

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

This study incorporates the Human Development Index (HDI) into the INFERNO fire modelling scheme of the JULES land surface model. In this implementation, the HDI decreases linearly the anthropogenic ignitions and the fraction of unsuppressed fires in the model. This modelling initiative is based on the assumption that increased socio-economic development, as approximated by the HDI, leads to more fire-suppressing policies and management. The study evaluates the impact of this new modeling framework using a simulation over the 1997-2016 period at the global scale, where the world is subdivided in 14 regions. Evaluation of the model without the HDI scaling (JULES-INFERNO, JI hereafter) and with the HDI scaling (JULES-INFERNO+HDI, JIH hereafter) is performed with respect to the burned area (BA) product of the Global Fire Emissions Database with small fires (GFED4s).

This study addresses an important and difficult objective: representing in global process-based fire models the influence of socio-economic factors on fire ignition and suppression. The study builds on the notion that societal approaches to fire management vary, and are not only captured by human population density and land use practices. While the socio-economic impact on fire activity depends on a number of complex factors with very large diversity across the world, this study explores if a simple linear scaling using the HDI can improve fire modeling. This is an important step for the fire modeling community, as methods to incorporate socio-economic influence are needed to improve fire modeling. Also, I believe that the modeling procedure of the authors is well-designed, and that their results are a contribution to the field. However, I have a major reservation concerning the presentation of the results. In particular, the presentation is misleading on the benefits of the linear and globally-uniform inclusion of HDI, where conclusions are not always well-supported quantitatively by the results. I note that this concern was already raised in the review of this study in a previous submission to the journal Biogeosciences, and has still not been sufficiently addressed. I detail this Major concern in this review, as well as two Minor comments, and other Specific comments. Nevertheless, I emphasize that, in my view, this study includes sufficient novel work and results that are relevant to the fire modeling community for meriting publication in Earth System Dynamics, but a more transparent presentation is required. Line numbers in this review correspond to the preprint manuscript.

We thank the reviewer for their thoughtful evaluation and for recognizing the novelty and potential contribution of this study to the fire modelling community. We appreciate the positive feedback regarding the rationale for incorporating socio-economic influences via HDI and the design of the modelling approach.

As stated in the replies below, we commit to revising the manuscript to present results more transparently, emphasizing both improvements and limitations of the HDI inclusion across different regions. In addition, we will provide quantitative comparisons to ensure that conclusions are fully supported by the results, highlighting where the HDI scaling improves model performance and where it does not.

We also make a commitment to ensure that the manuscript will explicitly discuss the assumptions, simplifications, and potential caveats associated with applying a globally linear HDI scaling, as well as the heterogeneity in socio-economic and fire regimes that may influence the results. These clarifications are intended to improve transparency and ensure that the conclusions are consistent with the evidence presented.

We thank the reviewer again for their constructive feedback and believe that these revisions address the major concern while retaining the novel contribution of the study.

Major comment: Inadequate phrasing and presentation of the manuscript

Title: The notion of “improving” should be removed.

We thank the reviewer for this suggestion. We agree and will change the manuscript title to “Assessing Historical Trends in the INFERNO Fire Model with the Human Development Index”

I14: “way to improve fire model performance”: this statement is too general and vague. Since there is no general improvement of performance, the aspects that are improved need to specified.

Thank you for this suggestion. We commit to changing this sentence to “this study demonstrates an effective and simple way to incorporate a socio-economic dimension in INFERNO through HDI.” In a revised version of the manuscript.

I16: A sentence in the abstract should be added to explain that a linear and globally-uniform inclusion of HDI as a simple approximation for socio-economic factors is a step forward but insufficient in many aspects.

We thank the reviewer for the constructive feedback. The authors will include a statement to make clear that this work is a step forward but insufficient in many aspects.

I211: “including socio-economic factors”: here and everywhere in the manuscript, the authors should not write that they include socio-economic factors, but that they include the HDI. The former wording suggests a more complex implementation than what is truly done, i.e., only including HDI. Nevertheless, this wording is repeatedly used (e.g., I278, I285, I389, I458, I465, I514, and many more).

