and Methodology

Although generally acceptable, I have several comments here, some relating to presentation clarity, some to sensitivity analyses, and others on noted weaknesses.

Answer:

We really appreciate all your encouraging comments and constructive comments. We have carefully addressed and revised the manuscript based on the points you raised. We hope these revisions meet with your approval and further strengthen the quality of the study.

2. The authors note that the ML method they developed agrees “well” with other predictions and trends. Can they please be more specific about how the model agrees well. This is vague and uninformative to the reader, and something more specific should be in the main

body of the paper.

Answer:

We agree with your suggestion. The evaluation of ML prediction performance is briefly discussed in the main text. In this revision, we conducted a more comprehensive assessment of ML performance across various global regions. Based on these regional evaluations and prior results, we have added the following sentences to the ***Global Wildfire Carbon Emissions under a Warming Future*** section to address this:

"*We applied this ML model to reconstruct global BA from 2000 to 2014, comparing the results with satellite observations from the GFEDv4.1s. The model successfully captured the observed global BA decline of -6.4 Mha yr^{-2}, with a similar rate of -6.0 Mha yr^{-2} (Supplementary Figs. 1 and 2(a–d)). Regional evaluations showed strong agreement (Pearson's R^2: 0.72–0.96, Supplementary Fig. 3), though predictions were generally underestimated, resulting in a mean annual predicted BA of 466.6 Mha yr^{-1}, slightly lower than the GFEDv4.1s value of 475.7 Mha yr^{-1}. Further assessment of global fire carbon emissions, based on ML-derived BA estimates, revealed a significant reduction in the bias of ESM-simulated emissions (from 0.9 Pg yr^{-1} to -0.2 Pg yr^{-1}), improving both the magnitude and spatial pattern compared to GFED4.1s estimates (Supplementary Figs. 1(c–d)), provide a reasonable basis for expanding from historical to future pathways. For detailed evaluation results, see Supplementary Text 2.*"

3. On the fire emissions, can the authors offer some assurance to the reader that the current GFED estimates will be accurate on what are likely to be very different vegetative regimes by the end of the century? This seems to be something readers might question.

Answer:

Thank you for your valuable comment. We understand the concern regarding the accuracy of current GFED estimates in the context of potential changes in vegetative regimes by the end of the century. The GFED estimates provide the most widely used global wildfire emissions data up to date, and future shifts in vegetation and land cover could affect emission patterns. Predicting the impact of such changes on wildfire emissions is inherently challenging as we recognize in the second paragraph of the Discussion section as one limitation.

In our study, we have used GFED burned area data as the baseline for historical estimates, and our burned area projections are based solely on expected climate change under different SSP scenarios. These projections assume that human intervention levels observed over the past 15 years (the period covered by the training data for the ML model) will probably continue. This means that our future emission projections primarily reflect the continuation of global burned area changes observed over the past 15 years, driven by global warming and biomass changes predicted by earth system models. Our approach indirectly accounts for changes in vegetative regimes from earth system model estimates. For the coarser 2° × 2.5° grid burned area predictions, vegetative regime changes would have some impact, but we remain cautious about whether such changes will significantly alter the primary conclusions of this study. As shown in the figure below (Figure 1), the burned area fraction within each grid cell is relatively small, with the exception of the high-emission regions in sub-Saharan Africa, where it constitutes 30–40%. In most other global regions, this value is below 10%.

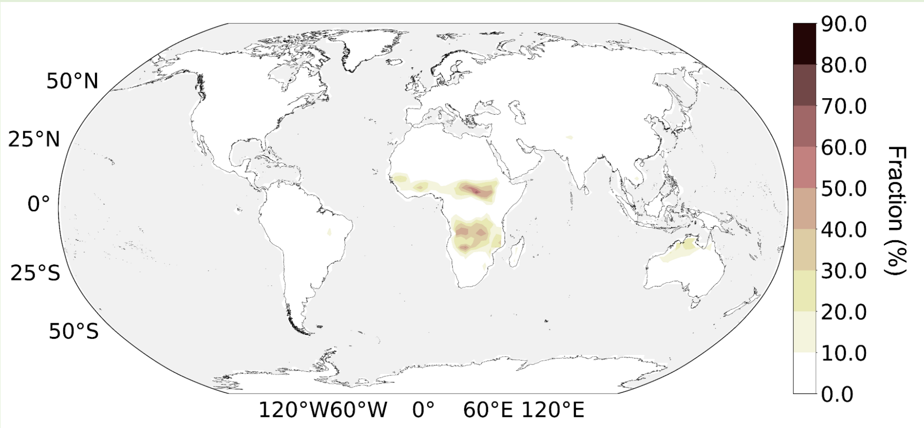


Figure 1. Fraction of burned area (average of 2095-2099 under SSP5-8.5 scenario) relative to each grid cell area (2°×2.5°).

However, we acknowledge that human land management, including fire suppression, land-use changes, and forest management, can influence fire dynamics and may introduce uncertainty into models of past fire occurrences. We have highlighted this limitation in the manuscript and suggest that future research could explore integrating land management data to enhance model predictions and better account for changing land-use types and their effects on fire behavior and emissions. This will help continue to improve projection estimates, particularly for higher-resolution projections, and uncover more detailed information. The relevant sentence in the ***Discussion*** has been revised as follows:

"*Additionally, human factors influencing fire activity, including prevention efforts and indirect land-use effects, present challenges for future projections—a limitation we acknowledge. Our projections for BA are based on past fire frequency and size, under the assumption that the level of human intervention observed over the past 15 years will persist in future scenarios. While biomass estimates from ESMs account for land-use changes, variations in emission factors due to land-use changes were incorporated into our future fire emission calculations (Methods). For the coarse 2° × 2.5° grid BA estimates, we remain cautious about the potential impact of land-use changes on the study's primary conclusions. Future work should explore methods to integrate land management data, improving the high-resolution estimates of land-use impacts on fire occurrences and enhancing our understanding of human-fire dynamics.*"

4. The assumption that the peatland contributions will remain fixed seems unrealistic given the increased chance of drought – could the authors offer some conjecture on what this exclusion would mean in the worst case for their estimates? Is there literature that the authors could cite on projected changes in peatland burning to put the importance of this assumption in context?

Answer:

Given the climate sensitivity of natural sources, we acknowledge that peatland emissions are likely to be affected by future climate change. Previous studies have highlighted that drier soils resulting from warming are a key factor driving peat fires[26], though they also note that several processes critical to peatland carbon balance remain poorly understood, leaving these issues for future research. Furthermore, uncertainties in historical emission estimates, such as the smoldering behavior of peat combustion and inaccurate location data for peat versus burned area, pose significant challenges when projecting future emissions[27,28]. Notably, future global emissions input data, a key deliverable of the ScenarioMIP experiment within CMIP6, do not predict peat burning emissions and assume they remain constant over time[29,30]. In light of these considerations, and the feasibility and workload of exploring future emissions changes, we also used constant emissions as what CMIP6 experiments have done in this study.

During our investigation, we found that CMIP6 publicly available data does not include process-based ESM estimates for peat fire emissions (https://esgf.github.io/index.html). However, a global study using an integrated assessment model estimated peatland greenhouse gas emissions under various future scenarios, reporting a potential increase of ~56.8% by 2100 under a pessimistic scenario. Based on the global average estimates of 54 Tg (C)/year from GFEDv4.1s, we applied this growth rate to project peatland fire emissions at 84 Tg (C)/year, which represents a small fraction (~2%) of the total global wildfire emissions (3 Pg (C)/year). We have revised the relevant descriptions in the ***Method*** section as follows:

" *We do not take into account projections of peatland fire emissions but instead use the future emissions trajectories from CMIP6, assuming global average estimates (2000-2014) of 54 Tg (C) year^{-1} from GFEDv4.1s remaining constant in the future (denoted as Peat(specie)$_{i,j}$). These emissions account for 1.8-2.2% of wildfire emissions during 2095-2099 under different future scenarios. However, considering the climate sensitivity of natural emissions, these may be affected by future climate change, as a global model study[31] suggests a potential increase of ~56.8% by 2100 under a pessimistic scenario compared to historical levels.*"

5. The authors assume equal toxicity in their health impact assessment (HIA) which is based on the undifferentiated $PM_{2.5}$ functions found in the GEMM model, but they are suggesting in the introduction that the wildfires could be more toxic. If they are suggesting this, a recent paper by R. Connelly et al. confronts this issue by adjusting the chronic exposures from $PM_{2.5}$ with acute wildfire estimates.

https://www.science.org/doi/10.1126/sciadv.adl1252. Could the authors implement a similar approach to show the sensitivity of their estimates to an explicit treatment of greater toxicity – the Connelly paper does give a global estimate for all-cause mortality that could potentially be used in this analysis to show the sensitivity of the reported estimates to inclusion of a dose-response function that accounted for greater toxicity in wildfire $PM_{2.5}$? This would give a first approximation of what a different dose-response function would mean for the HIA estimates, and it would probably show even greater impacts.

Answer:

Thank you for your thoughtful suggestion. We acknowledge that wildfire smoke may exhibit higher toxicity due to its unique chemical composition and the acute nature of exposure. After carefully reviewing the Connelly paper, following their methodology and input data, using short-term wildfire-specific $PM_{2.5}$ dose-response values (βws) alongside undifferentiated short-term $PM_{2.5}$ dose-response values to calculate the long-term wildfire-specific relative risk (β_RR) without age- and disease-specific differentiation. This β_RR was then incorporated into the mortality calculations for additional sensitivity analysis. The method is now described in the ***Methods*** section as follows:

"*In line with the approach used in Connelly et al.*[12]*, we use existing short-term wildfire-specific* $PM_{2.5}$ *dose-response values (βws) and short-term undifferentiated* $PM_{2.5}$ *dose-response values (βs) to calculate the long-term wildfire-specific relative risk (β_RR) using the following equation:*

$$\beta\_RR_c = \frac{\beta_{ws}}{\beta_s} \times RR_c \tag{11}$$

β_RR is a country-specific value calculated based on the age- and disease-specific RR integrated into the equation (10). β_{ws} *is derived from a global study*[24] *on country-specific mortality risks due to wildfire-sourced* $PM_{2.5}$*. For countries not included in the study, we use the global average value of 1.019 from Connelly et al.* [12]*.* β_s *is set at 1.0065, based on a recent meta-analysis*[32]*. Country-specific β_RR values are then applied in equation (9) to estimate* *wildfire-specific, dose-response-based $PM_{2.5}$-related mortalities."*

The corresponding results have been added to the second paragraph of the ***Wildfire-induced $PM_{2.5}$ mortality and direct radiative forcing*** section as follows:

"*A recent study*[12] *established a wildfire-specific ERF and suggested that using an undifferentiated ERF to estimate wildfire-related $PM_{2.5}$-induced mortality may lead to underestimation. To account for the potentially higher toxicity of wildfire smoke, we adopted a similar approach to calculate wildfire-specific premature deaths (Methods). Our results show an increase in mortality to 1.72 (95% CI: 0.99–2.56) million and 2.60 (95% CI: 1.48–3.83) million during 2095–2099 for SSP2-4.5 and SSP5-8.5 scenarios, respectively. This represents an increase of approximately 18.7–22.8% compared to the results using an undifferentiated ERF. While these results suggest a greater mortality impact from wildfires, we acknowledge the inherent uncertainty in this approach, as long-term wildfire-specific values have yet to be validated through primary research.*"

As mentioned in the ***Responses to Reviewers' Comments*** section, we have conducted a comprehensive reevaluation of the health impact assessment, incorporating feedback from all reviewers. This includes: downscaling $PM_{2.5}$ exposure data, adding Type II diabetes-related attributable deaths, utilizing high-resolution (0.1°×0.1°) exposure data, and recalculating results for both historical and future scenarios based on the latest GBD2021 country-level baseline mortality data. Consequently, the relevant data and figures have been updated throughout the manuscript.

6. The authors explain the integration of the population data into the 2 * 2.5 degree grids, but they also need to give the scale of the GBD data and discuss how this was harmonized.

Answer:

As noted in the previous response, we conducted a comprehensive reevaluation of the health impact assessment, which includes downscaling $PM_{2.5}$ exposure data and recalculating the results based on high-resolution (0.1°×0.1°) exposure data. The relevant methodology is now described in the ***Methods*** section as follows:

" *Given that the 2°×2.5° resolution of GEOS-Chem's $PM_{2.5}$ estimates is too coarse for accurate health burden assessment, we use the updated 2021 GBD exposure estimates*[19] *(0.1°×0.1°) for 2010-2014 to evaluate health impacts. For the historical period, fire contributions from GEOS-Chem (calculated as the difference between estimates with and without fire emissions, denoted as TMH and WFMH) were downscaled to 0.1°×0.1° resolution using nearest-neighbor interpolation to match the GBD exposure estimates (TGH). These were then multiplied by the GBD exposure estimates to derive historical fire-contributed $PM_{2.5}$ exposure (FH). For future scenarios, historical GBD exposure estimates were first adjusted by the ratio of change between GEOS-Chem $PM_{2.5}$ concentrations for future scenarios (TMS) and the historical period (TMH). These adjusted estimates were then multiplied by the corresponding future fire contributions (calculated as the difference between estimates with and without fire emissions for future scenarios, denoted as TMS and WFMS) to obtain future fire-contributed $PM_{2.5}$ exposure (FS). The calculation is as follows:*

$$FH\_PM_{2.5} = TGH\_PM_{2.5} \times \frac{(TMH\_PM_{2.5} - WFMH\_PM_{2.5})}{TMH\_PM_{2.5}} \tag{7}$$

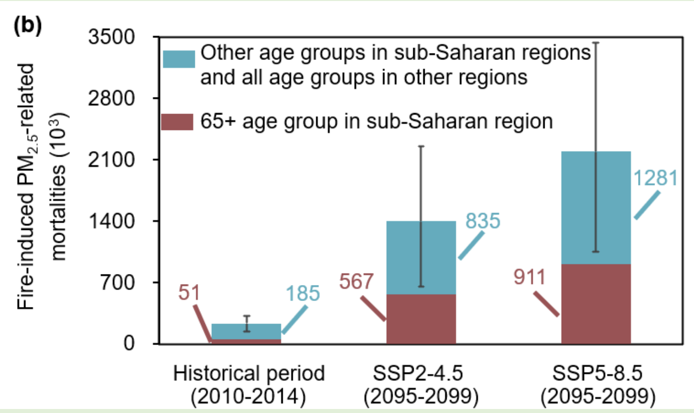
$$FS\_PM_{2.5} = TGH\_PM_{2.5} \times \frac{TMS\_PM_{2.5}}{TMH\_PM_{2.5}} \times \frac{TMS\_PM_{2.5} - WFMS\_PM_{2.5}}{TMS\_PM_{2.5}} \tag{8}$$

Finally, we aggregated the gridded population data to a spatial resolution of 0.1° × 0.1° to ensure compatibility with the estimated $PM_{2.5}$ exposure estimates."

7. Figure 1 has way too much information jammed into it, which makes it somewhat hard to follow for readers. Panel B seems to stick out and doesn't flow well. It is fine to make this point, just not on the same figure. When it says all other regions – does that mean in Africa. Sorry the plot confused me, and I am not sure what to recommend other than a re-thinking of what they are trying to say with this and to put more in the description on the figure.

Answer:

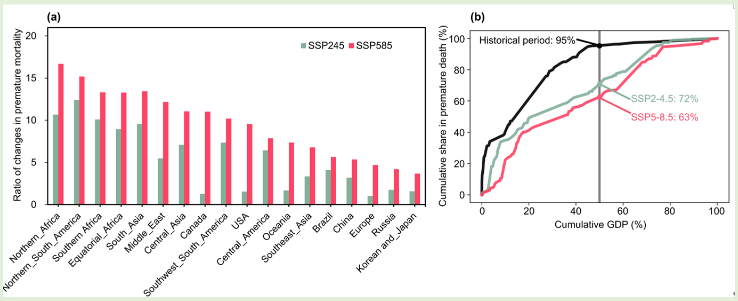
Thank you for your feedback. The purpose of including the three panels in Figure 1 was to capture the main findings of the study: (a) trends in emission changes, (b) mortality results, and (c) radiative forcing outcomes. In the revised version, the brown and blue bars in Panel B of Figure 1 represent fire-induced $PM_{2.5}$-related premature deaths in sub-Saharan Africa's population aged 65+ (brown) and in other age groups in sub-Saharan Africa and all age groups in other regions (blue). The blue bar represents the total fire-induced premature deaths, excluding those accounted for by the brown bar. The updated description has been added to the figure as follow:



8. Figure 4 – it was never explained as to why the lower emission scenarios resulted in large variation in the ratios presented. It was unclear why in Africa and the middle east the green bars showing the ratios of the lower emission scenarios tend to produce larger ratios and why in other places they produce much lower ratios than the red bars representing the higher ratios. Similarly, there are huge differences going in the other direction in wealthier places such as Europe and North America. Some further explanation is needed here. It also merits mentioning that the higher emission scenarios result in higher ratios in most places.

Answer:

In the original draft, these results primarily stemmed from the higher proportion of individuals aged 65+ in the SSP245 scenario. Than in SSP585 However, due to the coarser resolution (2°x 2.5°) and the use of global average baseline mortality, the estimates carried large uncertainty. As noted in the previous response, we conducted a comprehensive reevaluation of the health impact assessment in this revision. The revised projections provide more reasonable regional variations in premature deaths, with updated ratios as follow:



9. The first line of the abstract didn't agree well with the content of the paper and I would suggest changing this to something closer to the first line of the introduction.

Answer:

The current abstract emphasizes the broader context of carbon-neutrality and clean energy transitions, but we understand that a closer alignment with the paper's central theme—wildfire intensity and its drivers—would provide better clarity. We have revised the first line of the ***abstract*** to better align with the introduction and reflect the specific context of our study: " *Global warming and the increasing frequency of dry events are expected to intensify wildfires, thereby amplifying the release of greenhouse gases and air pollutants, underscoring the growing role of wildfires in shaping the global climate, air quality, and human health.*"

10. As noted above, wildfires are becoming projected to become more frequent and severe because of climate change, increased development at the wildland-urban interface, and forest mismanagement (broadly falling into too much fire suppression in some regions and too much intentional burning for agricultural conversion in others). The introduction only emphasizes the climate change and drought as driving factors, but the issues of WUI and forest mismanagement should be mentioned as well. These are covered in the conclusion, but the following line is somewhat inaccurate. The controlled fires to improve forest management are usually not intended to convert land to other uses, but they are meant to reduce fuel loads and stop larger fires from propagating into areas where they can grown rapidly. Land conversions in many instances such as in the Amazon are a root driving force behind higher wildfire risk because the land conversion from forests to agriculture often result in uncontrolled fires, so the authors need to be more accurate here.

