Reply to reviewer comments (original reviewer comment presented in bold)

This is my second review of this study, following the first round of revisions. First, the authors have greatly improved the narrative of the manuscript. The analysis is now more aligned with the results, and the claims are more accurately reflecting the actual model performance. Additionally, references to existing literature are better contextualised.

On the other hand, I believe that some of my comments related to methodological aspects have not been addressed. I previously raised these comments as "Minor", since I thought that they only required some clarification. While the authors have expanded the descriptions, some of the methods are still unclear, confusing, and mathematically inconsistent. This raises concerns about potential fundamental issues with the results. For this reason, I now raise the methodological issues of Section 2.2 as a Major comment. In addition, this review includes a Minor comment about the length and repetitive structure of the manuscript, and some Specific comments. I encourage the authors to address my comments, as well as potential comments from other reviewers, to make their manuscript up to the standards of Earth System Dynamics. Line numbers refer to the revised manuscript without tracked changes.

We thank the reviewer for the careful and constructive second review of the manuscript, and we appreciate the positive feedback.

We acknowledge the concerns raised regarding the methodological aspects, particularly in Section 2.2. We have revisited this section in detail to clarify the formulation, improve consistency, and ensure that all steps and assumptions are clearly described. The text has been revised to reduce ambiguity and improve readability, as we agree that clarity in the methodology is essential.

Regarding the comment on the length and repetitive structure, we have revised the Discussion & Conclusions, as well as the Introduction, reducing repetition and improving the focus while retaining the necessary context.

We thank the reviewer for the detailed constructive comments and have addressed all specific points in line below.

Major comment: Section 2.2

As mentioned above, these are mostly comments that I already made in my first review, and that have not been properly addressed.

(a) The burned area is "normalised" (L211). Please specify with respect to what it is normalised. If this refers to removing the mean and dividing by the standard deviation, then this process is called standardisation, not normalisation.

Thank you for this helpful comment. We agree that the term "normalised" was previously ambiguous. We have now clarified this in line 185 of the latest revised manuscript by explicitly stating that normalisation is applied independently to both variables (FWI and BA) over the study period using the min–max scaling method. We have also included the corresponding mathematical formulation for clarity.

(b) Equation (2). This equation suggests a detrending with respect to time, as t is the predictor. It does not regress out FWI, since this variable is not used as predictor ofdBA. Instead, the authors seem to make the regression parameter (βFWI) a function of FWI. First, this approach is a needlessly complex manner to regress out FWI from BA. Second, the explanation of how βFWI is computed is unclear (L212): "A linear regression of the FWI time series is fitted to estimate the local climate-driven trend". Is fitted to what? What are the predictor and predictand of this linear fit? Third, Why do the authors not simply regress out FWI using the following linear fit?

$dBA_{i,j}(t) = \beta_{i,j}FWI(t) + \alpha_{i,j}$

We thank the reviewer for pointing out the potential ambiguity in Equation (2). We clarify that our approach is not a regression of burnt area on FWI but rather uses FWI as a proxy for climate-driven fire risk. By fitting the FWI time series against time, we capture the temporal FWI trend that is mostly driven by climate variability. This is then assumed to represent the climate-driven component of the trend in burnt area. Subtracting this component removes the climate-driven influence, allowing us to isolate other factors affecting burnt area. We have revised the text to make this procedure and the meaning of $\beta_{i,j}^{FWI}$ explicit, including the linear fits and their predictors, to avoid misinterpretation. This can be found in lines 187 to 197 of the latest revised manuscript

(c) Remaining correlation between FWI and deweathered BA. Figure 2a shows that the correlation between FWI and deweathered BA is different from zero. This proves the point above: FWI has not been properly regressed out of BA. When a predictor is linearly regressed out of a variable, then the Pearson correlation between the predictor and the residuals is zero by definition.

We thank the reviewer for this comment. We note that our approach is not intended as a standard regression of burnt area on FWI, but rather as a removal of the climate-driven trend in FWI over the study period. Specifically, we estimate the temporal trend in FWI at each grid cell and use it to construct the climate-driven component in burnt area, which is then subtracted to obtain the deweathered time series of burnt area.

Because our method only removes the trend component of FWI over time, it does not remove all variability in FWI from the time series of burnt area. Therefore, the deweathered time series of burnt area retains correlation with the residual fluctuations of FWI around its trend. The Pearson correlation between FWI and the deweathered burnt area is not expected to be zero.