We agree and suggest changing this phrasing to “representing the socio-economic factors through HDI”.

I254: “This evidence highlights that HDI can be used as an indicator of the role socio-economic factors play in mitigating fire activity”. This sentence is too strong. This evidence only shows that HDI can regionally capture part of the variability in BA.

Thank you for this suggestion. We agree and suggest changing this to “This evidence suggests that HDI can regionally capture part of the variability in burned area, reflecting some influence of socio-economic factors.”

I264: “aligning the model more closely with observations” should be: aligning the model more closely with observations in terms of global mean dependence on HDI.

Thank you. We agree with the suggestion and will change it in a revised version of the manuscript.

I259: “leading to improvements over North America, Europe and Asia, as shown in Figure 8”. Figure 8 only shows the bias, so this statement should be: leading to reduced bias (...). In general, it is important to be more precise in the wording, rather than using general terms not supported by the Figures and/or numbers that are referred to.

We appreciate this suggestion and agree to change the wording to be more precise and consistent with what Figure 8 is showing.

I293: Here, the text only mentions the part of the histogram where JIH performs better than JI (i.e., BA fractions between 0.7 and 1.0). The authors avoid addressing the BA fractions <0.5, where JI strongly outperforms JIH, and which represent the large majority of the fire occurrences.

We agree with the reviewer that the text here is not balanced. In the revised manuscript, this analysis will be rectified to include a focus on the BA fractions <0.5 and the use of objective metrics as supporting evidence.

I305: “In EURO, the inclusion of socio-economic factors better represents both small and moderate burnt area fractions”. This is not true. Fig. 9g does not show a better performance of JIH compared to JI.

Thank you for spotting this incorrect interpretation. We will correct this in a revised version of the manuscript.

I325: “Nevertheless, discrepancies remain in some regions” should be changed to: Nevertheless, discrepancies are exacerbated in some regions.

Thank you for this suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript.

I326: “the inclusion of HDI represents a significant advancement”: based on which metric is this strong statement made?

We agree to review the use of “significant” to ensure whenever used it is supported by statistical metrics.

I345: Here, the authors list regions of larger bias of JIH, but they omit that the bias is also larger for the global scale.

We acknowledge this comment and commit to addressing it in a revised version of the manuscript, highlighting that the global bias is also increased.

I348: “a reduction of bias and RMSE in regions where improvements are most needed”: based on which criteria do the authors estimate that some regions are more in need of improvements than others?

We thank the reviewer for drawing attention to this important aspect. This refers to the focus on the larger relative biases in JULES-INFERN0 (e.g., regions where the relative bias is larger than 150%). We commit to re-writing this sentence to clarify which regions are being targeted and on what basis.

I354: “improving the representation of global burnt area variability”: this statement is false, as is clearly shown in Fig. 10a. The reason for this false statement is that the metric used by the authors (STD/STD_{GFED4s}) is inadequate. That is because the standard deviation influenced by the magnitude of the trend. To provide an adequate measure of inter-annual variability, the authors should compute the standard deviation after removal of a linear trend. In this case, I am almost certain (based on a visual analysis of Fig. 10a) that inter-annual variability at the global scale is larger in JI than in JIH.

Thank you for the valuable comment. Please note that while Figure 10a) shows the annual mean burned area, the metric STD/STD_{GFED4s} is rather calculated in the monthly mean data. This means that there is a difference between the interannual variability and the monthly variability in the data. However, we acknowledge that this is not mentioned in the current version of the manuscript and commit to correcting the statement to ensure clarity and alignment with the results from Figure 10a).

I396: This paragraph illustrates that there are also compensating biases in JIH that lead to a deceptively better skill of JIH than JI for some aspects such as the global trend. However, while the authors often use the wording “compensating biases” when describing the performance of JI, they never use this wording to describe the JIH performance.

We thank the reviewer for this valuable insight. We acknowledge that all model representations inherently include compensating errors. In the manuscript, the term “*compensating biases*” was used specifically in relation to the bias metric when discussing JULES-INFERN0. We will clarify this point in the revised version of the manuscript to avoid any ambiguity and to ensure consistent terminology is used when referring to JULES-INFERN0 as well as JULES-INFERN0+HDI.