“Wildfires can be partially mitigated through evidence-based fire management, as well as thoughtful planning and design of natural and urban landscapes32, including controlled fires initiated by humans for land conversion purposes.”

Answer:

We agree with your comments on the multiple drivers contributing to the increasing frequency and severity of wildfires. As suggested, we have added the following sentences to the first paragraph of the ***Introduction*** section: "*Additionally, increased development at the wildland-urban interface and forest mismanagement (e.g., intentional burning for agricultural conversion) may further exacerbate the frequency of fire occurrences globally*[33-35]."

To prevent any potential misinterpretation of the statement "controlled fires for land conversion purposes," we have revised the sentence as follows："*Wildfires can be partially mitigated through evidence-based fire management, as well as thoughtful planning and design of natural and urban landscapes*[36]*, including controlled fires used for forest management to reduce fuel loads and prevent larger fires from spreading into vulnerable areas. However, land conversion, such as deforestation for agricultural purposes, is often a primary driver of increased wildfire risk*[37]*……*"

11. The authors note that particles from wildfires tend to be more toxic. Here because there’s still considerable uncertainty on the toxicity of wildfire particles compared to undifferentiated PM, authors should moderate this statement by noting can be more toxic or have been shown in some studies to be more toxic would be better (this raises another methodological issue which is discussed under the methodology section of this review).

Related to this point, the authors have assumed that the exposure to wildfires represents a long-term or chronic exposure, but there are no existing studies that directly address the long-term impacts of wildfire-specific $PM_{2.5}$ on mortality at this time. So they are making a rather large assumption that the periodic nature of wildfire smoke also equals a chronic exposure. The Connelly article noted above addresses this issue in detail. The authors probably need to acknowledge this limitation in treating the exposure as chronic when there are no primary studies that address this.

Answer:

Thank you for your suggestion, which aligns closely with your second comment. We have revised the relevant statements. For further details, please refer to our response to the second comment.

12. The citation to Cohen et al. 2015 could be replaced or supplemented with the Burnett et al. 2018 report in PNAS because this latter study used arguably better methods to estimate the total burden of death from air pollution.

Answer:

We have updated the citation in the manuscript to include Burnett et al. 2018, which is now reflected in the first paragraph of the ***Wildfire-induced $PM_{2.5}$ mortality and direct radiative forcing*** section as follows:

"The global total mortality from all sources of $PM_{2.5}$ is estimated to be 7.7 (95% CI: 5.4–9.8) million, within the range of 6.9–8.9 million reported in previous studies[13,23,25] using the same exposure-response function (ERF)[15]."

Responses to Reviewer 3 comments:

1. This paper addresses future wildfire smoke and its impacts on health and radiative forcing of climate. Since wildfires have emerged in the past decade as an important unknown as climate changes, and because Earth System models (ESMs) are working to imbed fire occurrence internally but lack consistent approaches, this paper is timely. The authors use a novel method in which they use a historical (for recent years) fire emission inventory as well as climate parameters to train a machine learning model to predict fire occurrence and emissions in future scenarios to 2100 where climate is changing. Based on the introduction and references listed there, it seems that other studies have similarly used statistical models and machine learning models to explore relationships between climate variables and fire occurrence. But it seems that the present paper is the first to do this on a global scale and to apply the relationships found to future climate scenarios globally.

   I have several questions and concerns as described below. But I also expect that with satisfactory attention to these questions and concerns, and revisions to the paper where appropriate, that this can be an important addition to the current literature on the importance of climate change for future wildfire smoke and impacts.

   Answer:

   Thank you very much for all your valuable comments and constructive feedback. We have carefully addressed your questions and concerns in the revised manuscript to ensure the clarity and robustness of our methodology and results. We hope these changes meet with your approval and further enhance the quality of the study.

2. The idea to analyze historical fire emissions for climate drivers using machine learning is a good one. However, the machine learning methods are a bit of a black box. Very little text in the main paper describes the inputs to the machine learning, and only one sentence on it appears under Methods, although there is more text and some results in the supplement. Can more be said about how the machine learning is conducted, and why these methods vs. others are used?

Answer:

We agree that the description of the machine learning (ML) methodology could be more detailed, as much of the relevant information is currently in the Supplementary Texts. In response, we have expanded the description of the ML approach in the first paragraph of the ***Method*** section as follows:

"*ML has proven effective for wildfire predictions, particularly at a global scale for coarse grid BA estimates, where it outperforms traditional ESMs*[4]*. It also accurately predicts both extreme and normal fire events, capturing spatial trends*[38-40]*. Therefore, we trained the ML model to establish a nonlinear relationship between historical climate factors and satellite-based BA data (2000–2014). Hyperparameter optimization was initially performed to enhance model performance, followed by data preprocessing. Key climate factors influencing wildfire occurrence were aligned with BA data and integrated at a consistent horizontal resolution (2°×2.5°). Detailed information on the ML framework, data processing, and ESMs used is provided in Supplementary Text 1 and Tables 1-2. Model performance was thoroughly evaluated at global and regional scales using 15 years of historical data (Supplementary Text 2). Then the global monthly BA were then calculated using the following equations:*

$$\mathrm{C}_{i,t}^{\mathrm{train}} = f\left(\mathrm{X}_{i,t}^{n,\mathrm{train}}\right) \tag{1}$$

$$\mathrm{C}_{i,t}^{\mathrm{pred}} = f\left(\mathrm{X}_{i,t}^{n,\mathrm{pred}}\right) \tag{2}$$

Equations (1) and (2) describe the training and prediction processes. In Equation (2), C^{train} represents the monthly BA value for a specific geographical location of the i-th grid cell at time period of t during training period. X is the matrix of n-dimensional BA-related climate factors. During the training, supervised learning is used to establish the functional relationship, denoted as f, between BA and its related climate factors. In Equation (2), during prediction, the established relationship f is applied to the validation dataset to compute the predicted BA value, C^{pred}. The historical climate data used for training the ML model are *derived from simulations provided by four ESMs.*"

Additionally, recognizing the insufficient detail on the ML framework and input data in the SI text, we have expanded SI Text 1 to include the following information:

" ***Supplementary Text 1: ML framework for global burned area estimates***

We trained the XGBoost[2] *(machine learning algorithm under the Gradient Boosting framework) model to capture the nonlinear relationships between burned area (BA) and multiple climate factors that strongly affect fire occurrence*[3,4]*, and further estimate the global BA. Hyperparameter optimization is a crucial step in improving ML model performance. In this study, the GridSearchCV function, a function in Scikit-learn (https://scikitlearn.org/stable/modules/generated/sklearn.model_selection.GridSearchCV.html) was used for hyperparameter tuning, which includes both grid search and cross-validation. In the grid search, all parameter combinations within a specified range were explored by adjusting the parameters with a specified step size to train the learning algorithm. Cross-validation is a statistical analysis method used to verify the performance and generalization ability of algorithm classifiers. Finally, we chose the set of hyperparameters that yielded the highest average coefficient of determination (i.e., the* R^2 *regression score function) across all validation sets.*

The input data for the ML model consist of BA from Global Fire Emissions Database, Version 4.1 (GFED4.1s) [5]*, and climate data, including 2 m air temperature (T2), relative humidity (RH), total precipitation (TP), 10 wind speed (W10), leaf area index (LAI), evaporation from the top of canopy (Evaporation). These climate data are sourced from the monthly estimates of four Earth system models (ESMs) and the European Centre for Medium-Range Weather Forecasts Reanalysis version 5 (ERA-5) dataset*[6]*. Furthermore, to account for seasonal climate variations and the spatiotemporal heterogeneity of climate factors on fire occurrences, we incorporated the sampling times (Time) and geographic coordinates (Coordinate) of the input data into the ML framework. To address data discontinuity, these datasets were transformed into periodic functions, following methods recommended in* *previous studies[7-9]:*

$$\begin{bmatrix} M1 \\ M1 \end{bmatrix} = \begin{bmatrix} \cos\left(\text{month}\dfrac{2\pi}{12}\right) \\ \hline \sin\left(\text{month}\dfrac{2\pi}{12}\right) \end{bmatrix} \tag{1}$$

$$\begin{bmatrix} L1 \\ L2 \\ L3 \end{bmatrix} = \begin{bmatrix} \sin\left(\text{lat}\dfrac{\pi}{180}\right) \\ \sin\left(\text{lat}\dfrac{\pi}{180}\right)\cos(lat\dfrac{\pi}{180}) \\ -\cos\left(\text{lon}\dfrac{\pi}{180}\right)\cos(lat\dfrac{\pi}{180}) \end{bmatrix} \tag{2}$$

The terms month, lat, and lon refer to the sampling data's month, latitude, and longitude, respectively, while M1-M2 and L1-L3 denote the resulting calculations of periodic functions.

Due to large fluctuations in the input data, we applied log transformation and normalization to reduce variability. The dynamic range of predictors was constrained to the [−1,1] interval using min–max normalization, as recommended in prior research[10]:

$$Y_{norm} = \left[\frac{Y\text{-}Y_{min}}{Y_{max}\text{-}Y_{min}}\right] \tag{3}$$

where Y_{min} and Y_{max} are the minimum and maximum values in the data Y, respectively.

The transformed input data were matched to the BA data, which were aggregated into global monthly data on a 2°×2.5° grid. To evaluate the predictive performance of the ML model, we conducted annual predictions for historical data (2000-2014). For example, to predict 2014 BA, we used data from 2000-2013 for training, and for 2013 BA, data from 2000-2012 and 2014 were used, and so on. Detailed evaluation results are provided in Supplementary Text 2. Last for future projections (2015-2099), we trained the ML model on historical data and used ESM-estimated future climate factors for prediction."

3. I am not clear about how observations of climate (represented in the ECMWF ERA5 renalaysis) versus the ESM historical simulations are used in training the machine learning.

Supplement line 41 says that only the ESMs are used in training the machine learning, and not ECMWF, and supplement line 61 says that ECMWF is used to estimate historical burned area after the machine learning was conducted. The ESMs are commonly run with meteorology that “floats,” meaning that it does not necessarily represent historical meteorology within a given year, and it would miss important natural variations. For that reason, I would think it best to use ECMWF meteorology parameters, and not the ESMs, to best capture historical meteorological parameters for the machine learning. Why were the ESMs used for training the machine learning, and not ECMWF? Were the ESMs run with meteorology nudged to observations or were they freely floating?

Answer:

Thank you for your insightful comments. Indeed, we used historical and future predictions from ESMs to train the ML model and for future projections. Initially, we used ECMWF historical data (2000-2014) for training, while ESM climate projections were applied for the period 2015-2099. However, this approach resulted in a "discontinuity" between 2014 and 2015, as shown in Figure 2 below, where "Training with ERA5" and "Training with ESMs" represent the results from training and forecasting based on ERA5 and ESMs, respectively, in one of the previous experiments. As you correctly pointed out, ESMs have inherent biases, and the discontinuity may arise from the mismatch between the two datasets. To address this issue, we opted to use continuous ESM simulation data for both training and future projections. This change resolved the discontinuity, eliminating the "break" between 2014 and 2015 (Figure 2). We believe this decision was crucial for maintaining the consistency of the data, especially since the main goal of the study is to assess changes in fire emissions from the historical period (2010-2014) to the end of the century. Introducing discontinuities would significantly affect the assessment of such changes, introducing large uncertainties.

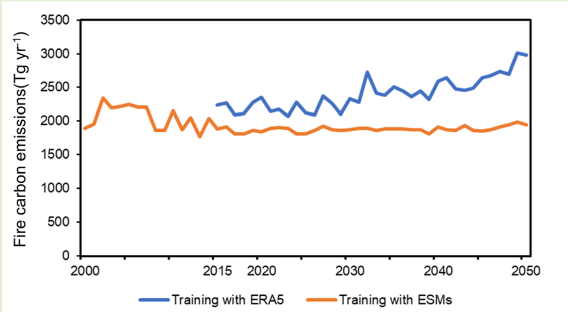
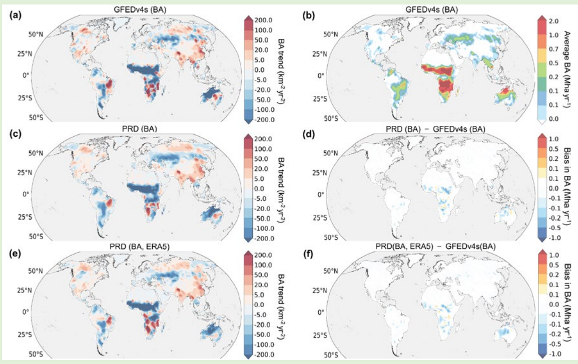


Figure 2. The historical and projected future fire carbon emissions based on ERA5 and ESMs meteorology parameters.

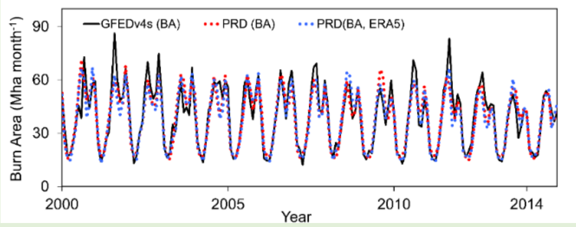
However, we also acknowledge that training with ESM data may introduce certain biases, which is why we included an evaluation based on both data sources in Supplementary Text 2. The results of this evaluation are presented as follows:

"However, the ESMs are known to have inherent biases[41] that may impact predictive performance. To further evaluate the accuracy of the ML model driven by ESMs' climate factors, we employed data from the ERA-5 to estimate global burned areas (PRD (BA, ERA5)). Our findings indicate that both PRD (BA) and PRD (BA, ERA5) exhibit minor differences in the spatial distribution of BA trends (Supplementary Fig. 2(c,e)) and biases in BA (Supplementary Fig. 2(d,f)) compared to satellite observations. Both models capture the monthly temporal variations in global mean BA (Supplementary Fig. 3). This suggests that the predictive performance of both models remains relatively consistent.



Supplementary Fig. 2 Historical burned area during 2000–2014 from GFEDv4.1s and predicted in this study using ML framework. *(a) represents the trend in burned area derived from GFEDv4.1s and those predicted in this study using climate factors from ESMs (c) and*

ERA5 (e). The trends in each grid cell are shown for using the linear least squares fitting method based on annual time series. The spatial distribution of annual mean burned area from GFEDv4.1s (b) and the disparities with predictions based on climate factors from ESMs (d) and ERA5 (f) compared to GFEDv4.1s.



Supplementary Fig. 10 Temporal trends of monthly burned area from GFEDv4.1s (black curve) and predicted in this study using climate factors from ESMs (dashed red curve) and ERA5 (dashed blue curve) during 2000-2014. "*.*

4. There would be much interest in understanding the historical climatic drivers of past wildfire burned area and emissions. The authors show the importance of different variables for future climate change in the two scenarios (Supplement Figures 6 and 7), but here they do not interpret the results. For example, what does it mean that "Coordinate" is the most important predictor variable? More importantly, a similar analysis of drivers is not given for the historical conditions analyzed, and I think this would be valuable. While it would be possible to have a follow-up paper focus on this analysis, adding it here can help the readers evaluate the results of the ML.

Answer:

We acknowledge that the original manuscript's description of "interpretable" was insufficient both in the main text and SI. As suggested, we have now included an analysis of SHAP values for the historical period and integrated it with the future trends section. This updated analysis has been added to Supplementary Text 4 along with the corresponding figures, as shown below:

"Supplementary Text 4: Driver analysis of global BA projection

Eight predictors, listed in Supplementary Table 2, were evaluated for their contributions to future BA predictions using the SHAP approach. The global average importance of these predictors for BA is summarized in Supplementary Tables 6 and 7, which represent the proportion of each feature variable's 5-year mean global average absolute SHAP value relative to the cumulative SHAP values of all feature variables, The values for 2000–2014 were derived from the BA evaluation process for the historical period (Supplementary Text 2), while the values for 2015–2099 were obtained from the future projection process. Notably, the coordinate feature shows a relatively high importance, indicating its significant influence during both training and prediction phases. Given the distinctive spatial patterns of global wildfire emissions[42], particularly the high value near the equator (10°S-10°N) their uneven distribution across regions, we hypothesize that the ML model is more sensitive to these parameters. As discussed in Supplementary Text 1, to account for the spatiotemporal heterogeneity of climate factors on fire occurrences, we incorporated the sampling geographic coordinates into the ML framework by transforming them into periodic functions. Notably, among the eight predictors, the 2m temperature (T2) demonstrated the most significant variation in importance. Its contribution increased from 6.7% to 7.7% and 9.6% between 2015 and 2099 under the SSP2-4.5 and SSP5-8.5 scenarios, respectively. This suggests a growing influence of T2 in BA prediction.

In Supplementary Fig. 11, the SHAP values refer to the change ratio of the 5-year mean global average SHAP value for each feature variable during 2015-2099 relative to those in 2010-2014, which presents the global average trends of each climate factor-related predictor and their contributions to BA prediction. A higher absolute SHAP value indicates a larger contribution to BA prediction compared to the preceding five years (2010-2014), while a lower SHAP value indicates a smaller contribution. Specifically, for T2, the SHAP value under the SSP5-8.5 scenario initially decreases, then increases around 2060. This trend aligns with observed BA trends from 2015-2059 and 2060-2099 (Supplementary Fig. 12). In the SSP5-8.5 scenario, sub-Saharan Africa experiences a declining BA trend in both periods, while most regions outside of sub-Saharan Africa show a more pronounced increase in BA from 2060-

2099. This upward trend in global average BA (Supplementary Fig. 6(a)) matches the observed trend in SHAP values for T2. Therefore, this study suggests that T2 may be a primary driver of future spatial and temporal trends in BA. Furthermore, several features related to drought, such as RH, Evaporation, and T2, show increasing global mean values indicating drier trends under the SSP5-8.5 scenario (Supplementary Fig. 11, dashed lines). Correspondingly, the related SHAP values increase (Supplementary Fig. 11, solid lines), and these variations are more pronounced compared to the SSP2-4.5 scenario. This may indicate that the ML model correctly captures the increased frequency and extent of wildfires under a future climate of global warming and increasing drought. In contrast, increased precipitation in the SSP5-8.5 scenario, relative to SSP2-4.5, is associated with smaller predicted BA values, as reflected in the decreasing SHAP values (Supplementary Fig. 11(f), solid line). Conversely, an increase in leaf area index (LAI), which adds more fuel, leads to larger predicted BA values (Supplementary Fig. 11(f), solid line)."

