Author Responses to Referees for RC1 and RC2

Review of Zhu et al. "Quantifying the current and future likelihood of the 2022 extreme wildfire weather conditions in France with anthropogenic climate change"

Manuscript: EGUSPHERE-2026-136

Unless otherwise specified, all page and line numbers mentioned in the reviewer comments refer to the originally submitted preprint, while the page and line numbers for the revised text and inserted text refer to the revised manuscript.

Author Responses to RC1

Summary

Zhu et al. present an analysis of the climatic conditions surrounding the extreme 2022 wildfire season in the Southwest of France. The authors use a country-level fire dataset, two different climate re-analysis derived fire weather index (FWI) datasets and CMIP6 model output to quantify how exceptional the 2022 climatic conditions were and how anthropogenic climate change (ACC) has made (and will make) these conditions more likely. The authors find that with increasing spatial-temporal resolution of the FWI, the signal becomes stronger and return period lengths increase by decades, suggesting that aggregating FWI over longer time periods (compared to the time of burning) and larger spatial scales will average out the FWI signal. Furthermore, the climate attribution analysis indicates that already ACC made the FWI conditions during the 2022 fires more than twice as likely compared to the "only natural forcings" run and that by the end of the century these conditions will be 10 to 100 times more likely to occur.

The study presents a nice concise analysis well within the scope of NHESS that is relevant for local stakeholders in the France as well as those in the wider European context. I do not have any concerns with the methods, assumptions or the interpretation of the results. The methodology is not entirely novel as studies of FWI on large spatial scales are common and attribution studies on extreme fire years have also been published before. However, I still think the study presents a scientific advance on the local and regional scale. I have a few major and minor comments which are listed below.

We thank the reviewer for this positive and very comprehensive review of our work. We appreciate all the suggestions and tried to clarify as much as possible the text, where needed.

General comments

One of the general comments is that there is almost no mention of ignition sources in the manuscript (except a little in the discussion about the ignition of the Landiras-2 fire from peat smoldering). It is not technically the scope of the paper to focus on ignitions because it focuses on the fire weather conditions surrounding these fires, if there are no ignitions sources there will be no fire. It would increase the novelty of the paper dramatically if these were taken into account in the analysis, even if it was a very simple approximation. For example, by making "ignition probability" a function of FWI.

We agree with the reviewer's suggestion that the ignition source is indeed a crucial component of wildfire activity and would definitely add to the scientific literature. However, as the reviewer also pointed out, ignition (whether anthropogenic or natural) is not the main focus of our study. Also, Ganteaume and Guerra (2018) showed that the fire cause is known in only 34% of cases. Nevertheless, we intend to examine this research question in a future study by using the ignition-related functionalities of the Firelihood model (Pimont et al., 2021) that takes into account the fire weather index as well as land use and land cover variables to provide the ignition probability, regardless of the cause. We expanded the discussion section.

Inserted at p. 16, L303: "Second, our analysis does not explicitly consider ignition sources and their spatiotemporal variability. In France, approximately 95 % of ignitions are related to human activities (Ganteaume and Guerra, 2018), and the realized fire activity therefore reflects the interplay between human pressure and fire weather conditions. As a simple approximation, the "fire start probability" could be expressed as a monotonically increasing function of the FWI (i.e., an ignition probability conditional on fire weather) using a probabilistic framework such as the Firelihood model (Pimont et al., 2021) combining FWI with land use and land cover information."

Ganteaume, A. and Guerra, F.: Explaining the spatio-seasonal variation of fires by their causes: The case of southeastern France, Appl. Geogr., 90, 69–81, https://doi.org/10.1016/j.apgeog.2017.11.012, 2018.

A second major comment concerns the readability of the paper. Generally, the manuscript is written in a very technical style that does not help with reaching a wider readership. If a single sentence in the results contains three abbreviations "FWI", "PC1" and "EOF" that were explained somewhere in the methods, you will lose possibly interested readers. The Conclusions section (5) was surprisingly accessible and I would urge the authors to use the writing style of this section and apply it to the entire manuscript, especially the Abstract and Results sections. It would also help if concepts like PC1 and EOF are shortly explained somewhere in the results, just to inform the reader why these metrics are important and what we can learn from them. The manuscript would benefit from a thorough language revision. I have provided several suggestions in the specific comments, although these are not exhaustive.

We agree with the reviewer's comments. The frequent use of technical terms (including abbreviations) may make it difficult for readers. We revised the text accordingly where needed and limited as much as possible the use of acronyms in the text, figures and captions to improve readability. We hope the text reads better now.

Specific comments

Title: "extreme wildfires weather", either pick "extreme wildfires in France" or "extreme wildfire weather conditions in France". Both apply to the study.

Response: The title has been revised as "Quantifying the current and future likelihood of the 2022 extreme wildfire weather conditions in France with anthropogenic climate change"

L7-10 'Our results show that the extremeness of FWI conditions generally increases with the spatiotemporal resolution, with the associated return periods increasing from 6 to 34 years, from 22 to 89 years, and from 6 to 101 years when moving from the coarsest to the finest spatiotemporal scale for the Landiras-1, Landiras-2, and La Teste-de-Buch wildfires, respectively. This sentence is not clear. How does resolution relate to extremeness?

Response: We agree that the phrasing was ambiguous. In the revised text, we explicitly defined "increasing spatiotemporal resolution" as the transition from larger to smaller grid cells and from longer to shorter time steps. Furthermore, we replaced "extremeness" by "rarity (return periods)". The revision now states that moving from coarse to fine scales reveals rarer FWI conditions (i.e., longer return periods).

Revised text p. 1, L7-10: "Our results demonstrate that the rarity of FWI conditions is generally the highest at local and fire duration scales with the associated return periods increasing from 6 to 34 years, from 22 to 38 years, and from 6 to 101 years when moving from the coarsest to the finest spatiotemporal scale for the Landiras-1, Landiras-2, and La Teste-de-Buch wildfires, respectively".

L17 "California in 2025", far more area burned in 2020 in California compared to 2025 (>8 times more, see https://www.fire.ca.gov/incidents/2025 and https://www.fire.ca.gov/incidents/2020) but more infrastructure was damaged in 2025 compared to 2020. I would at least mention 2020 in this sentence.

Response: We agree that the 2025 fires were particularly notable for their impacts on infrastructure while the 2020 California wildfire season was actually record-breaking in terms of burned area.

Revised text p. 1, L17-19: "The past decade has witnessed a number of unprecedented extreme wildfires across parts of the world (e.g., Australia in 2019–2020, Canada in 2023, or California in 2020 and 2025), causing widespread impacts on societies, ecological environments, and human life."

L22 "[…] and more than 14 times larger than the average in SW France (Fig. 1b)." Please revise this sentence. Separate the stats for France and SW France or rewrite in a different way to make the distinction clearer.

Response: We agree that the distinction between national and regional statistics was ambiguous. We have revised the sentence to clearly state that the 55,000ha national total represents a 6–7 fold increase over the national mean, whereas the 14-fold refers specifically to the burned area in SW France relative to its own average.

Revised text p. 1, L21-23: "At the national level, more than 55,000 hectares of forests and other natural vegetation were burned (IGN and MASA, 2025) — an area 6 to 7 times larger than the average over the preceding decade. In southwestern (SW) France specifically, the burned area was even more than 14 times larger than the regional average (Fig. 1b)."

L23 "a small number of wildfires" suggestion: "a small number of large wildfires"

Response: Agreed.

Revised text p. 2, L24-25: "This extensive burned area resulted in substantial biomass losses in Atlantic pine forests (Vallet et al., 2023) and was largely driven by a small number of large wildfires."

L27 "due to frequent wind shifts causing spread in multiple directions." Might need a reference, local source?

Response: The *Office National des Forêts* wrote a short experience report regarding those wildfires. Unfortunately, the report is not available online. Also, we double checked with ERA5-Land reanalysis and found similar abrupt wind changes during those wildfires.

Revised text p. 2, L26-29: "On 12 July 2022, two wildfires started simultaneously within the Gironde department: La Teste-de-Buch wildfire burned approximately 5,709 hectares over 12 days, while the second one in Landiras burned over 12,552 hectares over 14 days, due to frequent wind shifts causing spread in multiple directions (Office National des Forêts, personal communication)."

