Author's response to the reviews

Reviewer 1

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 have revised the manuscript to present results more transparently, emphasizing both improvements and limitations of the HDI inclusion across different regions. In addition, we provided 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 ensured that the manuscript explicitly discusses the assumptions, simplifications, and potential caveats associated with applying a linear and global assumption made on the 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.

<u>Major comment: Inadequate phrasing and presentation of the manuscript</u>

Title: The notion of "improving" should be removed.

We thank the reviewer for this suggestion. We agree and have changed the manuscript title to "Assessing the Impact of the Human Development Index on Historical Trends in the INFEERNO Fire Model"

l14: "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. The abstract of the manuscript has been revised to ensure a more quantitative and balanced presentation of the results.

l16: 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 have included a reference to the globally uniform assumption made about HDI dependency as implemented in INFERNO, in lines 9 and 21 of the revised manuscript

l211: "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 truely done, i.e., only including HDI. Nevertheless, this wording is repeatedly used (e.g., l278, l285, l389, l458, l465, l514, and many more).

We agree and have changed this to "representing the socio-economic factors through HDI" throughout the revised version of the manuscript, including the abstract.

l254: "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 have changed this to "This evidence suggests that HDI can regionally capture part of the variability in burned area, reflecting some influence of socio-economic factors.". Line 374 of the revised manuscript.

l264: "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. Line 385 of the revised manuscript.

l259: "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 we have now changed the wording to be more precise and consistent with what Figure 9 (previously figure 8) is showing, throughout the paragraph starting in line 397 of the revised manuscript, stating that HDI mostly reduces the burnt area in regions with high prosperity (high values of HDI), leading to reduced bias over North America, Europe and Asia, as shown in Figure 9. In addition, when compared to JULES-INFERNO, JULES INFERNO+HDI reduces the positive bias over South America and India, although it increases the negative bias over the boreal regions, Australia, and South East Asia.

l293: 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. This analysis has been rectified to include a focus on the burnt area fractions <0.5. In addition, to support this, and as suggested by the reviewer, the Wasserstein distance has been included providing a metric of the fit between the probability distributions of the JULES-INFERNO and JULES-INFERNO+HDI histograms and the GFED4s observations. The values for the Wasserstein distances have been added to each panel of Figure 10 (previously Figure 9). These changes are covered from line 415 to 465 in the revised manuscript.

l305: "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 corrected this in the revised version of the manuscript. Line 437.

l325: "Nevertheless, discrepancies remain in some regions" should be changed to: Nevertheless, discrepancies are exacerbated in some regions.

Thank you for this suggestion. This paragraph has been rewritten and replaced with the paragraph starting in line 459 of the revised manuscript, highlighting the deterioration in the representation of small and moderate burnt area fraction in JULES-INFERNO+HDI, which dominate fire occurrence globally.

l326: "the inclusion of HDI represents a significant advancement": based on which metric is this strong statement made?

This has been rephrased to better agree with the Wasserstein distance to read as "These results highlight the need for further refinement of the socioeconomic parametrisation and the inclusion of additional region-specific processes to improve the simulation of fire size distributions across all fire regimes.". Line 460 of the revised manuscript.

l345: 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 have addressed it in the revised version of the manuscript. A paragraph, starting in line 492 provides an analysis of the global scale bias and errors. In addition, the paragraph staring in line 501 highlights that the improvements from JULES-INFERNO+HDI in regions such as TENA, NHAF, and SHAF have a greater impact on the global metrics than the reduced performance seen for regions such as CEAM, NHSA, SHSA, EURO, and MIDE.

l348: "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-INFERNO (e.g., regions where the relative bias is larger than 150%). The paragraph in line 474 now clarifies this by stating that "the inclusion of socio-economic factors in INFERNO leads to regional improvements in the simulation of burnt area in regions where JULESINFERNO bias exhibits largest deviations from GFED4s (greater than 150 %). For example, the relative bias in TENA is reduced from 735.6 % in JULES-INFERNO to 44.5 % in JULES-INFERNO+HDI, in CEAM from 259.2 % to 24.2 %, in SHSA from 191.7 % to -1.7 %, in EURO from 258.8 % to -48.8 %, and in MIDE from 420.5 % to 231.8 %"

l354: "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/STDGFED4s) 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 11a) (previously Figure 10) shows the annual mean burned area, the metric STD/STDGFED4s is rather calculated from the monthly mean data. This means that there is a difference between the interannual variability and the monthly variability in the data.

