Reviewer response letter

We appreciate the reviewers for their detailed and constructive feedback. In response, we have carefully revised the manuscript to address all points and improve clarity, transparency, and scientific rigor. Key changes include:

1. Introduction & contextualization: Historical burned area data for Australia and Canada have been added to provide temporal comparisons, strengthening the link between climate change and fire weather conditions.
2. A new title following the suggestion by the reviewer: "Development of a Model for Global Burned Area Simulation within a DGVM-Compatible Framework"
3. Modeling rationale & transparency: Section 2.4 now clearly explains the rationale behind additive (Models 1–12) and multiplicative (Models 13–26) predictor formulations. A new "Rationale" column in Table 2 summarizes conceptual and statistical justifications for each model configuration.
4. Terminology & consistency: All instances of GPPI have been replaced with MEPI, table headers standardized, and the number of models and predictors verified for accuracy.
5. Results & figures: The manuscript now presents model evaluation using observational data prior to predictions. Figures and captions have been updated for clarity, including temporal coverage and cross-references to supplementary figures. Annual and seasonal evaluation results have been combined for better readability.
6. Discussion & conclusions: The Discussion now clearly articulates the novel contributions of the model, including the role of major drivers and regional HDI variations. The conclusion has been streamlined to focus on demonstrated outcomes, model performance, and potential for DGVM integration.

These revisions collectively address all reviewer comments, improve the manuscript's readability and methodological transparency and strengthen the presentation of the study's findings.

General comment

Overall, the revised manuscript shows noticeable improvements. However, there are several key points raised during the first-round review that have not yet been adequately addressed.

I strongly recommend that the authors provide a clear and direct response to each of these previously raised points, indicating precisely where the revisions have been implemented in the manuscript (e.g., by referencing specific line numbers). This will greatly facilitate the crosschecking process and ensure transparency in the revision. Not just answering "Yes, I will adjust..."

Additionally, the authors should exercise greater attention to detail in presenting information to avoid inconsistencies or errors—such as the total number of models, the number and names of predictors, table headers, and the agreed-upon terminology. For example, the term Gross Primary Productivity Index (GPPI) should consistently be replaced with Monthly Ecosystem Productivity Index (MEPI) as previously agreed.

Lastly, the manuscript's structure should be refined. Specifically, the explanation of model validation using observational data should precede any presentation or discussion of model predictions. This adjustment will enhance the logical flow and clarity of the results section.

Please refer to the detailed comments provided below for point-by-point feedback.

Response: Thank you for your thoughtful and constructive feedback. We have carefully reviewed all previous comments and have provided detailed responses to each, clearly indicating where the revisions have been made in the manuscript (by referencing specific line numbers). We have ensured that all issues are addressed directly, rather than providing vague responses. Also, we have thoroughly revised the manuscript to ensure consistent use of terminology, particularly replacing "Gross Primary Productivity Index (GPPI)" with "Monthly Ecosystem Productivity Index (MEPI)" throughout the document. Additionally, we have corrected any inconsistencies regarding the number and names of models, predictors, and table headers (e.g., Table 2, lines 272-273). As suggested, we have restructured the results section to present the model evaluation using observational data before model predictions. This adjustment improves the logical flow and clarity of the manuscript (see lines 351-385 for evaluation and lines 386-430 for predictions).

Detailed Comments

L1 "A statistical global burnt area model tailored for integration into Dynamic Global Vegetation Models"

> At this review stage, I personally find the current article title still not yet fully firm- particularly with regard to the mention of DGVM integration, which, based on your manuscript, has not yet been technically implemented (still an intention).
>
> Referring to your response to the first-round review, particularly at points **L14** and **L378**, I would suggest revising the title to better reflect the actual scope of the study. For instance:
>
> "**Development of a Biophysical and Socioeconomic-Based Generalized Linear Model for Global Burned Area Simulation**", or
>
> "**Development of a Biophysical and Socioeconomic-Based Statistical Model for Global Burned Area Simulation within a DGVM-Compatible Framework**"
>
> I hope this suggestion is helpful for your consideration, as the title plays a crucial role in accurately representing the core contribution and scope of your research.

Response: Thanks for the suggestion. We have revised the title of the manuscript accordingly and made made it concise and it now reads "Development of a Statistical Model for Global Burned Area Simulation within a DGVM-Compatible Framework" [Line 1-3]

L31 "The model presented should be compatible with most, if not all, DGVMs used to develop future scenarios."

Please delete this sentence.

This sentence could unintentionally weaken your argument, as it may suggest uncertainty regarding the model's compatibility with DGVMs.

Hope you'll consider to apply my suggestion on (**L1**) - indicating that it has been developed within a DGVM-compatible framework, without overstating its level of integration. This allows readers to understand that while integration with DGVMs is feasible, it is not necessarily straightforward. In practice, successful integration requires careful consideration of technical aspects, including differences in programming languages, data structures, and input–process–output mechanisms.

For instance, although it is quite complicated and challenging, SPITFIRE has been successfully implemented across multiple DGVMs within the FireMIP project, but such integration has required significant technical adaptation. Your model, if developed within a compatible framework, can follow a similar pathway.