L29-30 "largest wildfire in France since the 1940s." Please cite a reference here, what is the source?

Response: Actually, there is no reliable fire dataset in France extending so far. This assessment was based on historical archives and media reports. We clarified this in the text.

Revised text p. 2, L30-32: "When combined, the Landiras-1+2 wildfire burned over 19,676 ha, which makes it the largest wildfire in France since the Landes forest fire of August 1949 (Sarrau and Yagoub, 2025)."

Sarrau, J. and Yagoub, M. M.: Documentation of Historical Forest Fires and Hazard: Case of Gironde and Les Landes, France, ISPRS Ann. Photogramm. Remote Sens. Spatial Inf. Sci., X-G-2025, 771–778, https://doi.org/10.5194/isprs-annals-X-G-2025-771-2025, 2025.

L37 "contributed to reduce fuel moisture content" to a reduced fuel moisture content?

Response: Thanks. We have revised the sentence.

Revised text p. 2, L38-39: "This warmer atmosphere and elevated atmospheric aridity have contributed to a reduced fuel moisture content, thereby increasing landscape flammability."

L94 "To quantify how unusual were those conditions" please rephrase

Response: Done.

Revised text p. 5, L98-99: "To quantify the departure from the climatology (i.e., normal conditions), we removed the seasonality by computing deviations from the local mean seasonal cycle and expressed them as percentage anomalies."

L168-169 "Figure 3 illustrates the first two modes of May–September FWI over the observational 1959–2023 period with the spatial distributions of EOF loadings (left panel) and their corresponding principal components (right panel)." This is a very technical description of the first results that might be improved by bringing the results a bit more descriptive, what is the EOF loading saying and why is it relevant? And why are their PC important? Please rephrase this first section of the results to make it more accessible.

Response: We have revised the description of Figure 3 to better contextualize the findings for non-specialists. Specifically, we added brief, plain-language definitions of the EOF loadings and Principal Components (PCs), explaining their physical meaning.

Revised text p. 8, L179-184: "Figure 3 illustrates the first two dominant modes of May–September FWI variability over the 1959–2023 period, with the EOF loadings (left panels) and their corresponding PCs (right panels). The loadings characterize the spatial structure of a given mode, identifying regions where FWI anomalies vary either in phase (same sign) or in opposition. The PCs reflect the temporal evolution of each mode, highlighting years during which the associated spatial structure is either amplified or dampened. In other words, the initial FWI time series in a specific grid cell featuring a high positive loading will strongly look like the associated PC. A negative loading will in turn indicate that the FWI time series varies in opposition to the PC."

L184-185 "(note that 100% indicates FWI was twice larger than what we would expect from the average local conditions)" please rephrase, FWI is higher not larger. For example: "FWI is twice as high as expected under average local conditions."

Response: We corrected.

Revised text p. 9, L200-201: "Figure 4c shows positive anomalies three months before wildfires (note that 100% indicates that FWI was twice as high as expected from average local conditions), reaching 71 %, 106 %, and 155 % for small, medium, and large fires, respectively."

L186 "FWI anomalies were that time stronger during" stronger anomalies? Higher or lower please but not stronger.

Response: We corrected.

Revised text p. 9-10, L203-205: "A similar signal was observed in 2022 (Fig. 4b,d), but FWI anomalies were that time higher during the previous months and were 119 %, 137 %, and 180 % higher than mean conditions on the starting days of small, medium and large wildfires, respectively (Fig. 4d)."

L190 "indicates that annual maxima of the MA FWI" MA FWI is already maximum annual right? Please rephrase.

Response: We apologize for the ambiguity. Here, "MA" stands for "Moving Average" rather than "Maximum Annual." The MA is calculated as the arithmetic mean of daily FWI values over a sliding temporal window. This process smooths out the raw time series and reduces short-term variability. We then selected the annual maximum value from this smoothed series for our analysis. To prevent any oversight or misunderstanding by future readers, we explicitly reiterate the full name here.

Revised text p. 10, L207-210: "Figure 5 (left panels) indicates that the annual maxima of the moving-average (MA) FWI are consistently highest when computed at the finest spatiotemporal resolution (i.e., the fire-duration window at the 64 km² SAFRAN grid cell fire level). These annual maxima decrease when the temporal window is lengthened to 30 days or when FWI conditions are spatially averaged over the SW France region (~4.9×10^4 km^2).”

L207 “We then computed the RR” please write in full again first time in the results: risk ratio (RR)

Response: Done.

Revised text p. 12, L222: “We then computed the risk ratio (RR) for the Landiras-1 wildfire (Fig. 7) using all models.”

L225-226 “Indeed, we found that PC1 was strongly correlated with both temperature and rainfall anomalies over a large portion of western Europe (see Fig. S4 in the Supplement).” Interesting but this is a new result in the Discussion section, please move to results.

Response: We agree with the reviewer. We have moved this statement (including the reference to Fig. S4) from the Discussion to the Results section (revised manuscript, p. 8, L188–189).

L232 “45◦N (see Fig. S5 in the Supplement).” Again a new result in the Discussion, these need to be introduced in the Results.

Response: Done. We moved it to the Results section (revised manuscript, p. 9, L192–194).

L241 “The unprecedented levels of FWI in 2022” please rephrase to something like: “The exceptionally high FWI values observed [...]” in the original it is not clear if it is high or low.

Response: Yes, we clarified the direction of the anomaly. The text now reads as:

Revised text p. 15, L253-254: “The exceptionally high FWI values observed in 2022 in SW France, whether sampled locally or regionally, were conducive to a series of wildfires, with larger wildfires associated with larger FWI anomalies.”

L243 “their highest amplitude on the day or week of the wildfires” please rephrase, the day or week of ignition? As stated before, some of these larger fires burned for multiple weeks so it is important to be clear here.

Response: Right. The lead/lag plot is relative to the starting day of wildfires. We have revised the sentence to clarify that FWI peaks typically coincide (on average) with the day of ignition or the week surrounding it.

Revised text p. 15, L254-257: “We found that FWI levels reached their highest amplitude on the day of ignition (or the week surrounding it), boosted by either synoptic-scale heat waves or local wind bursts, as shown in previous studies over southern Europe (Ruffault et al., 2020) and France (Barbero et al., 2020; Pimont et al., 2021)."

Figures

Figure 1 Please write burned area (BA) in full in the legend title and y-axis label in panel b. Please remove the coordinates of the bounding box in panel a, they partly overlap with the fire locations and some are unreadable.

Response: Thanks for those suggestions that improved the figure.

Revised figure (Fig. 1):