In the revised version of the manuscript, we have now provided values of standard deviation after removal of a linear trend and corrected the results to reflect the new values. The paragraph starting in line 486 reflect these changes.

l396: 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. We acknowledge the presence of compensating biases in JULES-INFERNO+HDI and have revised the manuscript to reflect this (e.g., line 540 of the revised manuscript.)

l485: "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 has been changed this in the revised manuscript, line 634 of the revised manuscript.

l489: "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 have changed this paragraph to "In our analysis, we modelled the relationship between burned area 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.". Line 639 of the revised manuscript.

l499: The authors omit to write explicitly that JIH has an increased bias compared to JI at the global scale (same as comment above about l345).

We acknowledge the reviewer's valuable input.

It should be noted that the global-scale burnt area simulated by JULES-INFERNO 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-INFERNO+HDI. Considering that JULES-INFERNO'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-INFERNO global bias is lower than JULES-INFERNO+HDI, this is mostly due to compensating errors.

The manuscript has been revised to clarify this point in lines 660 to 666 of the revised manuscript.

l511: "discrepancies against observations remain in JULES-INFERNO+HDI" should be: discrepancies against observations are exacerbated in JULES-INFERNO+HDI.

We thank the reviewer for this valuable suggestion. We have rewritten this sentence to reflect the revised analysis of the histograms in Figure10 (previously Figure 9). This now reads "Conversely, in

Northern Hemisphere South America (NHSA) and Australia and New Zealand (AUST), JULES-INFERNO+HDI underpredict medium and large fire sizes, highlighting the regional trade-offs associated with the socio-economic parametrisation.". Line 672 of the revised manuscript.

l513: "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 have reflected this in a revised version of the manuscript, line 673 of the revised manuscript.

l517: "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 have now included BONA in line 679 of the revised manuscript.

l583: "Although this could be seen as a negative impact" should be: Although this further exacerbates the under-estimation of inter-annual variability in JULES-INFERNO+HDI (...).

We thank the reviewer for this valuable suggestion. We agree with the suggestion and have reflected this in line 761 revised manuscript.

l621: "provides a simple and linear representation of these effects": replace representation by approximation.

We agree with the reviewer's suggestion and have changed this in line 807 the revised manuscript.

l621: "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 have specified that this approach improves the representation of regions where JULES-INFERNO exhibits large positive biases, such as TENA, CEAM, SHSA, EURO, and MIDE. However, the improvements are regionally variable, and in some areas the inclusion of HDI can reduce variability or exacerbate negative biases, highlighting the trade-offs associated with this simplified socio-economic parametrisation. Line 807 of the revised manuscript.

<u>Minor comment 1: Some methodological aspects are unclear</u>

(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. This has been clarified in lines 208-209 (for the deweatherering process) and line 245 (for the Bayesian Linear Regression) of the revised manuscript.

In addition, a full description of the Linear regression for the deweathered data has been included from lines 211 to 221 of the revised manuscript.

