Author Responses to Referees' Comments on **"Impacts of Droughts on Biomass Burning Emissions, Air Quality, and Public Health in the Amazon"** by Ng et al. (MS No.: egusphere-2026-1614)

We thank the referees for their constructive feedback. Our point-by-point responses are provided below. The referees' comments are shown in *italics*, and our responses are in plain text. Any new or modified text incorporated into the manuscript is highlighted in **bold**, with line numbers referring to the revised version. A copy of the revised manuscript with tracked changes is appended for easy reference.

https://gocuhk-my.sharepoint.com/:b:/g/personal/amostai_cuhk_edu_hk/IQAN_nNmxTpCRbpgzK71ECcbAS9URbZHs4DgRG9jAjQ8bT8?e=z7zg7Q

Response to Referee #1

This paper examines the public health implications of fire-contributed air pollution in the Amazon. This is an important and growing topic given the influence that fires can have on regional air pollution and links with deforestation and climate. With that said, I have some questions about how the analysis is set up and connections with the existing literature. My major comments are that the authors need to justify: (1) their selection of 2010-16 as their analysis period rather than examining more recent years, (2) the use of a satellite-derived dataset in the validation and health impact analysis in combination with the CTM results, and (3) the use of annual dose-response functions developed for all-source annual air pollution rather than specifically for fires. Some more specific comments are listed below.

> We thank the reviewer for the thoughtful and constructive comments, which help us improve the manuscript substantially. According to your comments, we have revised accordingly to address the reviewer's concerns.

Introduction

L82-106: I would suggest reorganize this material a bit for clarity. First you could distill the key literature on PM2.5 and O3 health effects from all source pollution, then what is known about fire-specific pollution (see link below for a global study but there are several others), and finally discuss the health burden from smoke pollution that has been estimated by prior studies in the Amazon. The authors do already cite prior studies in the introduction that have estimated this health burden for the region but clearly describing what is missing from these studies beyond the drought connection would be helpful to better motivate this analysis.

https://doi.org/10.1016/S0140-6736(24)02251-7

> We thank the reviewer for these helpful suggestions regarding the organization of the introduction. In the revised manuscript, we have restructured and expand the paragraphs as to first summarize the key epidemiological evidence on health effects of all-source $PM_{2.5}$ and O_3, then highlight recent work specifically on landscape-fire smoke–related health impacts at the global and regional scales, and finally synthesize the existing estimates of the health burden from smoke pollution in the Amazon region.
>
> Line 83-114 "The impacts of **ambient air pollutants** on regional public health due to worsening air quality are a substantial concern (Kelly & Fussell, 2015; Pfleger et al., 2023). $PM_{2.5}$ and ground-level ozone are major concerns due to their pervasiveness and harmful health effects (Sun & Zhou, 2017; Yang & Omaye, 2009). Long-term exposure to $PM_{2.5}$ has been associated with a wide range of health issues. Epidemiological studies have demonstrated a positive relationship between ambient $PM_{2.5}$ concentration and the frequency

and severity of respiratory ailments, including non-communicable respiratory diseases (NCD) and lower respiratory infections (LRI) (Liang et al., 2020; Pope et al., 2018; Puett Robin et al., 2011; Wong Chit et al., 2015). For ground-level ozone, previous studies found that long-term exposure to ozone causes asthma (Zu et al., 2018), chronic obstructive pulmonary disease (Seltzer et al., 2018) and premature cardiovascular mortality (Jerrett et al., 2013; Turner et al., 2016). **There are more concerns on fire-specific air pollution in recent years, with growing evidence that $PM_{2.5}$ and smoke from landscape fires (including wildfires, deforestation fires, and agricultural fires) substantially increase mortality and cardiorespiratory morbidity worldwide (Rizzo & Rizzo, 2025; Xu et al., 2024). Time-series and multi-country epidemiological studies show that short-term increases in wildfire $PM_{2.5}$ are associated with significant rises in all-cause, cardiovascular, and respiratory mortality, and that the toxicity of fire-derived particles may exceed that of $PM_{2.5}$ from other sources (Lei et al., 2024).**

In order to estimate the public health impacts caused by the long-term exposure to $PM_{2.5}$ and ozone, various models and exposure functions were developed. Burnett et al. (2018) developed the Global Exposure Mortality Model (GEMM), which was used to evaluate premature mortality from NCD and LRI associated with $PM_{2.5}$ exposure in different age groups. Using the GEMM model, Butt et al. (2020) estimated that around 9,800 premature deaths in the Amazon basin from August to October 2012 were linked to forest fires, as observed $PM_{2.5}$ concentrations increased from 2 to 30–50 **$\mu g\ m^{-3}$** during the burning season in the southwestern Amazon region. Vohra et al. (2021) also applied the GEMM model and found approximately 8.9 million deaths worldwide were attributed to long-term exposure to $PM_{2.5}$ from all sources in 2015. **Additionally**, Jerrett et al. (2009) correlated the American Cancer Society Cancer Prevention Study II with air pollution data to estimate the contribution of long-term ozone exposure to the premature mortality risk from respiratory causes. A larger-scale epidemiological study involving more participants over a longer period, conducted by Turner et al. (2016), reaffirmed the premature mortality risk from respiratory causes and examined the risk from cardiovascular disease related to long-term ozone exposure. **A recent global assessment estimated that exposure to wildfire-related $PM_{2.5}$ is responsible for more than one million deaths annually, with the vast majority occurring in low- and middle-income countries, highlighting its disproportionate burden on vulnerable regions that contributes to environmental injustice (Xu et al., 2024).** These studies demonstrated the significant increases in premature mortality with enhanced $PM_{2.5}$ and ozone concentration. Consequently, under climate change, the intensified wildfires in the Amazon are expected to exacerbate public health impacts in Brazil with enhanced $PM_{2.5}$ and ozone concentrations."

L118: The authors need to set up here why this time period was selected rather than including more recent years. In the methods you discuss needing stable deforestation rates but I would like to understand more why anthropogenic deforestation and drought couldn't both be included in the analysis so that the interaction is also examined. Even if deforestation area is relatively constant over the period, could the spatial patterns be shifting and exposing different cities?

We thank the reviewer for raising this important point regarding our choice of study period and the treatment of deforestation versus drought. We have now clarified our rationale in the revised manuscript. Specifically, we focus on 2010–2015 because, at the time of conducting the simulations, the built-in GFEDv4s fire emission dataset in our modelling framework was reliably available only up to the late 2010s, and this period already encompasses multiple major fire seasons, including the pronounced drought years of 2010 and 2015, which rank among the highest-fire-emission years of the past decade (refer to annual fire emission in South America from GFEDv4). Moreover, the Brazilian Amazon deforestation rate during 2010–2015 was relatively low and stable compared with earlier and subsequent periods, which provides a practical experimental design to more cleanly isolate the influence of

drought-driven variability on fire emissions, air quality, and health impacts, without the confounding effect of large interannual swings or trends in deforestation extent. Our primary objective in this study is therefore to quantify the individual effect of drought on smoke-related air quality and public health under a quasi-constant deforestation background. We have added in the Methods section clarifying our rationale for selecting the 2010–2015 study period and for focusing on drought-driven variability under relatively stable deforestation rates.