I485: “an improved representation of the relationship between burnt area and HDI” should be: an improved representation of the globally-averaged linear relationship between burnt area and HDI.

We agree with the suggestion and will change this in the revised manuscript.

I489: “The observed linear relationship between burnt area and HDI”. There is no observed linear relationship between HDI and BA (Fig. 3). It is approximated as linear by the authors. The fact that the posterior distribution for the slope is significantly negative does not mean

that the relationship is linear. It only means that if we assume a linear relationship, then there is a significant non-zero slope.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will change this paragraph to “In our analysis, we modelled the relationship between burned area (BA) and the Human Development Index (HDI) using a linear form for simplicity. It is important to note that this does not imply that the relationship is inherently linear in the observed data. This result should be interpreted as conditional on the linear model; it reflects a statistical trend rather than a directly observed linear relationship.”

I499: The authors omit to write explicitly that JIH has an increased bias compared to JI at the global scale (same as comment above about I345).

We acknowledge the reviewer’s valuable input and will revise the text to clarify this point. It should be noted that the global-scale burnt area simulated by JULES-INFERNNO includes contributions from regions with large positive biases exceeding +150% (TENA, CEAM, SHSA, EURO, and MIDE). The combined bias from these regions is +76.96 Mha (Table A1). These are precisely the regions targeted for bias reduction in JULES-INFERNNO+HDI. Considering that JULES-INFERNNO’s overall global bias is –34.35 Mha, removing the compensating effect of these highly biased regions would imply a potential global bias of approximately –111.31 Mha. This highlights the importance of addressing regional biases and highlights that although JULES-INFERNNO global bias is lower than JULES-INFERNNO+HDI, this is mostly due to compensating errors.

I511: “discrepancies against observations remain in JULES-INFERNNO+HDI” should be: discrepancies against observations are exacerbated in JULES-INFERNNO+HDI.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript.

I513: “where the model continues to underpredict medium and large fire sizes” should be: where the model underpredicts medium and large fire sizes more strongly.

We agree with the reviewer’s suggestion and will reflect this in a revised version of the manuscript.

I517: “misrepresents the observed positive burnt area trends found in TENA, MIDE and SEAS”: the region BONA should also be listed here.

Thank you. We agree and will reflect this in the revised manuscript.

I583: “Although this could be seen as a negative impact” should be: Although this further exacerbates the under-estimation of inter-annual variability in JULES-INFERNNO+HDI (...).

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in the revised manuscript.

I621: “provides a simple and linear representation of these effects”: replace representation by approximation.

We agree with the reviewer’s suggestion and will change this in the revised manuscript.

I621: “This leads to an improvement in model performance, especially in developed regions.” This statement is debatable. Again, there is no general increase in performance. So, such a statement should be more specific on which aspects are improved.

We agree with the suggestion and will reflect this in the revised manuscript by providing a statement on what specific aspects are improved. For example, the large biases that are present in JULES-INFERNNO for TENA, CEAM, SHSA, EURO, and MIDE.

Minor comment 1: Some methodological aspects are unclear

(a) Figure 2. It is not clear how the FWI is linearly regressed out of the BA. Is this regression performed at the level of individual grid cells or regions or globally? Does it use every monthly

BA value at each grid cell, or is it based on the climatology of the FWI and BA? Please provide more detail, and also a figure of the linear regression in the Appendix, along with an R² statistic.

(b) The Bayesian fitting procedure of Figures 3 and 6 should be better explained.

We thank the reviewer for pointing out that the methodology was not clear here. Both the Bayesian Linear Regression in Figures 3 and 6, and the Linear regression for the deweathered data (Figure 2) are performed at the level of individual grid cells for every monthly burnt area value. We will provide more detail on how this is done in the revised manuscript.

I126: “ δ BA having a log-normal distribution with mean of zero and standard deviation of ten ” . If δ BA has a log-normal distribution, this means that $\log(\delta \text{ BA})$ follows a Normal distribution, and thus that $\delta \text{ BA}$ is constrained to be positive. But by comparing the legend in Fig. 3 with Eq. (1), it appears that the posterior mean of $\delta \text{ BA}$ is -6.57. There is an inconsistency in the explanation of the priors and/or Eq. (1), which needs to be corrected.