Response: The sentence stating, "The model presented should be compatible with most, if not all, DGVMs used to develop future scenarios," has been deleted as recommended. In its place, we emphasize that the model was developed within a DGVM-compatible framework, without overstating the level of integration (See lines 14-15). This revision clarifies that while integration with DGVMs is feasible, but not pursued in this study. However, we would like to point out, purely as a matter of historical accuracy and because some coauthors were involved in these activities, that SPITFIRE was implemented in many DGVMs independently from FireMIP. The role of FireMIP was to provide a systematic comparison of these implementations. Furthermore, the SPITFIRE model is far more complicated than the GLM presented here and integrating it into a DGVM requires more fundamental model development. Moreover, a European GLM-based model developed with the same approach but for Europe (Forrest et al. 2024), which also inspired the work here, has in the meantime been integrated within the LPJmL model, which was even for us surprisingly easy. For example, model skill was hardly reduced by replacing remote sensing-based vegetation input variables with those simulated by LPJmL. However, we are still finalizing the manuscript on this work and acknowledge that we cannot build upon these results for the manuscript here.

L37 "Notably, climate change has led to more severe fire weather in large parts of the world and record fires have recently occurred in Australia and Canada, burning more than 15 million and 7 million ha (Jain et al., 2024; MacCarthy et al., 2024)."

As previously suggested **(first round review L36),** I recommend including one historical burned area data for Australia and Canada for years prior to 2024 to provide a clearer basis for comparison.

Additionally, since the previous sentence states that "climate change led to more severe fire weather," it would strengthen the argument to include a temporal analysis of fire weather conditions in both countries. This would help substantiate the claim by demonstrating how fire weather patterns have evolved over time in response to climate change.

Response: We appreciate the reviewer's suggestion and have revised the introduction to include historical burned area data for both Australia and Canada to provide a clear temporal comparison. The passage now reads "*Globally, the impacts of climate change continue to manifest through extreme weather events and changes in weather patterns (Clarke et al., 2019). In Australia, for example, the mean annual burned area in forested regions was about 1.8 million ha per year between 1988–2001, increasing to 3.5 million ha per year between 2002–2018, before the 2019–20 "Black Summer" fires burned over 15 million ha nationally (Canadell et al., 2021; Australian Government, 2020). Similarly, in Canada, the 1986–2022 mean annual burned area was about 2.1 million ha, compared with the record-breaking 7.8 million ha burned in 2023 (Jain et al., 2024; Curasi et al., 2024; MacCarthy et al. 2024). These multi-decadal increases in burned area in both countries are consistent with evidence that climate change has intensified fire-conducive weather over time."*These temporal comparisons demonstrate a clear escalation in burned area over recent decades, consistent with published evidence that climate change has intensified fire-conducive weather conditions in both countries. [See lines 34 - 40]

L127 In Figure 1, the label currently reads "Monthly burnt area **GFEDv5**." To ensure consistency in the use of abbreviations throughout the manuscript, please standardize the format - either use GFED5 or GFEDv5 consistently across all figures, tables, and text.

Additionally, please clarify whether the prediction process is part of the Model Evaluation procedure or not?

If not, I suggest separating the Model Prediction process as a distinct subsection following the Model Evaluation. This will help to clearly distinguish between performance assessment and forward-looking application.

In the Model Prediction section, please specify the temporal coverage of the prediction- for example, from which year to which year the model was used to simulate burned area- and provide a clear rationale for this time frame.

Response: Great suggestions. We've now adopted GFED5 as the standard name and revised all sections of the manuscript accordingly. Also we separated the model training, testing , and prediction and evaluation components on figure 1 and specified the time periods. Additionally we added a new section which clarifies model training and testing (Section 2.5) in which the training and testing periods are clarified as ; *" We used data from the period 2002–2010 for model training, the period 2011–2018 for model testing, and the full period 2002–2018 dataset for predictions and model evaluation. These time periods were chosen to ensure that the testing data remained independent from the training data while also allowing predictions to span a sufficiently long timeframe to enhance the robustness of the analysis and evaluation. The essence of splitting*

training vs testing is to train the model on training data, and then check that the results are similarly good on the testing data (for example, no overfitting to the training data) before making predictions on the full dataset. During model testing we compared the performance of the model on training data vs training data to assess model robustness"[See lines 252-259].

L131 Monthly BA data for the periods **2002 and 2018** were derived from monthly mean fractional BA from the GFED5.

Please clarify whether this refers to the entire period from 2002 to 2018 (inclusive), rather than only the years 2002 and 2018. If so, I recommend revising the sentence to accurately reflect the time range, such as:

"Monthly BA data for the period 2002–2018 were derived from the monthly mean fractional BA provided by GFED5."

Response: Good suggestion. As described above we added a new section on model training and testing(2.5) which clearly describes how data are split and the rationale behind the selected periods. In this section the revised passage reads *" We used data from the period 2002–2010 for model training, the period 2011–2018 for model testing, and the full period 2002–2018 dataset for predictions and model evaluation. These time periods were chosen to ensure that the testing data remained independent from the training data while also allowing predictions to span a sufficiently long timeframe to enhance the robustness of the analysis and evaluation. The essence of splitting training vs testing is to train the model on training data, and then check that the results are similarly good on the testing data (for example, no overfitting to the training data) before making predictions on the full dataset. During model testing we compared the performance of the model on training data vs training data to assess model robustness"*[See lines 252-259].