L206-218 "Two sets of simulations ran from January 2009 to December 2015, with 2009 used as model spin-up and 2010-2015 retained for analysis. **This study period was chosen because it encompasses multiple intense fire seasons, including the major drought years 2010 and 2015, which are among the highest fire-emission years in the past decade. A comparison of regional DMB from the full GFEDv4s record (1997–2019) indicates that 2010 and 2015 are among the highest-emission years in the time series, while 2011–2014 spans lower to intermediate fire activities, suggesting that the 2010–2015 window samples both typical and extreme dry-season fire variability in the region (Fig. S1). Additionally,** deforestation rate remains low and stable during the experimental period (Fig. 1a), **providing a quasi-constant anthropogenic land-use background that allows us to more cleanly isolate the influence of drought-driven variability on fire emissions, air quality, and public health impacts by** comparing drought years (2010, 2012, 2014 and 2015) and non-drought years (2011 and 2013). **Within this window, we could capture substantial interannual variability in drought conditions while avoiding the confounding effects of large swings in deforestation extent, which would complicate attribution of observed changes in air pollutant exposure and health burden. This design enables us to quantify the individual effects of droughts on fire-related air quality and public health under a relatively stable deforestation regime.**"

We agree that explicitly examining the interaction between anthropogenic deforestation dynamics and drought, including potential shifts in the spatial pattern of clearing and associated smoke exposure for downwind cities, is an important next step. We now note this explicitly in the Discussion as a priority for future work, which would benefit from extended fire and land-use datasets beyond our current modelling configuration and from targeted experiments that vary both deforestation and drought conditions simultaneously.

L558-564 "**Another important limitation of our design is that we did not explicitly explore the interactions between anthropogenic deforestation dynamics and droughts. While the 2010–2015 period offers the advantage of relatively stable deforestation rates for isolating drought impacts, changes in the spatial pattern of clearing and associated fires could still alter which urban and rural populations are most exposed to smoke. Fully characterizing these interactions would require extended land-use and fire datasets beyond our current configuration, together with targeted modeling experiments that systematically vary both deforestation and drought conditions. We therefore view the joint assessment of deforestation–drought interactions, including shifting exposure patterns for specific cities, as a key priority for future work building on the present analysis.**"

Methods

L141: Why was the new GFED version not used here? Figure 5 in van der Werf et al. (2025) suggests rather substantial regional differences in fire emissions. I appreciate that it's a substantial amount of work to run these simulations, but the authors should at least include a justification for using an older version of the inventory given that a newer version is available if they are unable to update the analysis.

https://www.nature.com/articles/s41597-025-06127-w

We thank the reviewer for raising this important point. At present, our GEOS-Chem configuration natively supports GFEDv4s as the standard fire emissions inventory, and implementation of GFEDv5 into the model is still under active development by the GEOS-Chem community, with a multi-year timeline anticipated before a fully tested and documented integration is available (as stated in the following link). As a result, we are not yet able to use GFEDv5 in a way that is consistent, stable, and reproducible within the current modeling framework.

L547-556 "Previous studies found that the combination of net land use flux estimated from the Bookkeeping of Land Use Emissions (BLUE) and the net wildfire flux from a fire bookkeeping model (FATE) show similar interannual variability as GFEDv4s but the average flux from BLUE+FATE is 116% higher than GFEDv4s (Rosan et al., 2024). **A newer GFED version (GFEDv5) is now available and suggests notable regional differences in fire emissions relative to GFEDv4s. However, our GCHP configuration currently supports GFEDv4s as the standard inventory, and a fully tested, documented implementation of GFEDv5 within GEOS-Chem is still under development.** Future studies can incorporate the **updated** global fire emission inventory with a regional monitoring system, such as TerraBrasilis, to improve representation on natural and deforestation fires (F. G. Assis et al., 2019), and couple the drought impacts on biosphere processes into coupled climate-chemistry models to further investigate the potential impacts of sea surface temperature anomalies on drought occurrence, fire emission and air quality."

https://wiki.seas.harvard.edu/geos-chem/index.php?title=GEOS-Chem_model_development_priorities

L170-172: Figure 1 a shows an uptick in deforestation in more recent years. The authors should clearly articulate why this period wasn't included (see prior comment).

We thank the reviewer for this comment. We have revised the Methods section to clarify our choice of study period (stated in prior response).

L203: Why wasn't daily fire emissions data used? Monthly data can obscure peak exposures and GEOS-Chem is running at a subdaily timestep.

We appreciate the reviewer's insightful comment regarding the temporal resolution of fire emissions. Our choice of monthly GFEDv4s emissions reflects a balance between our scientific objectives and available computational resources. In our current configuration, each simulated month already requires on the order of 24 hours of runtime using 30 cores, with input/output of emission fields representing a major share of the computational cost. Using daily or sub-daily fire emissions would substantially increase the volume of data read and written at each timestep and thus the overall computational burden, making the full 2009–2015 simulation period impractical within our resource constraints. Moreover, the primary focus of this study is on interannual and seasonal-scale differences in fire emissions and their impacts on air quality and health between drought and non-drought years, rather than on resolving short-term peak exposure episodes. For these reasons, we employ monthly mean fire emissions, which are adequate for capturing the contrasts in seasonal and interannual smoke pollution relevant to our objectives. We now clarify this rationale in the Methods.

L220-227 "The first set of simulations ("fire-on") utilized the GFEDv4s (Randerson et al., 2018) monthly DMB data in 0.25° to represent global fire emissions (Fig. 1c), while the second set of simulations ("fire-off") were the control with all global biomass burning emissions turned off. **We use monthly mean fire emissions rather than daily fields**

because our primary focus is on seasonal and interannual differences between drought and non-drought years, and running the full 2009–2015 period with higher-frequency emissions would substantially increase computational cost due to the large input/output burden for emission fields. Within the 2010–2015 study period, the DMB recorded by GFEDv4s peaks in every August or September in each year, and the peak magnitudes in identified drought years (2010, 2012, 2014 and 2015) are generally substantially higher than that in the two non-drought years (2011 and 2013), with 2010 showing the largest peak among all years."

L219-220: Please justify why this satellite PM2.5 dataset was used rather than sticking with the GEOS-Chem data. The satellite PM2.5 product is available at a finer resolution, so was it regridded to match the GEOS-Chem output? Can you give validation statistics?