We appreciate the reviewer’s careful observation. The posterior mean value of δBA (–6.57) is presented in the natural (untransformed) space, rather than in the log-transformed space, to facilitate a more intuitive interpretation of the results. We acknowledge that this distinction was not clearly stated in the manuscript and will revise the text to clarify that while δBA was modelled using a log-transformed prior distribution, the reported posterior values are shown in their natural units.

Figures 3 and 6: since the quantity of interest is $\delta \text{ BA}$, please show the posterior distributions of this coefficient in the main manuscript instead of the Appendix. Figures 3 and 6: Please specify if the grey points of the scatter plot represent all monthly BA values at all grid cells of GFED4s (Fig. 3) and of the models (Fig. 6). I believe so, but it is not explained.

We thank the reviewer for this valuable suggestion. We agree and will reflect this in the revised manuscript.

I128: Why did the authors chose to represent the posterior uncertainty with 145 posterior samples? This choice seems arbitrary.

The choice of 145 posterior samples was made pragmatically to provide a sufficiently large sample size to represent the posterior uncertainty while avoiding excessive clutter in the figures. This number ensures a clear visual representation of the spread and central tendency without compromising interpretability.

I123: “optimization over a normal posterior distribution” : do the authors mean a normal likelihood?

We thank the reviewer for this valuable suggestion. We agree and, to improve clarity, will change this to read “Parameter estimation was carried out within a Bayesian framework using the No-U-Turn Sampler (NUTS). The data likelihood was assumed to follow a normal distribution, and posterior samples were used to quantify parameter uncertainty.”

(c) The analysis of Figure 9, is very qualitative. I recommend that, in each subplot of Figure 9, the authors provide a quantitative metric of the fit of the JI and JIH histograms to the GFED4s histogram, for example the Wasserstein distance. Their analysis (from I283 to I331) would benefit from a more quantitative description of the performance.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will more quantitative analysis in the revised manuscript.

(d) Too many trends are analyzed in Figure 13 (11 model configurations times 15 spatial entities = 165 trend values). I recommend to show in Figure 13 only the trend values that are significantly different from zero, and to limit the analysis (I434 to I478) to the significant trends only.

We agree with the suggestion and will reflect this in a revised version of the manuscript.

Minor comment 2: Referencing of literature

The referencing in the manuscript does not currently meet the standards expected for Earth System Dynamics. Several statements lack appropriate citations where references are clearly needed, while others cite sources that do not adequately support the claims being made. In some cases, multiple references (often more than four) are grouped at the end of a paragraph to justify the entire content, which makes the specific contributions of individual studies unclear. It would be more informative to cite specific examples from the literature in direct connection with the relevant claims. Additionally, some important contributions from the existing literature that are highly relevant to this study are missing entirely. In this Minor comment, I try to provide specific instances where the referencing could be improved, and I hope that this will help the authors to better reference existing literature in the manuscript.

I21: “decline of 1.27% per year” : requires a citation.

We thank the reviewer for this comment, and will include a citation to Andela et al., 2017 to support this statement.

I24: “Climate is a key factor that also influences fire activity (Archibald et al., 2010; Andela et al., 2017; Jones et al., 2022; Kelley et al., 2019).” I do not think that 4 citations are needed to state this well-know fact. If these 4 studies are relevant here, then please indicate the specific aspects of these studies that are important to highlight.

Thank you for this comment, and considering this to be a well-known fact will remove the citations from the sentence.

I30: I believe that it is critical to cite the work of Marlon et al. (2008) here. In particular this study shows how anthropogenic factors have affected changes in fire activity over multi-decadal to multi-centennial time scales.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript.

I36: I believe that, in this paragraph, it is critical to cite the work of Forkel et al. (2019). Their quantification in a data-driven framework of the anthropogenic versus climate drivers of fire activity is very relevant to this study. (I recommend the authors to have a look at Fig. S13a,b, which they might find interesting).

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript.