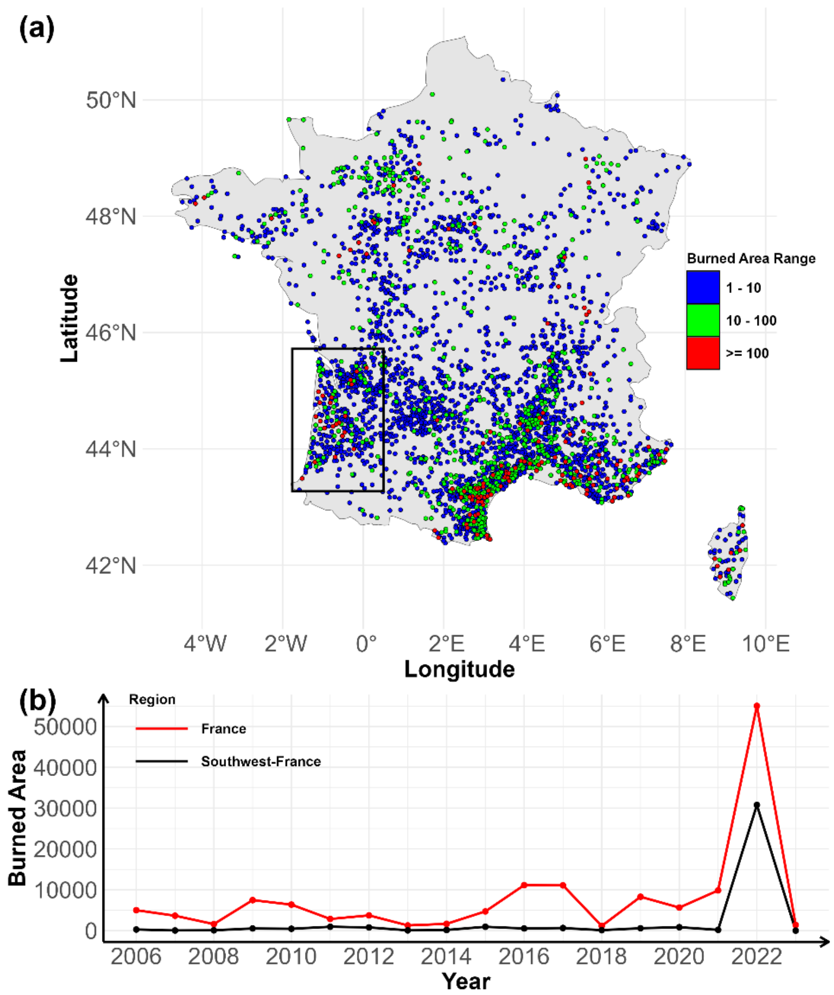

Figure 1. (a) Wildfires ≥ 1 ha recorded in the BDIFF database from 2006 to 2023. Color denotes burned area (BA) classes and the black box delineates the southwestern France region (~ 4.9×104 km2). (b) Total burned area

during the warm fire season (May–September) for France (red line) and the southwestern region (black line).

Figure 3 many abbreviations in the figure make it very hard to interpret, EOF FWI PC#1, it makes it unnecessary technical. Please reconsider changing the text in this figure that a wide range of audiences immediately can grasp what the figure is showing. Also, it is totally unclear to me what a high EOF or low EOF means.

Response: We understand the suggestion but for the sake of readability, we prefer to stick with acronyms and provide the full term in the caption, otherwise the figure would be too busy. Regarding the meaning of EOFs, we clarified this point above in our response to RC1's comment on L168–169 and in the revised manuscript (p. 8, L179–184).

Revised caption (Fig. 3): Leading two modes of mean May–September Fire Weather Index (FWI) over France from 1959 to 2023. (a) First empirical orthogonal function (EOF) (variance explained 62 %) with (b) its corresponding principal component (PC-1) time series. (c) Second EOF (variance explained 13 %) with (d) its corresponding PC-2 time series.

Figure 4 Please write FWI in full on y axis (fire weather index)

Response: Done.

Revised figure (Fig. 4):

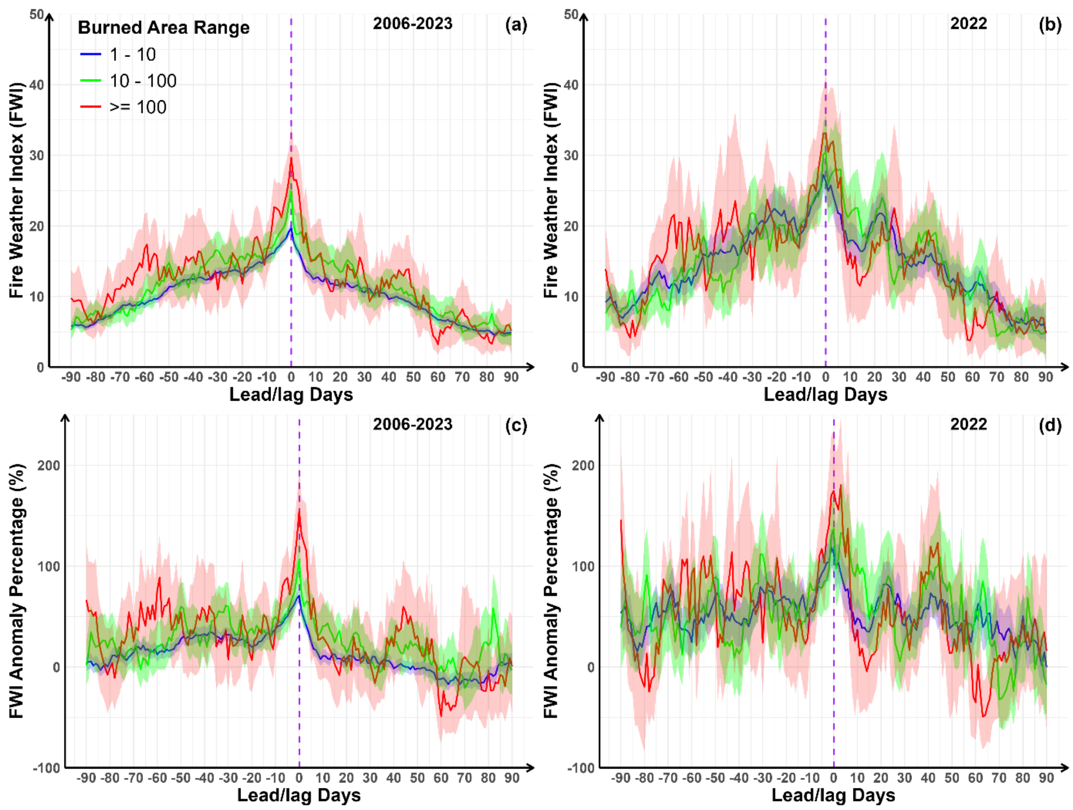

Figure 4. Lead–lag time series of FWI (a, b) and percent anomalies (c, d) relative to wildfire dates for three fire size classes over 2006–2023 (a, c) and 2022 only (b, d) in SW France. Anomalies were computed relative to the long-term (1959–2023) mean local seasonal cycle. Blue, green, and red curves denote BA = 1–10 ha, BA = 10–100 ha, and BA ≥ 100 ha, respectively. Shaded bands indicate 95 % bootstrap confidence intervals. The x-axis shows lead/lag days from −90 to +90 relative to the wildfire starting day (day 0; purple dashed line).

Author Responses to Referee RC2

Summary

This paper describes an event attribution analysis of fire weather conditions associated with three 2022 fires in the southwest of France and how the likelihood of these conditions changes under a future warming scenario. The authors consider multiple event definitions to test the sensitivity of their results. I find the concept of this study to be worthwhile and the manuscript was generally well written and easy to follow. My main concerns are about the robustness of the results.

General comments

How do you account for the (sometimes large) difference in resolution between the CMIP6 models and the observations?

Response: We agree that the resolution difference between the observational datasets and the CMIP6 models warrants more explanation. In our analysis, the observational data (SAFRAN at 8 km) is used solely to estimate the exceedance probability (p_{OBS}) of a given wildfire; it is not used for a direct comparison with, or evaluation of, CMIP6 outputs. The attribution analysis is then carried out independently with each CMIP6 model by comparing the ALL and NAT simulations at the model's native resolution. Because the key result relies on the ratio of p_{ALL} to p_{NAT} (and not a direct model–observation comparison), no downscaling was necessary. We clarified in the Methods section.

Inserted at p. 6, L119 : "Each CMIP6 model is analysed at its native spatial resolution."

Revised text p. 7 , L145-154: "To quantify the impact of anthropogenic climate change (ACC), we employed a commonly used approach to calculate the exceedance probability of each wildfire-related FWI (p_{OBS}) (Barbero et al., 2020), following the procedure applied in the previous section to the 1959–2023 SAFRAN observations. Here, SAFRAN is used only to estimate the exceedance probability of the observed event (p_{OBS}). We then compared the exceedance probabilities under two scenarios: (i) the ALL scenario, including all anthropogenic and natural forcings (hereafter p_{ALL}), and (ii) the NAT scenario, which includes only natural forcings (hereafter p_{NAT}). For the GEV distribution fitted to the ALL-scenario simulated annual maxima of the MA-FWI time series, we inverted its cumulative distribution function (CDF), $F_{ALL}(x)$, to find the FWI level in the p_{ALL} scenario such that $1 - F_{ALL}(FWI_{ALL}) = p_{OBS}$. We then applied this same threshold FWI_{ALL} to the GEV distribution fitted to the NAT-scenario simulated annual maxima of the MA-FWI time series – using its CDF $F_{NAT}(x)$ – to compute $p_{NAT} = 1 - F_{NAT}(FWI_{ALL})$. Because the attribution metric relies on the within-model ratio between p_{ALL} and p_{NAT}, no spatial downscaling was required."