As discussed in Section 3.1, our GEOS-Chem configuration systematically underestimates surface $PM_{2.5}$ compared with satellite-derived estimates over the study region, particularly during non-drought years. To obtain more realistic absolute $PM_{2.5}$ levels for the health-impact calculations, we therefore relied on the satellite $PM_{2.5}$ product as our baseline concentration field and used the GEOS-Chem simulations to provide the relative fire contribution. Concretely, we computed the ratio between the "fire-on" and "fire-off" simulations and apply this grid-specific scaling factor to the satellite-derived $PM_{2.5}$ to approximate the counterfactual and fire-impacted concentrations used in the GEMM-based mortality estimates. The satellite dataset, originally available at finer spatial resolution, is regridded to match the GEOS-Chem output grid before applying this scaling. Validation statistics for the $PM_{2.5}$ fields and model biases, including seasonal performance and correlation with the satellite product, are provided in the Supplementary Material (Fig. S5).

In order to compare the results between using GCHP output and satellite data, we extend and show the related plots in Supplementary Materials (Fig. S2).

Supplementary Materials

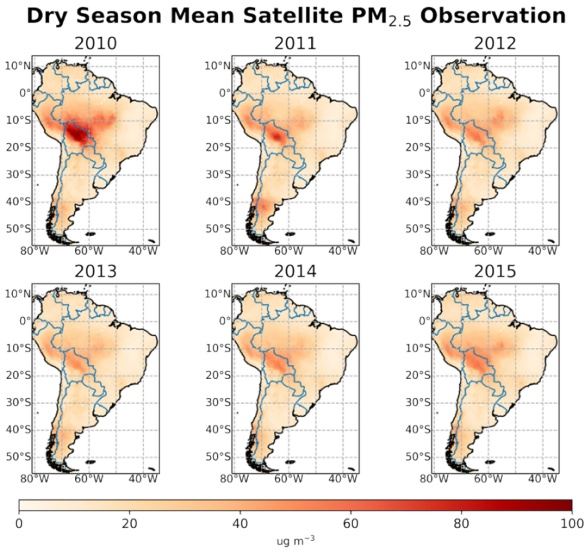


Figure S2. Dry season mean $PM_{2.5}$ Observation derived from satellite."

L225: Recent research has suggested the use of fire specific dose response functions rather than those developed for general air pollution. Can you include a justification here for this approach.

We appreciate the reviewer's helpful comment. Recent work has adopted diverse approaches to concentration–response functions for smoke, with some using general all-source ambient $PM_{2.5}$ and ozone functions and others exploring fire-specific relationships. In our study, however, the exposure fields used in the mortality calculations include both fire emissions and other anthropogenic and natural sources, because in the fire-off simulations non-fire sources (e.g. fossil fuel and biogenic emissions) remain present. For internal consistency, we therefore applied the same general long-term concentration–response functions (GEMM for $PM_{2.5}$ and established ozone functions) to both the fire-on and fire-off exposure fields and then attribute the difference in estimated premature deaths to the incremental fire contribution. Applying a fire-specific function only to the fire-on case while using a general function for the fire-off (counterfactual) case would mix different risk models within a single attribution framework and complicate interpretation of the resulting fire-attributable burden. On this basis, our choice to use general functions for both fire-on and fire-off exposures yields internally consistent and likely conservative estimates of fire-related health impacts, given that new fire-specific risk coefficients often suggest higher per-μg risks.

L230: If I'm understanding correctly, the counterfactual PM2.5 is basically assuming that the satellite PM2.5 is correct and trying to back out the non-fire fraction. But then in line 231 you say that the counterfactual is 2.4 ug/m3. Can you articulate more clearly how the counterfactual and GEOS-Chem runs were incorporated into the PM2.5 equation? Another approach could be to estimate the fire-specific PM2.5 component(fires-on - fires-off from GEOS-Chem) to use as the exposure variable.

We apologize for the lack of clarify in the original manuscript. Recent applications of the Global Exposure Mortality Model (GEMM) adopt 2.4 μg m^{-3} as the counterfactual $PM_{2.5}$ concentration, defined as the lowest long-term mean concentration observed across the 41 cohorts used to derive the exposure–response relationship, below which no additional change in hazard ratio is assumed (Burnett et al., 2018). In our analysis, this counterfactual was therefore prescribed by the GEMM framework and is not intended to represent a "fires-off" $PM_{2.5}$ level from our simulations. Instead, we used the GEOS-Chem fire-on and fire-off runs to quantify the fire-attributable fraction of total $PM_{2.5}$ and applied this as a scaling factor to the satellite-derived $PM_{2.5}$ field, which we adopted as the baseline concentration due to the systematic underestimation of surface $PM_{2.5}$ in our model. Concretely, we (i) regridded the satellite $PM_{2.5}$ product to the model grid, (ii) computed, for each grid cell and month, the ratio between the fire-on and fire-off GEOS-Chem simulations, and (iii) used this ratio to construct paired fire-on and fire-off $PM_{2.5}$ exposure fields from the satellite product. These paired exposure fields were then input to GEMM, with 2.4 μg m^{-3} as the counterfactual concentration, to estimate premature deaths in the fire-on and fire-off cases; the difference between them is interpreted as the mortality attributable to fire emissions. We now clarify this procedure in the Methods and explicitly distinguish the GEMM counterfactual (2.4 μg m^{-3}) from the modeled "fires-off" scenario.

L240-248 "To **obtain realistic absolute $PM_{2.5}$ levels while retaining the model-derived fire signals**, we **regridded the** satellite-derived monthly $PM_{2.5}$ concentrations (V5.GL.04), provided by the Atmospheric Composition Analysis Group of the Washington University in St. Louis (Shen et al., 2024) **and adopted it as the baseline exposure field for health impact analysis (Fig. S2). We then coupled it with** a monthly grid-specific correction factor derived from the **ratio between fire-on and fire-off simulated surface $PM_{2.5}$ to construct a corresponding "no fire $PM_{2.5}$" exposure field for the baseline exposure field, the** field is given by:

$$\textbf{No fire } \mathbf{PM_{2.5}} = \frac{\text{GCHP fire off PM}_{2.5}}{\text{GCHP fire on PM}_{2.5}} \times \text{satellite PM}_{2.5}, \quad (1)$$

The GEMM NCD+LRI model estimates $PM_{2.5}$-induced premature deaths using age-specific and sex-specific hazard ratios, baseline mortality and population data (Burnett et al., 2018). **Applying the two exposure fields,** the hazard ratio (HR) is given by:"

L252-254 "… 2.4 μg m^{-3} as the counterfactual $PM_{2.5}$ concentration described by Burnett et al. (2018)**, corresponding to the lowest $PM_{2.5}$ observed across the 41 cohorts, which is prescribed by GEMM.** The number of $PM_{2.5}$-induced premature death (M) is given by:"

L264-265 "**Finally, the difference in estimated premature deaths between two cases was interpreted as the mortality attributable to fire-emitted $PM_{2.5}$.**"

Burnett, Richard, Hong Chen, Mieczysław Szyszkowicz, Neal Fann, Bryan Hubbell, C. Arden Pope III, Joshua S. Apte et al. "Global estimates of mortality associated with long-term exposure to outdoor fine particulate matter." Proceedings of the National Academy of Sciences 115, no. 38 (2018): 9592-9597.