L151 Please review the formatting and content of Table 1 carefully:

1) The table caption should be placed above the table, in accordance with standard journal formatting conventions.
2) Please ensure that the "Repeat Header Row" option is enabled. Currently, the header on pages 7 and 8 differs from that on page 6. The column headings should consistently read "Temporal Resolution" and "Source", not "Temporal" and "Predictor".
3) Additionally, please include appropriate citations for the PGC and AGB data sources in the table to ensure transparency and proper attribution.
4) I recommend adding an additional column titled "Temporal Coverage". This will help clarify the specific years associated with each predictor dataset used in the analysis and enhance the transparency and reproducibility of the study.

Response: Great suggestions regarding Table 1. We have carefully revised the table to address all points:

1. The table caption has been moved above the table in accordance with journal formatting conventions.

2. The "Repeat Header Row" option has been enabled to ensure consistent headers across all pages.

3. The column headings have been updated to consistently read "*Temporal Resolution*" and "*Source."*

4. Appropriate citations for the PGC and AGB data sources have been added to enhance transparency and ensure proper attribution.

5. We have added a new column titled "*Temporal Coverage"* to clarify the specific years associated with each predictor dataset, thereby improving the reproducibility and clarity of the analysis. [See lines 151 – 152]

L157 "We used **eight** vegetation predictor variables…"

On Line 158, only **six** vegetation predictors are listed, whereas Table 1 includes **nine** vegetation-related predictors.

Please review and clarify the exact number of vegetation predictors used in the analysis to ensure consistency across the manuscript.

Response: We appreciate the reviewer for pointing out this discrepancy. Upon review, we have clarified that we used nine vegetation predictors in the analysis. This update has ensured consistency between the text and Table 1. [See line 156]

L166 I recommend reviewing the opening phrases of each paragraph in this sub section or other section if any. The phrase "we used" is repeated frequently, which may reduce the overall readability and stylistic variation of the text.

Additionally, please consider improving the logical flow and continuity between paragraphs to ensure a more cohesive narrative. Enhancing transitions and reducing redundancy will help maintain reader engagement and strengthen the overall presentation of your work.

Response: Thanks for the feedback. We have revised the opening phrases throughout the subsection to reduce the repetitive use of *"we used"* and improve stylistic variation. For example: some of the passages were revised begin as as follows *"PGZC, PRC, PTNC and PTC were used to evaluate the relationship between landcover and burnt area distribution"* (See lines 166-167); *"GPP, AGB, and FAPAR were proxies for vegetation health and productivity and type, and fuel load"* (See lines 168); *" To evaluate how topography can influence the occurrence and spread of fires, we incorporated topographic positioning index (TPI)"* (See lines 189-190)

Additionally, we have enhanced the logical flow and continuity between paragraphs by refining transitions and minimizing redundancy. [See lines 156-187]

L249 I would like to reiterate a key point from the first round of review: **I recommend avoiding the use of the phrase "seamless integration into DGVMs."**

This is not a matter of opposition to the idea itself, but rather a concern about accuracy and scientific rigor. In the current study, you have developed a Generalized Linear Model (GLM) under the DGVM framework, with the intention that it may be integrated into DGVMs in the future. However, as the model has not yet been implemented or tested within an actual DGVM - nor validated across multiple DGVM platforms - the claim of "seamless integration" is not currently supported by sufficient evidence.

For this reason, I suggest using more cautious and evidence-based wording to reflect the current status of your model development and its potential for future integration.

Response: We agree that "seamless integration" was potentially misleading. We have revised the manuscript to replace the phrase "seamless integration into DGVMs" with more cautious and evidence-based wording that reflects the current status of our model. Specifically, we emphasize that the GLM was developed to be *"DGVM-compatible"* or *"for ease of transference to other modelling frameworks"* (See lines 251-252). While this particular GLM-based model has not yet been implemented or tested within an actual DGVM, framing it as DGVM-compatible highlights its design alignment and flexibility, without implying untested integration.

L251 The sentence "Calibration of the model utilized data from 2002 to 2010 while testing utilized data from 2011 to 2018" should be revised for clarity and consistency.

First, please explicitly mention that the data used are from GFED5.

Second, kindly verify the accuracy of this statement, as Figure 1 indicates that GFED5 data from 2002–2010 were used for model training, while data from 2011–2018 were used for model evaluation.

Lastly, I recommend using terminology consistently throughout the manuscript. Please choose a uniform set of terms - such as training, testing, calibration, evaluation or validation, and ensure that their usage aligns with standard definitions, as each term has a distinct meaning in modeling studies.

Response: We have revised the sentence for clarity and consistency. Specifically, we now explicitly state that the data are from GFED5 in Figure 1. We have also clarified the periods for training, testing, precision and evaluation using consistent terminology in the new section we added (Section 2.5). There is passage in this section which clarifies these periods as follows: *"We used data from the period 2002–2010 for model training, the period 2011–2018 for model testing, and the full period 2002–2018 dataset for predictions and model evaluation. These time periods were chosen to ensure that the testing data remained independent from the training data while also allowing predictions to span a sufficiently long timeframe to enhance*

the robustness of the analysis and evaluation. The essence of splitting training vs testing is to train the model on training data, and then check that the results are similarly good on the testing data (for example, no overfitting to the training data) before making predictions on the full dataset. During model testing we compared the performance of the model on training data vs training data to assess model robustness." [See lines 254-261]. *The term "validation" is not used anymore as it might indicate that a model is generally valid and is often confused with evaluation, which we think is more appropriate for a model like here.*

L266 The sentence "A total of 25 model runs were conducted, each…" appears to be inconsistent with the information provided in Table A1, which lists 26 models.