Inserted at p. 7, L147: “Here, SAFRAN is used only to estimate the exceedance probability of the observed event (p_{OBS}).”

Revised text p. 12, L218-220: “Finally, we examined how ACC altered the probability of those FWI conditions. Figure 6 illustrates, for one model (NorESM2-LM, r1i1p1f1) and one spatiotemporal set-up (local and fire duration), how p_{NAT} varies relative to the reference probability p_{OBS} (= p_{ALL} by construction).”

If I understand correctly, this study calculates the risk ratios for four different model realizations individually and then takes the median across these results. My concern is that the uncertainty for each is high and the range across the models is large, so when the median is reported without an uncertainty range, it conveys a level of confidence and precision that I don’t think is there.

Response: We agree with the reviewer that the uncertainty within each individual model as well as the uncertainty across the models is substantial. In the original Figure 7, we chose to display only confidence intervals for the four individual model realizations, as adding an additional uncertainty band for the multi-model median in the same panel would have made the figure difficult to read. However, we added a supplementary figure (see new Supplementary Fig. S6 below) showing the multi-model median RR together with its pooled ensemble uncertainty range. For each year, we pooled together the bootstrap replicates from all models into a single set of 400 samples (4 models × 100 bootstrap replicates each), computed the 5th and 95th percentiles of the pooled set as the lower and upper bounds of the uncertainty range. This pooled approach is intended to reflect both the within-model sampling uncertainty and the spread across models.

Revised text p. 7, L168-175: “To quantify the sampling uncertainty surrounding RR and FAR, a parametric bootstrap approach was implemented as follows:

(1) Generate new samples of ALL and NAT scenarios from the estimated non-stationary GEVs.
(2) Re-estimate the non-stationary GEVs based on these new samples and compute the RR and the FAR.
(3) Repeat steps 1–2 100 times to derive model-specific parametric confidence intervals.

Finally, attribution scores (RR and FAR) from individual models were aggregated across models using a multi-model median. In that case, we pooled 100 bootstrap replicates from each of the four models. This pooled ensemble range is intended to reflect both within-model sampling uncertainty and the spread across models.”

New Supplementary Figure (Fig. S6):

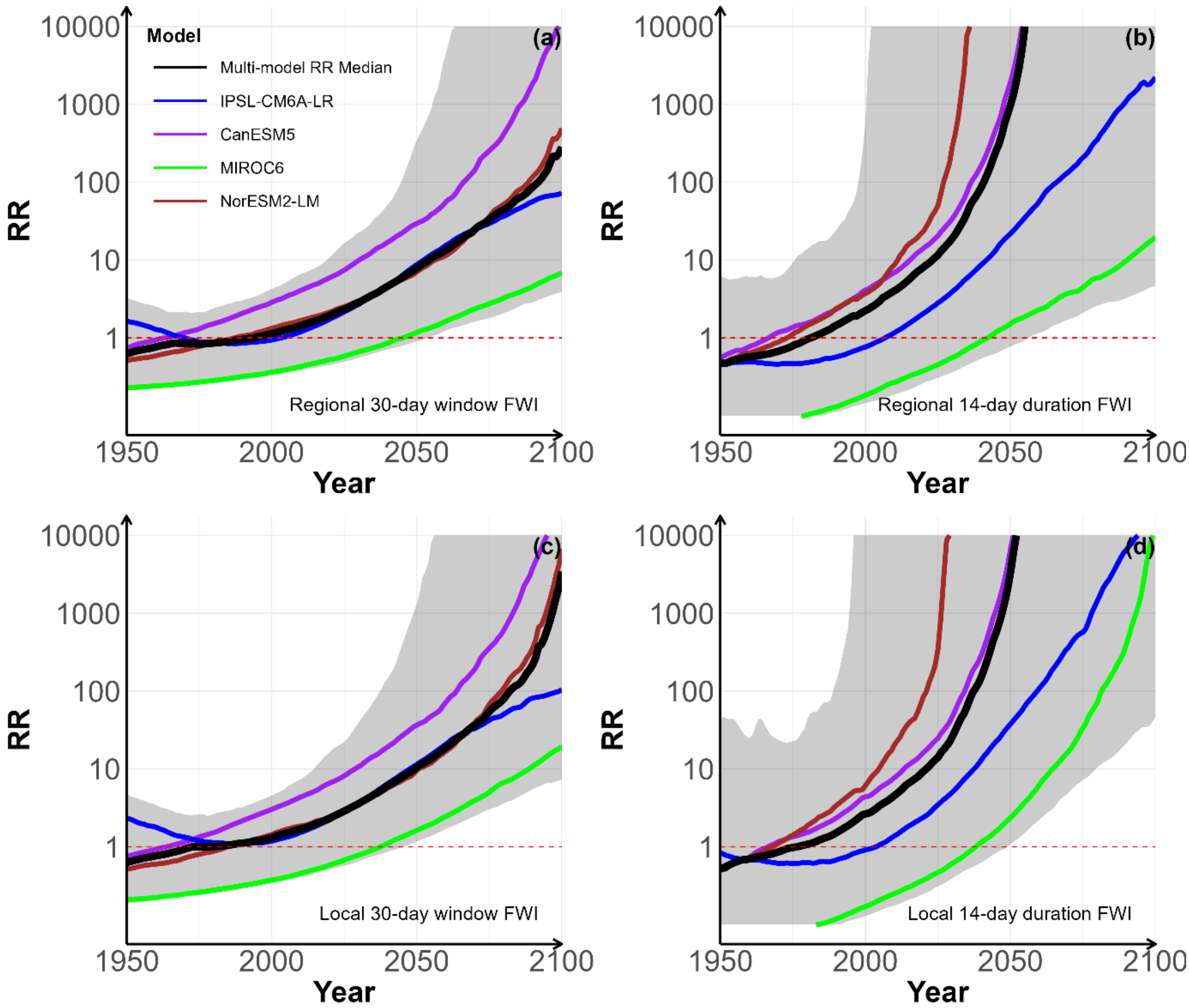

Figure S6. Same as figure 7 except that the uncertainty range was computed for the multi-model median RR. For each year, the grey shaded envelope denotes the corresponding 5th–95th percentile range of the pooled bootstrap replicates from four models (4 × 100 = 400 samples). This pooled range reflects both within-model sampling uncertainty and inter-model spread.

We also revised Figure 8. In the original manuscript, the confidence intervals for the multi-model median FAR were computed by taking, for each bootstrap replicate, the median FAR across the four models, thereby forming a set of 100 cross-model median values, and then using the 5th and 95th percentiles of that set as the confidence bounds. While this approach quantifies the uncertainty of the median itself, it does not fully retain the within-model variability and may therefore underestimate the total uncertainty. We have now replaced this with the same pooled ensemble uncertainty range method described above. The updated Figure 8, its caption, and the related text in the manuscript have been revised accordingly to clarify this treatment of uncertainty.