L230: Was this performed at the monthly or annual scale? Is the dose response relationship for specific exposure intervals?

We calculated $PM_{2.5}$-attributable mortality at the monthly scale using monthly surface $PM_{2.5}$ concentrations and annual baseline mortality rates from the Global Burden of Disease. Specifically, we first applied GEMM to each grid cell and month, then summed over all grid cells to obtain regional monthly premature deaths and finally averaged these 12 monthly values to derive annual mean mortality. The GEMM NCD+LRI function is applied to long-term (chronic) outdoor $PM_{2.5}$ exposure and was developed for an exposure range of approximately 15–84 μg m^{-3}, within which our $PM_{2.5}$ fields largely fall (Burnett et al., 2018).

Burnett, Richard, Hong Chen, Mieczysław Szyszkowicz, Neal Fann, Bryan Hubbell, C. Arden Pope III, Joshua S. Apte et al. "Global estimates of mortality associated with long-term exposure to outdoor fine particulate matter." Proceedings of the National Academy of Sciences 115, no. 38 (2018): 9592-9597.

L260-262: How were these O3 counterfactuals determined? Should they represent the no-fire concentration?

Regarding the ozone counterfactual concentrations, we followed the values prescribed by the epidemiological studies that underlie the exposure–response functions, rather than using the modeled 'no-fire' ozone as the counterfactual. These counterfactuals are defined in the original studies (Jerrett et al., 2009; Turner et al., 2016) to shape the concentration–response relationship and represent a threshold below which no additional risk is assumed. In our analysis, we applied the same exposure–response functions and their associated counterfactual concentrations consistently to both the fire-on and fire-off simulations, and then attributed the difference in estimated premature deaths between these two cases to fire emissions. We now clarify this procedure in the Methods and explicitly distinguish the prescribed epidemiological counterfactual from the modelled no-fire ozone concentrations.

L284-288 "… counterfactual concentration 26.7 ppb. **These ozone counterfactual concentrations were taken directly from the underlying epidemiological studies and were used to define the shape of the concentration–response function, rather than to represent a modeled "no-fire" ozone level. We applied these prescribed counterfactuals and exposure–response functions consistently to both the fire-on and fire-off simulations, and attributed the difference in resulting premature deaths to the contribution of fire emissions.**"

Results

L281: Did the authors validate the PM2.5 simulations with the same satellite data that you used to determine the counterfactual? My understanding is that the Shen et al. data also uses GEOS-Chem as an input to the satellite-derived model, which wouldn't make it an independent dataset. Did the authors consider using station data to validate as with the O3 data?

We appreciate the reviewer's helpful comment. We did validate our $PM_{2.5}$ simulations against the same satellite-derived $PM_{2.5}$ product that we later use in the health impact calculations, so this evaluation is not fully independent in the strict sense that the satellite product itself incorporates GEOS-Chem as one input. However, the satellite dataset also blends multiple satellite aerosol optical depth retrievals with ground-based AERONET sun-photometer observations, which are the same underlying ground network used to constrain the aerosol–$PM_{2.5}$ relationship and explain most of the variance in measured surface $PM_{2.5}$. In this context, the satellite product provides a spatially and temporally extensive benchmark that is much more representative of regional conditions than the very limited number of surface $PM_{2.5}$ monitors currently available in our study domain. In contrast, for ozone there is no analogous regional gridded product, so we relied on the available station data for model evaluation. We now clarify in Section 3.1.

L315-323 "We also validated the simulated surface $PM_{2.5}$ concentrations by comparing them with satellite-derived surface $PM_{2.5}$, which is from a global monthly mean dataset with 1° spatial resolution (named as V5.GL.04), provided by the Atmospheric Composition Analysis Group of the Washington University in St. Louis (Shen et al., 2024). **This dataset combines aerosol optical depth from multiple satellite instruments with GEOS-Chem simulations and ground-based AERONET sun-photometer observations to estimate surface $PM_{2.5}$. Because GEOS-Chem contributes to the construction of this product, the $PM_{2.5}$ evaluation is not fully independent in a strict sense; however, the inclusion of AERONET and multiple satellite retrievals provides an observationally constrained, regionally representative benchmark that is more informative than the very sparse in situ $PM_{2.5}$ monitoring network currently available in the Amazon.** We compared the performance between dry (Jul-Nov) and wet (Jan-May) season for every year, including both drought and non-drought years (Fig. S**5**)."

L305: This assumes that the satellite-derived PM2.5 is more accurate in the region. Can you provide validation statistics from the Shen et al. datasets specific for this time period and region?

There is, to our knowledge, no dedicated validation of the Shen et al. $PM_{2.5}$ dataset focused specifically on our study period and the Brazilian Amazon. The published evaluation provides global performance statistics, including for South America as a whole, but not a separate analysis tailored to our exact domain and years. In our case, long-term surface $PM_{2.5}$ monitoring in the Brazilian Amazon is extremely sparse, which limits the possibility of constructing an independent, region-specific validation for this period. At the same time, our own comparison indicates that GEOS-Chem systematically underestimates $PM_{2.5}$ in the region, reflecting biases from GFEDv4s and/or the aerosol scheme, so using the raw model concentrations directly would likely lead to underestimation of the health burden. We therefore use the model primarily to diagnose the relative fire-induced fraction (via the fire-on vs. fire-off ratio) and apply this ratio to the Shen et al. satellite–ground fused product, which has been globally validated, to obtain more realistic absolute $PM_{2.5}$ levels. This hybrid approach explicitly acknowledges the limitations of both the monitoring network and the model, and it implies that our resulting fire-attributable health impacts should be interpreted as conservative, given the underlying model underestimation and the remaining uncertainties in the satellite product for this specific region and period. We now note this explicitly in the Discussion.

