Response to Reviewer 1 (RC1)

We thank the reviewer for taking the time to comment on our paper, and appreciate the positive comments on the comprehensive data, development of methods and approach, and organisation of the manuscript. Please find our responses to each point below.

Reviewer Comment 1:
L162: The statement that "global total burned area (BA) and emissions are decreasing" may need some clarification. While global burned area has shown a clear declining trend, global fire emissions have remained relatively stable, with previous studies reporting only a statistically non-significant decreasing trend (e.g., Zheng et al., 2021). I suggest distinguishing these contrasting trends, which may also better support the following discussion of increasingly severe regional fire impacts (e.g., increasing forest fire activity).

Thank you for this important comment. We agree that we can make the point more clearly that emissions are not showing as strong a decreasing trend as burned area. We have revised the text accordingly:

~~Research is showing that while global total burned area (BA) and emissions are decreasing, regional impacts are becoming more severe. Fires are moving into extratropical carbon-rich forests and peatland areas (Jones et al., 2024a; Zheng et al., 2021), and extreme heat and drought are driving abnormally intense fires across Mediterranean biomes (Ghasemiazma et al., 2026).~~ While global total burned area (BA) is decreasing, both BA and fire emissions are rising in extratropical carbon-rich forests and peatlands (Jones et al., 2024a; Zheng et al., 2021). Meanwhile, fire intensity is also rising globally and abnormally intense fires across Mediterranean biomes have been linked to extreme heat and drought (Cunningham et al., 2024; Ghasemiazma et al., 2026).

Reviewer Comment 2:
L384–386: If I understand correctly, the GFED5 fire emissions used in this study consist of two components: the standard GFED5 record through 2022 and GFED5-NRT (Chen et al., 2026) for 2023–2026. Although GFED5-NRT relies on different data sources and methodological approaches to extend the historical record, it has been shown to be broadly consistent with the standard GFED5 product. I suggest explicitly clarifying these two components and their respective temporal coverage so that readers can clearly understand the composition of the GFED5 emission record used in this study.

We fully agree, and we have added clarifications as follows:

The first substantial change to our input data was the replacement of the Global Fire Emissions Database (GFED) version 4.1s (GFED4.1s, van der Werf et al., 2017; as used in Kelley et al., 2025) carbon emissions with GFED5 (van der Werf et al., 2025; Chen et al., 2026a). Our emissions record combines standard GFED5 estimates through 2022 with the GFED5 near-real-time extension (GFED5NRT; Chen et al., 2026a) from 2023 to 2026. GFED5 substantially revises GFED4s by using a larger, better-constrained BA estimate, though at a relatively coarse resolution of 0.25° (Chen et al., 2023). It captures more small and agricultural fires than GFED4s and MCD64A1, and major updates from GFED4.1s include a 500 m fuel-consumption model aggregated to 0.25° and updated emission factors (Binte Shahid et al., 2024), including dynamic savanna emission factors (Vernooij et al., 2023). GFED5 generally gives higher carbon emissions than GFED4s, globally amounting to a 65% uplift in total emitted carbon. However, the uplift is uneven, with especially large increases in fragmented or human-dominated regions such as Southeast Asia, Europe and Central America, while equatorial Asia decreases slightly because of recalibrated peat-fire emissions in GFED5 (van der Werf et al., 2025). For the period 2023-2026, our emissions record uses the GFED5 near-real-time extension (GFED5NRT; Chen et al., 2026a). GFED5NRT extends the record using VIIRS active-fire observations, converting active-fire counts to burned area using biome- and region-specific effective-fire-area relationships calibrated against standard GFED5, and estimating emissions using corresponding GFED5-derived fuel-consumption relationships. Although this differs from the full GFED5 methodology, evaluation over the overlapping period shows that GFED5NRT broadly reproduces GFED5 spatial patterns, seasonal cycles and interannual variability.

Reviewer Comment 3:
L3012–3021: One potential direction for future editions of the State of Wildfires Report could be to complement the long-term climate projections with a near-term outlook for the upcoming fire season. Given the annual nature of the report, it may be valuable to explore whether probabilistic forecasts of wildfire activity (e.g., burned area or the likelihood of above-normal fire activity) could be provided for the following fire season. If feasible, this could be an interesting avenue to consider in future editions, potentially further enhancing the practical value of the report for preparedness and early warning.

Thank you for this suggestion. In previous reports, we have indeed published a near-term outlook and we agree that it can be of interest to our readers. Unfortunately, this has been logistically challenging given our commitment to the peer review process, with the time from submission to publication often closely matching the period over which forecasts are most reliable (e.g. up to 2-3 months). Hence, upon publication the forecast became outdated. It was on this basis that we took the decision to remove this analysis in this edition of the report, but we intend to find alternative ways of publishing near-term forecasts stemming from published methods (e.g. Di Giuseppe et al., 2025), including through our website. This would allow outlooks to be updated as new forecast information becomes available, independently of the report's publication cycle, while maintaining a clear link to the peer-reviewed methodology and communicating forecast uncertainty and limitations.