The aim of our approach is solely to isolate and remove the climate-driven trend in burnt area, while preserving residual variability that may be influenced by other factors such as human activity or land management. We have revised the manuscript, line 200, to clarify this point in the methods section.

(d) Significance testing in Figure 2. In the previous manuscript version, the authors had not implemented any correction for multiple hypothesis testing in their significance assessment in Figure 2. I raised this issue in my first review, and the caption now specifies that "Stippling indicates grid points where the Pearson correlation is statistically significant at the 5 % level after controlling for the false discovery rate using the Benjamini and Yekutieli (2001) procedure". However, I compared the updated Figure 2 with the one of the previous manuscript version that had no false discovery rate correction. It appears that the stippling has not changed. This is impossible if the false discovery correction is applied. I hope this is an oversight from the authors.

We thank the reviewer for pointing this out and apologise for the oversight. Indeed, the stippling in Figure 2 was not updated in the previous revision to reflect the false discovery rate correction. This has now been corrected, and the figure in the revised manuscript properly shows grid points that are statistically significant at the 5 % level after controlling for the false discovery rate using the Benjamini and Yekutieli (2001) procedure. As a result of this correction, most grid points are not statistically significant. The corresponding text (lines 204 to 207) has also been revised to reflect this updated assessment of significance.

(e) Bayesian linear regression of BA on HDI. The authors have expanded the description of their Bayesian linear regression method. Worryingly, the additional descriptions do not clarify the confusion. The equation concerned is:

$\log(BA*) \sim BA0 + \delta BA \times HDI$

There are many inconsistencies with respect to this equation.

- The authors write that they assign the prior (L245) "δBA ~ LogNormal(0, 10)". This means that δBA is constrained to be positive (elementary property of the LogNormal distribution). In turn, this implies that log(BA∗) increases with HDI, and therefore that BA∗ increases with HDI. However, Figure 3 shows that BA∗ decreases with HDI.

- The regression equation shown in Figure 3 is: y = −6.57x+19.42. Since the authors write in the caption that "the results are presented here in natural space for interpretability", I interpret this equation as: BA∗ = 19.42−6.57×HDI. If we plug in the value HDI = 1, this gives BA∗ = 12.85%. However, the fit in Figure 3 shows that BA∗ ≈ 0.1% at HDI = 1. This issue is not because of the interpretation of natural- versus log-space, because if log(BA∗) = 12.85, then BA∗ ≈ 4 × 105%, which is clearly unrealistic.

For all these reasons, I am concerned that there are fundamental problems with the deweathering of the BA values, and with the Bayesian regression on HDI.

Thanks for pointing out concerns about the Bayesian linear regression of BA on HDI. We agree that the previous version of the manuscript contained several inconsistencies and lacked clarity in the description and implementation of the Bayesian Linear Regression (BLR), which contributed to the confusion highlighted. In the revised manuscript, we have addressed the following issues:

1. Clarified the model formulation: The original equation was written using abbreviated notation and did not explicitly define the likelihood or residual term. We have now reformulated the model explicitly as in Equation 4.

$$\log\left(\mathrm{BA}_i^*\right) = BA_0 + \delta BA \cdot \mathrm{HDI}_i + \epsilon_i$$

   clearly specifying the predictor, predictand, and error structure.

   In addition, the reviewer correctly noted that substituting HDI = 1 into the reported relationship (e.g., BA^* = 19.42 - 6.57 x HDI) yields inconsistent values. This discrepancy arose from two issues in the previous manuscript: (i) an erroneous transformation of the regression results to natural space, and (ii) the resulting misrepresentation of the intercept term BA_0.

   Importantly, the regression itself was correctly implemented in log-space. This is evident when comparing with Figure 7, where results were presented in log-transformed space and do not exhibit the inconsistencies highlighted above. The issue therefore lay in the presentation and natural space transformation of Figure 3, rather than in the underlying model.

   To resolve this, we now present all regression results consistently in log-space, ensuring direct correspondence between the mathematical formulation and the graphical representation.