L566-578 "**In addition, there are limitations in our representation and evaluation of $PM_{2.5}$ and ozone concentrations for health impact assessment. Our configuration tends to underestimate surface $PM_{2.5}$ over the study region, reflecting biases in GFEDv4s fire emissions and/or aerosol processes, so we used the simulations primarily to diagnose the relative fire-induced fraction and applied this to a satellite–ground fused $PM_{2.5}$ product rather than relying on raw model concentrations. The satellite dataset has been globally and regionally validated but is not fully independent of the model and lacks a dedicated validation focused on our exact domain and period, while surface $PM_{2.5}$ monitoring in the Brazilian Amazon remains sparse. Overall, the $PM_{2.5}$ concentrations and the number of premature deaths is slightly higher when using the satellite-derived data (Fig. S8, S9). Similarly, for health impact calculations we employed general long-term exposure–response functions for all-source ambient $PM_{2.5}$ and ozone (e.g. GEMM and established ozone functions) rather than emerging fire-specific functions, to ensure internal consistency between fire-on and fire-off scenarios and comparability with broader burden-of-disease assessments. Given evidence that model $PM_{2.5}$ is biased low and that wildfire $PM_{2.5}$ may be at least as toxic as average ambient $PM_{2.5}$, these choices imply that our estimates of fire-attributable mortality should be interpreted as conservative, with true smoke-related health burdens potentially larger than reported here.**"

L309: Why are CO and PM2.5 lumped together in this section? Were CO concentrations validated?

We thank the reviewer for this helpful comment and the opportunity to clarify our rationale. In this section, we present CO and $PM_{2.5}$ together because both are primary combustion products from biomass burning, respond similarly to drought-enhanced fire activity, and thus jointly illustrate the fire-driven degradation of air quality in the region. CO is also an important air pollutant and a useful tracer of combustion, but compared to $PM_{2.5}$ it has a more limited direct health impact in the long-term mortality context we quantify, which is why it is not included in the subsequent health impact calculations. For this reason, we used CO alongside $PM_{2.5}$ primarily to characterize the atmospheric response to fires and droughts, while $PM_{2.5}$ and ozone are carried forward into the exposure–response and mortality analysis. We did not perform a dedicated validation of CO against ground-based observations analogous to our ozone and $PM_{2.5}$ evaluations. The CO results are therefore interpreted in a process-based, relative sense (fire-on vs. fire-off and drought vs. non-drought), rather than as independently validated absolute concentrations.

L407: Are the health effects of PM2.5 on O3 considered to be independent?

Yes, the estimated premature deaths attributable to $PM_{2.5}$ and ozone are calculated separately using distinct exposure–response functions and baseline mortality rates in our analysis, which implicitly assumes that their health effects are independent. This approach is standard practice in large-scale burden-of-disease and air quality health impact assessments. The underlying epidemiological cohort studies used to derive these functions typically adjust for the presence of the other pollutant in their risk models. This statistical adjustment supports treating $PM_{2.5}$ and ozone as having largely independent effects on mortality. However, we acknowledge that in reality, exposures to these pollutants are highly correlated in fire plumes, and there could be unmeasured synergistic effects or minor double-counting when summing their respective burdens. We now discuss this in the Methods to clarify that $PM_{2.5}$ and ozone impacts are calculated independently, and a note in the Discussion regarding this assumption.

L290-292 "**Because the epidemiological studies underlying our exposure–response functions control for co-pollutants, we calculated the premature deaths attributable to $PM_{2.5}$ and ozone independently and summed them to represent the total fire-induced health burden.**"

L578-581 "**Finally, while we calculated the health impacts of $PM_{2.5}$ and ozone independently based on established multi-pollutant risk models, exposures to these pollutants are highly correlated in fire plumes. This approach assumes additive risks and may not fully capture potential synergistic effects or minor double-counting when assessing the combined health burden.**"

Discussion

-I would also like to see a discussion of the influence of different toxicity and temporal patterns of fire-specific PM2.5 compared to non-fire PM2.5 and how this would influence the results of this study. In addition, this study is applying dose response curves and long-term exposure metrics that were not developed specifically for fires. How could this influence the results?

We thank the reviewer for this excellent suggestion. We agree that the unique toxicity and episodic nature of fire emissions are critical factors that influence health impact assessments. Because fire-specific $PM_{2.5}$ often contains higher concentrations of toxic organic compounds and exhibits greater oxidative potential than typical urban $PM_{2.5}$, applying dose–response curves developed for general, all-source air pollution likely underestimates the true health burden of fire smoke. Therefore, our results should be viewed as conservative, lower-bound estimates.

Furthermore, fire emissions exhibit distinct temporal patterns, characterized by extreme, episodic spikes during the dry season (and exacerbated by droughts as what we found in this work). The long-term exposure metrics and dose–response functions we used are based on chronic, year-round ambient exposures. Applying these long-term metrics to episodic fire smoke averages out the acute concentration peaks, meaning our methodology captures the chronic baseline shifts but likely misses additional acute cardiovascular and respiratory mortality triggered by short-term, extreme smoke episodes. We have now added a dedicated paragraph to the Discussion section to explicitly address how the distinct toxicity and temporal patterns of fire-specific $PM_{2.5}$ influence our findings.

L583-594 "**An additional source of uncertainty arises from the unique chemical properties and temporal dynamics of fire emissions. Emerging toxicological and epidemiological evidence suggests that fire-specific $PM_{2.5}$ may be more harmful to human health per unit mass than general ambient $PM_{2.5}$ because it is highly enriched in carbonaceous species, including toxic organic compounds such as polycyclic aromatic hydrocarbons, and exhibits greater oxidative potential (Ma et al., 2025; Rizzo & Rizzo, 2025). Because we applied general dose–response functions developed primarily for all-source urban ambient pollution, our methodology does not explicitly capture this enhanced toxicity, meaning that our premature death estimates are likely conservative. Furthermore, fire pollution is highly episodic, driven by dry-season peaks and exacerbated by drought events. The long-term exposure metrics used in our assessment are designed to capture chronic health effects averaged over an annual scale. This approach inherently smooths out the extreme, short-term concentration spikes typical of severe fire seasons, potentially missing the acute cardiovascular and respiratory mortality impacts triggered by intense smoke waves. Developing robust, fire-specific dose–response functions that account for both enhanced toxicity and episodic exposure remain a critical need for future health impact assessments.**"

-The above comment could be incorporated into a broader discussion of the uncertainties of this work.

agree that a comprehensive discussion of the uncertainties is essential for contextualizing our findings. In the revised manuscript, we have substantially expanded the Discussion section to

provide a broader and more integrated overview of the limitations and uncertainties inherent in our approach.