Additionally, we have added descriptions in the second paragraph of the ***Global wildfire carbon emissions under warming future*** section**:**

"*We used the SHapley Additive exPlanation method to assess the importance of each predictor in our ML model and quantify their contributions to BA predictions, enabling us to evaluate the relative impact of variables such as 2-meter temperature, which was found to be the primary driver of spatial and temporal variations in BA, outweighing other factors such as precipitation, evaporation, relative humidity, etc.—particularly under the SSP5-8.5 scenario, where its influence is projected to intensify in the future. The trend in drought-related SHAP values also suggests that the ML model successfully captures the expanded extent of wildfires under a future climate of global warming and increasing drought. (Supplement Text 4).* "

5. In addition to meteorological drivers, I understand that human land management and forestry can play an important role in determining fires (I'm not an expert in this area). The authors acknowledge opportunities to use fire management in the future to adapt to climate change, but the authors did not acknowledge that human actions may add uncertainty to

their machine learning model of past fire occurrence, nor did they include any land management variable in the machine learning.

Answer:

In the current study, we did not include land management variables, such as human interventions or forestry practices, in the ML model. The focus was primarily on climate and vegetation-related factors that are known to significantly influence wildfire occurrence.

Given the availability of land-use datasets from historical periods to SSP future scenarios, such as those generated by the Land-Use Harmonization (LUH) 2 project[43], we performed an additional experiment to show the impact of including land management variables in our machine learning model and results. We incorporated LUH land-use data statistics (including areas of forested and non-forested primary land, managed pasture, rangeland, secondary biomass carbon density, urban land, etc.) as input parameters for training the ML model and projecting future BA. The results based on one of the ESMs are shown below in Figure 3. PRD_no-LUH and PRD_with-LUH represent predictions without and with considering land-use data, respectively. The global averages for the period 2095–2099 are 406 Mha and 418 Mha, respectively, showing a slight difference, though spatial variations exist. Additionally, SHAP analysis revealed some unexplained patterns, i.e., in the urban parameter, where the contribution of SHAP values remained constant despite an increasing urban area over time. We attribute this to the annual scale resolution of LUH data, which may not capture significant monthly variations like in other input parameters. Additionally, the coarse grid resolution (2°×2.5°) may limit the ability to detect fine-scale land-use changes. Given the computational cost and the minor differences in results, we thus did not pursue further investigation into this.

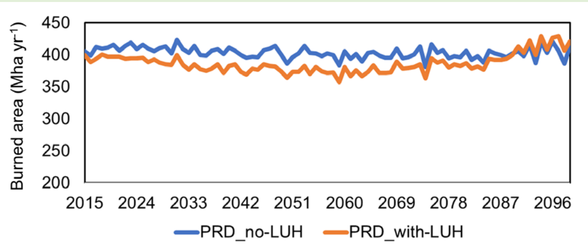


Figure 3. Projected future burned area with and without land management variables.

Overall, we acknowledge that human land management, including fire suppression, land-use changes, and forest management, can influence fire dynamics and introduce uncertainty into ML models of past fire occurrences. We have highlighted this limitation in the manuscript and suggest that future research could explore integrating land management data to enhance model predictions and better evaluate the interaction between human activities and fire dynamics. The relevant sentence in the ***Discussion*** has been revised as follows: "*Additionally, human factors influencing fire activity, including prevention efforts and indirect land-use effects, present challenges for future projections—a limitation we acknowledge. Our projections for BA are based on past fire frequency and size, under the assumption that the level of human intervention observed over the past 15 years will persist in future scenarios. While biomass estimates from ESMs account for land-use changes, variations in emission factors due to land-use changes were incorporated into our future fire emission calculations (Methods). Future work should explore methods to integrate land management data, improving the high-resolution estimates of land-use impacts on fire occurrences and enhancing our understanding of human-fire dynamics.*"

6. Looking at the novelty of the fire projections, the authors say in the introduction that ESMs disagree with one another, adding motivation for a machine learning estimate. But then in the results, the estimates here for the future agree well with the ESMs (although the ESMs themselves do not agree?).

Answer:

As you pointed out, we discuss the uncertainty in wildfire emission estimates from ESMs without observational constraints in the introduction. However, your mentioned "*the estimates here for the future agree well with the ESMs*" in the results might refer to the opening sentence of the final paragraph in "***Global wildfire carbon emissions under a warming future***" We acknowledge that this may cause some confusion, and have revised the sentence for clarity as follows:

"*The projected spatial trends in fire emissions are generally similar to those estimated* *by ESMs (Supplementary Fig.4), showing an overall increase across the Northern Hemisphere and a decline in the African region.*"

It is important to note that here we refer to the similar spatial trends (increase and decrease) in fire emissions, rather than the emission levels mentioned in the introduction. We further analyze the differences between our projections and ESMs, highlighting that our results are more aligned with currently reported observational studies. This is detailed as follows:

"*However, relative differences still exist, primarily in regions recognized as limited vegetation cover at present*[4]*, particularly within the 15°N–30°N latitude range in Africa and the 15°N–60°N latitude range in West Asia. Over there, ESMs predict substantial future rises in fire carbon emissions, marking notable discrepancies from our projections (Supplementary Fig. 4). Additionally, the ESMs' simulation results indicate limited or negligible growth in fire emissions within the Siberian Arctic (Supplementary Fig. 4), contradicting the recent studies*[44-46] *which highlight a substantial increase in fire occurrences due to accelerated warming and circulation changes over the Arctic. Our observation-constrained ML projection approach tends to provide a more reasonable projection for these areas compared to estimations from prognostic ESM models.*"

7. In line 409, the authors describe the methods of using biomass from ESMs along with their machine learning estimates of fires to get the emissions. If the fire occurrence within the ESMs differs from that projected using these methods, would the available biomass be different, and would that then affect emissions? More broadly, are there reasons to think that errors in the methods used here might grow over the coming century, or are the authors confident that these methods can be used reasonably for projections that far into the future?

Answer:

Thank you for your comment. Indeed, the fire occurrence in the ESMs differs from our results, and this disparity affects the available biomass, which in turn influences emissions. However, we would like to clarify a few points.

First, in the original draft (line 409), the "fuel consumption" in Equation (1) is derived from the change ratio in ESM-simulated vegetation biomass (Equation 2), multiplied by the fuel consumption estimated in the historical period using GFED4.1s data. This means we are not using the absolute fuel consumption values directly predicted by the ESMs, but rather the change in vegetation biomass. The reason for this approach is that fuel consumption depends on the amount of flammable biomass and the degree of combustion completeness (CC). However, there is a significant disparity between the vegetation biomass estimated by ESMs and the biomass used in the GFEDv4.1s dataset. To avoid large discrepancies between the first prediction year (2015) and the historical emissions in 2014, we applied the biomass change ratio from ESMs to estimate future fuel consumption. As shown in Supplementary Fig. 6 (b, d, f), the biomass estimated by ESMs is in the range of 200-300 Pg(C)/yr, while the global fire carbon emissions predicted by these models are around 3-4 Pg(C)/yr. The key point is that while fire occurrence in the ESMs influences biomass, the difference in magnitudes (about 1% of total biomass) means the effect on biomass change rates used in this study is likely minimal.

Moreover, we place more confidence in the model's simulated trends based on forcing data rather than absolute quantities, as we have used state-of-the-art ESMs to simulate biomass changes driven by climate and other forcing factors. While future uncertainties cannot be entirely ruled out, we believe the methods used here are reasonable for projecting long-term trends, given that this reasonability is grounded in the current understanding of vegetation biomass variation under climate change as simulated by state-of-the-art ESMs. We hope our explanation addresses your concerns.

8. I do not understand the results presented (Fig. 1c) because it is not clear what time period DRF is relative to. If the preindustrial, then how are preindustrial conditions modeled?

Answer:

The DRF from fire emissions is calculated using online-based sensitivity experiments for the perturbation results for the radiative balance between a simulation considering fire emissions with respect to a sensitivity simulation omitting fire emissions, both during the same time period, which is similar to previous studies on aerosol DRF[9,47], rather than the change in Effective Radiative Forcing (ERF) from preindustrial conditions to current level which typically referenced in IPCC reports. In Figure 1c, the black bars represent the 2010–2014 average for historical periods, while the green and red bars correspond to the 2095–2099 averages for future scenarios. To clarify, we have added the following sentence to the ***Methods*** section: "*Fire emissions contribution to ambient $PM_{2.5}$ concentrations and their induced DRF is calculated as the difference between simulations with and without fire emissions, conducted over the same period.*"

9. Wildfires emit both gases and particles. The authors estimate DRF only for aerosols (accounting for the secondary production of aerosols by emitted gases, I assume), but they leave out changes to greenhouse gases including methane and ozone through atmospheric chemistry. I thought that for fires, the effect on methane and ozone would be relevant for climate, but perhaps I am wrong. The authors should expand to include other forcings, or should justify showing results for only aerosol DRF.

Answer:

Thank you for your constructive comments. Indeed, wildfires emit both aerosols and greenhouse gases, including methane and O_3 precursors (such as CO, NO_x, and organic compounds), which influence radiative forcing through atmospheric chemistry. However, CO_2, CH_4, O_3, water vapor, and nitrous oxide are important long-term climate forcers. We acknowledge that including the net radiative effect of both gases (CO_2, O_3, CH_4, etc.) and aerosols (e.g., sulfate, nitrate, ammonium salts) for both present and future scenarios would provide a more comprehensive baseline for Earth's climate system, as demonstrated in the recent Nature paper by Alfonso et al.[48] on radiative effects from other natural sources.

However, incorporating these into radiative forcing calculations within a climate feedback system requires complex treatment of atmospheric chemistry and dynamic interactions, entailing a substantial computational workload. The GEOS-Chem model used in this study is commonly employed for atmospheric chemistry simulations. While it is coupled with the RRTMG radiation module to calculate some gas radiative forcing and aerosol direct radiative forcing, it is not capable of providing a comprehensive net radiative effect of gases and aerosols as reported in Alfonso et al. Off-line coupling with additional radiation modules, including CH_4, O_3, and trace carbon gases, as well as aerosol-cloud-albedo effects, would significantly increase both computational demand and complexity. Moreover, as noted in the Alfonso et al. paper, while they used an atmospheric chemistry model nested within Earth System Models (ESMs) to estimate the net radiative effects of gases and aerosols, considering the feedback of fire-related greenhouse gases on climate and the subsequent impact on fire occurrence would require even more complex ESM simulations, further multiplying the workload and difficulty.

Given that this study focuses on projecting future wildfire emissions and their associated $PM_{2.5}$-related health impacts, we have limited our analysis to fire-induced direct radiative forcing (DRF), due to its more immediate and well-characterized climate impact based on the current methodology. As you suggested, we are considering expanding this work in the future by incorporating a fully coupled Earth System Model to account for the full range of fire-induced net radiative effects, including climate feedback from both gases and aerosols. This point has been added to the Discussion section as follows:

" *Hence, future research should employ more comprehensive parameterization schemes to drive ESMs, incorporating observational constraints for online calculations that account for fire-vegetation-human-climate feedback, and aim to provide a thorough assessment of fire-induced net radiative effects to establish a more accurate natural baseline for climate system.*"

10. Given that the authors present CO_2 emissions from fires and the DRF from aerosols, it may be possible to present the net climate effects of the fires from all forcings, and as a climate feedback factor (additional forcing from fires per degree warming).

Answer:

Please refer to the details response provided in your ninth comment for that issue.

11. It’s hard to know what to conclude from the DRF shown. The abstract, for example, doesn’t give any conclusion about the DRF estimated. One option the authors might consider would be to focus this paper on $PM_{2.5}$ and health, and leave the DRF climate consequences for follow-on papers - but I will leave that decision to the authors.

Answer:

Thank you for your insightful comments on the DRF results. As mentioned previously, we consider a comprehensive radiative effect analysis as a key focus for future work. In this paper, we provide an initial assessment, and we have added the following sentences to the ***Abstract*** and ***Discussion*** sections to clarify this point:

Abstract: "*From 2010–2014 to 2095–2099, climate change alone is projected to increase fire carbon emissions by 23% under the Socioeconomic Pathway (SSP) 2-4.5 and 53% under SSP5-8.5. Increased fire-induced aerosols enhance the cooling effect in mid-latitudes up to 2.3-fold.*"

Discussion: "*Our estimation of future fire emissions was derived from offline calculations, which did not incorporate bidirectional feedback mechanisms, such as the impact of fire-induced greenhouse gases on climate change. Compared to historical periods, our estimates show a 1.4-2.3-fold increase in fire-induced aerosol DRF over the mid-latitudes of the Northern Hemisphere. Given the potential for even greater fire-induced greenhouse gas radiative effects, future work should aim to provide a thorough assessment of fire-induced net radiative effects to establish a more accurate natural baseline for climate system.*"

12. The 2x2.5 degree resolution modeling is generally good for estimating fire occurrence, but is coarse for estimating mortality, as significant differences in concentration can occur within a grid cell – for example between urban and rural regions. The state of the art today would typically use finer resolution for health impacts, so this choice adds to the overall uncertainty.

Answer:

As mentioned in the ***Responses to Reviewers' Comments*** section, we have conducted a comprehensive reevaluation of the health impact assessment, incorporating feedback from all reviewers. This includes: downscaling $PM_{2.5}$ exposure data, adding Type II diabetes-related attributable deaths, utilizing high-resolution (0.1°×0.1°) exposure data, and recalculating results for both historical and future scenarios based on the latest GBD2021 country-level baseline mortality data. Consequently, the relevant data and figures have been updated throughout the manuscript. The relevant methodology for downscaling $PM_{2.5}$ exposure data is now described in the ***Methods*** section as follows:

" *Given that the 2°×2.5° resolution of GEOS-Chem's $PM_{2.5}$ estimates is too coarse for accurate health burden assessment, we use the updated 2021 GBD exposure estimates[19] (0.1°×0.1°) for 2010-2014 to evaluate health impacts. For the historical period, fire contributions from GEOS-Chem (calculated as the difference between estimates with and without fire emissions, denoted as TMH and WFMH) were downscaled to 0.1°×0.1° resolution using nearest-neighbor interpolation to match the GBD exposure estimates (TGH). These were then multiplied by the GBD exposure estimates to derive historical fire-contributed $PM_{2.5}$ exposure (FH). For future scenarios, historical GBD exposure estimates were first adjusted by the ratio of change between GEOS-Chem $PM_{2.5}$ concentrations for future scenarios (TMS) and the historical period (TMH). These adjusted estimates were then multiplied by the corresponding future fire contributions (calculated as the difference between estimates with and without fire emissions for future scenarios, denoted as TMS and WFMS) to obtain future fire-contributed $PM_{2.5}$ exposure (FS). The calculation is as follows:*

$$FH\_PM_{2.5} = TGH\_PM_{2.5} \times \frac{(TMH\_PM_{2.5} - WFMH\_PM_{2.5})}{TMH\_PM_{2.5}} \tag{7}$$

$$FS\_PM_{2.5} = TGH\_PM_{2.5} \times \frac{TMS\_PM_{2.5}}{TMH\_PM_{2.5}} \times \frac{TMS\_PM_{2.5} - WFMS\_PM_{2.5}}{TMS\_PM_{2.5}} \tag{8}$$

Finally, we aggregated the gridded population data to a spatial resolution of 0.1° × 0.1° to ensure compatibility with the estimated $PM_{2.5}$ exposure estimates."

13. l. 130 – The authors show that their present-day estimate of $PM_{2.5}$-related deaths agrees well with the Global Burden of Disease study, but they are comparing with the GBD of several years ago. The most recent GBD is here: https://www.thelancet.com/journals/lancet/article/PIIS0140-6736(24)00933-4/fulltext. More importantly, the authors are using GEMM which has been estimated to give much larger numbers of $PM_{2.5}$ deaths globally than the 4 million here and in the GBD studies. That leaves the question of why the global estimate here is so much lower than other estimates using GEMM.

Answer:

Thank you for pointing out the need for clarification. As mentioned in the previous response, we conducted a comprehensive reevaluation of the health impact assessment, including recalculations using the newly updated GBD 2021 country-level baseline mortality data. The discrepancy between the original draft's global estimate based on the GEMM model and results from other studies stems from the use of global average baseline mortality data in the earlier calculations. In this reevaluation, we specifically addressed this issue by using country-level data for more accurate estimations. The updated results are described in the first paragraph of the ***Wildfire-induced $PM_{2.5}$ mortality and direct radiative forcing*** section as follows:

" *The annual global premature deaths induced by $PM_{2.5}$ from fire emissions are estimated to be 0.24 (95% Confidence Interval: 0.15–0.32) million for the years 2010–2014 , accounting for ~3.1% of total global $PM_{2.5}$-related deaths, which agrees with the estimate of McDuffie et al[14]. The global total mortality from all sources of $PM_{2.5}$ is estimated to be 7.7 (95% CI: 5.4–9.8) million, within the range of 6.9–8.9 million reported in previous studies[13,23,25] using the same exposure-response function (ERF)[15].*"

14. l. 30 – I think the number here is per year, not accumulated over the 5 years.

Answer:

Yes, the number refers to the annual number. The corresponding sentence has been revised as follows: "*Projections show a surge in premature deaths, ranging from 1.40 to 2.19 million annually between 2095 and 2099, roughly 6–9 times higher than current levels.*"

15. l. 37 – How does the diminishing disparity underscore the need to address … ?

Answer:

To clarify this point, we have revised the corresponding sentence as follows:

"*As the world warms, wildfire-related health disparities across countries will diminish, with northern hemisphere countries facing greater risks, underscore the need for coordinated efforts to address health risks.*"

16. l. 123 – On what basis do you conclude that your estimates are more reasonable?