2. Slope prior parameter: As correctly noted by the reviewer, the manuscript previously stated that $\delta BA \sim \mathrm{LogNormal}(0{,}10)$, which would constrain the slope to be strictly positive and is inconsistent with the observed decrease in burnt area with increasing HDI. However, this was an error in the description only. In the actual BLR implementation, the slope parameter was assigned a normal prior, $\delta BA \sim \mathcal{N}(0{,}10)$, allowing both positive and negative relationships.

   We have now corrected the manuscript to accurately reflect the model specification used in the analysis – lines 221 to 223 of the latest revised manuscript. As mentioned before, we confirm that this discrepancy was limited to the written description and does not affect the results presented.

3. Consistent treatment of the log-transformation: The revised manuscript (line 219) now explicitly states that burnt area is expressed as a fraction (0–1) prior to log-transformation, ensuring mathematical consistency and clarity in interpretation.

4. Correction of Figure 3: An error occurred when transforming the regression results back to natural space, leading to an inconsistency between the mathematical formulation and the

plotted relationship in Figures 3 and 6. This resulted in the mismatch identified by the reviewer.

To resolve this and avoid further ambiguity, we now present all regression results consistently in log-space. This ensures direct correspondence between the model equation and the visual representation, and avoids potential misinterpretation associated with back-transformations.

5. Improved description of Figures 3 and 6: The caption and axis labels of Figures 3 and 6 have been revised to clearly indicate that the results are shown in log-space, and to explicitly define the meaning of the plotted regression lines and uncertainty.

We sincerely thank the reviewer for their thorough and careful assessment of this section. Their comments were instrumental in identifying inconsistencies in the description and presentation of the BLR. We apologise for the inaccuracies in the original manuscript and appreciate the opportunity to clarify and correct these points. The revisions in Section 2.2 have now improved the clarity, consistency, and transparency of the analysis.

Minor comment: text length

The revised manuscript is excessively long. In particular, the Section 4. Discussion & Conclusions is 6 pages long. I strongly recommend to shorten Section 4 to maximum 3 pages, and to shorten other sections of the manuscript by removing details that are not necessary to the key messages of this study.

Section 4 can easily be shortened as there are a lot of repetitions. I list here some of them.

- L656-694: this is a repetition of the Results, and should be strongly shortened.

We thank the reviewer for this comment. We have revised this paragraph (now starting in line 625 in the revised manuscript) to reduce repetition and improve conciseness, while retaining the key message regarding the role of compensating regional biases in the apparent global performance.

- L685-L688: this is the same information as in L750-757.

We thank the reviewer for pointing out this duplication. The text in L685–L688 in the previous version of the manuscript has been removed, and the information is retained in L669–674 (previously lines 750 – 757) of the revised manuscript to avoid repetition and improve the flow of the manuscript.

- L704-712: this repeats almost exactly L116-124. The Discussion should not be a 2nd version of the Introduction.

We thank the reviewer for this comment. We agree that this text was repetitive of the Introduction and have therefore removed it to avoid duplication and improve the focus of the Discussion.

- L728-730: this is the same information as in L706.

We thank the reviewer for this comment. We agree that this text was repetitive and have therefore removed it.

- L741-745: this is the same information as in L676-680.

We thank the reviewer for this comment. We have removed this duplicated text (lines 741 – 745 of the previous version) to avoid repetition and to keep the Conclusions section focused and concise.

- 774-778: this is the same information as in L750-757.

We thank the reviewer for this comment. We agree that this text was repetitive and have therefore removed the text in lines 774 – 778 of the previous version of the manuscript.

- L800-806: this is the same information as in L702-712.

We thank the reviewer for this comment. We have removed (lines 800 – 806 of the previous version of the manuscript) to keep the Conclusions section focused and concise.

- L809-811 and L816-817: these two sentence are quasi identical, only a few lines apart.

We agree that the two sentences were quasi-identical, and we have therefore removed the repetition by deleting L816 – 817 lines of the previous version of the manuscript to improve clarity and conciseness.

In addition to these repetitions, a lot of unnecessary details can be removed from the Discussion, which often reads more as a literature review than a focused Discussion of the present study.

We thank the reviewer for the helpful suggestions regarding conciseness and repetition in the Discussion. We have revised the Discussion and Conclusions to reduce repetition and make the interpretation of results more direct and focused. This has substantially reduced this section. In addition, the Introduction has also been revised to remove redundant text, improve focus, and reduce repetition throughout.