Revised figure (Fig. 8):

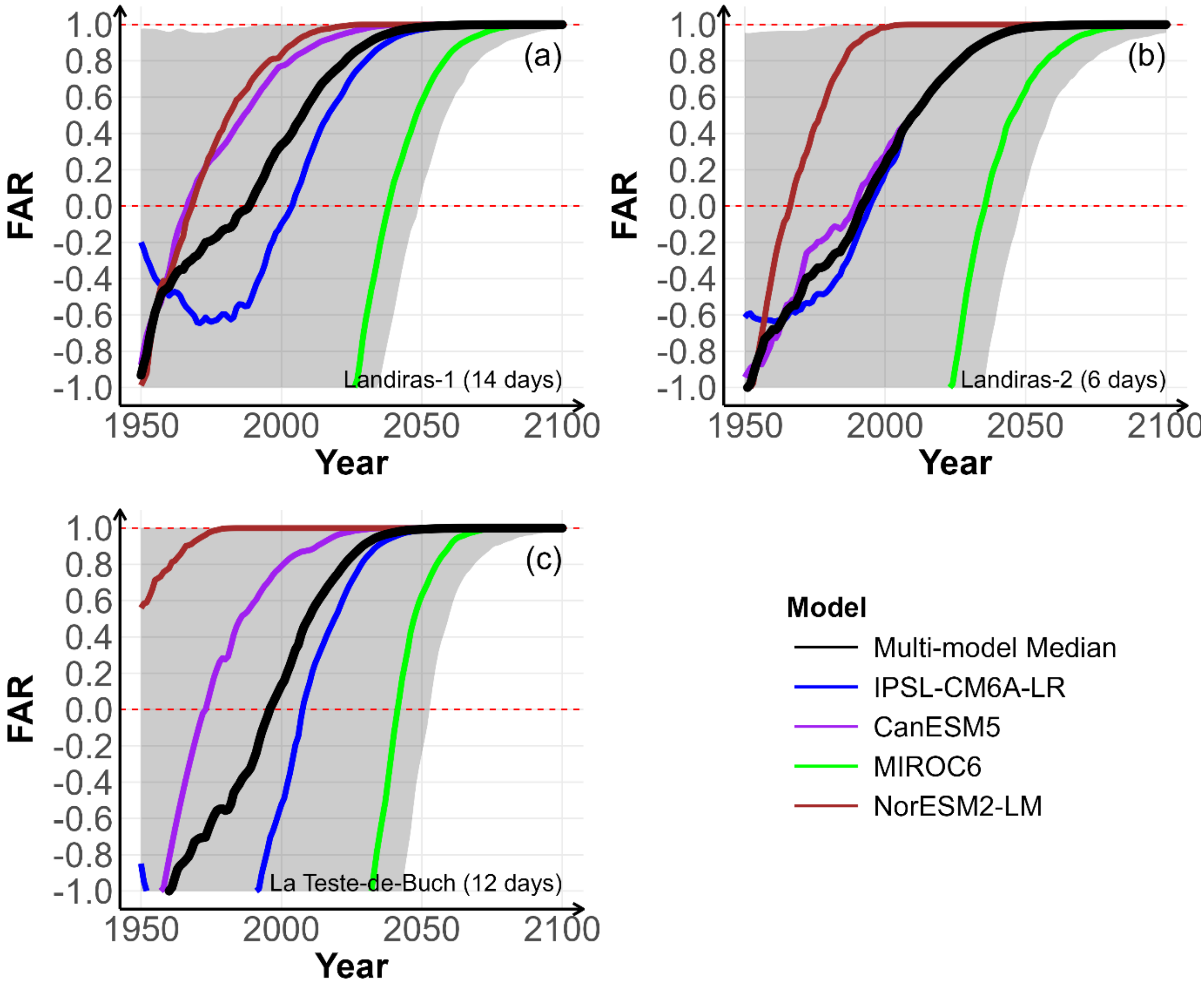

Figure 8. Fraction of attributable risk (FAR, 1 − 1/RR) using the local FWI over the event-duration window for (a) Landiras-1 (14-day), (b) Landiras-2 (6-day), and (c) La Teste-de-Buch (12-day). The black curve shows the multi-model median across models. The y-axis is truncated to [−1,1]; red dashed lines indicate FAR = 0 (no anthropogenic contribution) and FAR = 1 (fully attributable). The shaded envelope indicates the 90 % pooled ensemble uncertainty range for the multi-model median, obtained by pooling the bootstrap replicates from the four models and taking the 5th and 95th percentiles of the pooled distribution. This approach captures both within-model sampling uncertainty and inter-model spread.

As stated by the reviewer, the uncertainty (whether sampled at the model level or across models) is quite large and we believe that this deserves to be emphasized throughout the text. We mentioned it in the Results and Discussion sections.

Revised text p. 15-16, L278-281: "Our study suggests that climate change increased the risk of such conditions by 2–10 times in 2022 and will continue to do so by several orders of magnitude by the end of the twenty-first century under a medium-level radiative forcing scenario. However, the pooled ensemble uncertainty range indicates substantial uncertainty across and within models (see Fig. S6)."

It was not clear why this study was limited to only one realization from each of four CMIP6 models. This limited sample size for fitting extreme value distributions results in larger uncertainty. If calculating the FWI is limiting, there are datasets available where this computation has already been performed (e.g., [https://doi.org/10.5194/essd-15-2153-2023]).

Response: We agree that relying on a single realization per model limits our ability to characterize within-model (internal) variability, and we acknowledged this limitation more explicitly in the revised manuscript. Our choice was primarily driven by data availability and by the need to maintain a strictly comparable experimental design across models. For NorESM2-LM, the publicly accessible CMIP6 archive provides only one realization (r1i1p1f1) for the ssp245-nat experiment. To avoid mixing models with unequal ensemble sizes, we therefore used a single member for all four models; otherwise, RR/FAR curves from models with more available members would appear artificially smoother simply because they are based on more simulations. In addition, each realization represents a distinct and internally consistent climate trajectory associated with a specific set of initial conditions, and pooling members through an average or median would break this physical consistency. If multiple realizations mainly differ by initial conditions, simply averaging them would tend to smooth variability and extremes, rather than necessarily reduce the uncertainty of the GEV fit. We nevertheless acknowledge that this choice limits the characterization of within-model variability, and we clarified this point in the Data and Methods section.

Inserted at p. 6, L119: "Each CMIP6 model is analysed at its native spatial resolution. For each model, we used a single member (r1i1p1f1) in both the ALL and NAT experiments. This choice was primarily motivated by data availability: for NorESM2-LM, only one realization was publicly available for the ssp245-nat experiment in the CMIP6 archive. For consistency, we used a single member for all four models."

Regarding the dataset suggested by the reviewer (Quilcaille et al., 2023), we agree that it is a valuable resource, but it does not include natural-forcing-only experiments (e.g., hist-nat or ssp245-nat), which are essential for our attribution analysis. In addition, it only provides annual-scale FWI indicators rather than the daily time series we need to estimate FWI levels across multiple temporal scales.

Related to the previous comment and considering the different climate sensitivities of the models, would using GSAT as a predictor in the nonstationary distribution instead of year allow for a more even comparison between models?

Response: We thank the reviewer for this insightful suggestion. Using GSAT as a covariate instead of year is indeed an appealing idea, because it places models with different climate sensitivities on a more common physical basis. Following this suggestion, we explored an alternative formulation of the nonstationary GAMLSS-GEV in which each model's 30-year smoothed GSAT time series (see Fig. R1) was used as the covariate.

Figure R1:

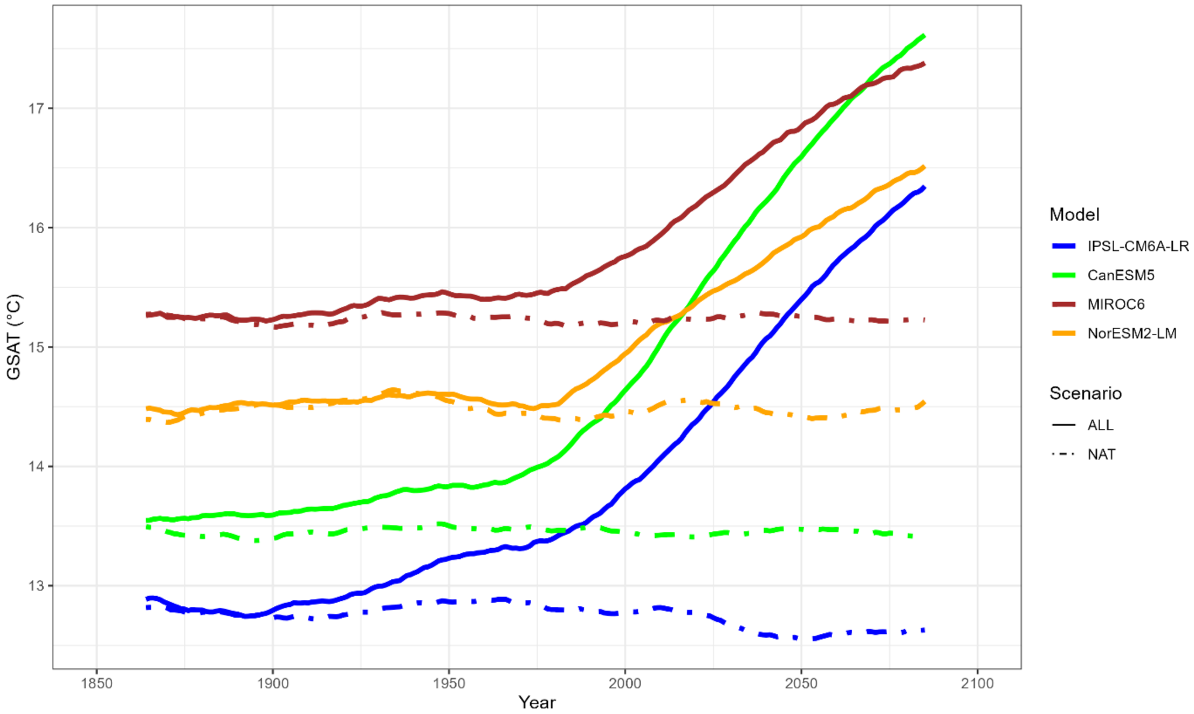

Figure R1: Global Surface Air Temperature (GSAT) across multiple CMIP6 models under the ALL (anthropogenic + natural forcing) and NAT (natural-only forcing) scenarios for the r1i1p1f1 simulation. The GSAT time series are smoothed using a 30-year moving average to highlight long-term trends only.

However, we found that RR (Fig. R2) and FAR (Fig. R3) estimates were substantially more variable from one year to another with several erratic spikes. This may arise from the asymmetry of the covariates (GSAT ALL vs GSAT NAT) whereas both ALL and NAT were fitted against the same covariate when using the calendar year. We thus prefer to stick with the year-based approach in the main paper.

Figure R2:

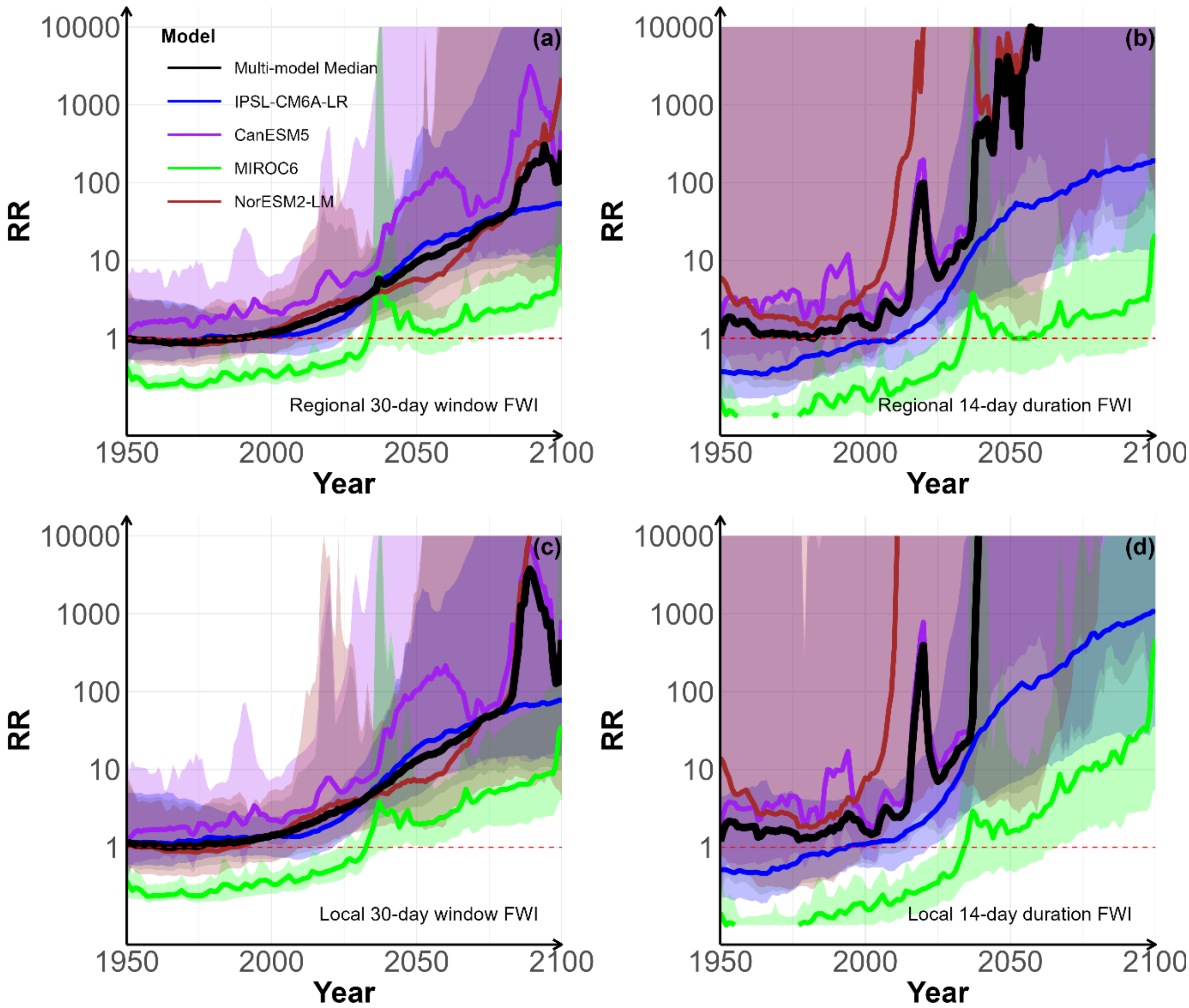

Figure R2. Same as Fig. 7, except that the risk ratio (RR) of FWI conditions associated with the Landiras-1 wildfire was estimated from GAMLSS-GEV models fitted to annual maxima MA-FWI using each model's global surface air temperature (GSAT), rather than year, as a covariate.

Figure R3:

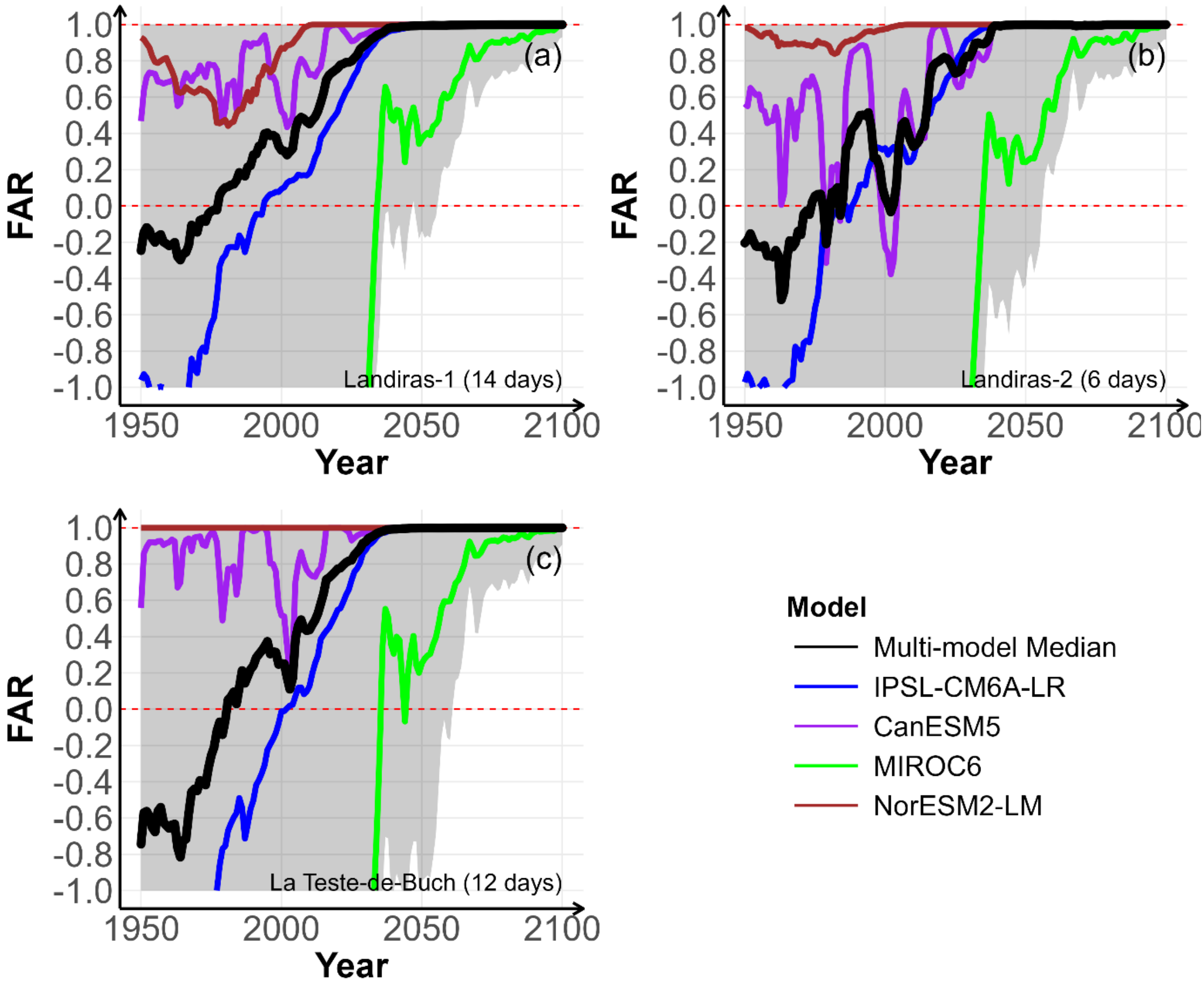

Figure R3. Same as Fig. 8 except that the fraction of attributable risk (FAR) was estimated from GAMLSS-GEV models fitted to annual maxima MA-FWI using each model's global mean surface air temperature (GSAT), rather than year, as a covariate.

For the Landiras fires, how do you reconcile conducting the analysis with the annual maximum when the maximum value could only come from one of these events?

Response: We appreciate the opportunity to clarify that the annual maxima MA-FWI for 2022 is used solely as the annual extreme input for fitting the GEV distribution, and is not intended to represent the fire weather conditions of any individual wildfire. In Figure 5, we indicated the fire weather level of each of the three wildfires (FWI aggregated over the burning duration) but those numbers were not used in the GEV fitting. They are therefore distinct from, and not equivalent to, the 2022 annual maxima MA-FWI used in the GEV fitting. We clarified in the Data and Methods section to avoid any ambiguity.

Revised text p. 6, L141-143: "Note that the annual maxima of the MA-FWI time series were used only as annual extreme inputs for fitting the GEV distribution, and do not represent the fire weather conditions of any individual wildfire. The FWI level observed during each wildfire is therefore distinct from the annual maxima used in the GEV fitting."

Specific comments

L30: Does "since the 1940s" mean that there was a larger fire in the 1940s or is this when the comparable records start?

Response: Indeed, this was ambiguous. Our intention was to refer to a specific fire: the Landes forest fire of August 1949 in the Gironde department, which burned approximately 50,000 hectares and remains the largest wildfire on record in France. To avoid this confusion, we have revised the text and added a reference.

Revised text p. 2, L30-32: "When combined, the Landiras-1+2 wildfire burned over 19,676 ha, which makes it the largest wildfire in France since the Landes forest fire of August 1949 (Sarrau and Yagoub, 2025)."

Sarrau, J. and Yagoub, M. M.: Documentation of Historical Forest Fires and Hazard: Case of Gironde and Les Landes, France, ISPRS Ann. Photogramm. Remote Sens. Spatial Inf. Sci., X-G-2025, 771–778, https://doi.org/10.5194/isprs-annals-X-G-2025-771-2025, 2025.

L45: Should North America here be the western United States?

Response: Absolutely, the cited studies actually focused on the western United States.

Revised text p. 2, L46-49: "However, fire weather conditions (combining multiple meteorological variables) have received less attention, although a number of efforts have been made in the western United States (Abatzoglou and Williams, 2016; Williams et al., 2019; Brown et al., 2023), Canada (Kirchmeier-Young et al., 2019a), Australia (van Oldenborgh et al., 2021), and France (Barbero et al., 2020; Lanet et al., 2024)."

L76: The reference for the Canadian FWI System should be Van Wagner (1987)

Response: Good catch. We corrected.

L93: What happens if a fire covers more than one grid cell?

Response: This is a good question. Because the BDIFF dataset provides only the city of the fire and not the exact fire perimeter, each fire was matched to the nearest SAFRAN grid cell. A wildfire may indeed cover multiple grid cells but this should not happen for the vast majority of fires, given the size of the SAFRAN grid cell (64 km^2 = 6,400 ha). Moreover, sampling additional neighboring grid cells is not expected to change much the results given the strong spatial autocorrelation of the FWI, including at daily scale. We clarified this in the Data and Methods section and discuss this limitation explicitly in the revised manuscript.

Revised text p. 5, L94-98: "To quantify the relationship between local FWI conditions and wildfire events, we extracted for each wildfire the daily FWI time series from the nearest SAFRAN grid cell, over a window extending from 90 days before to 90 days after the wildfire start. Note that BDIFF does not provide wildfire perimeter and that multiple grid cells may potentially intersect with the actual wildfire perimeter. However, this effect should be limited given the size of the SAFRAN grid cell (64 km^2) and the inherent spatial autocorrelation of FWI."

Figure 2: Should the "=" between FWI_ALL and FWI_NAT be an arrow instead?

Response: Good catch. We have modified Figure 2 accordingly.

Revised figure (Fig.2):

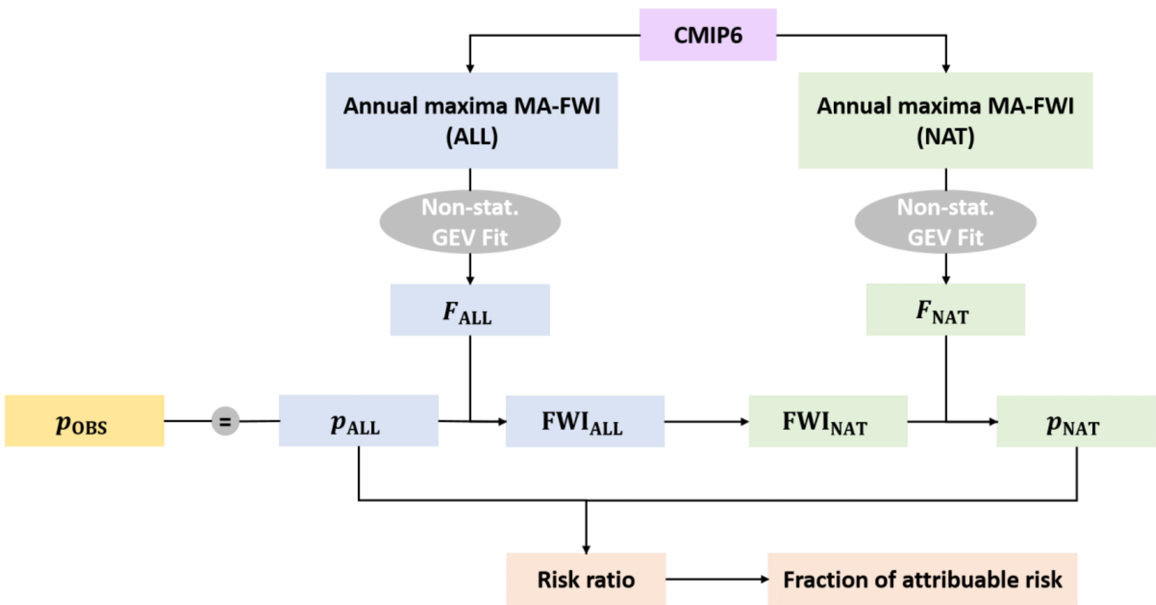

Figure 2. Schematic workflow used to estimate exceedance probabilities (denoted by p) under ALL and NAT forcings from CMIP6 annual maxima MA-FWI: a non-stationary GEV fit provides the cumulative distribution functions (CDFs) $F_{ALL}(x)$ and $F_{NAT}(x)$ (where F denotes the fitted GEV CDF); FWI_{ALL} is obtained by inverting F_{ALL} such that $1 - F_{ALL}(FWI_{ALL}) = p_{OBS}$, and p_{NAT} is then computed as $1 - F_{NAT}(FWI_{ALL})$.