Please clarify whether 25 or 26 model runs were actually performed and ensure consistency throughout the manuscript.

Response: We appreciate the reviewer for identifying this inconsistency. Upon review, we confirm that 26 model runs were conducted, as correctly listed in Table A1. The manuscript has been updated to reflect this number consistently throughout, ensuring alignment between the text and the table. Also, all other numbers have been carefully checked. [See line 290]

L628 Could you please explain why you moved Table 2 (initial manuscript) to the appendix as Table A1 in the current revised version?

In my view, the information presented in Table A1 is essential to the core content of the manuscript, as it outlines the structure and configuration of Models 1 through 26 - an integral part of the model development process presented in this study. Including this table in the main text would improve clarity and allow readers to more easily follow the comparisons and results throughout the paper.

If the relocation was in response to a specific request from another reviewer, I might understand the consideration. Alternatively, if necessary, guidance from the editor may be helpful in determining the most appropriate placement for this table.

Response: The relocation of Table 2 (initial manuscript) to the appendix as Table A1 was made following a suggestion from the editor during the revision process. This change was intended to streamline the main text while preserving all essential methodological details for transparency and reproducibility. We agree that Table A1 provides important information on the structure and configuration of Models 1 through 26. We have ensured that the main text clearly references Table A1 at relevant points, so readers can easily follow the comparisons and results presented in the study. Besides we have now included an abridged version of Table 2 which summarises models (M1–M26) with corresponding formulas, performance metrics, and rationale for predictor inclusion or interaction terms [274-277]

L289 The sentence "The initial models (Model 1 to Model 3) progressively include more variables, however, a noticeable jump in deviance explained when PNTC is added (Model 3: 0.5298)." Should be improved for clarity and consistency.

While it is understandable that "Model 3: 0.5298" refers to the deviance explained by Model 3, the current phrasing may be unclear to some readers. I suggest rephrasing the sentence to explicitly state that Model 3 explains 52.98% of the deviance or adjusting the deviance writing style on the sentence.

Additionally, there is inconsistency in how models and their deviance values are presented in this sub-section. For example, "Model 15 (~0.5664789)" in Line 295 follows a different format.

Please revise the entire section to ensure a consistent and reader-friendly presentation of model identifiers and their corresponding deviance explained values.

Response: Thanks for these constructive suggestions. The first sentence has been revised for clarity and now reads *"The initial models (model 1 to model 3) had many variables and a significant improvement is observed in model 3 which explained 52.98% following the inclusion of PNTC. Models 4 to 8 involve adding vegetation (FAPAR) and various land use types (PCC, PPS, PRC, PGC). This is accompanied by marginal improvement in deviance explained, indicating these factors provide some additional predictive power but are not as impactful as existing vegetation covariates (such as GPP). Models 10 to 12 introduce polynomial terms for PTC. This results in an increase in performance, explaining 55.88% in Model 12. Models 13 to 16 incorporate interactions between HDI and land use types (e.g., PCC and PRC), resulting in marginal improvement in performance with the highest recorded in Model 15 which explained 56.65%. Models 19 to 26 fine-tune the overall performance by incorporating various variables and their interactions. Model 24, which includes a comprehensive set of climatic, vegetation, human, and topographic variables along with their interactions, achieves the highest performance as it explained 57.20%. The marginal improvements observed in subsequent models indicate that, while additional variables contribute to the model performance, the primary influencing factors were already identified by Model 19, however it was not the simplest model (~ parsimonious) and included variables for which future projections are currently unavailable (e.g., RD), due to the lack of established projection models or datasets. Therefore, model … was chosen as the final model."* [See lines 321-328]

L300 The phrase "and consisted of other variables that we don't have future projections for (e.g., RD)" is not sufficiently formal for a scientific manuscript.

I suggest rephrasing it to something more appropriate, such as:

"and included variables for which future projections are currently unavailable (e.g., RD), due to the lack of established projection models or datasets."

This revision would provide a clearer and more scientifically sound justification. If possible, please support this statement with references or evidence to strengthen the rationale for excluding such variables from future scenario modeling.

Response: Good suggestion. The sentence has been revised to a more formal and scientifically precise wording: *"and included variables for which future projections are currently unavailable (e.g., RD), due to the lack of established projection models or datasets."* A supporting statement providing a clear rationale for excluding these variables modeling was provided and it reads *"Since the main objective of the study was to produce a DGVM compatible model, availability of future projections for these datasets was indispensable to model building."*[See line 323 -326]

L635 Could you please explain how the predictor formula in each model is determined, the reasons for summing or multiplying the predictors? **This question was asked since the first review round (L277) but I couldn't find the explanation in the revised manuscript.**

I believe it would be helpful - for both myself and the broader readership, to provide a clear rationale for the modeling approach used. Specifically, Models 1 to 12 apply only the sum of predictor variables, while subsequent models begin incorporating multiplicative interactions between predictors.

Was this modeling structure formula based on established references, or was it determined subjectively defined by the authors?