Specifically, we have consolidated the discussion of uncertainties into several key areas:

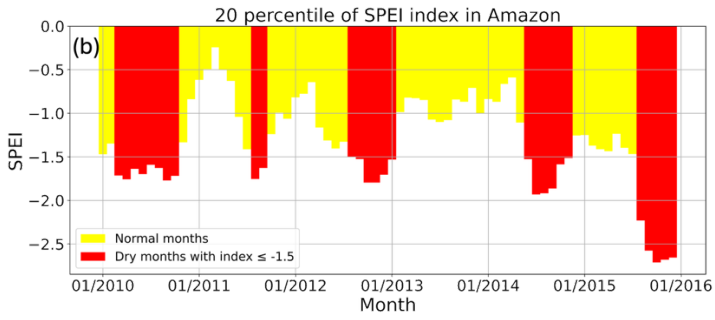
1. Fire Emission Inventories and Model Biases: We acknowledge the uncertainties in the GFEDv4s inventory, noting that it may underestimate net land-use carbon fluxes and regional fire emissions compared to newer version. We also discuss the systematic underestimation of absolute $PM_{2.5}$ concentrations in our GEOS-Chem configuration, which necessitated the use of a satellite-derived $PM_{2.5}$ baseline.
2. Deforestation and Climate Interactions: We note that our experimental design holds deforestation relatively stable to isolate drought impacts, meaning we do not explicitly resolve shifting spatial patterns of deforestation or the complex, two-way feedback mechanisms between drought, the biosphere, and atmospheric chemistry.
3. Dose–Response Functions: We discuss the limitations of applying general, all-source exposure–response functions to fire smoke. We explicitly address how the unique chemical properties and episodic temporal dynamics of fire-specific $PM_{2.5}$ are not fully captured by chronic, all-source metrics, meaning our calculated health burdens are likely conservative, lower-bound estimates.
4. Independence of Pollutant Risks: We clarify the assumption that the health impacts of $PM_{2.5}$ and ozone are treated as independent, despite co-exposure in fire plumes.

By grouping these points in the revised Discussion, we provide readers with a transparent assessment of the methodological constraints and clearly define the trajectory for future research, including the need for fire-specific dose–response functions and coupled climate–chemistry–vegetation models. See previous responses for the detailed description of all of these points.

Minor:

-I suggest adding a color bar to Fig 1b rather than explaining in the caption.

We thank the reviewer for the suggestion. We have added a color bar to Fig. 1b and updated the figure caption accordingly in the revised manuscript.



-I think that the Figure S3 caption is wrong (orange=dry season, blue=wet season).

The caption for Fig. S3 has been corrected so that the colors for the dry and wet seasons are now described accurately in the revised Supplementary Materials.

-Does Figure S4 show the simulated PM2.5 for the fire-only fraction or all sources?

Figure S4 shows simulated surface $PM_{2.5}$ from all sources.

-I'm not sure that Table S1 is necessary if the authors can cite the most recent GFED paper.

Thank you for this suggestion. We agree that the detailed emission factors are available from the most recent GFED documentation, so Table S1 is not strictly necessary. We have therefore removed Table S1 in the revised Supplementary Materials.

Response to Referee #2

The Amazon is an important source of wildfire emissions into the atmosphere, and understanding drivers of wildfire emission variability in this region has potential for global significance. The authors study the relationship between drought influence on wildfire and the resulting impacts on fire emissions, air quality and health. They use satellite derived fire emissions in a global chemical transport model (GEOS-Chem) to evaluate impacts between 2010 and 2015, categorizing results by drought intensity. Additionally, health impacts are evaluated using a fire sensitivity study and exposure response algorithms for PM2.5 and ozone.

Overall the study is comprehensive and rigorous. The paper is well written, with a thorough discussion. Please find my specific comments below.

We thank the reviewer for the invaluable comments, which help us improve the manuscript substantially. According to your comments, we have revised accordingly to address the reviewer's concerns.

Main Comments:

My main question is whether the length of time simulated is enough to fully capture the variability space. I suggest a brief statement about the fire emission variability during the 2010-2015 period compared to the longer record of GFEDv4s to quantify whether the study covers typical variability seen in the region.

We thank the reviewer for this thoughtful comment. To address it, we have compared the dry-matter-burned (DMB) time series over our study domain from the full GFEDv4s record (1997–2019). This comparison shows that 2010 and 2015 correspond to two of the largest regional DMB peaks in the record, and that the intervening years (2011, 2013 and 2014) span a wide range of fire activities from relatively low to moderately high levels. In other words, our 2010–2015 window captures both typical dry-season fire emissions and several extreme drought-related fire seasons within the broader GFEDv4s variability space, while maintaining relatively stable deforestation rates for attribution purposes. We have added a brief statement in the Methods describing this comparison to clarify that the simulated period is representative of the typical and high-end fire variability observed in the region.

Supplementary Materials:

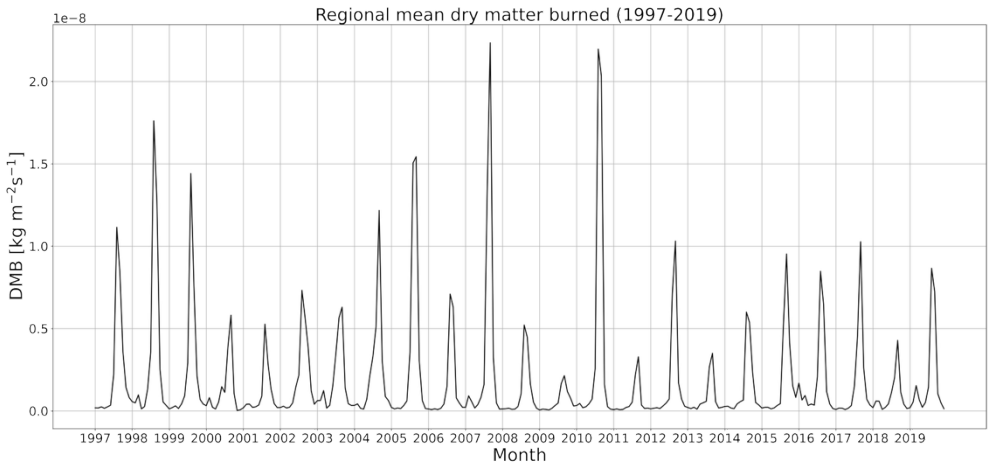


Figure S1. 1997-2019 dry matter burnt in study area recorded by GFEDv4."

L206-211 "Two sets of simulations ran from January 2009 to December 2015**, with 2009 used** as model spin-up **and 2010-2015 retained for analysis. This study period was chosen because it encompasses multiple intense fire seasons, including the major drought years 2010 and 2015, which are among the highest fire-emission years in the past decade. A comparison of regional DMB from the full GFEDv4s record (1997–2019) indicates that 2010 and 2015 are among the highest-emission years in the time series, while 2011–2014 spans lower to intermediate fire activities, suggesting that the 2010–2015 window samples both typical and extreme dry-season fire variability in the region (Fig. S1).**"

Minor Comments:

Abstract (and elsewhere): Add standard deviations to the estimates, for example in the abstract it is stated fires contribute 50% to regional CO, I suggest to clarify this is an average and add plus or minus a specific range.

Methods: Perhaps I missed the definition of the spatial bounds of the Amazon, presented in regional averages for many results. Please add a definition.