I42: “However, most CMIP6 models do not adequately account for these suppression mechanisms, resulting in an overestimation of burned area and fire-related carbon emissions.” This statement is false. Please see Figure 2 It is clear that most CMIP6 models represent well global total BA and fire C emissions. And they tend to under-estimate rather than over-estimate these quantities.

The main focus of this whole paragraph is the decline of burnt area over the last two decades. This is not well reproduced by CMIP6 models, as shown by both Li et al. (2024) and (Andela et al. (2017). For clarity, the authors suggest rewriting the respective paragraph to read as below, ensuring it is clear that the focus is on burnt area trends.

“The study by Li et al. (2024) shows that the Earth System Models (ESMs) used to provide state-of-the-art climate projections for Phase 6 of the Coupled Model Intercomparison Project (CMIP6; Eyring et al., 2016) fail to reproduce the observed decline in global burned area and fire carbon emissions over the past two decades. They identify the primary reason for this discrepancy as an

underestimation of anthropogenic fire suppression in fire models. Key human-driven factors—such as agricultural expansion, land-use changes, fire management policies, and landscape fragmentation—have significantly reduced fire activity, particularly in tropical savannas (Andela et al., 2017). However, most CMIP6 models do not adequately represent these suppression mechanisms, leading to an overestimation of the temporal trend in burned area and associated fire-related carbon emissions.”

I48: At this point of the introduction, I believe that it would be valuable to shortly describe how existing fire models quantify changes in fire activity from climate change drivers versus anthropogenic influence. With regards to this aspect, referencing the study of Burton et al. (2024) would be relevant.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript.

I51: At this point of the introduction, I believe that it would be valuable to shortly explain that simulating fire accurately is also important for climate projections, because of two-way feedback processes between climate and fire. With regards to this aspect, referencing the study of Verjans et al. (2025) would be relevant.

We agree with the suggestion and will reflect this in a revised version of the manuscript.

I55: “The HDI has been used in various studies to better understand the socio-economic impacts on the Earth System (ES) (Ture, 2013; Hickel, 2020; Roy et al., 2023).” If these 3 studies are relevant here, then please indicate the specific aspects of these studies that are important to highlight.

We thank the reviewer for this valuable suggestion. We commit to expand on the specific aspects of these studies that are relevant for this work.

I57-61: I find this paragraph confusing because the notions of inter-annual variability and fire activity are used interchangeably. Please note that Chuvieco et al. (2021) only focus on inter-annual variability, and re-phrase the paragraph accordingly.

We agree with the suggestion and will change the paragraph to read as per below, ensuring more clarity on the notions of inter-annual variability and fire activity, as intended by the authors.

“Chuvieco et al. (2021) demonstrates that the HDI is strongly correlated with the inter-annual variability of burned area. Regions with higher HDI show lower variability, largely because increased mechanization and a shift away from agrarian livelihoods reduce the need for fire in agricultural practices. Conversely, areas with lower HDI exhibit greater variability, reflecting continued reliance on fire as a land management tool. Incorporating socio-economic indicators such as HDI into fire models significantly enhances their ability to reproduce observed patterns of variability.”

I63: “However, their approach was limited to agricultural fires and did not account for broader human factors in fire management.” This statement is false. Li et al. (2013) use a GDP-based parameterization that is not limited to agricultural fires. Please revise this paragraph.

Upon revaluation, we agree that our previous phrasing was misleading. While Li et al. (2013) do include agricultural fires, their GDP-based parameterization indeed extends to broader human influences on fire management. We will revise the paragraph to accurately reflect this, emphasizing the comprehensive scope of their approach.

I63: I believe that it is critical to cite the work of Perkins et al. (2024) here. In particular, please discuss recent more sophisticated attempts to incorporate anthropogenic drivers in fire modeling.

We agree with the suggestion and will reflect this in a revised version of the manuscript.

I77: “Several studies have shown that in developed regions, land and fire management policies play a more significant role in controlling fire ignitions than other human behaviours

(Nikolakis and Roberts, 2022; Jacobson et al., 2022; Ford et al., 2021; Curt and Frejaville, 2018; Carreiras et al., 2014; Mourão and Martinho, 2014).” If these 6 studies are relevant here, then please indicate the specific aspects of these studies that are important to highlight.