Answer:

To clarify this point, we have revised the corresponding sentence as follows:

" *The projected spatial trends in fire emissions are generally similar to those estimated by ESMs (Supplementary Fig.4), showing an overall increase across the Northern Hemisphere and a decline in the African region. Despite general trends, regional differences persist, particularly in areas with currently limited vegetation cover*[4]*, such as the 15°N–30°N latitude range in Africa and 15°N–60°N in West Asia. Here, ESMs project significant increases in fire carbon emissions, contrasting sharply with our projections, which show minimal emissions (Supplementary Fig. 4). Additionally, ESMs predict negligible growth in fire emissions in the Siberian Arctic (Supplementary Fig. 4), conflicting with recent studies*[44-46] *that indicate substantial increases in fire frequency due to warming and circulation changes. Our observation-constrained ML approach tends to provide more reasonable projections, showing an upward trend in wildfire emissions in these regions, compared to the ESM estimates.*

17. l. 133 – Is it coincidence that both scenarios give the same future deaths from wildfire smoke? Wouldn't we expect the more severe climate change scenario to yield more deaths, since it had more fire and fire C emissions (Fig 1)?

Answer:

In the original draft, the results were consistent up to two decimal places, as mentioned, with the higher fraction of people aged 65+ in the SSP245 scenario. However, due to the coarser resolution (2°x 2.5°) and the use of global average baseline mortality, the uncertainty in the estimates was relatively large. As noted in the previous response, we conducted a comprehensive reevaluation of the health impact assessment in this revision. The revised projections show premature deaths reaching 1.40 million and 2.19 million per year in SSP245 and SSP585, respectively, for 2095-2099.

18. l. 138 – Radiative forcing is defined for a change in state, often from one year to another. By convention radiative forcing is most commonly defined relative to preindustrial concentrations. It's not clear in this paragraph what years are being compared when giving radiative forcing outcomes. Is DRF in the future relative to the present? Or relative to preindustrial? How about the historical DRF presented?

Answer:

This seems similar to your eighth comment; please refer to the detailed response provided for that issue.

19. l.154 – If I understand correctly, the "POP" scenarios do not require another simulation of GEOS-chem, but rather would use the same GEOS-chem simulation with different population inputs for the mortality calculation.

Answer:

Yes, the "POP" scenarios do not require a separate GEOS-Chem simulation. Instead, they use the same GEOS-Chem simulation outputs with different population data for the mortality calculation. This approach allows us to assess the health impacts under varying population scenarios without the need for additional GEOS-Chem runs. To clarify this point, we have revised the corresponding sentence as follows: "*..., we conducted four additional sensitivity experiments.*"

20. l. 159 – Again, if I understand correctly, the “POP” scenarios change both total population and age structure, from which it would not be possible to separate the influence of population growth from the age structure. But here the authors attribute the change to the age structure alone.

Answer:

To clarify this point, we have revised the corresponding sentence as follows:

"*Our estimations indicate that 1.51 (95% CI: 0.89–2.04) million and 1.87 (95% CI: 1.11–2.54) million $PM_{2.5}$-related premature deaths from fire emissions are due to combined changes in both population size and age structure at the end of the 21st century for SSP2-4.5 and SSP5-8.5 scenarios (SP2-POP, SP5-POP in Fig. 3), respectively*."

21. l. 160 – The numbers of deaths here and in Figure 3 can only be understood as contributors to the overall impact of fires on deaths, which is given earlier as 1.19 million. I suggest they be presented as such.

Answer:

To clarify this point, we have revised the corresponding sentence as follows:

"*Our estimations indicate that 1.51 (95% CI: 0.89–2.04) million and 1.87 (95% CI: 1.11–2.54) million $PM_{2.5}$-related premature deaths from fire emissions at the end of the 21st century, due to combined changes in both population size and age structure for SSP2-4.5 and SSP5-8.5 scenarios (SP2-POP, SP5-POP in Fig. 3), respectively, are contributions to the overall fire-related mortality. In addition, fire-induced $PM_{2.5}$-related deaths resulting from climate and emission changes are projected at 0.24 (95% CI: 0.11–0.41) million and 0.34 (95% CI: 0.16–0.56) million for SSP2-4.5 and SSP5-8.5 scenarios (SP2-EM, SP5-EM in Fig. 3), respectively*."

22. l. 179 – Here the authors clarify that different population assumptions are made in the two SSP scenarios. While it’s true that the SSPs have different population, it might be more straightforward to keep population the same between the scenarios, to focus on the impacts

of fires.

Answer:

Thank you for your suggestion. The SP2-EM and SP5-EM scenarios were designed to assess the impact of climate and emission changes on $PM_{2.5}$-related premature mortality under future scenarios. These scenarios control for population size and age distribution to match those of the historical period (2010–2014), while using pollutant concentrations driven by climate and emission projections for the future. We believe that the results from these experiments reflect the influence of climate and emission changes. Additional descriptions of these results have been added to the third paragraph of the ***Drivers of the Surge in Wildfire-induced PM2.5 Mortality*** section to support our conclusions, as follows:

"*Our projection identifies sub-Saharan Africa as the region that will be impacted the most by fire-induced premature deaths in future warming scenarios. The premature deaths due to $PM_{2.5}$ from fire emissions will markedly increase from 0.05(95% CI: 0.03–0.07) million in the historical period (2010–2014) to 0.57 (95% CI: 0.26–0.88) million and 0.91 (95% CI: 0.45–1.35) million for the 65+ age group (blue bars in Fig. 1(b)) during the end of the 21st century (2095–2099) in SSP2-4.5 and SSP5-8.5 scenarios, respectively. However, it is important to emphasize that in the sub-Saharan region, we observe a decrease in fire emissions' contributions to ambient $PM_{2.5}$ concentrations during the end of the 21st century compared to the historical period, primarily due to a projected decline in fire emissions over this region (refer to Supplementary Figs. 4 (a), (b) and Supplementary Figs. 7 (a), (b)). These changes are associated with shifts in the age structure of the region, particularly the substantial growth of the population aged 65+ between 2095–2099 compared to 2010–2014 (Supplementary Table 5). As evidenced by the SP2-EM and SP5-EM results, when population size is held constant, premature deaths in the 65+ age group in sub-Saharan Africa decrease relative to historical levels (Figure 3), which is primarily attributed to the lower fire-contributed $PM_{2.5}$ concentrations in this region.*"

23. l. 183 – In the text it is not clear whether the authors are comparing future wildfire smoke

deaths with a) present wildfire smoke deaths or b) all $PM_{2.5}$-related deaths, when they reach the conclusion that disparities diminish. Figure 4a clarifies that it is compared to present wildfire smoke deaths. That should be clarified in the text, and the authors might consider comparing with all $PM_{2.5}$ deaths since they also estimate that.

Answer:

To clarify this point, we have revised the corresponding sentence as follows:

"*Premature deaths attributed to $PM_{2.5}$ resulting from increased fire emissions were evaluated across 18 continental regions (Fig. 4(a) and Supplementary Fig. 8), comparing future wildfire smoke deaths to those in historical period.*"

24. l.198 and Figure 4 – Consider presenting disparities in smoke relative to percents of population, rather than percent of global GDP. For an exposure and health issue, I'm not sure how GDP is relevant.

Answer:

Given that historical emission hotspots were primarily concentrated in the Southern Hemisphere near the equator, our calculations reveal a trend of wildfire emission hotspots shifting northward. This shift underscores that future global warming, driven by both national emissions and human activities (with greater contributions from northern hemisphere countries), will result in an increased shared risk from wildfire emissions across northern hemisphere nations, many of which are more affluent. To highlight this relationship and imbalance, GDP is used to describe the historical-to-future changes, rather than the direct correlation between wildfires and GDP.

25. l. 206 – Might more be said here to put the fire smoke $PM_{2.5}$ deaths into context of current or projected deaths from PM2.5 more generally?

Answer:

As suggested, we have added the following statements in the ***Discussion*** section:

"*This intensifying wildfire activity is expected to significantly raise greenhouse gas and air pollutant emissions, exacerbating global warming and increasing $PM_{2.5}$-related premature deaths, with the contribution of global wildfires to total $PM_{2.5}$-related deaths projected to rise from 3.1% in the historical period to 14.0–18.5% under future scenarios.*"

26. l.409 – What is PRD? I don't see it defined.

Answer:

To clarify this point, we have revised the corresponding sentence as follows:

" *The projected fire carbon emissions (PRD(C)) are calculated by multiplying projected burned area (PRD(BA)) by fuel consumption for 2015–2099 under SSP2-4.5 and SSP5-8.5 scenarios.*"

27. l. 428 – "Species" is both singular and plural. "specie" isn't a word.

Answer:

We have revised the "*specie*" to "*species*".

28. Figure 3 – The labels on the 2nd bar seem wrong as 346 looks bigger than 536.

Answer:

We have carefully reviewed and corrected the mislabeled figures.

We appreciate the careful and thoughtful appraisal of our work by the Editors/Reviewers, along with their many helpful suggestions. We hope that the corrections made in response to your feedback will meet with your approval.

References

1 Randerson, J. T., Chen, Y., van der Werf, G. R., Rogers, B. M. & Morton, D. C. Global burned area and biomass burning emissions from small fires. *Journal of Geophysical Research: Biogeosciences* **117** (2012). https://doi.org/https://doi.org/10.1029/2012JG002128

2 Chen, T. & Guestrin, C. in *Proceedings of the 22nd ACM SIGKDD International Conference on Knowledge Discovery and Data Mining* 785–794 (Association for Computing Machinery, San Francisco, California, USA, 2016).

3 Zhu, Q. *et al.* Building a machine learning surrogate model for wildfire activities within a global Earth system model. *Geosci. Model Dev.* **15**, 1899-1911 (2022). https://doi.org/10.5194/gmd-15-1899-2022

4 Yu, Y. *et al.* Machine learning–based observation-constrained projections reveal elevated global socioeconomic risks from wildfire. *Nature Communications* **13**, 1250 (2022). https://doi.org/10.1038/s41467-022-28853-0

5 van der Werf, G. R. *et al.* Global fire emissions estimates during 1997–2016. *Earth Syst. Sci. Data* **9**, 697-720 (2017). https://doi.org/10.5194/essd-9-697-2017

6 Dee, D. P. *et al.* The ERA-Interim reanalysis: configuration and performance of the data assimilation system. *Quarterly Journal of the Royal Meteorological Society* **137**, 553-597 (2011). https://doi.org/https://doi.org/10.1002/qj.828

7 Gregor, L., Kok, S. & Monteiro, P. M. S. Empirical methods for the estimation of Southern Ocean CO2: support vector and random forest regression. *Biogeosciences* **14**, 5551-5569 (2017). https://doi.org/10.5194/bg-14-5551-2017

8 Gade, K. A Non-singular Horizontal Position Representation. *The Journal of Navigation* **63**, 395-417 (2010). https://doi.org/10.1017/S0373463309990415

9 Zhao, J. *et al.* Improving Estimates of Dynamic Global Marine DMS and Implications for Aerosol Radiative Effect. *Journal of Geophysical Research: Atmospheres* **129**, e2023JD039314 (2024). https://doi.org/https://doi.org/10.1029/2023JD039314

10 Wang, W. L. *et al.* Global ocean dimethyl sulfide climatology estimated from observations and an artificial neural network. *Biogeosciences* **17**, 5335-5354 (2020). https://doi.org/10.5194/bg-17-5335-2020

11 GBD. 2019 risk factors collaborators. Global burden of 87 risk factors in 204 countries and territories, 1990–2019: a systematic analysis for the Global Burden of Disease Study 2019. *Lancet* **396**, 1223–1249 (2020). https://doi.org/10.1016/S0140-6736(20)30752-2

12 Connolly, R. et al., Mortality attributable to $PM_{2.5}$ from wildland fires in California from 2008 to 2018. *Sci. Adv.***10**, eadl1252 (2024). https://doi.org/10.1126/sciadv.adl1252

13 Pozzer, A. *et al.* Mortality Attributable to Ambient Air Pollution: A Review of Global Estimates.

GeoHealth **7**, e2022GH000711 (2023). https://doi.org/https://doi.org/10.1029/2022GH000711

14 McDuffie, E. E. *et al.* Source sector and fuel contributions to ambient PM2.5 and attributable mortality across multiple spatial scales. *Nature Communications* **12**, 3594 (2021). https://doi.org/10.1038/s41467-021-23853-y

15 Burnett, R. *et al.* Global estimates of mortality associated with long-term exposure to outdoor fine particulate matter. *Proceedings of the National Academy of Sciences* **115**, 9592-9597 (2018). https://doi.org/10.1073/pnas.1803222115

16 Manisalidis, I., Stavropoulou, E., Stavropoulos, A. & Bezirtzoglou, E. Environmental and Health Impacts of Air Pollution: A Review. **8** (2020). https://doi.org/10.3389/fpubh.2020.00014

17 Cohen, A. J. *et al.* Estimates and 25-year trends of the global burden of disease attributable to ambient air pollution: an analysis of data from the Global Burden of Diseases Study 2015. *The Lancet* **389**, 1907-1918 (2017). https://doi.org/https://doi.org/10.1016/S0140-6736(17)30505-6

18 Murray, C. J. L. *et al.* Global burden of 87 risk factors in 204 countries and territories, 1990–2019: a systematic analysis for the Global Burden of Disease Study 2019. *The Lancet* **396**, 1223-1249 (2020). https://doi.org/https://doi.org/10.1016/S0140-6736(20)30752-2

19 Brauer, M. *et al.* Global burden and strength of evidence for 88 risk factors in 204 countries and 811 subnational locations, 1990–2021: a systematic analysis for the Global Burden of Disease Study 2021. *The Lancet* **403**, 2162-2203 (2024). https://doi.org/10.1016/S0140-6736(24)00933-4

20 Anenberg Susan, C. *et al.* Estimates of the Global Burden of Ambient PM2.5, Ozone, and NO2 on Asthma Incidence and Emergency Room Visits. *Environmental health perspectives* **126**, 107004 https://doi.org/10.1289/EHP3766

21 Chowdhury, S. *et al.* Global and national assessment of the incidence of asthma in children and adolescents from major sources of ambient NO2. *Environmental Research Letters* **16**, 035020 (2021). https://doi.org/10.1088/1748-9326/abe909

22 Connolly, R. et al., Mortality attributable to $PM_{2.5}$ from wildland fires in California from 2008 to 2018. *Sci. Adv.***10**, eadl1252 (2024). https://doi.org/10.1126/sciadv.adl1252

23 Gordon, J. N. D. *et al.* The Effects of Trash, Residential Biofuel, and Open Biomass Burning Emissions on Local and Transported PM2.5 and Its Attributed Mortality in Africa. *GeoHealth* **7**, e2022GH000673 (2023). https://doi.org/https://doi.org/10.1029/2022GH000673

24 Chen, G. *et al.* Mortality risk attributable to wildfire-related PM2.5 pollution: a global time series study in 749 locations. *The Lancet Planetary Health* **5**, e579-e587 (2021). https://doi.org/10.1016/S2542-5196(21)00200-X

25 Lelieveld, J. *et al.* Air pollution deaths attributable to fossil fuels: observational and modelling study. *BMJ* **383**, e077784 (2023). https://doi.org/10.1136/bmj-2023-077784

26 Turetsky, M. R. *et al.* Global vulnerability of peatlands to fire and carbon loss. *Nature*

Geoscience **8**, 11-14 (2015). https://doi.org/10.1038/ngeo2325

27 Mathijssen, P. J. H., Tuovinen, J.-P., Lohila, A., Väliranta, M. & Tuittila, E.-S. Identifying main uncertainties in estimating past and present radiative forcing of peatlands. *Global change biology* **28**, 4069-4084 (2022). https://doi.org/https://doi.org/10.1111/gcb.16189

28 Rodríguez Vásquez, M. J., Benoist, A., Roda, J. M. & Fortin, M. Estimating Greenhouse Gas Emissions From Peat Combustion in Wildfires on Indonesian Peatlands, and Their Uncertainty. *Global Biogeochemical Cycles* **35**, e2019GB006218 (2021). https://doi.org/https://doi.org/10.1029/2019GB006218

29 Gidden, M. J. *et al.* Global emissions pathways under different socioeconomic scenarios for use in CMIP6: a dataset of harmonized emissions trajectories through the end of the century. *Geosci. Model Dev.* **12**, 1443-1475 (2019). https://doi.org/10.5194/gmd-12-1443-2019

30 Feng, L. *et al.* The generation of gridded emissions data for CMIP6. *Geosci. Model Dev.* **13**, 461-482 (2020). https://doi.org/10.5194/gmd-13-461-2020

31 Doelman, J. C., Verhagen, W., Stehfest, E. & van Vuuren, D. P. The role of peatland degradation, protection and restoration for climate change mitigation in the SSP scenarios. *Environmental Research: Climate* **2**, 035002 (2023). https://doi.org/10.1088/2752-5295/acd5f4

32 Orellano, P., Reynoso, J., Quaranta, N., Bardach, A. & Ciapponi, A. Short-term exposure to particulate matter (PM10 and PM2.5), nitrogen dioxide (NO2), and ozone (O3) and all-cause and cause-specific mortality: Systematic review and meta-analysis. *Environment International* **142**, 105876 (2020). https://doi.org/https://doi.org/10.1016/j.envint.2020.105876

33 Shyamsundar, P. *et al.* Fields on fire: Alternatives to crop residue burning in India. *Science* **365**, 536-538 (2019). https://doi.org/10.1126/science.aaw4085

34 Radeloff, V. C. *et al.* Rapid growth of the US wildland-urban interface raises wildfire risk. *Proceedings of the National Academy of Sciences* **115**, 3314-3319 (2018). https://doi.org/10.1073/pnas.1718850115

35 Schug, F. *et al.* The global wildland–urban interface. *Nature* **621**, 94-99 (2023). https://doi.org/10.1038/s41586-023-06320-0

36 Bowman, D. M. J. S. *et al.* Vegetation fires in the Anthropocene. *Nature Reviews Earth & Environment* **1**, 500-515 (2020). https://doi.org/10.1038/s43017-020-0085-3

37 Aragão, L. E. O. C. *et al.* 21st Century drought-related fires counteract the decline of Amazon deforestation carbon emissions. *Nature Communications* **9**, 536 (2018). https://doi.org/10.1038/s41467-017-02771-y

38 Tang, R. *et al.* Quantifying wildfire drivers and predictability in boreal peatlands using a two-step error-correcting machine learning framework in TeFire v1.0. *Geosci. Model Dev.* **17**, 1525-1542 (2024). https://doi.org/10.5194/gmd-17-1525-2024

39 Buch, J., Williams, A. P., Juang, C. S., Hansen, W. D. & Gentine, P. SMLFire1.0: a stochastic

machine learning (SML) model for wildfire activity in the western United States. *Geosci. Model Dev.* **16**, 3407-3433 (2023). https://doi.org/10.5194/gmd-16-3407-2023

40 Li, F. *et al.* AttentionFire_v1.0: interpretable machine learning fire model for burned-area predictions over tropics. *Geosci. Model Dev.* **16**, 869-884 (2023). https://doi.org/10.5194/gmd-16-869-2023

41 Xu, Z., Han, Y., Tam, C.-Y., Yang, Z.-L. & Fu, C. Bias-corrected CMIP6 global dataset for dynamical downscaling of the historical and future climate (1979–2100). *Scientific Data* **8**, 293 (2021). https://doi.org/10.1038/s41597-021-01079-3

42 Senande-Rivera, M., Insua-Costa, D. & Miguez-Macho, G. Spatial and temporal expansion of global wildland fire activity in response to climate change. *Nature Communications* **13**, 1208 (2022). https://doi.org/10.1038/s41467-022-28835-2

43 Hurtt, G. C. *et al.* Harmonization of global land use change and management for the period 850–2100 (LUH2) for CMIP6. *Geosci. Model Dev.* **13**, 5425-5464 (2020). https://doi.org/10.5194/gmd-13-5425-2020

44 Chen, A. *et al.* Long-Term Observations of Levoglucosan in Arctic Aerosols Reveal Its Biomass Burning Source and Implication on Radiative Forcing. *Journal of Geophysical Research: Atmospheres* **128**, e2022JD037597 (2023). https://doi.org/https://doi.org/10.1029/2022JD037597

45 Descals, A. *et al.* Unprecedented fire activity above the Arctic Circle linked to rising temperatures. *Science* **378**, 532-537 (2022). https://doi.org/10.1126/science.abn9768

46 Zheng, B. *et al.* Record-high CO2 emissions from boreal fires in 2021. *Science* **379**, 912-917 (2023). https://doi.org/10.1126/science.ade0805

47 Zhao, J. *et al.* Changes in global DMS production driven by increased CO2 levels and its impact on radiative forcing. *npj Climate and Atmospheric Science* **7**, 18 (2024). https://doi.org/10.1038/s41612-024-00563-y

48 Saiz-Lopez, A. *et al.* Natural short-lived halogens exert an indirect cooling effect on climate. *Nature* **618**, 967-973 (2023). https://doi.org/10.1038/s41586-023-06119-z

Responses to reviewers' comments

We sincerely thank all four reviewers for their thoughtful and constructive feedback on our manuscript entitled "***Global warming escalates wildfire smoke health burden while narrowing global inequality disparities***". We have carefully addressed all comments and revised the manuscript accordingly.