As a modeler, particularly from the perspective of DGVM applications, this distinction is crucial. In DGVMs, each variable or parameter is typically defined through well-established empirical or mechanistic relationships, reflecting biophysical, physiological, and ecological processes. The interactions between environmental drivers - such as soil, vegetation, disturbance, and atmospheric variables, lightning – as well as anthropogenic driver such as population density - are governed by interdependent equations grounded in process-based understanding.

Therefore, introducing mathematical operations such as summation or multiplication without a strong theoretical or empirical basis may present challenges for future model integration and validity. Arbitrary combinations may not align with the underlying mechanisms of DGVMs and could compromise the scientific robustness of future applications. I encourage the authors to explain and clarify the conceptual or empirical justification for the mathematical formulations applied in each or overall model structure.

I suggest adding this explanation in **Section 2.4**, positioned sequentially *before* explanation about model performance assessment.

Response: We thank the reviewer for raising this point again and apologise that our previous revision did not sufficiently clarify the rationale for the model formulations. In response, we have substantially revised Section 2.4 and we now have a new section (2.6) that is dedicated

to describe the model selection procedure. A clarifying passage in this section reads "*We employed a sequential model-building approach, beginning with additive structures ((M1–M12) to estimate the independent contribution of climate, vegetation, and human variables on burned area (Table 2). This approach aligns with established fire risk modeling practices (e.g., Forrest et al 2024). Additional predictors were introduced if they represented ecologically meaningful processes (e.g., drought severity, vegetation productivity) and improved model fit (deviance explained and Normalised Mean Error). Multiplicative interaction terms (M13 onward) were added only when fire ecology theory suggested synergistic effects (e.g., human ignitions under extreme weather, vegetation dryness and temperature) and retained if deviance explained improved. This stepwise approach ensures both statistical rigor and ecological interpretability rather than ad hoc formula selection."* To improve transparency, we have added a Rationale column in Table 2 summarizing the conceptual and statistical justification for each model formulation. This ensures our modeling approach is systematic, theory-informed and empirically grounded, addressing the reviewer's concern about potential arbitrariness. [See lines 264 - 277]

L325 "Predictor variables were Gross Primary Production Index (GPP)…"

It appears that the term **Gross Primary Production Index (GPP?)** is still used in several parts of the manuscript, despite your agreement in the first-round review's response (see comment L200) to consistently use MEPI, as also shown in Figure 3.

For clarity and consistency, I recommend revising the manuscript to uniformly refer to this variable as MEPI throughout the text, figures, and tables.

Response: We thank the reviewer for highlighting this issue. We have carefully revised the manuscript to ensure that the variable is referred to as MEPI on figure 3 caption [see line 349] and throughout the text, and tables, in accordance with our previous agreement. Where GPP still exists, it's representing the inputs for determining MEPI not GPPI (as it was formerly addressed)

L330 Please search additional references related to similar GLM modeling that have similar or lower explained deviance values than yours, and providing explanations to strengthen your results that the values are accepted. **This point has been asked since the first review round (L308) but I couldn't find any change in this section.**

The comparison with Haas et al. (2022), which reports a deviance explained value of 69%, may inadvertently weaken the presentation of your own model's performance (56.8%).

Since this comparison appears in Section 3 (Results), I recommend reconsidering its inclusion. The Results section should primarily focus on presenting your own findings, while comparative analysis with previous studies would be more appropriately placed in the Discussion section - if sufficiently supported.

If no additional references or contextual justification are available to frame this comparison constructively, it may be better to omit it altogether to maintain a focused and balanced presentation of your results.

Response: We appreciate the recommendation. We have removed the comparison with Haas et al. (2022) from the Results section. We however note also that Haas et al. fitted a model for annual burned area, not capturing seasonal variation like here, and the number of used predictors was more and not constrained by DGVM applications in the discussion section [Lines 105-106]. Thus, a comparison of model performance might not be very useful. However, in the discussion we still mention that the performance here is comparable to other fire-enabled DGVMs on ability to capture interannual variability ".*We note that in the recent comparison of fire-enabled DGVMs in the Fire Model Intercomparison Project (FireMIP) project (Hantson et al., 2020), all models did a poorer job of matching the interannual variability than the spatial patterns by a considerable margin. The seven acceptably-performing models achieved a mean spatial NME (across all data and model comparisons) of 0.84 with respect to spatial patterns, but an NME of 1.15 for interannual variability*.." (See lines 541-544…).

L345 Figure 4: As previously mentioned in the **first-round review (see comment L24**), it is necessary to present a comparison between the observed and predicted burned area data before displaying the map titled "Predicted Burnt Area: 2011–2018."

In general modeling practice, model performance should be evaluated prior to making and presenting predictions. This includes comparing observed and predicted data using spatial analyses or summary statistics, and ideally supported by visualizations such as scatter plots.

If the model demonstrates satisfactory performance, the presentation of the spatial prediction for the 2011–2018 period will be better justified and more scientifically robust.

Response: We agree and have added a new section (3.2) dedicated to model performance evaluation preceding the presentation of predictions. In this section, we present results demonstrating the strength of the relationship between observed and predicted burned area across GFED regions. These results are now visualized spatially in Figure 4, allowing a clear assessment of model performance prior to the presentation of the 2011–2018 predicted burned area map. [See line 353 - 392]

L356 Figure 5: Could you please clarify whether the predicted burned area shown covers the period from 2002 to 2018, or only from 2011 to 2018 only?