We thank the reviewer for this valuable suggestion. We will reflect this in a revised version of the manuscript.

I491: “For instance, the gross national income index indicates that higher HDI regions typically have more funding available for fire prevention and suppression efforts (Rideout et al., 2017).” This specific fact is not mentioned in Rideout et al. (2017).

The study by Rideout et al. (2017) is an example on how appropriate allocation of budgets impacts fire prevention and suppression efforts. We agree that this is not specifically mentioned in the study and will rephrase the sentence to appropriately reflect the intent of this citation.

I493: “Similarly, the life expectancy index suggests that these governments are more likely to implement policies aimed at mitigating the negative impacts of fire on their population (Rizzo and Rizzo, 2024).” This specific fact is not mentioned in Rizzo and Rizzo (2024).

We acknowledge this comment and will clarify this sentence in a revised version of the manuscript.

I494: “ Additionally, the education index highlights that educational initiatives can enhance community awareness and preparedness regarding fire risks and environmental stewardship (Prestemon et al., 2010).” Please be careful here, as this statement suggests a very general fact, while the study of Prestemon et al. (2010) focuses only on the state of Florida.

Thank you for the insight we commit to explicitly mention that the work from Prestemon et al. (2010) focuses only on the state of Florida in a revised version of the manuscript.

I540: “Although HDI does not encompass explicitly the impacts of fire management policies, these results are consistent with other studies, which show that for developed regions, land and fire management policies have a greater role than other human behaviours in controlling ignitions (Nikolakis and Roberts, 2022; Ford et al., 2021; Jacobson et al., 2022; Carreiras et al., 2014; Mourão and Martinho, 2014).” If these 5 studies are relevant here, then please indicate the specific aspects of these studies that are important to highlight.

We thank the reviewer for this valuable suggestion. We commit to expand on the specific aspects of these studies that are relevant for this work.

I552: “Several authors have also shown that declines in burnt area in the Mediterranean have occurred irrespective of increases in fire weather, as well as extensions to the fire weather season length, which is attributed to increased fire prevention and in combating and mitigating fire impacts (Jones et al., 2022; Urbieta et al., 2019; Carreiras et al., 2014; Mourão and Martinho, 2014).” If these 4 studies are relevant here, then please indicate the specific aspects of these studies that are important to highlight.

We commit to expand on the specific aspects of these studies that are relevant for this work.

I560: The facts about fire in the Amazonia region stated here should use citations at the end of the specific sentences, rather than grouping 3 citations together at the end of the paragraph.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript.

I573: “However, INFERNO has been developed for Earth System Modelling resolutions and timescales, and it is not expected to be able to capture the representation of the processes that drive large and severe fires” . Please be careful here, because this wording suggests that

this is a limitation inherent to all Earth System Models. However, this limitation is rather due to the use of BAPFT in Eq. (2). Please cite counter-examples, for example Lasslop et al. (2014).

We thank the reviewer for drawing our attention to this important aspect. Although implementations such as Lasslop et al. (2014) are based on modelling the representation of fire spread, the ability to represent this will also be impacted by Earth System Modelling resolutions and timescales, as the resolution that we run these systems do not allow all the details that are needed to fully represent fires spread processes to be captured. With this in mind, we commit to clarifying this in the revised manuscript, while also highlighting INFERNO's limitation due to the use of BAPFT.

I595: “In addition, biases in the underlying vegetation can significantly impact modelled burnt area” . Please also refer to the work of Forkel et al. (2019) here, as they demonstrated some widespread shortcomings of fire models in capturing the sensitivity of BA to leaf area index and plant productivity.

We agree with the suggestion and will reflect this in a revised version of the manuscript.

I603: “it is known that JULES vegetation has few needle-leaf trees across the boreal regions compared to observations” . Please provide a citation here.

Thank you. We agree with the suggestion and will reflect this in a revised version of the manuscript.

Specific comments

I1: “Earth System Models (ESM), have struggled to reproduce the historical decline in burnt area” : this statement is too crude. See for example Fig. 1 of Teckentrup et al. (2019), and it is also shown by Li et al. (2024) that most CMIP6 models capture the 1850-2010 trend.