L184: Could this be related to an early start to the fire season?

Response: Absolutely. The persistent positive FWI anomalies in the months preceding wildfire may partly reflect an earlier onset of the fire season, in addition to antecedent dry conditions. We clarified in the revised manuscript.

Inserted at p. 9, L202: "These persistent pre-wildfire positive anomalies may reflect not only prolonged antecedent hot and dry conditions, but also, to some extent, an earlier seasonal onset of the fire weather season."

Figure 4: Suggest making the green shading darker to increase visibility.

Revised figure (Fig. 4):

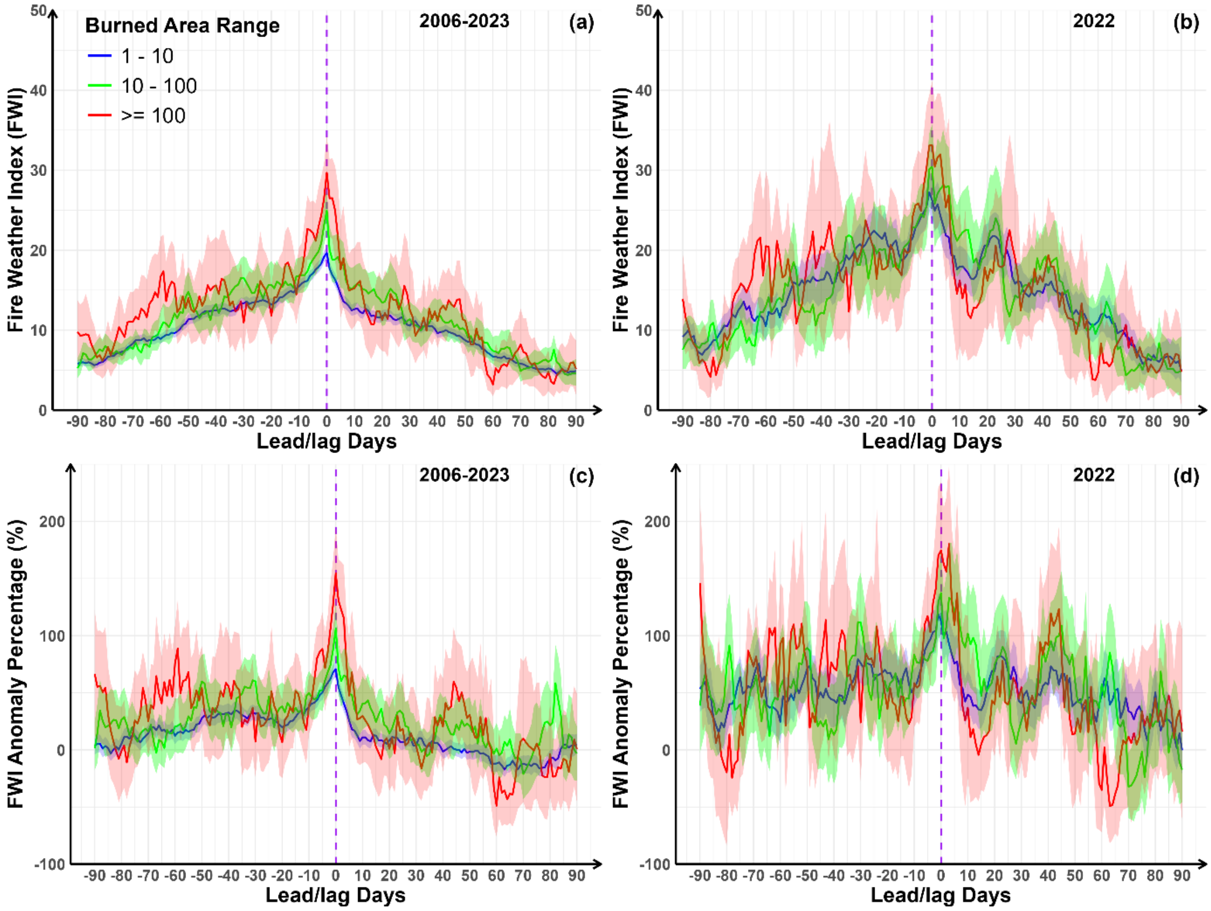

Figure 4. Lead–lag time series of FWI (a, b) and percent anomalies (c, d) relative to wildfire dates for three fire size classes over 2006–2023 (a, c) and 2022 only (b, d) in SW France. Anomalies were computed relative to the long-term (1959–2023) mean local seasonal cycle. Blue, green, and red curves denote BA = 1–10 ha, BA = 10–100 ha, and BA ≥ 100 ha, respectively. Shaded bands indicate 95 % bootstrap confidence intervals. The x-axis shows lead/lag days from −90 to +90 relative to the wildfire starting day (day 0; purple dashed line).

L196: These are ranges of best estimate values from the different definitions, not uncertainty ranges. It would be helpful to clarify this in the text here.

Response: Agreed. We corrected to avoid any misunderstanding.

Revised text p. 10, L212-215: "Overall, the rarity of those conditions also increases with the resolution (Fig. 5, right panels), with the best-estimate RPs increasing from ≈6 to ≈34 years, from ≈22 to ≈38 years, and from ≈6 to ≈101 years when moving from the coarsest to the finest spatiotemporal scale for Landiras-1 (Fig. 5b), Landiras-2 (Fig. 5d), and La Teste-de-Buch (Fig. 5f) wildfires, respectively, illustrating how sensitive the RPs are to the chosen scales."

Figure 6: I found this figure and the accompanying text to be confusing. Would the observed probability (and subsequently p_{ALL}) not change through time as well? One suggestion would be to choose the FWI_{ALL} threshold based on the $p_{ALL} = p_{OBS}$ in 2022, but to then use the same FWI_{ALL} threshold for all other years. This might better reflect the probability of those fire conditions in different years.

Response: We tried to clarify. Figure 6 was designed to illustrate the logic underlying the RR computation in Figure 7, not to depict how the probability of exceeding a fixed 2022 FWI threshold evolves over time. In our framework, $p_{ALL} = p_{OBS}$ is used as a reference, and the figure illustrates how p_{NAT} varies relative to that reference, from which the attribution metrics are derived. So yes, the observed probability and subsequently p_{ALL} do not change with time. We revised the figure caption and accompanying text to avoid misunderstanding.

Revised caption (Fig. 6): Example of the NAT-only exceedance probability for the Landiras-1 wildfire (local and fire-duration set-up) using NorESM2-LM (r1i1p1f1): the NAT-only exceedance probability p_{NAT} (black; median) is shown relative to the reference probability $p_{OBS} = p_{ALL}$ (blue) that does not change with time. Shaded envelope indicates the 80 % parametric-bootstrap confidence interval.

Revised text p. 12, L218-220: "Finally, we examined how ACC altered the probability of those FWI conditions. Figure 6 illustrates, for one model (NorESM2-LM, r1i1p1f1) and one spatiotemporal set-up (local and fire duration), how p_{NAT} varies relative to the reference probability p_{OBS} (= p_{ALL} by construction)."

L242: "proportional" here implies a linear relationship. Was this intended or could a different word be chosen?

Response: We agree that it was a poor choice of words. Our analysis only shows that larger wildfires are associated with larger FWI anomalies, based on a composite analysis and not a formal linear fit. We therefore revised the wording.

Revised text p. 9, L198-199: “Figure 4a shows that FWI increases until the wildfire day and decreases in the following days, with higher FWI values for larger wildfires.”

Revised text p. 15, L253-254: “The exceptionally high FWI values observed in 2022 in SW France, whether sampled locally or regionally, were conducive to a series of wildfires, with larger wildfires associated with larger FWI anomalies.”

L296: The use of only a single ensemble member needs to be mentioned much earlier in the manuscript.

Response: We agree with this point. We addressed it above in our response to the previous General Comment on the use of a single ensemble member, and we now mention it much earlier in the revised manuscript (p. 6, L119–122).