For clarity, our point-by-point responses to each reviewer's comment are provided below, with our replies highlighted in green-shaded text. In particular, in response to the first comment from Reviewer #4, we conducted a review of recent literature on projected wildfire activity and associated health impacts to better contextualize and emphasize the technical novelty of our study.

Revisions made to the manuscript are clearly indicated: new additions appear in red text, and deletions are shown with strikethrough formatting for full transparency.

Response to Reviewer #1

1. I appreciate the authors' great efforts to improve the manuscript. I have the following suggestions, which are mostly minor.

Response:

We appreciate the reviewer's time and effort in reviewing our responses and providing valuable suggestions to further improve the manuscript.

2. Intentional burning for agricultural conversion is not necessarily considered "forest mismanagement." While I understand the authors' intent, some readers may misinterpret the text as implying that all intentional burning is mismanagement. The authors do acknowledge the benefits of controlled fires for forest management, but only in the Discussion section rather than the Introduction. I suggest either removing the parenthetical phrase or clarifying the distinction more explicitly in the Introduction.

Response:

Following the reviewer's suggestion, we have removed the parenthetical phrase from the first paragraph of the ***Introduction***. The revised sentence now reads:

"*Additionally, increased development at the wildland–urban interface and forest mismanagement may further exacerbate the frequency of fire occurrences globally.*"

3. The GDP presented in Figure 4 does not specify the time range. The total GDP amount and country-specific shares are unlikely to be the same between 2010-2014 and 2095-2099.

Response:

We have clarified the data sources of GDP in the caption of Figure 4 as follows.

"*Country-specific GDP data are sourced from the SSP Public Database Version 2.0 (https://tntcat.iiasa.ac.at/SspDb/dsd?Action=htmlpage&page=welcome). For the historical period, GDP values from the year 2010 are used. For the SSP2-4.5 and SSP5-8.5 scenarios, GDP projections for the year 2100 are used.*"

Response to Reviewer #2

1. The authors have responded well to the comments with many useful revisions that have improved the clarity and content of the paper. I have no further suggestions.

Response:

We sincerely thank the reviewer for the positive feedback on our revised manuscript.

Response to Reviewer #3

1. Overall the authors were thorough in their responses to my earlier comments, with some rather minor remaining questions highlighted below. They also made some efforts to improve estimates, such as by better accounting for fine-scale $PM_{2.5}$ population exposure. I also had new observations on the equity analysis below. I think this is an interesting study that is well-conducted overall, that uses some new ML methods to project future fire occurrence and its impacts.

Response:

We sincerely thank the reviewer for the thoughtful and encouraging feedback on our efforts to improve the manuscript. Below, we address the remaining questions raised.

2. Regarding the inequality analysis, two comments. First, I think the authors are estimating premature deaths by regions (Figure 4a) or countries, and that is the basis for determining inequity on the basis of GDP of those regions or countries. That is a simplification that misses differences among GDP within countries, in which rural populations may be exposed to greater fire smoke generally than urban populations. Of course some countries have extremely large population. It may be appropriate to clarify the methods used and the limitations they pose. Second, it may be worthwhile to compare inequality due to fire smoke with that due to total $PM_{2.5}$, at present and in the two future scenarios, to better put the inequality into a broader context. Do changes in fire smoke $PM_{2.5}$ make existing inequalities for $PM_{2.5}$ worse or do they alleviate them? Perhaps 3 new lines could be added to Figure 4b.

Response:

We thank the reviewer for this insightful comment highlighting both methodological limitations and the broader context of inequality.

First, regarding the use of national-level GDP, we have clarified this limitation in the section ***Reshaped global inequality patterns of wildfire health impacts*** as follows:

"*Our analysis is based on national-level GDP data and does not capture intra-national disparities in exposure, such as differences between rural and urban populations. With more spatially resolved socioeconomic projections, this framework could be extended to assess within-country inequalities in future studies.*"

Second, in line with the reviewer's suggestion, we added three new curves to Figure 4b to compare inequality in fire-related $PM_{2.5}$ premature deaths from fire emissions versus total $PM_{2.5}$, under the present and two future scenarios (SSP2-4.5 and SSP5-8.5). We have also revised the corresponding discussion in the main text. The updated paragraph now reads:

"*Global inequality (regional differences) in $PM_{2.5}$-related premature deaths attributable to fire emissions is projected to decline in the future (solid curves in Fig. 4(b)), broadly mirroring trends in total $PM_{2.5}$-related deaths (dashed curves in Fig. 4(b)). This shift is driven by two main drivers: a global increase in fire emissions resulting from climate change, particularly in currently less-affected regions, and a concurrent decline in anthropogenic $PM_{2.5}$ due to economic development and air pollution controls. For instance, the least developed countries (bottom 50% of global Gross Domestic Product (GDP)) account for around 94% of total $PM_{2.5}$-related deaths in 2010–2014, but only 70% and 62% by 2100 under SSP2-4.5 and SSP5-8.5 scenarios, respectively.*

However, fire-related $PM_{2.5}$ mortality shows persistent and distinct inequality patterns. Countries comprising the poorest 20% of global GDP are projected to experience 49% and 41% of fire-related deaths by 2100 under SSP2-4.5 and SSP5-8.5 scenarios, respectively—lower than today's 62% but higher than their future shares of total $PM_{2.5}$ deaths (approximately 20%). This is because fire activities remain concentrated in tropical regions. Meanwhile, wealthier nations (top 30% of GDP) are projected to experience a growing share of fire-related mortality (15% in SSP5-8.5), exceeding both today's 2% and their share of total $PM_{2.5}$ mortality (11%), driven by increasing fire activity across the mid-latitudes of the Northern Hemisphere. These patterns indicate rising health burdens in previously less affected, often more developed regions. Fire-related health impacts could persist or even intensify in some high-income regions, challenging efforts to reduce overall health burden."

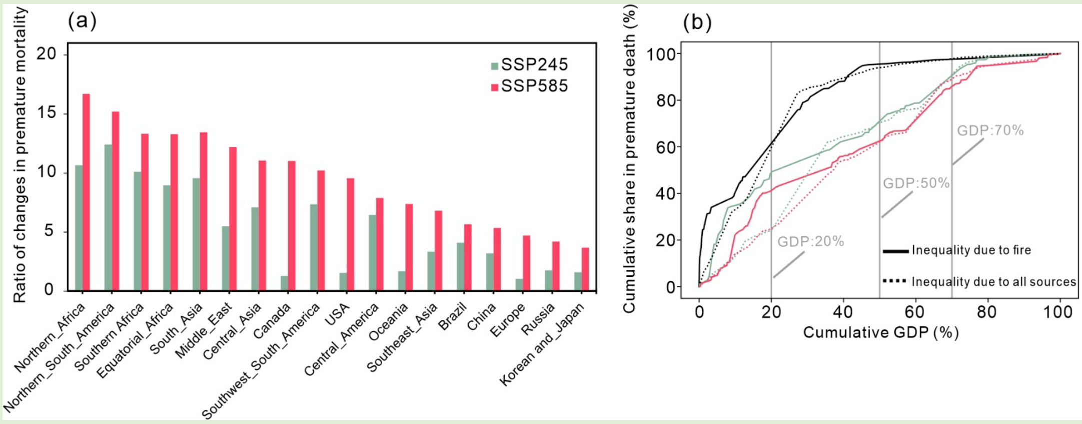


Figure 4. Regional premature deaths attributed to $PM_{2.5}$ exposure resulting from fire emissions and premature deaths inequality related to GDP for the historical period (black curve), and end of the 21st century under the SSP2-4.5 (green curve) and SSP5-8.5 (red curve) scenarios. (a) Change ratio of annual mean regional $PM_{2.5}$-related premature deaths due to fire emissions in end of the 21st century of the SSP2-4.5 scenario (green bar) and the SSP5-8.5 scenario (red bar) compared to the historical period. The region definition refers to Supplementary Fig. 8. (b) The horizontal axis displays the cumulative GDP, arranged from the least affluent country on the left to the wealthiest country on the right. Country-specific GDP data are sourced from the SSP Public Database Version 2.0 (https://tntcat.iiasa.ac.at/SspDb/dsd?Action=htmlpage&page=welcome). For the historical period, GDP values from the year 2010 are used. For the SSP2-4.5 and SSP5-8.5 scenarios, GDP projections for the year 2100 are used.

3. In their response to my question #3, I don't see that the authors addressed the question of whether the ESMs were run with fixed (actual historical) meteorology or meteorology that floats. Also, are all 5 models (4 ESMs and ERA5) provided as input predictors of meteorological parameters, and then the ML decides how to weight the different predictors?

Response:

We thank the reviewer for the follow-up question. To clarify: the ESMs were run with internally consistent, projected meteorological fields—not fixed historical meteorology. The ML model was trained and applied using meteorological predictors from the four ESMs only.

ERA5 data were used solely for independent evaluation of model performance and were not included as input features.

To clarify this point in the manuscript, we have added the following to the second paragraph of ***Supplementary Text 2***: "*The ML model was trained and applied using meteorological data from ESMs with internally consistent projections. ERA-5 reanalysis data were used only for independent evaluation and not as input predictors. The ML model therefore determines the relative weights of the four ESM inputs alone.*"

4. Regarding my question #5, the historical training period is short enough that perhaps no big changes in land use and fire management occurred. But I suspect that had the authors trained over a much longer historical period (acknowledging the BA data would not be available), land management factors and particularly fire suppression seems like it would be quite important – perhaps a caution on future projections. The authors' response is sufficient unless they want to strengthen the discussion to include this point.

Response:

We appreciate the reviewer's point and have strengthened the discussion accordingly. The following statement has been added to the ***Discussion*** section:

"*Due to the short training period constrained by the availability of historical burned area data, our ML model may not fully capture the long-term impacts of land use change and fire management —particularly suppression practices—on fire dynamics. While our study emphasizes the potential health risks of climate-driven increase of fire, the analytical framework can be extended in future work to account for land management influences as longer-term and higher-resolution datasets become available.*"

5. Regarding #8, the change in text is helpful. I would suggest putting a description of what DRF is relative to in the figure caption, so it is clear what quantity is plotted. But that is the authors' choice.

Response:

To improve clarity, we have added the following definition to the Figure 2 caption:

"*Fire-induced DRF is defined as the difference between simulations with and without fire emissions over the same period.*"

6. Regarding #16, I don't see how your estimates are "more reasonable". It is good to point out that they are different, as you do. But I don't see that you have a basis for concluding that one is more reasonable than another.

Response:

We have revised the corresponding sentence as follows: "*Additionally, ESMs predict negligible growth in fire emissions in the Siberian Arctic (Supplementary Fig. 4), conflicting with recent studies[4,5,32] indicating substantial increases in fire frequency due to warming and circulation changes. In contrast, our observation-constrained ML approach aligns with these previous studies, showing an upward trend in wildfire emissions over these regions.*"

7. Regarding #26, the authors are clear in the definition of PRD(..), but it seems strange to use the PRD name for a variable if it does not stand for anything.

Response:

"PRD" is used consistently throughout the manuscript as a shorthand for "prediction", including in figures and tables. While it is not an acronym, we opted to retain it for brevity and consistency. However, if the abbreviation is deemed confusing, we are happy to revise it in a future version.

Response to Reviewer #4

1. In their manuscript, Zhao et al. use a machine learning approach and chemical transport model to project future wildfire-induced $PM_{2.5}$ pollution under two different climate scenarios, and then use the long-term exposure estimates generated by this approach to estimate changes in the number of deaths attributable to wildfire-induced $PM_{2.5}$. They also investigate the drivers of these changes (e.g., exposure/climate change, population, etc.) and the impact of changing emissions on Earth's radiative balance.

   I appreciate the authors' work to address the first round of reviewer comments. Overall, the writing and data visualizations are good and the methodology seems relatively sound. I would have no problem recommending this manuscript for publication in a disciplinary journal after addressing my comments and other comments from the other reviewers. To me, the question for a Nature submission becomes what new insight has been gained from this analysis. This is where the manuscript comes up short for me, especially since there are recent papers on this topic (e.g., https://iopscience.iop.org/article/10.1088/1748-9326/ad1b7d/meta). Perhaps this a question for the journal editors and also an opportunity for the authors to think about the novelty that their results represent and ways to discuss that novelty.

Response:

We sincerely thank the reviewer for the constructive and thoughtful feedback, particularly regarding the novelty of our study. We appreciate the opportunity to clarify how our study advances the current understanding of future wildfire emissions, air quality impacts, and associated health burdens in the context of recent literature.

(1) Comparison with Park et al. (2024)

We thank the referee for pointing us to the study by Park et al. (2024)[1], which examined future wildfire-related health impacts using a land surface model (LSM). As we noted in our introduction, accurate projections of future wildfire emissions are essential for designing effective mitigation and adaptation strategies. However, the parameterization in Park et al.

(2024)[1]—relying on empirical relationships with GDP, population density, and lightning ignition—introduces substantial uncertainties. This is reflected in the wide discrepancies among process-based models, including the one used by Park et al., when simulating historical fire emissions compared to satellite-based observations (Yu et al., 2022[2]). Moreover, Park et al. (2024) [1] calculated wildfire emissions using an offline method and did not incorporate the complex, bidirectional interactions between climate change, vegetation dynamics, and fire behavior—key factors that influence future fire activity.

In contrast, our study employs an interpretable machine learning framework constrained by satellite-based observations to dynamically project burned areas under evolving climate conditions. This approach accounts for climate feedback and incorporates vegetation biomass from fully coupled CMIP6 Earth system models (ESMs), which simulate interactive responses between climate and vegetation. These technical innovations allow us to generate a climate-responsive, observation-constrained wildfire future emissions dataset that represents a key methodological improvement over Park et al. (2024) [1]. Furthermore, building on feedback from the first round of reviews, we expand beyond undifferentiated $PM_{2.5}$ mortality estimates to include wildfire-specific mortality risks using the latest dose-response relationships from Connolly et al. (2024)[3]. We also integrate the newly updated GBD 2021 dataset, enabling high-resolution (0.1° × 0.1°) global estimates of wildfire-related health burdens.

Using our newly developed emissions dataset, we uncover several important insights not captured by previous studies. Notably, we project a rise in fire-related premature deaths across high-latitude regions of the Northern Hemisphere and a substantial increase in mortality across Africa, primarily driven by demographic aging in the future. In contrast, Park et al. (2024)[1] projected declining mortality in Eurasian high latitudes and inconsistent trends across Africa. While Park et al. (2024)[1] offers useful regional perspectives, it does not incorporate dynamic feedback between climate, vegetation, and wildfire emissions, nor does it quantitatively disentangle the drivers of future wildfire-related mortality changes. Our study addresses these limitations by introducing a more integrated and physically consistent framework for projecting wildfire emissions and associated health impacts under climate change.

(2) Review of global fire projections and associated health impacts

Global warming is expected to intensify wildfire activity by increasing aridity and lengthening fire seasons, thereby releasing more greenhouse gases and reinforcing a positive feedback loop between climate change and wildfire emissions[4,5]. Accurate projections of future wildfire emissions and their consequences are essential to inform targeted mitigation and adaptation strategies. However, wildfire behavior is shaped by a complex interplay of climatic, ecological, and human factors, making emission projections an interdisciplinary challenge. ESMs attempt to simulate human–vegetation–fire–climate interactions using process-based approaches[6], but substantial uncertainties remain due to nonlinear feedback and data limitations. Over the past decade, a range of modeling approaches has been developed to estimate future fire trends and their health implications. Below, we review these studies to position our work in the broader context and to clarify the methodological advantages and added value of our approach.

a. Process-driven wildfire emission projection

ESMs simulate the coupled interactions among the atmosphere, land, ocean, sea ice, and biosphere, and are widely used to explore long-term climate dynamics and future Earth system responses. ESMs typically include process-based fire modules, which enable the assessment of fire-climate feedback and projections of future wildfire activity.