Kindly ensure that your response is consistent with the point raised in comment **L127**. If the prediction only covers a subset of the years, please adjust the graph accordingly to reflect the correct prediction period.

Additionally, I recommend improving the x-axis labeling by clearly indicating both the start and end years. If space is limited, consider using italicized text or reduced font size so that all years (e.g., 2002–2018) can be displayed legibly. This will enhance clarity and make the graph more reader-friendly.

Response: The data used in Figure 5 cover the full period from 2002 to 2018 showing a comparison between observed and predicted data based on the full dataset. We have updated the x-axis labeling to clearly indicate both the start and end years (2002–2018), adjusting the font size to ensure legibility. [See lines 409-413]

L361 Please write the full name of the abbreviation SHAF, SHSA, NHAF, CEAS and so on (because it has not been explained before). **This point has been asked since the first review round (L338) but I couldn't find any additional information.**

For improved clarity and accessibility, I recommend including the full definitions of all of these abbreviations used in Table A2.

Response: We thank the reviewer for pointing this out. We have included the full names of all abbreviations (e.g., SHAF, SHSA, NHAF, CEAS) in lines 365-369 for improved clarity and accessibility, and we have included those in Table A2 caption as well.

L370 Figure 6b presents the validation results of the selected model through a comparison between observed and predicted burned area data.

I suggest repositioning this figure before Figure 4, as it is more appropriate to present and discuss model validation prior to showing the prediction outputs. This will improve the logical flow of the results section.

Please also clarify the temporal coverage of the comparison: for example, confirm that both GFED5 and model data span 2011–2018, and specify whether the comparison is based on annual average data.

Additionally, please revise the figure caption to include this information and improve clarity.

This comment is closely related to your response to **point L345.** I recommend to show the scatter plot comparison between predicted and observed GFED5 data on the Supplementary file in accordance with this Figure 6 to increase the validation clarity.

Response: Thank you for your valuable feedback regarding Figure 6b and its placement within the manuscript. We have carefully considered your suggestions and made the following revisions:

1. In response to your suggestion, we have moved Figure 6b to precede Figure 4, in order to present the model evaluation results prior to the model prediction outputs. This adjustment enhances the logical flow of the results section and we believe it improves the

clarity and structure of the manuscript.

2. We have added the requested clarification regarding the temporal coverage of the comparison between the observed and predicted burned area data. Specifically, we confirm that both the GFED5 and model data cover the period 2011–2018. Additionally, we specify that the comparison is based on annual average data, as per your recommendation. This clarification has been incorporated into the revised manuscript (see lines 290-291).

3. The figure caption for Figure 6b has been revised to include the temporal coverage and further details on the comparison methodology to improve clarity. The updated caption now reads: *"Figure 4. Evaluation of the selected model using observed burned area data from GFED5 predicted data (2011-2018). The map shows r-square values highlighting the model's performance for interannual (a) and seasonal variability (b) per GFED region."*

4. In line with your suggestion, we have included the scatter plot comparison between predicted and observed GFED5 data in the Supplementary Material (Figure A4), which complements Figure 4 in the main manuscript. [See lines 716-721]

L393 Figure 8. This figure presents the results of model validation using seasonal observation data.

In line with my previous comment, I recommend combining both the annual and seasonal validation results into a single figure to facilitate direct comparison and enhance clarity. Once the validation results are clearly presented and discussed, you may proceed with displaying and interpreting the prediction outputs.

This revised order will improve the logical flow of the results and allow readers to better assess model performance before evaluating its predictive capabilities.

Response: We appreciate the suggestion. We have now combined the annual and seasonal evaluation results into a single figure (now Figure 4) to facilitate direct comparison and enhance clarity. We revised the presentation to allow readers to assess model performance more effectively before interpreting the prediction outputs. [See lines 382-386]

L395 Section 4. Discussion

I recommend maintaining the subsection title 4.1. Main Drivers of Global Burned Area to help clearly categorize and structure the Discussion section.

As noted in my first-round review (comment L372), my suggestion was to delete only the intervening paragraph between the main section title (4. Discussion) and subsection 4.1, not the subsection itself.

Therefore, the structure should be:

4. Discussion

4.1. Main Drivers of Global Burned Area

Response: Thanks for the clarification. We have retained the subsection title 4.1. Main Drivers of Global Burned Area and removed only the intervening paragraph between the main Discussion heading and this subsection, as suggested. This revision restores the intended structure and improves the clarity and organization of the Discussion section. [See line 434-441].

L396 I recommend removing the sentence "We found a DGVM-compatible parsimonious global statistical model made of FWI, PNTC, PTC, TPI, MEPI, HDI, VAT, and NDD."

Placing this statement at the beginning of the paragraph may be inappropriate, as the claim of "DGVM-compatible" is not yet strongly substantiated within the manuscript.

Instead, I suggest focusing the paragraph on discussing the main drivers of global burned area, in line with the theme of subsection 4.1. This will ensure better alignment with the section's objective and maintain a coherent flow of discussion based on the model results.