The spatial bounds of the Brazilian Amazon used for our regional averages are defined in the Section 2.4 (at line 222 in the revised version) (5°N–15°S, 47°W–73°W), but we recognize this may have been easy to miss. We have now made this definition more prominent by explicitly restating these bounds when introducing the regional averages in the Results section.

L348 "Figure 2a shows its emissions in the Amazon area **(5°N-15°S, 47°W-73°W)**,"

L394 "Figure 4a shows the Amazon **(5°N-15°S, 47°W-73°W)** monthly mean NO_x emission in both simulations."

L171 to L172: It would be helpful to be slightly more specific that the stable deforestation rates from 2010-2015 were the reason this time period was chosen for the study presented, and that outside these times the high variability in deforestation could mask any signal from drought impacts. For example on line 200 to 201.

The manuscript has been revised to mention the reason of the study period.

L206-218 "Two sets of simulations ran from January 2009 to December 2015**, with 2009 used** as model spin-up **and 2010-2015 retained for analysis. This study period was chosen because it encompasses multiple intense fire seasons, including the major drought years 2010 and 2015, which are among the highest fire-emission years in the past decade. A comparison of regional DMB from the full GFEDv4s record (1997–2019) indicates that 2010 and 2015 are among the highest-emission years in the time series, while 2011–2014 spans lower to intermediate fire activities, suggesting that the 2010–2015 window samples both typical and extreme dry-season fire variability in the region (Fig. S1). Additionally,** deforestation rate remains low and stable during the experimental period (Fig. 1a), **providing a quasi-constant anthropogenic land-use background that allows us to more cleanly isolate the influence of drought-driven variability on fire emissions, air quality, and public health impacts by** comparing drought years (2010, 2012, 2014 and 2015) and non-drought years (2011 and 2013). **Within this window, we capture substantial interannual variability in drought conditions while avoiding the confounding effects of**

large swings in deforestation extent, which would complicate attribution of observed changes in air pollutant exposure and health burden. This design enables us to quantify the individual effect of drought on fire-related air quality and public health under a relatively stable deforestation regime."

L179 to L180: The description of SPEI values is confusing, please double check. I think the word "larger" should be replaced with "greater" or "less negative than". Also, from Figure 1, The SPEI is a 3 month index, so it is confusing that two bars would not indicate a 6 month values and therefore be "more than 3 month" definition of drought.

We thank the reviewer for these helpful clarifications regarding the SPEI description. We have corrected the wording to avoid confusion.

L185-188 "The indices less than or equal to –1.5 are labeled with red color bars, indicating the occurrence of a severe drought, while yellow color bars refer to non-drought conditions with the index **greater** than –1.5. In this study, the severe dry months were considered as experiencing a drought if they were extended to more than three months **(more than 3 consecutive red color bars in the plot) (Fig. 1b)**."

L202 to L204: Clarify whether the "fire off" emissions are global fires off, or only Amazon/South America fires off.

We thank the reviewer for this comment. In our simulations, the 'fire-on' case includes biomass burning emissions from GFEDv4s globally, whereas the 'fire-off' case turns off biomass burning emissions worldwide, not only over the Amazon or South America. We have clarified this in the Methods to avoid ambiguity.

L220-222 "The first set of simulations ("fire-on") utilized the GFEDv4s (Randerson et al., 2018) monthly DMB data in 0.25° to represent **global** fire emissions (Fig. 1c), while the second set of simulations ("fire-off") were the control **with all global biomass burning emissions turned off**."

L204 to L206: Clarify whether you are comparing DMB just for the 2010 to 2015 period, or the whole GFEDv4s period. Mention somewhere about the limitation of data impacts drawing robust conclusions, a broad statement like "drought years generally double to triple that in non-drought years" could be a little misleading – because there are only two non-drought years and three drought years.

In the revised manuscript, we now explicitly state that the comparison of dry matter burned (DMB) between drought and non-drought years refers to the 2010–2015 study period only. We also soften the wording to avoid overgeneralizing from a small number of years and acknowledge that the limited sample size constrains how robustly we can generalize this pattern.

L225-227 "**Within the 2010–2015 study period, t**he DMB recorded by GFEDv4s peaks in every August or September **in each year**, **and** the **peak** magnitudes in **identified** drought years **(2010, 2012, 2014 and 2015) are** generally **substantially higher than** that in **the two** non-drought years **(2011 and 2013)**, **with 2010 showing the largest peak among all years.**"

L206 to L210: Convoluted and long sentence. Consider rewording for clarity.

We have simplified and reworded this sentence to improve clarity.

L227-231 “By utilising the GFEDv4s data in GCHP, we obtained the fire-induced chemical emissions by multiplying **DMB with** species-specific and fire-type-specific emission factors. **We then** analyzed a temporal analysis of shortlisted chemicals in monthly level from Jan 2010 to Dec 2015 within the Brazilian Amazon area (5°N-15°S, 47°W-73°W), and spatial analysis of **these** chemicals in monthly level from January 2009 to December 2015 in South America (15°N-25°S, 40°W-90°W).”

L213: The PM2.5 are not satellite-observed PM2.5, reword to satellite-informed or satellite-derived.

We have reworded the text to refer to the $PM_{2.5}$ product as “satellite-derived” rather than “satellite-observed” in the revised manuscript.

L240-241 “To **obtain realistic absolute $PM_{2.5}$ levels while retaining the model-derived fire signal,** we **regridded the satellite-derived** monthly $PM_{2.5}$ concentrations (V5.GL.04), …”

L276 to L280: Clarify how the model might be impacted by plume height – are the plumes low in reality and the plumes are too high in altitude in GEOS chem, or are the plumes too low in the model compared to reality. How exactly is this impacting comparison between model and measurements? e.g., does the plume height impact chemistry and the ozone production in the plume? Why was the updated emission scheme not used in this study?

We have now clarified the plume height and emission scheme issues in the revised manuscript. In reality, especially during drought years, fire plumes in the region tend to remain relatively low (with characteristic heights decreasing from about 1100 m to 800 m) (Gonzalez-Alonso et al. (2019), whereas the standard GEOS-Chem configuration used here likely injects a larger fraction of emissions at higher altitudes through its default plume-rise and vertical mixing treatment (Zhu et al., 2018). This vertical mismatch can affect the comparison with measurements because plume height influences dilution, photolysis rates, and distribution of precursors and radicals, thereby affecting in-plume ozone production and downwind ozone levels (e.g. Val Martin et al., 2010). At present, we lack a quantitative estimate of how large this effect is in our specific simulations.