Recent studies have employed ESMs and land surface models (LSMs) to explore how key drivers influence future fire emissions. For instance, Park et al. (2023)[7] used a calibrated process-based fire module within an LSM to isolate the effects of meteorology, biomass, land use, population density, and GDP per capita on future carbon emissions from wildfires across multiple scenarios. Nurrohman et al. (2024)[8] used an enhanced process-based LSM to improve the representation of Siberian wildfire dynamics, showing that warming and rising biomass may drive substantial increases in emissions. Similarly, Kinnunen et al. (2024)[9] used an LSM to project changes in fire seasonality, frequency, and burned area in Fennoscandia under future climate scenarios, identifying temperature and precipitation as primary drivers of increasing fire risk. Dong et al. (2024)[10] analyzed outputs from multiple CMIP6 ESMs, showing that

Arctic wildfire emissions could more than double by 2100 due to persistent drought and high temperatures, even in the presence of a 31% increase in precipitation. In contrast to these process-based studies, Brey et al. (2021)[11] applied a statistical regression model based on environmental predictors and CMIP5 climate projections to estimate future burned areas in the western U.S. While such statistical approaches are computationally efficient and informative, they generally lack feedback mechanisms and may fail to capture nonlinear scaling relationships under future climate regimes.

Despite the ability to incorporate biophysical and socioeconomic processes, many process-based fire models remain weakly constrained by observations and struggle to reproduce the spatial and temporal variability of historical wildfires., in particular extreme fires These deficiencies contribute to large uncertainties, especially over multidecadal projections. For example, Kloster and Lasslop (2017)[12] found that fire simulations from nine CMIP5 models underestimated the global burned area by more than 50% relative to satellite observations. While Li et al. (2024)[13] reported improvements in CMIP6 models—particularly in the global total burned area, spatial distribution, and seasonal timing—substantial challenges persist. Most models fail to capture the observed global decline in burned area and emissions over the past two decades and exhibit large mismatches with satellite-derived spatial patterns. These findings underscore the limitations of unconstrained, process-based approaches in accurately representing wildfire dynamics at global scales.

b. Data-driven wildfire emission projection

To overcome the limitations of process-based models and reduce computational demands, recent studies have increasingly turned to machine learning (ML) approaches to simulate wildfire activity from regional to global scales (Jain et al., 2020[14]). These models are typically trained on satellite-derived burned area datasets and use a wide range of climate and vegetation predictors to capture the complex, nonlinear relationships governing wildfire behavior. ML-based methods have been applied to regional fire forecasting (e.g., Wang et al., 2021[15]; Wang et al., 2022[16]; Kondylatos et al., 2022[17]; Buch et al., 2023[18]; Tang et al., 2024[19]) and to improve fire parameterizations in process-based land models (e.g., Zhu et al., 2022[20];

Liu et al., 2024[21]). Due to their strong performance in handling nonlinear dynamics and reliance on observational constraints, ML approaches have demonstrated higher predictive accuracy—up to 90% improvements—and lower computational costs compared to traditional process-based fire models.

Several recent studies have applied ML approaches to project future burned area at both regional (Li et al., 2023[22]) and global (Zhang et al., 2024[23]) scales, as well as to reconstruct historical burned area from 1901 to 2020 (Guo et al., 2025[24]). However, most of these efforts are limited to burned area prediction and do not integrate outputs from ESMs, thereby omitting key climate-vegetation-fire feedbacks that influence future fire behavior. As a result, they are unable to estimate fire emissions or downstream atmospheric impacts. Yu et al. (2022)[2] combined ML with an emergent constraint approach to project fire carbon emissions but did not include vegetation biomass—a key driver of fuel availability—which may lead to underestimation of emissions under future climate scenarios. Wang et al. (2023)[25] took a similar approach to estimate future wildfire-derived $PM_{2.5}$ over the contiguous U.S., yet their method was not designed to generate emissions compatible with atmospheric chemistry models. Thus, while these ML-based studies represent important progress in wildfire forecasting, they generally do not provide emissions data that can be used to quantify air quality, health outcomes, or radiative forcing under future scenarios.

In contrast to prior work, our study introduces an interpretable ML framework constrained by satellite-based burned area observations and designed to capture dynamic feedback between climate and fire activity. We further integrate vegetation biomass projections from ensemble CMIP6 Earth System Model simulations, enabling the estimation of future fuel loads in a manner that reflects bidirectional climate–vegetation interactions. This allows us to project global wildfire emissions that respond to both climate and ecosystem changes. Importantly, our emission dataset is physically consistent with the satellite-derived GFEDv4.1s historical record, providing a robust basis for simulating the atmospheric and health impacts of wildfire emissions from the historical period through the end of the 21st century.

c. Wildfire-induced $PM_{2.5}$ health impacts

As wildfire activity intensifies under warmer and drier future conditions, several studies have investigated the resulting impacts on ambient $PM_{2.5}$ concentrations and associated health burdens. For instance, Knorr et al. (2017)[26] coupled land surface and atmospheric chemistry models to project wildfire-driven $PM_{2.5}$ impacts through 2100. Sarangi et al. (2023)[27] employed process-based coupled climate–ecosystem models to examine future wildfire $PM_{2.5}$ levels in North America, while Xie et al. (2022)[28] used multiple linear regression to relate CMIP6 wildfire emissions and climate variables to $PM_{2.5}$ in the western U.S., projecting substantial increases in pollution and mortality. Collectively, these studies point to wildfires as an increasingly severe and persistent threat to air quality and public health—particularly in vulnerable regions such as the Pacific Northwest, where fire-related $PM_{2.5}$ concentrations are expected to rise two- to three-fold.

Several additional studies have assessed fire-related health impacts across different regions and timeframes. Ford et al. (2018) [29] used regionally coupled models to estimate future $PM_{2.5}$ exposure and mortality across the U.S. under RCP4.5 and RCP8.5 scenarios, projecting substantial increases in fire-attributable deaths. Neumann et al. (2021)[30] combined regression models based on historical burned area and climate data with ESM outputs to estimate a two–to threefold rise in $PM_{2.5}$-related premature mortality from wildfires in the western U.S. Borchers-Arriagada et al. (2021)[31] extended this approach to Paraguay, using a machine learning framework and ESM projections to estimate more than 3,500 annual wildfire-related deaths by 2100. Lou et al. (2023)[32] conducted a long-term national analysis for China, identifying a peak in fire-related mortality between 2021 and 2040. However, their estimates relied on CMIP6-derived fire emissions, which, as noted earlier, are subject to high uncertainty due to limitations in fire modeling and observational constraints.

These studies collectively underscore the growing contribution of wildfires to air pollution in a future shaped by declining anthropogenic emissions. However, most focus on specific regions and emphasize health effect estimation, often relying on simplified or externally sourced wildfire emission projections. A recent study by Qiu et al. (2024, preprint)[33] developed a statistical/ML-based framework linking climate variables to wildfire biomass emissions and projected a 76% increase in annual U.S. fire-related deaths by 2050 relative to 2011–2020. While this approach helped reduce uncertainty in emission estimates, it converted emissions to $PM_{2.5}$ concentrations using statistical relationships, without accounting for changes in future atmospheric chemistry. Qiu et al. also employed an empirical exposure-response function based on 2006–2019 U.S. mortality records, which is a methodological advance over literature-based assumptions, but their analysis did not extend to global scales.

In contrast to prior studies, our study does not rely on simplified or regionally limited approaches to estimate future wildfire emission or health impact modeling. We develop an ML-based global wildfire emission dataset, constrained by observations of historical burned areas and responsive to future climate and vegetation changes, and use it to drive a global atmospheric chemistry model spanning both historical and future periods. This enables spatially explicit simulations of $PM_{2.5}$ concentrations attributable specifically to wildfire activity. We pair this with the newly released GBD 2021 dataset and updated fire-specific exposure-response functions from Connolly et al. (2024) to quantify both total and wildfire-attributable premature deaths. Crucially, our analysis offers a global perspective, identifying rising mortality in Northern high-latitude regions and substantial increases in Africa largely due to demographic aging, which has been overlooked in many prior assessments. These findings highlight the growing relevance of wildfires to public health planning and climate adaptation policy.

(3) Concluding remarks

In summary, while prior research has yielded important insights into future wildfire trends and their health consequences, most studies are regionally focused, use simplified or externally generated emission projections, and lack integrated global assessments. Our study advances the field by developing an observation-constrained, ML-based global wildfire emission projection dataset and coupling it with a global atmospheric chemistry model. This integrated framework allows us to quantify wildfire-specific $PM_{2.5}$ exposure, associated mortality, and radiative forcing across both historical and future timeframes. By combining updated epidemiological evidence, the latest GBD 2021 health burden data, and the high-resolution $PM_{2.5}$ exposure dataset, our analysis provides a comprehensive and policy-relevant understanding of the escalating public health risks posed by wildfire smoke in a warming world.

2. While I do not doubt the authors' findings that global inequity with respect to fire-sourced $PM_{2.5}$ -attributable mortality is projected to decrease, I'm personally not sure whether this finding is a positive one as the authors have framed it (e.g., in the title "while narrowing global inequality disparities", "diminished global inequity", etc.). This narrowing is accomplished by increases virtually everywhere but that are uneven such that northern mid-latitude countries "catch up" to places like Africa. Therefore there are no winners - everyone will suffer the impacts of changing wildfire smoke and associated health impacts. Can you rephrase these occurrences to something more realistic about why it is that inequality is projected to narrow in the future?

Response:

We appreciate the reviewer's comment and agree that our original phrasing may have suggested that the narrowing of inequality seems like a positive outcome. To avoid this potential misinterpretation, we have revised the article ***title*** to:

"*Global warming amplifies the health impacts of wildfire smoke and reshapes inequality patterns***"**

Additionally, we modified the final sentence of the ***abstract*** to:

" The changes of this burden also become more evenly distributed across countries of different economic development levels, shifting the traditional patterns of health inequality with an increased share in the wealthier countries."

In the main text, we revised the title of ***Section 4*** to:

"Reshaped global inequality patterns of wildfire health impacts**"**

We also rewritten the third paragraph in this section to:

"However, fire-related $PM_{2.5}$ mortality shows persistent and distinct inequality patterns. Countries comprising the poorest 20% of global GDP are projected to contribute 49% and 41% of fire-related deaths by 2100 under SSP2-4.5 and SSP5-8.5 scenarios, respectively— lower than today's 62% but higher than their future shares of total $PM_{2.5}$ deaths (approximately 20%). This is because fire activities remain concentrated in tropical regions. Meanwhile, wealthier nations (top 30% of GDP) are projected to experience a growing share of fire-related mortality (15% in SSP5-8.5), exceeding both today's 2% and their share of total $PM_{2.5}$ mortality (11%), driven by increasing fire activity across the mid-latitudes of the Northern Hemisphere. These patterns indicate rising health burdens in previously less affected, often more developed regions. Fire-related health impacts could persist or even intensify in some high-income regions, challenging efforts to reduce overall health burden."

3. In the discussion (Line 314ff), the authors mention "Given the potential for even greater fire-induced greenhouse gas radiative effects"; however, I thought that the authors found that increased fire-induced aerosols enhance the cooling effect in much of the world (e.g., line 200ff, figure 2)? Please clarify and adjust text as needed.

Response:

We agree that the original phrasing could lead to confusion between the cooling effects of fire-induced aerosols and the warming effects of fire-induced greenhouse gases. To clarify, we have revised the sentence in the ***Discussion*** section as follows: "*While the results indicate a 1.4–2.3-fold increase in aerosol-induced cooling (negative DRF) over the mid-latitudes of the Northern Hemisphere, fire emissions also release greenhouse gases that exert a warming (positive) radiative effect. Given the potential for substantial warming contributions from fire-induced greenhouse gases, future work should aim to provide a thorough assessment of fire-induced net radiative effects to establish a more accurate natural baseline for climate system.*"

4. I feel that the section on projecting future fire emissions (lines 489ff) could use some more explanation, specifically regarding what drivers of future land-use changes and therefore emissions are accounted for in their machine learning approach.

The authors mention that biomass estimates from ESMs account for land-use changes (line 321), but it wasn't clear to me whether these are climate-change induced impacts on land use (e.g., changing precipitation patterns lead to less arable land) versus exogenous events (e.g., deforestation for increased agriculture). It would be great to understand the extent to which these factors are accounted for and whether or not their predictions at least roughly follow projections of land-use changes from the literature (e.g., https://www.sciencedirect.com/science/article/pii/S0959378016303399#fig0015; https://www.nature.com/articles/s41598-024-61035-0). I'm also curious as to the ability of the approach used by the authors to estimate changing emissions and whether or not this approach and/or any assumptions of the machine learning algorithm (e.g., linearity, etc.) can capture potential non-linearities in land-use changes.

Response:

The ESMs used in this study adopt the Land-Use Harmonization 2 (LUH2)[34] dataset as the land-use forcing[35,36] for their land components under ScenarioMIP. LUH2 incorporates land-use changes driven by exogenous human activities and reflects a range of socioeconomic developments and policy decisions. It is disaggregated according to each each SSP scenario, based on land-use projections from models coupled with, or part of, Integrated Assessment Models[37], thus enabling consistent representation of land-use dynamics across different future scenarios. Accordingly, we have added the following clarification to the first paragraph after Equation 4 in the ***Methods*** section: "*These biomass estimates are derived from ESMs that use the Land-Use Harmonization 2 (LUH2)[34] dataset as the land-use forcing[35,36], which accounts for land-use changes linked to human activities (e.g., deforestation for agricultural expansion) and socioeconomic scenarios. The LUH2 dataset is disaggregated for each SSP land-use scenario based on outputs from land-use models that are either coupled with or part of Integrated Assessment Models[37], thereby reflecting different socioeconomic development trajectories and policy assumptions.*"

In the current study, we did not include land management variables, such as human interventions or forestry practices, in the ML model. The focus was primarily on climate and vegetation-related factors that are known to substantially influence wildfire occurrence. Given the availability of land-use datasets from historical periods to SSP future scenarios, such as those generated by LUH2 project[34], we performed an additional experiment to show the impact of including land management variables in our machine learning model and results. We incorporated LUH land-use data statistics (including areas of forested and non-forested primary land, managed pasture, rangeland, secondary biomass carbon density, urban land, etc.) as input parameters for training the ML model and projecting future BA. The results based on one of the ESMs are shown below in Figure 1. PRD_no-LUH and PRD_with-LUH represent predictions without and with considering land-use data, respectively. The global averages for the period 2095–2099 are 406 Mha and 418 Mha, respectively, showing a slight difference, though spatial variations exist. Additionally, SHAP analysis revealed some unexplained patterns, i.e., in the urban parameter, where the contribution of SHAP values remained constant despite an increasing urban area over time. We attribute this to the annual scale resolution of LUH data, which may not capture seasonal variations like in other input parameters. Additionally, the coarse grid resolution (2°×2.5°) may limit the ability to detect fine-scale land-use changes and their impacts on fire occurrence. Given the computational cost and the less affected results, we did not pursue further investigation into this. One of the reviewers suggested that these results might be attributed to the short historical training period, the model may not adequately capture long-term land use impacts on fire occurrence.

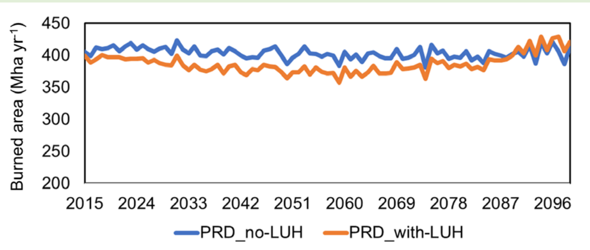


Figure 1. Projected future burned area with and without land management variables.

Overall, we acknowledge that human land management, including fire suppression, land-use changes, and forest management, can influence fire dynamics and introduce uncertainty into ML models based on past fire occurrences. We have highlighted this limitation in the manuscript and suggest that future research could explore integrating high-resolution land management data to enhance model predictions and better evaluate the interaction between human activities and fire dynamics. The relevant sentence in the ***Discussion*** has been added as follows: "*Our projections for BA are learned from past fire frequency and size, under the assumption that the level of human intervention observed over the past 15 years will persist in future scenarios. Due to the short training period constrained by the availability of historical burned area data, our ML model may not fully capture the long-term impacts of land use change and fire management—particularly suppression practices—on fire dynamics. While our study emphasizes the potential health risks of climate-driven increase of fire, the analytical framework can be extended in future work to account for land management and fire management influences as longer-term and higher-resolution datasets become available. While biomass estimates from ESMs account for land-use changes, variations in emission factors due to land-use changes were incorporated into our future fire emission calculations (Methods). Future work should explore methods to integrate land management data, improving the high-resolution estimates of land-use impacts on fire occurrences and enhancing our understanding of human-fire dynamics.*"

5. Lines 29-32: I would argue that the authors' results regarding the growing burden of disease is alone enough to underscore the need for coordinated efforts to address wildfire-related health threats. As it's currently written, it seems as if this call to action is only valid because health disparities will diminish. Suggest rephrasing.

Response:

As suggested, we have revised the corresponding sentence as follows: "*As the world warms, wildfire-related health burdens are projected to rise globally, underscoring the urgent need for coordinated efforts to address this growing global health threat. This burden will also become more evenly distributed across countries of different economic development levels, shifting the traditional patterns of health inequality.*"

6. Lines 221-223: Since the number of attributable deaths (1.51 and 1.87 million for the two scenarios) are higher than the total number found using the non-Connolly RR approach

(i.e., line 185ff), are these results found using the Connolly approach? Please clarify and make this distinction in the text.

Response:

We employed a non-Connolly RR approach. To clarify, we have revised the relevant sentence to: *"Our estimations based on undifferentiated ERF indicate that 1.51 (95% CI: 0.89–2.04) million and 1.87 (95% CI: 1.11–2.54) million $PM_{2.5}$-related premature deaths..."* Additionally, we added the following explanation in the same paragraph: *"In the SSP2-POP scenario, where climate and emission conditions remain constant, no substantial decrease in fire-contributed $PM_{2.5}$ concentrations is observed over Sub-Saharan Africa (Supplementary Fig. 7(b)). Consequently, fire-induced premature deaths for the 65+ age group are higher than under SSP2-4.5 (as shown by the brown bars in Figures 1 and 3), leading to higher total fire-induced premature mortality in SSP2-POP compared to SSP2-4.5."*

7. Lines 245ff: These phrases feel non-sequitur. The authors mention that fire emissions are expected to decline over Sub-Saharan Africa and then “these changes are associated with shifts in the age structure...”. In this case A != B and there could be emissions changes without a change in the population structure. Please revise. Maybe “…decline in fire emissions over this region. At the same time, there are shifts in the age structure…”

Response:

Thank you for the suggestion. We have revised the sentence as follows: "*At the same time, there are shifts in the age structure of the population in each region, particularly a substantial growth of the population aged 65+ between 2095–2099 compared to 2010–2014 (Supplementary Table 5).*"

8. Line 329: “*A* previous study”

Response:

We have revised the "Previous study" to "A previous study".