Response: We agree. In the revised manuscript, we have removed the original sentence referring to a *"DGVM-compatible"* model from the beginning of the paragraph. The opening statement has been replaced with: *"We found that the candidate variables, namely, FWI, PNTC, PTC, TPI, MEPI, HDI, PPN and NDD, had strong influence on burnt areas."* This revision focuses the paragraph on the main drivers of global burned area, aligning with the theme of subsection 4.1 and maintaining a coherent flow of discussion based on the model results, without making claims that are not yet substantiated. [See lines 435-437].

L459 The sentence *"Our model has contributed novel insights to the existing understanding of the factors influencing global fire trends (Joshi and Sukumar, 2021; Kraaij et al., 2018; Mukunga et al., 2023)"* remains vague and insufficiently supported.

Could you please explain whether they say that your model contributed novel insights to the global fire trends?

As previously noted in my first-round review **(comment L432),** but authors didn't make any change on this point. This claim should be substantiated by explaining how your model offers novel insights. Simply stating the contribution without elaboration or contextual comparison weakens the impact of your findings.

I recommend strengthening this statement by clearly articulating what specific advancements or new perspectives your model provides - such as integrating novel predictor combinations, improved spatial resolution, or enhanced predictive accuracy - and then positioning these insights relative to previous studies.

A clearer structure could be:

"Our model has contributed novel insights to the existing understanding of the factors influencing global fire trends because "reason A" (Reference), "reason B" (Reference), so on if any. Previous studies, such as Andela …"

Response: We appreciate the suggestion. We have strengthened the statement to clearly articulate the novel contributions of our model. The revised text now specifies what previous studies explored and the strengths of our model compared to earlier studies, for example its integration of novel combinations of predictor variables and improved temporal resolution. Additionally, we contextualize these contributions relative to existing literature, highlighting how our findings extend the understanding of factors influencing global fire trends (e.g., Joshi and Sukumar, 2021; Kraaij et al., 2018; Mukunga et al., 2023; Andela et al., 2017). The revised section reads as follows: *" Previous studies have improved our understanding of drivers of fire but differ in approach and attributional focus for fire trends. For instance, Joshi and Sukumar (2021) employed region-specific multilayer neural networks to reveal spatially varying sensitivities between fire and socio-environmental drivers, providing strong spatial diagnostics but limited transparency on attributions of burnt area trends. Kraaij et al. (2018) provided detailed biome-level attribution of destructive fires by linking drought, fuel state and vegetation context in case studies (e.g., fynbos/plantation complexes), emphasizing vegetation and weather controls at local scales. Mukunga et al. (2023) used random-forest analyses to quantify the added value of human predictors for ignition probability, focusing on anthropogenic controls of ignitions rather than burnt area extent. Building on these approaches, our study contributes novel attributional insight because it explicitly integrates a compact set of DGVM compatible fire-weather and fuel indices (FWI, PTC, TPI, PNTC) with a socio-economic indicator (HDI) within a parsimonious statistical framework for burnt area trends. This allows direct attribution of directional effects (for example, the negative association between HDI and burnt area) across regions"* [See lines 496-506].

L468 "This highlights the significant influence of HDI in projecting the purported negative global ire trend".

The response provided by the authors to comment **L442 in the first-round review** does not appear to be fully implemented in the revised manuscript. While the authors agreed to include a discussion on HDI regional classification to improve the logical flow between sentences, this has not been clearly reflected in the current version.

For example, the sentence **"This highlights the significant influence of HDI in projecting the purported negative global fire trend."** is immediately followed by **"HDI is a rather broad socioeconomic indicator, which we assume acts as a proxy for factors..."**

The connection between these two sentences is weak, and the paragraph lacks a transitional explanation or regional perspective that would justify the claim. I recommend adding a brief discussion on HDI variability across regions or classifications (e.g., low-, middle-, high-HDI

countries), and how this influences fire trends. This addition would strengthen the coherence of the paragraph and enhance the reader's understanding of HDI's role in your model.

Response: Thanks for the suggestion. In the revised manuscript, we have strengthened the discussion of HDI by explicitly incorporating a regional perspective. We now briefly describe HDI variability across regions and classifications (e.g., low-, middle-, and high-HDI countries) and explain how these differences influence fire trends. The passage now reads "*This highlights the significant influence of HDI in projecting the purported negative global fire trend. Importantly, HDI is not uniform worldwide but varies substantially across regions and levels of socioeconomic development. For instance, in high-HDI countries, greater financial resources, infrastructure, and institutional capacity often translate into stronger investments in fire control technologies, improved surveillance systems, and more effective prevention campaigns. By contrast, in low- and middle-HDI countries, limited resources and weaker institutional frameworks may constrain fire management capabilities, resulting in greater reliance on natural fire dynamics or less formalized suppression efforts. As many countries continue to develop, it translates improvements in HDI and fire management strategies. Although strategies are often implemented independently and on a smaller scale, their cumulative impact on global fire trends is substantial. Thus, HDI serves as a broad socioeconomic indicator that we assume acts as a proxy for the combined effects of investments, advancements in fire control methods, surveillance, technology, and outreach strategies that increase awareness (Teixeira et al., 2023).*" This provided a clearer connection between the sentence on HDI's influence in projecting global fire trends and the subsequent discussion of HDI as a broad socioeconomic proxy. [See lines 513-523].

L496 "Globally, our model predicts a notable peak in burnt areas during February and August."

Could you please include a cross-reference to an image that states this?