In addition to the reviewer comments addressed above, we have made a few minor updates including co-author affiliations that have changed in the last 2 months during the review period. Additionally, the underlying HadGEM3-A dataset used in parts of the analysis has been updated to include additional ensemble members that became available in the last 2 months during the review period. We therefore incorporated these members and re-ran the analysis. This resulted in minor quantitative changes to the HadGEM3-A FWI attribution estimates and associated figures for the focal regions, but did not affect the interpretation or conclusions of the study. The revised text and figures have been marked in the manuscript. During this update, we also identified and corrected an issue in the processing of the cumulative dry-days metric used in the burned-area attribution analysis, whereby gaps in some ensemble time series artificially reset the cumulative dry-day count. This correction produced revised burned-area attribution estimates, most notably in Northwest Iberia. This change primarily affects the central estimate for burned-area attribution results; associated uncertainty ranges and likelihood assessments changed little, and projections remain unchanged, and the study's overall interpretation and conclusions remain unchanged.

Response to Reviewer 2 (RC2)

We thank the reviewer for their comprehensive and positive review of our manuscript. Please find our responses to each point below.

1. The report explicitly reports on fire C emissions, but does not discriminate between emissions of CO2 and CH4, despite the big difference in global warming potential (GWP). The emission factors of CH4 and CO2 depend heavily on fuel characteristics and change with changing fire patterns: e.g., late-dry-season (LDS) burning of dry, highly flammable grasses in southern African savannahs have a smaller CH4:CO2 ratio than early-dry-season fires. The fire C emissions may be larger in LDS (due to higher fuel loads), yet the total GWP is smaller because most C emissions are in the form of CO2, see, e.g. a critical note by Paul Laris and references therein (https://doi.org/10.1038/s41467-021-25141-1). Do the authors account for differences in GWP of C emissions in their assessment?

Thank you for this insightful comment. We agree that the composition of fire emissions, including differences in the relative contributions of CO2 and CH4, is important when considering their impacts on climate. Our analysis does not currently distinguish between individual chemical species, instead reporting total C emissions, consistent with other annual reporting efforts such as the Global Carbon Budget (Friedlingstein et al., 2026). We recognise that this distinction, and its implications for interpreting our results, should be made clearer to readers.

We also recognise the need for care when interpreting the climate impacts of fire emissions, particularly in distinguishing gross emissions during combustion from their longer-term climate effects. Gross fire emissions do not account for subsequent carbon uptake during post-fire ecosystem recovery, which influences the longer-term net contribution of fires to atmospheric CO_2. Applying GWPs to gross emissions without considering these processes could therefore lead to misleading interpretations of their net climate impact.

Overall, we agree that our reported C emissions should not be interpreted directly as a measure of climate warming, and have revised the Methods and Results sections to clarify these limitations, as follows.

Methods (section 2.1.2.2):

"Emissions estimates from both GFED5 and GFAS represent the total carbon (C) emitted from fires, encompassing carbon released as CO_2, CO, CH4 and other carbon-containing species. Although both datasets provide estimates of individual emitted species, our

analysis does not distinguish between them or account for differences in their global warming potentials (GWP). Importantly, these gross emissions estimates also exclude subsequent carbon uptake during post-fire ecosystem recovery, which influences the longer-term net contribution of fires to atmospheric CO_2. We therefore report emissions in C terms and do not convert them into CO_2-equivalent climate impacts, which would require consideration of both the composition of emitted species and the longer-term carbon balance of affected ecosystems."

Results (section 3.1):

"These values represent gross carbon emissions from fires, rather than their net contribution to atmospheric CO_2 or climate warming. The latter depends on the composition of emitted gases, subsequent carbon uptake during ecosystem recovery, and the timescale considered (see Section 2.1.2.2)."

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2. The decision to publish the regional reports separately is appreciated, because it makes the regional findings and efforts more visible and can receive credit in their own right.

We thank the reviewer for noting this change, and agree it improves the visibility of the regional reports which are important standalone pieces.

3.On line 796 it is mentioned that MERRA2 is used for Northwest Iberia, whereas for all other regions, ERA5 is used. Can the authors elaborate on their decision to use another model in this particular region?

Thank you for making this important point that we omitted to fully explain this in the submitted paper. We excluded MERRA2 from the analysis for Chile as 2026 data wasn't available at the time of processing. We also excluded it for Canada, as it was found when we processed the data that the seasonality and spatial pattern compared to data from ERA5 and National Research Council Canada were found to be incorrect for this region, including not representing any winter lull in fire risk. We have added to the text accordingly:

We do the same for reanalysis products. We use ~~(~~ERA5 for all focal regions, and MERRA2 for Northwest Iberia. MERRA2 is excluded for Chile as 2026 data wasn't available at the time of processing, and for Canada as the seasonality and spatial pattern were found to be incorrect compared to data from ERA5 and National Research Council Canada~~)~~.

Please find additional minor comments below.

ll.123-124: "(...) while BA extent in the region is 46 times greater." -> This subsentence follows two statements on (increased) likelihoods, which affects its readability. I suggest to write this in a new sentence to form a bridge to the subsequent statements on "record or high-ranking BA and fire C emissions".