9. Lines 535 and Equation 4: What does the superscript 245/585 represent?

Response:

We have revised the corresponding sentence as follows: "*Where i and j represent the row and column of the simulation grid, t represents the month of year, and 245/585 refer to the SSP2-4.5 and SSP5-8.5 scenarios, respectively.*"

10. Lines 582-583: Calculated in real time sounds a little weird and more like something that would be used for a short-term air quality forecasting model. Maybe "were calculated online using MERRA-2 meteorological fields" would be better?

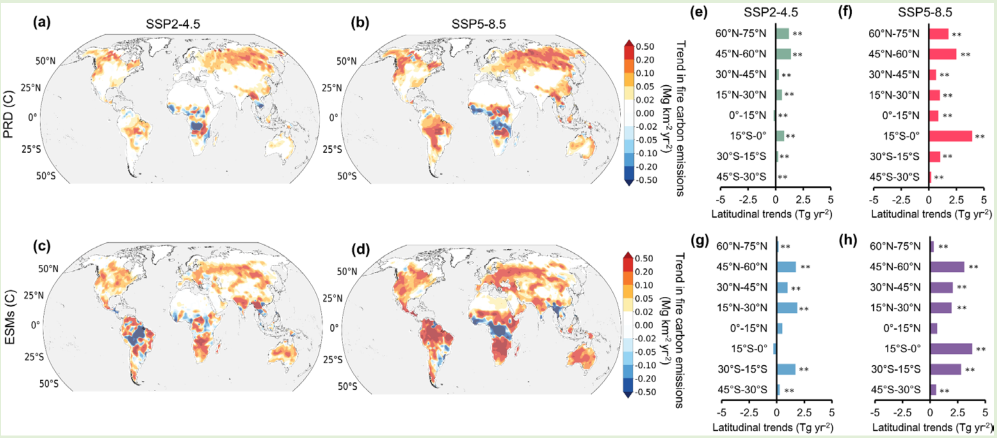
Response:

We have revised the corresponding sentence as follows: "*Emissions of dust, biogenic VOCs, sea salt, soil NO_x, and lightning NO_x were calculated online using the MERRA-2 meteorological field.*".

11. Line 155: Was significance assessed formally?

Response:

We have added the significance results to Supplementary Fig. 4 and revised the figure accordingly:



Supplementary Fig. 4 Spatial distributions of trends of fire carbon emissions during 2015-2099. (a), (b) and (c), (d) represent trends of fire carbon emissions in our projection and ESMs estimates under SSP2-4.5 and SSP5-8.5 scenarios, respectively. (e–h) depict the corresponding latitudinal trends. The trends in each grid cell are shown using the linear least squares fitting method based on annual time series. Statistically significant trends ($p < 0.05$) are indicated by asterisks (∗∗) in latitudinal trends.

References

1 Park, C. Y. *et al.* Future fire-PM2.5 mortality varies depending on climate and socioeconomic changes. *Environmental Research Letters* **19**, 024003 (2024). https://doi.org/10.1088/1748-9326/ad1b7d

2 Yu, Y. *et al.* Machine learning–based observation-constrained projections reveal elevated global socioeconomic risks from wildfire. *Nature Communications* **13**, 1250 (2022). https://doi.org/10.1038/s41467-022-28853-0

3 Rachel, C. *et al.* Mortality attributable to PM_{2.5} from wildland fires in California from 2008 to 2018. *Science Advances* **10**, eadl1252 (2024). https://doi.org/10.1126/sciadv.adl1252

4 Randerson, J. T. *et al.* The Impact of Boreal Forest Fire on Climate Warming. *Science* **314**, 1130-1132 (2006). https://doi.org/10.1126/science.1132075

5 Halofsky, J. E., Peterson, D. L. & Harvey, B. J. Changing wildfire, changing forests: the effects of climate change on fire regimes and vegetation in the Pacific Northwest, USA. *Fire Ecology* **16**, 4 (2020). https://doi.org/10.1186/s42408-019-0062-8

6 Rabin, S. S. *et al.* The Fire Modeling Intercomparison Project (FireMIP), phase 1: experimental and analytical protocols with detailed model descriptions. *Geosci. Model Dev.* **10**, 1175-1197 (2017). https://doi.org/10.5194/gmd-10-1175-2017

7 Park, C. Y. *et al.* Impact of climate and socioeconomic changes on fire carbon emissions in the future: Sustainable economic development might decrease future emissions. *Global Environmental Change* **80**, 102667 (2023). https://doi.org/https://doi.org/10.1016/j.gloenvcha.2023.102667

8 Nurrohman, R. K. *et al.* Future projections of Siberian wildfire and aerosol emissions. *Biogeosciences* **21**, 4195-4227 (2024). https://doi.org/10.5194/bg-21-4195-2024

9 Kinnunen, O., Backman, L., Aalto, J., Aalto, T. & Markkanen, T. Projected changes in forest fire season, the number of fires, and burnt area in Fennoscandia by 2100. *Biogeosciences* **21**, 4739-4763 (2024). https://doi.org/10.5194/bg-21-4739-2024

10 Dong, X. *et al.* Historical Simulation and Future Projection of Arctic-Boreal Fire Carbon Emissions and Related Surface Climate by 17 CMIP6 ESMs. *Journal of Geophysical Research: Atmospheres* **129**, e2024JD041806 (2024). https://doi.org/https://doi.org/10.1029/2024JD041806

11 Brey, S. J., Barnes, E. A., Pierce, J. R., Swann, A. L. S. & Fischer, E. V. Past Variance and Future Projections of the Environmental Conditions Driving Western U.S. Summertime Wildfire Burn Area. *Earth's Future* **9**, e2020EF001645 (2021). https://doi.org/https://doi.org/10.1029/2020EF001645

12 Kloster, S. & Lasslop, G. Historical and future fire occurrence (1850 to 2100) simulated in CMIP5 Earth System Models. *Global and Planetary Change* **150**, 58-69 (2017). https://doi.org/https://doi.org/10.1016/j.gloplacha.2016.12.017

13 Li, F. *et al.* Evaluation of global fire simulations in CMIP6 Earth system models. *Geosci. Model*

Dev. **17**, 8751-8771 (2024). https://doi.org/10.5194/gmd-17-8751-2024

14 Jain, P. *et al.* A review of machine learning applications in wildfire science and management. *Environmental Reviews* **28**, 478-505 (2020). https://doi.org/10.1139/er-2020-0019

15 Wang, S. S. C., Qian, Y., Leung, L. R. & Zhang, Y. Identifying Key Drivers of Wildfires in the Contiguous US Using Machine Learning and Game Theory Interpretation. *Earth's Future* **9**, e2020EF001910 (2021). https://doi.org/https://doi.org/10.1029/2020EF001910

16 Wang, S. S. C., Qian, Y., Leung, L. R. & Zhang, Y. Interpreting machine learning prediction of fire emissions and comparison with FireMIP process-based models. *Atmos. Chem. Phys.* **22**, 3445-3468 (2022). https://doi.org/10.5194/acp-22-3445-2022

17 Kondylatos, S. *et al.* Wildfire Danger Prediction and Understanding With Deep Learning. *Geophysical Research Letters* **49**, e2022GL099368 (2022). https://doi.org/https://doi.org/10.1029/2022GL099368

18 Buch, J., Williams, A. P., Juang, C. S., Hansen, W. D. & Gentine, P. SMLFire1.0: a stochastic machine learning (SML) model for wildfire activity in the western United States. *Geosci. Model Dev.* **16**, 3407-3433 (2023). https://doi.org/10.5194/gmd-16-3407-2023

19 Tang, R. *et al.* Quantifying wildfire drivers and predictability in boreal peatlands using a two-step error-correcting machine learning framework in TeFire v1.0. *Geosci. Model Dev.* **17**, 1525-1542 (2024). https://doi.org/10.5194/gmd-17-1525-2024

20 Zhu, Q. *et al.* Building a machine learning surrogate model for wildfire activities within a global Earth system model. *Geosci. Model Dev.* **15**, 1899-1911 (2022). https://doi.org/10.5194/gmd-15-1899-2022

21 Liu, Y. *et al.* ML4Fire-XGBv1.0: Improving North American wildfire prediction by integrating a machine-learning fire model in a land surface model. *Geosci. Model Dev. Discuss.* **2024**, 1-23 (2024). https://doi.org/10.5194/gmd-2024-151

22 Li, F. *et al.* AttentionFire_v1.0: interpretable machine learning fire model for burned-area predictions over tropics. *Geosci. Model Dev.* **16**, 869-884 (2023). https://doi.org/10.5194/gmd-16-869-2023

23 Zhang, G., Wang, M., Yang, B. & Liu, K. Current and Future Patterns of Global Wildfire Based on Deep Neural Networks. *Earth's Future* **12**, e2023EF004088 (2024). https://doi.org/https://doi.org/10.1029/2023EF004088

24 Guo, Z. *et al.* Reconstructed global monthly burned area maps from 1901 to 2020. *Earth Syst. Sci. Data Discuss.* **2025**, 1-28 (2025). https://doi.org/10.5194/essd-2024-556

25 Wang, S. S. C., Leung, L. R. & Qian, Y. Projection of Future Fire Emissions Over the Contiguous US Using Explainable Artificial Intelligence and CMIP6 Models. *Journal of Geophysical Research: Atmospheres* **128**, e2023JD039154 (2023). https://doi.org/https://doi.org/10.1029/2023JD039154

26 Knorr, W., Dentener, F., Lamarque, J. F., Jiang, L. & Arneth, A. Wildfire air pollution hazard during the 21st century. *Atmos. Chem. Phys.* **17**, 9223-9236 (2017). https://doi.org/10.5194/acp-17-9223-2017

27 Sarangi, C. *et al.* Projected increases in wildfires may challenge regulatory curtailment of PM2.5 over the eastern US by 2050. *Atmos. Chem. Phys.* **23**, 1769-1783 (2023). https://doi.org/10.5194/acp-23-1769-2023

28 Xie, Y. *et al.* Tripling of western US particulate pollution from wildfires in a warming climate. *Proceedings of the National Academy of Sciences* **119**, e2111372119 (2022). https://doi.org/10.1073/pnas.2111372119

29 Ford, B. *et al.* Future Fire Impacts on Smoke Concentrations, Visibility, and Health in the Contiguous United States. *GeoHealth* **2**, 229-247 (2018). https://doi.org/https://doi.org/10.1029/2018GH000144

30 Neumann, J. E. *et al.* Estimating PM2.5-related premature mortality and morbidity associated with future wildfire emissions in the western US. *Environmental Research Letters* **16**, 035019 (2021). https://doi.org/10.1088/1748-9326/abe82b

31 Borchers-Arriagada, N., Schulz-Antipa, P. & Conte-Grand, M. Future fire-smoke PM2.5 health burden under climate change in Paraguay. *Science of The Total Environment* **924**, 171356 (2024). https://doi.org/https://doi.org/10.1016/j.scitotenv.2024.171356

32 Lou, S. et al. Projections of mortality risk attributable to short-term exposure to landscape fire smoke in China, 2021-2100: a health impact assessment study. *The Lancet Planetary Health* **7**, e841-e849 (2023). https://doi.org/10.1016/S2542-5196(23)00192-4

33 Qiu, M. *et al.* Wildfire smoke exposure and mortality burden in the US under future climate change. (2024).

34 Hurtt, G. C. *et al.* Harmonization of global land use change and management for the period 850–2100 (LUH2) for CMIP6. *Geosci. Model Dev.* **13**, 5425-5464 (2020). https://doi.org/10.5194/gmd-13-5425-2020

35 Lawrence, D. M. *et al.* The Community Land Model Version 5: Description of New Features, Benchmarking, and Impact of Forcing Uncertainty. *Journal of Advances in Modeling Earth Systems* **11**, 4245-4287 (2019). https://doi.org/https://doi.org/10.1029/2018MS001583

36 Döscher, R. *et al.* The EC-Earth3 Earth system model for the Coupled Model Intercomparison Project 6. *Geosci. Model Dev.* **15**, 2973-3020 (2022). https://doi.org/10.5194/gmd-15-2973-2022

37 Hasegawa, T., Fujimori, S., Ito, A., Takahashi, K. & Masui, T. Global land-use allocation model linked to an integrated assessment model. *Science of The Total Environment* **580**, 787-796 (2017). https://doi.org/https://doi.org/10.1016/j.scitotenv.2016.12.025

Responses to reviewers' comments

We sincerely thank all reviewers for their thoughtful and constructive feedback on our manuscript entitled "***Global warming amplifies wildfire health burden and reshapes inequality***". We have carefully addressed all comments and revised the manuscript accordingly.

For clarity, our point-by-point responses to each reviewer's comment are provided below, with our replies highlighted in green-shaded text. Revisions made to the manuscript are clearly indicated: new additions appear in red text, and deletions are shown with strikethrough formatting for full transparency.

Response to Reviewer #3

1. The authors have responded adequately to my previous comments and concerns with one exception.

   The authors now clarified that they used output of 4 ESMs run with floating (not historical) meteorology to train the AI, and use the meteorological reanalysis ERA5 to test the results. I remain confused on the motivation to do this. While the 4 ESMs likely have realistic representations of present-day meteorology, they will not capture year-to-year variability correctly. It seems to me that should limit the abilities of the ML to diagnose the relationships between meteorological variables and BA. For example, one would expect that fires would occur during hot and dry conditions. In a year when a fire occurred, the ESMs would be as likely as not to identify this as a drier than normal year. The variability would be random. How then can the ML diagnose the right relationships? I don't understand why 4 ESMs would be used for this purpose rather than ERA5, which would get the variability right. I'd appreciate if the authors would clarify the reasons for this choice, and whether such an approach is used commonly in the literature. I'll also comment that this is presented only briefly in the supplement, without discussion.

Response:

We appreciate the reviewer's thoughtful comments and the opportunity to clarify our methodological choice. Our study specifically targets century-scale projections of wildfire occurrence and impacts, necessitating an internally consistent meteorological dataset that extends seamlessly from present-day conditions to future climate scenarios. The choice of CMIP6 Earth System Models (ESMs), despite their limitations in precisely reproducing present-day year-to-year meteorological variability observed in reanalysis datasets like ERA5, allows us to maintain methodological consistency and avoid the potential discontinuities introduced when merging present-day (ERA5) and future (ESM-projected) climate datasets.

To ensure the validity of this approach, we explicitly evaluated the performance of our machine learning (ML) model trained on ESM data for the present-day period (2000–2014).

The results demonstrated that our model effectively captures global and regional burned area trends and variability, as confirmed by comparisons with the observational satellite-based GFED 4.1s dataset (Supplementary Text 2).

Furthermore, to robustly assess the impact of the choice of meteorological data, we conducted an additional validation by independently training and driving our ML model using ERA5 data for 2000–2014. This ERA5-based ML reconstruction closely matched our ESM-based results (Supplementary Fig. 1), providing strong evidence that our ML approach accurately resolves the underlying relationships between meteorological variables and burned area. Thus, we are confident that our methodology is suitable for capturing climate-fire relationships essential for projecting future wildfire dynamics, especially targeting multi-year averages.

We acknowledge that this method choice was presented briefly in the supplement; accordingly, we have expanded the discussion in Supplementary Text 2 to clarify our reasoning, address this concern explicitly, and highlight supporting evidence from the ERA5 comparison.

We have added the following discussion to the Supplementary Text 2:

" *To avoid discontinuities between historical and future periods, we used a single, continuous set of climate forcings from ESMs for both ML training and projection. While this strategy may introduce model-specific biases, a parallel experiment driven by observed forcings shows only modest deviations, indicating that our main conclusions are not sensitive to the forcing choice.*"

2. I will also comment that I read the points of Reviewer 4 on the novelty of the paper and the long response by the authors. Since I do not work in this immediate field, it is hard for me to judge the novelty. While the long response clarifies the novelty, I will just comment that very little of this is written in the paper itself, and so it seems that the paper is missing a discussion of why it is novel. The paper is a well-done study, apart from my confusion on the use of ESMs to train the ML, and it seems that it can be a solid addition to this field. But the decision of whether or not it is sufficiently novel for publication in Nature lies with the editor, based on the authors' discussion of novelty.

Response:

We thank the reviewer for carefully reading our detailed response and recognizing our clarification of the research novelty. Given the length constraints of the main manuscript, we intend to make the comprehensive literature review provided in response to Reviewer 4 publicly available through Nature's transparent peer review process upon publication. This approach ensures interested readers can access additional context and insights supporting the novelty of our work.

Additionally, we have revised the Introduction to explicitly emphasize the key points underpinning the novelty of our study, as follows:

"*Prior research has yielded important insights into future wildfire trends*[1,2] *and their health consequences*[3-6]*, highlighting wildfires as an increasingly severe and persistent threat to air quality and public health. However, existing studies are predominantly region-specific and primarily focus on health impact estimation, employing simplified or externally generated emission projections. Few studies have comprehensively evaluated global future wildfire emission pathways, along with their associated health and climate consequences, under different warming scenarios. This gap limits our capacity to inform effective wildfire management policies amid uncertain warming futures.*

Here we developed an interpretable ML framework to project global wildfire emissions under two Shared Socioeconomic Pathway (SSP) scenarios[7] *—SSP2-4.5, representing moderate future warming aligned with continued current global development trends, and SSP5-8.5, representing high-emission, fossil fuel-intensive futures without effective mitigation policies. Our ML-based projection approach, trained on observed historical burned areas and sensitive to projected climate and vegetation changes, quantitatively elucidates the primary drivers shaping global wildfire emission trends. We then utilized these projected emissions in the global chemical transport model GEOS-Chem to assess wildfire impacts on $PM_{2.5}$ concentrations, $PM_{2.5}$-associated excess mortality, and $PM_{2.5}$ direct radiative forcing (DRF) based on a series of model simulations (Methods). Comparisons between historical periods* *and future scenarios highlight the increasing significance of wildfires in driving global $PM_{2.5}$ air pollution and related health and climate impacts, providing critical insights to inform global wildfire management and adaptation strategies.*"

3. l. 31 – Clarify that the deaths are related to fire smoke.