Response: A cross-reference to support this statement has been added. The sentence now directs readers to Figure 8, which visually illustrates the predicted peaks in burnt areas during February and August. This ensures that the textual description is clearly linked to the corresponding visual evidence. [See line 548].

L512 Could you please provide evidence to support this assertion that it is due to climatic conditions in those regions? You can compare seasonal fire patterns and climatic conditions in those regions and discuss the result in this sub-section. **This point has been asked since the first review round (L471) but I couldn't find any additional information.**

Author responded "Evidence supporting the assertion that climatic conditions influence fire dynamics in specific regions will be provided by comparing seasonal fire patterns with climate variables."

This can be addressed with a relatively straightforward approach. I suggest plotting the seasonal fire pattern alongside the relevant seasonal climate variables discussed in this section, to strengthen your argument.

You may include this plot as a supplementary figure, and provide an appropriate crossreference to it within the main text.

Response: Thank you for your continued and insightful feedback. We apologize for not adequately addressing your previous request regarding the influence of climate on fire dynamics in specific regions. In response to your comment, we have made the following revisions:

1. We have revised the manuscript to provide a more robust explanation of how climate influences fire dynamics through the FWI to better represent the climatic conditions that drive seasonal fire patterns. We now explicitly show how the FWI is particularly effective in regions with distinct climatic patterns, and as a more comprehensive monthly climate variable. This revision is discussed in detail in Section 4.2 (see lines 480-483), where we highlight how regions with seasonal variability in climatic factors exhibit stronger relationships between the FWI and fire dynamics. The revised passage reads "*The stronger performance in these areas is likely due to the well-defined and predictable fire regimes in these regions. Since fire activity here is strongly governed by distinct wet-dry seasonal cycles, which align closely with fire weather, enabling our model to capture these patterns effectively using linear functions (Fig. A5), hence better model generalization.*"

2. We have now included a plot (now Figure A5 in the Supplementary Material) that compares seasonal fire patterns (both observed and predicted) with the relevant seasonal climate variable in our model (FWI). This comparison strengthens our argument that climatic conditions are a primary driver of fire dynamics in certain regions. The plots in Figure A5 clearly illustrate the temporal alignment between seasonal fire patterns and climate variability, reinforcing the role of climate in influencing fire behavior. We have also cross-referenced this supplementary figure in Section 4.2 (see lines 483) to guide readers to the additional information.

L587 "We sought … , both globally and regionally."

I recommend deleting the first paragraph of the Conclusion section, as it is not appropriate to restate the research objectives in this part of the manuscript. As mentioned in my **firstround review (comment L531),** the Conclusion should focus on explaining how the research objectives were achieved, based on the results and discussion presented.

I suggest starting with a concise summarizing statement such as:

Line 592 "We present a parsimonious statistical model specifically tailored for global burned area simulation."

Please remove the phrase "with the goal of integration into DGVMs" at this point, should emphasize demonstrated outcomes rather than intentions.

Then after Line 592, please discuss points 1) how the major drivers in the model you use, how the major drivers accommodate the fire incident or burnt area factors. 2) how the model can be integrated in DGVM and 3) how your model performs against interannual and seasonal observational data - global and regional.

Note: Please ignore the quotation 1), 2) and 3) -> my intention writing the quotation number to make ease the explanation of your research objective sequentially. Please write the content in a continuous, flowing narrative that cohesively summarizes how the model addresses and fulfills the research objectives.

Response: The first paragraph of the Conclusion section has been removed, as recommended, to avoid restating the research objectives. The revised section now begins with a concise summarizing statement: "*We present a parsimonious statistical model specifically tailored for global burned area simulation*." The subsequent discussion has been restructured into a continuous, cohesive narrative and now reads *" We found the drivers FWI, TPI, and PNTC are positively associated with BA, whereas MEPI, HDI, PPN, and NDD exhibit negative relationships, and PTC showed a unimodal response with strongest effect at intermediate tree cover. The diversity of these drivers underscores the multifaceted influence of both climatic and socio-economic drivers on fire dynamics. Our model explicitly accommodates these drivers, capturing how variations in climate, vegetation productivity, and human development interact to modulate fire occurrence and extent. Notably, the use of HDI to represent societal development as a proxy for fire management capacity and the transition away from fire-dependent agricultural practices provides a coarse but global socioeconomic driver beyond GDP and population density. Including this in DGVMs can improve fire, vegetation and human feedbacks, particularly with respect to Shared Socioeconomic Pathways (SSPs, O'Neill et al., 2017) or other scenarios.*" (See lines 641-652].

L604 "We hope that our research outcomes will stimulate a more rigorous implementation of global fire models within DGVM frameworks."

Please rewrite the sentence, perhaps something like this would be better

"The parsimonious statistical model developed in this study has demonstrated strong performance in simulating global burned area patterns. With further development, it holds potential for integration into DGVMs to enhance the representation of fire dynamics…"

Response: We appreciate the suggestion. The sentence has been revised as suggested to emphasize demonstrated outcomes rather than intentions. It now reads: *"The parsimonious statistical model developed in this study has demonstrated strong performance in simulating global burned area patterns. It holds potential for integration into DGVMs to enhance the representation of fire dynamics, albeit it remains to be tested how well the model performs*

when remote-sensing-derived vegetation and land cover variables are replaced with those simulated by a DGVM" [See lines 654-657]