We note that, while we have reviewed the Discussion to improve conciseness and reduce unnecessary repetition, we maintain links to existing literature. These references are used to place the model results from this study into context, support the interpretation of regional mechanisms, and highlight consistency or differences with previous studies. We consider this integration important for strengthening the interpretation and robustness of the model evaluation.

Specific comments

First, please note that I do not provide Specific comments on Section 4, as I expect the authors to thoroughly modify this section (see Minor comment).

Abstract. This abstract is too long. Please reduce it to a number of words typical of abstracts in Earth System Dynamics.

We thank the reviewer for this comment. We have revised the manuscript and shortened the abstract to bring it in line with the typical length and style of abstracts in Earth System Dynamics.

L30-33. These sentences should not be individual paragraphs.

Thank you for the suggestions, this has now been merged into a single paragraph in the revised version of the manuscript in lines 21 – 23 of the revised manuscript.

L47-48. This sentence has a wording issue due to "with...and...and".

Thank you for the suggestions, this has now been addressed in the revised version of the manuscript (lines 34 – 36).

L54. Remove "(mathematical representation of a real-world system)".

Thank you, "(mathematical representation of a real-world system)" has been removed in the revised version of the manuscript.

L55. Replace "data-driven" by observational.

Thank you for the suggestions, "data-driven" has been replaced by observational in the revised version of the manuscript (line 40).

L87. Rephrase to: "correlated with the magnitude of inter-annual variability in burned area", otherwise the statement is misleading. As it is now, the sentence suggests that inter-annual BA variability is temporally correlated with inter-annual HDI variability.

Thank you for the suggestions, this has been applied in the in line 68 of the revised version of the manuscript.

L109. “this is increasingly urgent” is a structural error in the sentence.

Thank you for the comment. This has been addressed in line 87 of the revised manuscript.

L155. “socio-economics” should be socio-economical.

Thank you for the comment. This has been addressed in line 130 the revised manuscript.

L177. Typo: “raging”.

Thank you for the comment. This has been addressed in the revised manuscript (line 132).

L166. I believe that this should be normalised GNI per capita.

Thank you for the comment. This has been addressed in the revised manuscript (line 141).

L171. I believe that the description of the derivation of the GDP data is irrelevant to this study, and can be removed.

Thank you, we agree and have removed this in the revised manuscript.

L182. Typo: “bunrt”.

Thank you for the attention, this has been addressed in the revised manuscript (line 155).

L195. Add a comma after “dataset”.

Thank you for the comment. This has been addressed in the revised manuscript (line 168).

L195. Use either “e.g.,” or “such as”, not both.

Thank you, we agree and have removed this in the revised manuscript (line 168).

L196. Replace “driven by” by from.

Thank you for the comment. This has been addressed in the revised manuscript (line 169).

L199. Replace “forces” by forcing.

Thank you for the comment. This has been addressed in the revised manuscript (line 172).

L201. Replace “These moisture codes” by These fuel moisture levels.

Thank you. “These moisture codes” have been replaced by These fuel moisture levels in the revised manuscript (line 173).

L232. “Southern Africa” should be Central Africa.

Thank you for the comment. This has been replaced in a revised version of the manuscript (line 208).

Figures 3 and 6. It is unclear what the blue and red curves represent. Are the red curves posterior samples? And what are the blue curves?

We thank the reviewer for this helpful comment. We agree that the previous caption did not clearly distinguish the different elements shown in the figure. We have revised the caption to explicitly define each component. In the updated Figure 3, the blue solid lines represent posterior predictive samples from the BLR, illustrating the range of plausible fitted relationships given the posterior distribution. The black dashed line represents the mean posterior fit. The red lines represent the uncertainty derived from the posterior predictive distribution.

L268. “weather”: this is not true, as fire spread is not decoupled from weather because FPFT depends on weather.

We agree that fire spread is not fully decoupled from weather due to the dependence of FPFT on meteorological conditions. We have therefore removed the reference to “weather”. (line 247)

Figure 4. “ppl” is not defined.

We thank the reviewer for pointing this out. The abbreviation “ppl” was unclear, and we have now replaced it with “people” in both the figure and the caption.

Figure 7. In (a), the histogram of GFED4s agrees with the slope shown in Figure 3 (centered on -6.57), but the one of JULES-INFERNO+HDI does not agree with the slope shown in Figure 6b (not centered on -5.40). Why?