Response:

The relevant sentence has been revised as follows: "*Projections show a surge in premature deaths from wildfire smoke*,".

Response to Reviewer #4

1. Thanks to the authors for addressing my earlier comments. As I read through their responses, the revised manuscript, and supplement, several additional comments arose. For those that do not directly stem from changes they introduced in their revisions, I apologize for not catching these earlier. However, I believe that they should be clarified before this paper can be considered for publication as future readers may have similar questions.

Response:

We thank the reviewer for the thoughtful re-evaluation and additional comments. We understand the importance of clarifying these points for the benefit of future readers and have addressed all of them carefully in our revised manuscript.

2. The authors quote climate change only driven changes in fire emissions (23% under SSP2-4.5 and 53% under SSP5-8.5) in the abstract and main text (Lines 28-30, 108ff). However, I'm confused about whether these changes truly isolate climate change alone since, to the best of my understanding of their study, the biomass estimates are derived from ESMs that use a land use/land cover data that accounts for changes related to human activity (e.g., deforestation) and socioeconomics (Lines 497-499). Please clarify and adjust, if needed.

Response:

We appreciate the reviewer highlighting this important point. We acknowledge that our original phrasing, "climate change alone", could be misleading. Indeed, the projected changes in wildfire emissions are based on biomass and emission factors derived from Earth System Models (ESMs), which inherently include effects not only from climate change but also from land-use changes driven by human activities. Although our burned area projections did not explicitly incorporate direct land-use impacts on fire occurrence, indirect influences from land-use changes may still affect fire emission projections through their impacts on vegetation biomass and emission factors. To clarify this and avoid potential misinterpretation, we have revised the relevant sentence in the ***Abstract*** as: "*From 2010–2014 to 2095–2099, fire carbon emissions are projected to increase by 23% under SSP2-4.5 and by 53% under SSP5-8.5.*"

3. The authors' ML BA dataset was trained on data from GFED, along with other datasets such as the ERA-5 reanalysis (Lines 85-89, SI Text 1-2). But then the authors say that they conducted a performance assessment by comparing their ML predictions of BA with satellite observations from GFED (SI Text 2). First, what do the authors mean by *satellite observations* from GFED? GFED is an emissions inventory and not a satellite instrument. Second, isn't there some problematic circular logic if GFED is used as an input/in training and then the performance of the newly-created dataset is assessed also using GFED and they conclude that the model successfully captured the observed global BA changes (Lines 89-90)? Please clarify.

Response:

We thank the reviewer for raising these important points.

First, our evaluation compared ML-predicted burned area (BA) with the GFED4.1s dataset, which provides satellite-derived burned area data supplemented with small and missing fires based on the MODIS observations. To clarify this, we have revised the relevant sentence in the manuscript as follows: "*We conducted a performance assessment by comparing the ML predictions of BA with the satellite-derived burned area data used within the GFED4.1s framework for the period 2000–2014.*"

Second, regarding potential circular logic, this issue was also noted by Reviewer #1 in the previous review round. To robustly assess the predictive capability of our ML model, we performed annual out-of-sample predictions for the historical period of 2000–2014. Specifically, to predict BA for a given year, the model was trained using data from all other available years, excluding the target year. For instance, the 2014 BA prediction was based on training data from 2000–2013, while the 2013 BA prediction utilized training data from 2000–2012 and 2014, and so forth. This cross-validation strategy is thoroughly described in the final paragraph of ***Supplementary Text 1***: "*To evaluate the predictive performance of the ML model, we conducted annual out-of-sample predictions for historical data 2000 – 2014. For example, to predict 2014 BA, we used data from 2000–2013 for training; for 2013 BA, data from 2000–*

2012 and 2014 were used, and so on. Detailed evaluation results are provided in Supplementary Text 2. For future projections (2015–2099), we trained the ML model on historical data and employed ESM-projected future climate factors for prediction.".

4. One of the authors' calls to action is "expanding regulatory scopes" (Lines 271-272). This is a pretty vague statement, so I'd encourage more specificity. In particular, wildland fire smoke is much more difficult to regulate than emissions coming out of, say, a tail pipe or flue, so I'm curious what regulations they have in mind.

Response:

We agree that the original phrase "expanding regulatory scopes" was vague and could benefit from greater specificity, particularly given the complexity of regulating wildfire emissions compared to anthropogenic sources like vehicles or industrial facilities.

To address this, we have revised the corresponding paragraph in the manuscript as follows:

"*Essential strategies, including cross-boundary coordination, incorporation of wildfires into air quality regulatory frameworks, satellite-ground integrated real-time monitoring, and enhanced early warning and emergency response planning, are imperative to counteract the mounting impact of wildfire smoke on air quality.*"

5. There are many occasions in the paper where the text isn't clear or where there are typos that hinder an easy understanding of the text. It would enhance the readability and interpretability of this text if the authors could review their manuscript with this in mind. A few, but not all, that I jotted down include "Part regions" (line 167), "the coordinate space notation (coordinate)" in SI Table 2, "assumingthat" (line 283), "The future meteorologies" (line 557), and several places where the tense changes from past to present in an unexpected way.

Response:

We have thoroughly reviewed and revised both the main text and the Supplementary Information to improve grammar, fix typographical errors. Specifically, we have corrected the following:

Main text

"*This framework account…*" → " *This framework accounts…*"

" *The changes of this burden also become more evenly distributed…*" → "*The distribution of this burden also becomes more even…*"

"*Fire-induced fine particulate matter ($PM_{2.5}$) degrades global…*" → " *Fire-induced fine particulate matter ($PM_{2.5}$) has degraded global…*"

" *This surge is linked to the rise in BA and vegetation biomass over this latitude band…*" → " *This surge is linked to increases in both BA and vegetation biomass across this latitude band…*"

"*Global warming and the increased dry events…*" → " *Global warming and increased dry events…*"

" *which causes damage…*" → " *which cause damage…*"

" *enabling…*" → " *enables…*"

" *used as in GFEDv4.1s…*" → "*used as GFEDv4.1s…*"

" *ML model successfully captures…*" → " *ML model successfully captured…*"

" *wildfire emissionsover…*" → " *wildfire emissions over…*"

" *McDuffie et al*[39]*…*" → " *McDuffie et al.* [39]*…*"

" *a data-driven approach that account for…*" → " *a data-driven approach that accounts for…*"

"*exacerbate global warming and increaseing…*" → " *exacerbate global warming and increase…*"

"*Then the global monthly BA were then…*" → "*The global monthly BA were then…*"

"...BA value for a specific geographical location of the i-th grid cell at time period of t during training period " → "...BA value for a specific geographical location of the i-th grid cell at time t during the training period "

" During the training, supervised learning is used to..." → " During the training, supervised learning was used to..."

" …during prediction, the established relationship f is applied to..." → " …during prediction, the established relationship f was applied to..."

" This predicted BA data was then combined..." → " These predicted BA data were then combined..."

"...we used the change ratio of ESMs simulated vegetation biomass to..." → "...we used the change ratio of ESM-simulated..."

" We used the fire emissions projected in this study for the future scenarios simulation..." → " We used the fire emissions projected in this study for the future scenario simulations..."

"…and their induced DRF is calculated..." → "…and their induced DRF are calculated..."

"…we use the updated 2021..." → "…we used the updated 2021..."

"…with the estimated $PM_{2.5}$ exposure estimates." → "…with the estimated $PM_{2.5}$ exposure."

"…were calculated with following equation:" → "…were calculated with the following equation:"

"…, we use existing short-term" → "…, we used existing short-term"

"…, we use the global average value of 1.019" → "…, we used the global average value of 1.019"

" Country-specific β_RR values are then applied in equation (9)..." → " Country-specific β_RR values were then applied in equation (9)..."

Supplementary Information

" GridSearchCV function, a function in Scikit-learn…" → " GridSearchCV tool in Scikit-learn…"

" 10 wind speed (W10)" → " 10m wind speed (W10)"

" we employed data from the ERA-5 to…" → " we employed data from ERA-5 to…"

" The observed trend of increasing vegetation biomass and burnable area..." → " The observed trend of increasing vegetation biomass and burned area..."

" we employed the SHapley Additive exPlanation regression (SHAP)…" → " we employed the SHapley Additive exPlanation (SHAP)…"

" …has a positive beneficial impact..." → " …has a positive impact..."

" …refers to the prediction value of BA..." → " refers to the predicted value of BA..."

6. In Lines 289ff, the authors state that "while biomass estimates from ESMs account for land-use changes, variations in emission factors due to land-use changes were incorporated into our future fire emission calculations." Should there be a *not* in the second part of that sentence?

Response:

The future fire emissions were projected for total carbon first at the grid scale, and were then converted to the other air pollutant species (i.e., NO_x, CO, SO_2, NH_3, BC, OC, and NMVOCs) based on emission factor ratios. These emission factors, which vary across fire types, are linked to land-use categories (as detailed in van der Werf et al.[13]). Therefore, we incorporated land-use datasets specific to each future scenario during the conversion from carbon to pollutant emissions, which is an offline process and not part of the ML-based future projection of fire carbon emissions. To enhance clarity, we have rewritten the sentence as follows: "*While biomass estimates from ESMs that account for land-use changes were incorporated into our future fire emission projections (Methods), future work should further explore approaches to explicitly integrate land management data, improve high-resolution estimates of land-use impacts on fire occurrence, and enhance our understanding of human-fire dynamics.*"

7. The authors describe spin-up for the meteorological inputs that are then input to the GEOS-Chem model in Lines 559-561. By talking about 19 years of spin-up in the 2040s (which is a time period they didn't specifically analyze or model), I think there's some unnecessary confusion introduced since it was initially unclear that this spin-up was referring to the meteorological inputs. Personally, I'm not sure whether they even need to describe one of the inputs for GEOS-Chem with this level of detaiI if these inputs have a reference, and I would encourage them to rework this part of the text.

Response:

Following the reviewer's suggestion, we have revised the sentence to improve clarity and conciseness, and have cited the relevant reference. The updated sentences now read: "*The simulations were conducted with a horizontal resolution of 2° × 2.5° and 40 vertical layers. To ensure a chemistry–climate steady state for the GEOS-Chem model, initial conditions for future simulations were generated using a spin-up period when meteorological fields become available in the GCAP data archive, following the approach adopted in our previous study*[8]".

8. The caption of Figure 4 (Lines 353-354) contains materials and methods, specifically the source and version of country-specific GDP data, and I don't see these data referenced in the actual materials and methods part of their paper. I would not recommend that the authors place critical study materials and methods in a figure caption.

Response:

We have moved the description of the country-specific GDP data to the ***Methods*** section as follows: "*The country-specific GDP data used in the analysis of regional $PM_{2.5}$-related premature deaths and associated inequality were obtained from the SSP Public Database Version 2.0 (https://tntcat.iiasa.ac.at/SspDb/dsd?Action=htmlpage&page=welcome). For the historical period, GDP values for the year 2010 were used. For future scenarios (SSP2-4.5 and SSP5-8.5), projections for the year 2100 were applied.*". The caption of ***Figure 4*** has been revised accordingly to read: "*See Methods for description of country-specific GDP data.*".

9. The authors mention that they assess $PM_{2.5}$-attributable mortality for the "six leading

causes of death" (Lines 595-596). This is not an accurate statement, as these six endpoints are not the leading causes of premature death per se but are those for which the GBD has deemed there is sufficient evidence to link to long-term $PM_{2.5}$ exposure from their burden of proof methods. I encourage the authors to read some of the related papers from IHME/GBD authors on this topic and adjust the text accordingly.

Response:

Thank you for pointing this out. After reviewing the relevant publications from the GBD framework, we have revised the text to ensure accuracy. The updated sentence is as below: "*With these data and results, we estimated premature mortality from six causes of death included in the GBD attributable mortality framework—ischemic heart disease (IHD), stroke, chronic obstructive pulmonary disease (COPD), lung cancer (LC), lower respiratory infections (LRI), and type II diabetes (DM) —for which sufficient evidence supports a causal relationship with long-term $PM_{2.5}$ exposure.*"

10. As someone who is an advocate for FAIR principles and open science, the authors' statement that "source codes for the analysis of this study are available from the corresponding author upon reasonable request" doesn't cut it for me. Who is the arbiter of what constitutes a "reasonable request", and how does this statement jive with Nature's guidelines? https://www.nature.com/nature-portfolio/editorial-policies/reporting-standards#availability-of-data

Response:

We appreciate the reviewer's advocacy for FAIR principles and open science, and we fully agree that transparency and accessibility are essential for ensuring reproducibility. The core GEOS-Chem model code used in our atmospheric simulations is already publicly available (*https://doi.org/10.5281/zenodo.5500536*). In addition, we commit to making all relevant scripts and analysis code, including those related to the machine learning-based burned area and emission projections, as well as the health impact assessments, accessible via a public repository (*https://github.com/zunyir/wildfire_projection*) upon the acceptance of the paper.

Accordingly, we have revised the Code Availability statement as follows:

"*The GEOS-Chem v13.2.0 model code is available at https://doi.org/10.5281/zenodo.5500536. All other analysis codes used in this study will be made publicly available at https://github.com/zunyir/wildfire_projection upon acceptance of the paper for publication.*"

References

1 Dong, X. *et al.* Historical Simulation and Future Projection of Arctic-Boreal Fire Carbon Emissions and Related Surface Climate by 17 CMIP6 ESMs. *Journal of Geophysical Research: Atmospheres* **129**, e2024JD041806 (2024). https://doi.org/https://doi.org/10.1029/2024JD041806

2 Zhang, G., Wang, M., Yang, B. & Liu, K. Current and Future Patterns of Global Wildfire Based on Deep Neural Networks. *Earth's Future* **12**, e2023EF004088 (2024). https://doi.org/https://doi.org/10.1029/2023EF004088

3 Wang, S. S. C., Leung, L. R. & Qian, Y. Projection of Future Fire Emissions Over the Contiguous US Using Explainable Artificial Intelligence and CMIP6 Models. *Journal of Geophysical Research: Atmospheres* **128**, e2023JD039154 (2023). https://doi.org/https://doi.org/10.1029/2023JD039154

4 Xie, Y. *et al.* Tripling of western US particulate pollution from wildfires in a warming climate. *Proceedings of the National Academy of Sciences* **119**, e2111372119 (2022). https://doi.org/10.1073/pnas.2111372119

5 Lou, S. *et al.* Projections of mortality risk attributable to short-term exposure to landscape fire smoke in China, 2021-2100: a health impact assessment study. *The Lancet Planetary Health* **7**, e841-e849 (2023). https://doi.org/10.1016/S2542-5196(23)00192-4

6 Park, C. Y. *et al.* Future fire-$PM_{2.5}$ mortality varies depending on climate and socioeconomic changes. *Environmental Research Letters* **19**, 024003 (2024). https://doi.org/10.1088/1748-9326/ad1b7d

7 O'Neill, B. C. *et al.* The Scenario Model Intercomparison Project (ScenarioMIP) for CMIP6. *Geosci. Model Dev.* **9**, 3461-3482 (2016). https://doi.org/10.5194/gmd-9-3461-2016

8 Zhao, J. *et al.* Changes in global DMS production driven by increased CO_2 levels and its impact on radiative forcing. *npj Climate and Atmospheric Science* **7**, 18 (2024). https://doi.org/10.1038/s41612-024-00563-y

Review Report for Nature-2024-09-18281 “Global warming escalates wildfire smoke health burden while narrowing global inequality disparities”

Comments to Authors

The authors have developed a pipeline to evaluate global burned areas and wildfire emissions until the end of this century based on climatic factors and further quantify the premature deaths and direct radiative forcing associated with fire-induced particulate matter ($PM_{2.5}$) pollution during both the contemporary period (2010–2014) to century end (2095–2099). The project is very ambitious, combining multiple modeling strategies. The finding on narrowing global inequality disparities is new, but, to my understanding, it is subject to caveats as it solely focused on fire $PM_{2.5}$-related deaths.

While the project is innovative in combining multiple modeling approaches, a key vulnerability lies in the accuracy of these models. If any model is inaccurate, the errors could compound as results are passed through a sequence of interconnected models (as one’s inputs use the other’s outputs), leading to significant uncertainty in the final projections. Additionally, the premature death is restricted to fire $PM_{2.5}$-related deaths, which may not fully capture the whole picture of the health burden of wildfire. A natural follow-up question is how global wildfire carbon emissions influence climate conditions, and how these climatic changes, in turn, affect human health on a larger scale.

The following list includes my comments regarding the data, model, and methodology.

1. Given that a machine learning (ML)-based model was used, a key question is how accurate these models are. For example, what is the highest R^2 value the ML model achieved? Additionally, I am concerned that model performance may vary significantly across different regions.
2. I am unclear on which outcome variable was used to train the ML model. I assume the outcome is burned area (BA), but where exactly does the training data come from? The authors reference “satellite observation-based BA” as the outcome but isn't that data from GFEDv4.1s (BA)? If so, what is the rationale behind comparing PRD (BA) to GFEDv4.1s (BA) when the latter was already used for training? **This is a major concern.**
3. The authors mention using an “interpretable” ML framework but did not clarify what they mean by "interpretable" in this context.
4. The fire behavior triangle consists of weather, fuel, and topography. Were topographic variables, like elevation, included in the ML model?
5. I concern that the uncertainty of the results is hard to quantify, e.g., Projections indicate a surge in premature deaths attributed to wildfire smoke PM2.5 exposure, reaching 1.19 million between 2095 and 2099. But how likely the premature death is 1.1 or 1.5 million? Are 95% confidence intervals plausible?
6. The emissions considered include pollutants like NOx, CO, SO_2, NH_3, and VOCs, which are not necessarily $PM_{2.5}$. These pollutants can also have direct health impacts. I wonder why these health impacts were not accounted into the premature death calculations.

7. Does the cross-county/cross-region travel of $PM_{2.5}$ impacts the results, given $PM_{2.5}$ can travel 2000 km and did not respect the geographic boundaries? How is this accounted in the regional premature deaths attributed to PM2.5 exposure?

Minor comments:

1. The colors of Figure 2(a)(c)(e) seem to be directly related to the population size of each country, since the colors are directly related to mortality. Mortality rate may be more intuitive.
2. "fire-induced mortality on the y-axis of Figure 3 implies a much broader meaning compared to "fire-induced $PM_{2.5}$-related premature deaths."