Based on our understanding, the updated biomass burning emission treatment was developed and tested in GEOS-Chem v9 using satellite-observed fire radiative power, which is typically used in “top-down” fire emission products such as GFAS and QFED. In contrast, our study uses the GFED inventory, which follows a bottom-up approach based on burned area and emission factors to derive fire emissions. As this updated treatment has not been adapted or implemented for use with GFED in the current GEOS-Chem configuration, it was not applied in our simulations.

Alvarado, M. J., Wang, C., & Prinn, R. G. (2009). Formation of ozone and growth of aerosols in young smoke plumes from biomass burning: 2. Three-dimensional Eulerian studies. Journal of Geophysical Research: Atmospheres, 114(D9).

L305-313 “Moreover, performance discrepancies may also be caused by the plume height. **In reality, especially during drought years, fire plumes from tropical forest and savanna burning in this region tend to remain relatively low, with characteristic heights decreasing from about 1100 m to 800 m (Gonzalez-Alonso et al., 2019). The standard GCHP configuration used here likely injects a larger fraction of emissions at higher altitudes through its default plume-rise and vertical mixing treatment** (Zhu et al., 2018). **This vertical mismatch can affect the comparison with measurements because plume height controls dilution, photolysis rates, and the distribution of precursors and radicals, thereby influencing in-plume ozone production and downwind ozone levels**

(e.g., Alvarado et al., 2009; Val Martin et al., 2010). At present, we lack a quantitative estimate of how large this effect is in our specific simulations and therefore treat it as an important but unquantified source of uncertainty."

Figure 2: Add units to the y-axis of Fig. 2d

We have added the appropriate units to the y-axis of all temporal plots in the revised manuscript.

L363 to L365: For clarity, it would be valuable to swap the order of discussion: Isoprene in the fire on and fire off simulations are equivalent, which means that the simulation of isoprene is independent of wildfire impact. Consider mentioning here that wildfires might impact biogenic emissions through the interaction of aerosols with vegetation in reality, but the GEOS-Chem model does not include this feedback mechanism.

We thank the reviewer for this helpful suggestion.

L399-406 "Figure 4b shows the simulated isoprene, one of the major biogenic VOC (BVOC), emissions in Brazilian Amazon, which is simulated by MEGAN **in GCHP**. **In our configuration, MEGAN biogenic emissions are independent of wildfire impacts, so isoprene emissions are identical in the fire-on and fire-off simulations and peak every September with magnitudes between** 5.2 to 8.1×10^{-10} kg m^{-2} s^{-1}. **In reality, wildfire smoke and aerosols may influence vegetation and thus biogenic emissions, but such feedbacks are not included in the current GEOS-Chem model. Despite the identical emissions,** the annual minimum of monthly mean isoprene concentration in the fire-on simulation is found between August and September **and is 3.2–4.7 ppb, which** is about 1–2 ppb lower than in the fire-off simulation over the same period (Fig. 4c)."

L369 to L370: Add in a reference for the 0.16 isop:NOx threshold indicates a NOx limitation of ozone production.

We thank the reviewer for this suggestion.

L406-407 "We also estimate the ratio of NO_x emission over isoprene emission in Fig. 4d. The ratio remains in a low proportion (below 0.16) **(e.g. Ren et al., 2022)**,"

L391 to L393: The text suggests Fig. 6 is a comparison of ozone between the fire on and fire off simulations, however, the caption of Fig. 6 does not state this. Please clarify.

We thank the reviewer for noting this potential ambiguity. We have revised the text and the caption of Fig. 6 to clarify that the figure shows ozone concentrations from the fire-on simulation only, and does not include a direct comparison to the fire-off simulation.

L442 "Figure 6: Dry season mean ozone concentration **in "fire-on"** simulation between 2010 and 2015."

Figure 5: I suggest to alter the plot so the ozone scatterplot points are more centered. Perhaps start the y-axis at 10. Also, add units to the y-axis.

We have now revised Fig. 5 so that the ozone scatter points are more centered by adjusting the y-axis (now starting at 10), and we have added the appropriate units to the y-axis in the revised manuscript.

Section 3.4: It seems Figure 8 and 9 are discussed in the main text before Figure 7. I would suggest to move Figure 7 after Figure 8.

We have now reordered the figures accordingly, moving Figure 7 to appear after Figure 8 in the revised manuscript so that the figure sequence matches the order of discussion in the main text.

L413 to 414: Check the ozone mortality values and the reference to the figure – Should it be Fig 8?

We thank the reviewer for highlighting this issue. We have rechecked the ozone mortality values and confirmed that the figure reference in the text was incorrect. After reordering the figures as suggested, the relevant results are now shown in Fig. 7, and we have updated the manuscript accordingly to refer to Fig. 7 instead of Fig. 8.

Figure 9: This mortality data is modeled. Is there a way to validate this data against reality? The authors suggest ozone and PM2.5 would be dominated by fire emissions, so the overall mortality for the investigated diseases may be dominated by and follow a sum of the ozone and PM2.5 versus SPEI patterns shown in Figures 9b and 9c (although total true mortality would be higher). I suggest the authors briefly look into publicly available mortality data for the studied deiseases, if available, which could strengthen this argument considerably.

We have examined publicly available Global Burden of Disease (GBD) estimates for the diseases considered in our analysis over Brazil and find an overall increasing trend in total mortality over the study period, consistent with previous reports of growing disease burdens attributable to air pollution and other risk factors. For the specific cardiovascular and respiratory outcomes we investigated, the total annual number of deaths in Brazil is on the order of 300,000 per year, whereas the fraction explicitly attributed to ambient ozone and $PM_{2.5}$ exposure in GBD is substantially smaller.

Because our modeled mortality is restricted to the component attributable to fire-induced ozone and $PM_{2.5}$, and the publicly available GBD data provide country-level totals rather than fire-specific or drought-stratified mortality, a direct quantitative validation of our modeled values against "true" mortality is not feasible.

Technical Corrections:

L42-43: Is this sentence meaning half of the fire emissions from the Amazon was sourced from old-growth forest burning?

Yes.

L99: Besides → Additionally,

L174: evapotranpiration → evapotranspiration

L272: "in which the modeled ozone level…"

L273 and L275: overestimates

L274: reproduces

L293: "Although model underestimation…"

L301: to gether → together

L324: "dry season mean CO concentration for each year from …"

L360: In the meantime → Additionally,

L365 to L366: Add Fig. 4b and Fig 4c notes to this sentence to make it less confusing.

L393: fire-emitted ozone → fire-influenced ozone

L395: fire emission contribution of ozone → fire contribution to ozone

L396: "Therefore, fire emissions are…"

L508: Besides → Additionally,

> We thank the reviewer for these helpful technical corrections. We have implemented all suggested edits (including wording changes, spelling corrections, and clarified phrases) throughout the revised manuscript, and the sentence at L42–43 now explicitly reflects that approximately half of the fire emissions from the Amazon originate from old-growth forest burning.