We thank the reviewer for this observation. The discrepancy identified between Figures 6 and 7 arises from the same issue previously identified in Figure 3 and discussed previously.

In the original version, Figure 6 was also affected by this erroneous transformation, which led to inconsistencies between the reported slope and the corresponding slopes displayed in the histogram of Figure 7. Importantly, Figure 7 was always computed directly and consistently in log-space and therefore remained correct.

In the revised manuscript, this issue has been resolved by removing the incorrect back-transformation and presenting Figure 6 consistently in log-space. As a result, the slope values and distributions shown in Figures 6 and 7 are now fully consistent.

Figures 9 and 13. Please use an irregularly space color bar to improve visibility (as in Figure 8).

Thank you for the suggestion. Both Figures 9 and 13 have been updated to include an irregularly spaced colour bar, and the caption of the figures updated to note this.

L494. Typo: “burnrt”.

Thank you for the comment. This has been addressed in the revised manuscript (line 433)

L495. “exacerbates” should be exacerbating.

Thank you for the comment. This has been addressed in the revised manuscript (line 434).

L456. The numbers provided for JULES-INFERNO and JULES-INFERNO+HDI should be swapped.

Thank you for the comment. This has been addressed in the revised manuscript (line 436).

L457. “additional suppression”. I believe it could also be due to reduced ignitions (Eq. 6).

We thank the reviewer for this comment. We agree that the wording “additional suppression” was too specific, as the HDI formulation affects both ignition and suppression processes in the model. We have revised the text (lines 436 – 437 of the revised manuscript) to describe the combined effect of reduced ignitions and increased suppression associated with HDI.

Figure 12. Please also show statistics for NHAF, SHAF, EQAS, and SEAS. There is no reason why these 4 regions should be discarded from this Figure.

We thank the reviewer for this suggestion. We agree that including NHAF, SHAF, EQAS, and SEAS improves the completeness of the analysis. We have therefore updated Figure 12 to include these four regions

Figure 12. Correlation should not have units.

Thank you for the comment. This has been addressed in the revised manuscript.

L499. Wording error: “with initial where”.

Thank you. This has been addressed in the revised manuscript (line 479).

L504. More precis wording would be: any relative difference in STD/STDGFED4s between both are small.

Thank you for the suggestion. This has been addressed in the revised manuscript (line 484).

L553-558. This is vague. Please remove, and refer to the Discussion for limitations.

Thank you for the comment. This has been addressed in the revised manuscript (line 534).

Figure 14. Why are there no confidence intervals on experiments other than control? If this is a choice from the authors, it should be specified in the caption.

Confidence intervals are shown only for GFED4s and the control experiment to avoid clutter in an already complex figure with multiple sensitivity experiments We thank the reviewer for this comment. We agree that the caption does not clearly state this choice and have now clarified this explicitly in the revised figure caption.

L583. “their reference”: should this be the control?

Thank you for the suggestion. This has been addressed in the revised manuscript (line 560).

L588. “can have opposite effects”: this is unclear. Opposite to what?

Thank you for the suggestion. This has been addressed in the revised manuscript (lines 565 – 566) to refer this is in relation to the sign of the trend.

L595-597. Figure 14 shows that Ndep is a significant driver of burned area trends in GLOBAL for JULES-INFERNO+HDI.

We thank the reviewer for this comment. We agree that the original wording was too strong and did not correctly reflect the role of biogenic drivers shown in Figure 14. We have revised the text in lines 572 to 575 to better align with the results, clarifying that nitrogen deposition is a significant driver of global burnt area trends in JULES-INFERNO+HDI, while atmospheric carbon dioxide assimilation and nitrogen deposition also contribute regionally.

L607. “AUS” should be AUST.

Thank you for the suggestion. This has been addressed in the revised manuscript (line 586).

L610. Figure 14 does not show that precipitation is the dominant driver in MIDE for JULESINFERNO.

We thank the reviewer for this comment. We agree that the original wording incorrectly overstated the role of precipitation as the dominant driver in MIDE in JULES-INFERNO, which is not supported by Figure 14. We have therefore revised the text to remove this claim and instead describe precipitation and temperature in terms of their respective contributions to the burnt area trends in line 589 of the revised manuscript.