Thank you very much for this helpful comment and for pointing out these relevant studies. We agree that our original statement was too general. In the revised manuscript, we will clarify that our comment refers specifically to the difficulty of Earth System Models in reproducing the decline in burnt area observed over the last two decades, rather than the full historical period.

I4: “formulation” should be plural.

We agree with the suggestion and will correct this in the revised manuscript

I5: Specify period of the trend.

We agree with the suggestion and will include this in the revised manuscript

I7: Change “reflects” to aims to reflect.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I7: Change “and, in turn” to , which in turn.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I9: Specify: reduces biases in annual burnt area for some regions, particularly (...)

We agree with the suggestion and will add more specific text in the revised manuscript

I15: Change “human-environment” to human-fire.

We agree with the suggestion and will change it in the revised manuscript

I18: Specify: climate change and variability.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I60: This is the first time that fire is mentioned as a land management tool. Up to here, the Introduction only focuses on fire suppression. This notion of land management needs to be introduced before.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I68: The objective is rather to model the influence of human populations on fire activity.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I97: Comma is missing after “Section 4” .

We agree with the suggestion and will reflect this in a revised version of the manuscript

I101: Here and in the remainder of the manuscript, correlation values should not be given in %.

We agree with the suggestion and will reflect this in a revised version of the manuscript

Figure 2: When testing for significance, did the authors apply a correction for false discovery rate? If not, this is needed here (please see Wilks, 2016).

The authors thank the reviewer for this observation. A correction for false discovery rate was not applied in the current analysis. We will implement this correction in the revised version of the manuscript to ensure a more robust assessment of statistical significance.

Figure 2 legend: “significant with a 95% confidence level” should be: significant at the 5% level.

We agree with the suggestion and will change this in the revised manuscript

I107: In the analysis of Fig. 2b, the first and most important aspect to focus on is that over most of the globe, the correlation between deweathered BA and HDI is not significant. The analysis should then only focus on the areas with statistical significance.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I107: “Figure 2 shows the spatial correlation coefficient” : I believe that Figure 2 shows the temporal correlation coefficient.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I114: Specify: a strong positive correlation with FWI.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I130: Typo: this method shows with an s.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I130: Specify: the observations show a log-linear decline.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I130: Specify: with a posterior mean slope of -6.57.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I140: If this is correct, specify: BAPFT is the average burnt area per fire for each PFT.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I142: “This decouples the fire spread stage from local meteorology” : I believe that this is not entirely true, because the FPFT depends on local meteorology.

Although FPFT depends on meteorology, it does not take into account wind or other localise effects. We commit to clarify this in a revised version of the manuscript.

I145: Please remove “significantly” .

We agree with the suggestion and will reflect this in a revised version of the manuscript

I186: Typo: dataset

Thank you, we will reflect this in a revised version of the manuscript

Figure 4: This figure should show a second sub-panel of fNS as a function of PD at different HDI levels.

We agree with the suggestion and will reflect this in a revised version of the manuscript

I202: The LIS-OTD climatology provides total lightning flash density. What parameterization is used to convert total flash density to cloud-to-ground flash density? Please specify this important aspect in the text.

We agree with the suggestion and will reflect this in a revised version of the manuscript, by specifying how this is partitioned.

I211: “(...) for the ignitions and suppression of fires. This is reflected in the BAPFT values (...)” should be rephrased to (...) for the ignitions and suppression of fires, which affects the BAPFT values (...).

We agree with the suggestion and will reflect this in a revised version of the manuscript

Table 1: Are these all the PFTs of the JULES model? Please specify in the caption.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript by including in the caption that these are all the PFTs of the JULES model.

Section 2.4: In my view, this section can be moved to the Appendix.

We agree with the suggestion and will reflect this in a revised version of the manuscript

Equations (9),(10,(11): The small n should be capital N.

We thank the reviewer for this valuable observation and will reflect this in a revised version of the manuscript

I245: “additional noise” is inappropriate wording here, and should be replaced by: residual variability.

We thank the reviewer for this valuable suggestion. We agree with the suggestion and will reflect this in a revised version of the manuscript