Thank you for this suggestion, we have edited as follows:

In the Midwestern Canadian Shield Forests, the epicentre of Canada's anomalous season, we find that anthropogenic climate change has doubled the likelihood of extreme fire weather in this region and made the observed BA around 61 times more likely during July-August 2025. ~~, while~~ BA extent in the region is found to be 46 times greater. Many parts of Europe also experienced record or high-ranking BA and fire C emissions…

ll.218-219: "above-average global BA" and "BA extent are both projected to increase" -> This is in contrast to the observation that globally, BA is decreasing. If these statements pertain to the Mediterranean ecoregions (or some other region), this may need to be more clearly stated.

Thank you for pointing out that this isn't written very clearly. We have revised the text as shown below, which we hope clarifies the point that this is pertaining to global changes in burned area due to climate change alone, separate to socio-economic changes in population and agriculture which have a suppressive effect globally:

The contribution of climate change to ~~increases in~~ global BA is rising each year, partially offsetting socio-economic suppression, and increasing the probability of experiencing months with above-average global BA by 22% (Burton et al., 2024b).

l. 1003: Remove spurious "out"

We have changed this to "from" to hopefully make this clearer:

BuRNN exists from ~~out of~~ 55 Long Short-Term Memory networks (LSTMs)...

l. 1115: "half below average" -> Do you mean "half of the average value?" Please rephrase for clarity.

Thank you for this suggested re-phrasing, this does indeed help clarify the meaning. We have changed accordingly:

South America, by contrast, experienced a much lower fire season than in 2024-2025: continental BA and fire carbon emissions were both around half of the ~~below~~ average value.

ll. 1190-1208: These paragraphs present absolute totals and differences in addition to relative differences and are somewhat difficult to read. Please consider rephrasing or putting the numbers into a table or figure for clarity.

Thank you for this suggestion. We agree that presenting several measures of population exposure together can make these paragraphs too information-dense. We have therefore made changes throughout to simplify the reporting of exposure statistics and improve readability, while retaining the distinction between areas with the largest numbers of people exposed and those experiencing the most unusual exposure relative to their historical baselines. The various revisions implemented can be viewed in the track-changes version of the manuscript, section 3.2.1.

Readers are able to access the data via the Zenodo repository linked to in the Methods and the Data Availability sections, as well as through the interactive apps which display those data on the State of Wildfires website.

ll. 1284-1289: "Industrial sites (...) at regional scale." -> These are important considerations of the limitations of the data set, but it appears at an unexpected location, as this might be more suited in either the section describing data sets or in the discussion. In any case, the statement should be made more specific regarding how the static mask "could potentially affect the analysis at regional scale."

Thank you for this suggestion. We agree that this discussion of uncertainties associated with GFAS is better placed in the Methods section. We have therefore moved it to the 'Air Quality Impact Assessment' subsection and revised the wording to reflect the screening procedures used in GFAS v1.4.2. We have also clarified how residual spurious detections could affect simulated PM2.5 concentrations at regional scale. The revised text is provided below, and the changes can also be seen in the track-changes version of the manuscript.

Text added to the Methods (section 2.2.3):

"GFAS v1.4.2 applies screening procedures to minimise spurious fire detections associated with industrial sites, reflective surfaces and gas flares. However, these procedures may not eliminate all non-vegetation thermal signals, potentially introducing false fire emissions that could affect simulated PM2.5 concentrations, particularly in regions with relatively little vegetation fire activity."

Text removed from the results (section 3.2.3):

"Industrial sites, reflective surfaces, and gas flares can introduce spurious signals into the analysis. GFAS emissions minimise these spurious detections by applying a screening mask to ensure that the data is largely based on vegetation fires. However, the screening procedure relies on a static mask rather than one generated in real time, and newly

established industrial sites could potentially affect the analysis at regional scale.”

l. 1616. "wildfire activity, driven primarily by extensive BA and emissions in Canada." -> This is a case of reversed logic: extensive BA and emissions in Canada are what define the extreme wildfire acitivity, i.e., more like symptoms than drivers. Please rephrase the statement, possibly by replacing "driven primarily by" with "characterised by".

Again a very pertinent and astute edit, thank you for picking this up. We have revised according to your suggestion:

The 2025-2026 fire season in North America marked a third consecutive year of extreme wildfire activity, characterised ~~driven primarily~~ by extensive BA and emissions in Canada.

l. 1850: "that produced" -> "with"

Edited as suggested:

Consistent with large wildfire years in the boreal forests of Canada, the focal region experienced a prolonged heat wave in early May 2025 with ~~that produced~~ substantial drying across the central portion of the Canadian shield.

ll.3055-3056: "These political commitments provided evidence that countries need better data..." -> Strictly, the commitments only show that nations are joined in wishing for better data; it doesn't prove that they need it. Please revise the statement accordingly.

True. We have revised as follows:

These political commitments demonstrate the need for ~~provided evidence that countries need~~ better data…