The Bayesian Linear Regression fitting procedure has been further detailed in lines 242 to 255 of the revised manuscript.

l126: " 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 (δ BA) follows a Normal distribution, and thus that δ BA is constrained to be positive. But by comparing the legend in

Fig. 3 with Eq. (1), it appears that the posterior mean of δ 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. Likewise, the results in Figures 3 and 6 are alo presented in the natural space. We acknowledge that this distinction was not clearly stated in the manuscript and have now included a statement to ensure this is clear to the reader in line 250 of the revise, manuscript and have corrected the captions of Figures 3 and 6. This clarifies 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 δ 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 have moved Figure A2 to the main manuscript, this is now Figure 7 in the revised manuscript. In addition, the captions of Figures 3 and 6 have been revised to clarify that the grey dots represent the scatter plot for all monthly burnt area values at all grid cells.

l128: 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. This has now been explicitly mentioned in line 252 of the revised manuscript.

l123: "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, have changed this to read "Posterior inference was carried out using the No-U-Turn Sampler (NUTS)" in line 248 of the revised manuscript. 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 quantiative metric of the fit of the JI and JIH histograms to the GFED4s histogram, for example the Wasserstein distance. Their analysis (from l283 to l331) would benefit from a more quantitative description of the performance.

We thank the reviewer for this valuable suggestion. We agree with the suggestion have revised the results referring to Figure 10 (previously Figure 9) from lines 415 to 465. This includes the use of Wasserstein distance to provide a quantitative metric of the fit.

(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 (l434 to l478) to the significant trends only.

We agree with the suggestion and have implemented this change in lines 580 to 626 of the 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.

Thank you for this constructive comment.

l21: “decline of 1.27% per year”: requires a citation.

We thank the reviewer for this comment, and have included a citation to Andela et al., 2017 to support this statement in line 27 of the revised manuscript.

l24: “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 we have removed the citations from the sentence (line 34 of the revised manuscript).

l30: 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 have included a reference to Marlon et al. (2008) in line 40 of the revised manuscript.

l36: 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 have included a reference to Forkel et al. (2019) work in line 55 of the revised manuscript.

l42: “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 have rewritten the respective paragraph to read as below, ensuring it is clear that the focus in 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.”

This can be found in line 51 of the revised manuscript.

l48: At this point of the introduction, I believe that it would be valuable to shortly describe how existing fire models quntify changes in fire activity from climate change drivers verus 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 the paragraph starting on line 70 of the revised manuscript to describe how existing fire models quantify changes in fire activity from climate change drivers versus anthropogenic influence.

l51: 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 have included a new paragraph starting on line 75 to explain that simulating fire accurately is also important for climate projections, because of two-way feedback processes between climate and fire.

l55: “The HDI has been used in various studies to better understand the socio-economic impacts on the Earth System (ES) (T¨ure, 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 helpful suggestion. We have revised the text (lines 84 to 86) to clarify the specific contributions of Türe (2013), Hickel (2020), and Roy et al. (2023), explicitly outlining how each study uses HDI to analyse distinct dimensions of socio-economic impacts on the Earth System.

l57-61: 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 have changed 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.”

This can be found in from lines 87 to 92 of the revised manuscript.

l63: “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 have revised the paragraph starting in line 99 of the revised manuscript to accurately reflect this, emphasizing the comprehensive scope of their approach.

l63: 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 have included a new paragraph starting in line 92 of the revised manuscript to introduce the work developed by Perkins et al. (2024) on WHAM!

l77: “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 helpful suggestion. We have revised the text (lines 116 to 129) to clarify the specific contributions of these authors, explicitly outlining how each study shows that in developed regions, fire occurrence and impacts are strongly shaped by governance and policy, through prevention and suppression capacity, resourcing, and coordination, rather than individual behaviour alone.

l491: “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 was not specifically mentioned in the study and have rephrased the sentence to appropriate reflect the intent of this citation in line 645 of the revised manuscript.

l493: “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).

Rizzo and Rizzo (2024) highlight that wildfire smoke has substantial adverse health impacts and therefore motivates mitigation and adaptation actions (e.g., risk communication and preparedness/early-warning measures) aimed at reducing exposure and protecting vulnerable populations. This has now been clarified in line 648 of the revised manuscript.

l494: “ 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 have explicitly mentioned that the work from Prestemon et al. (2010) focuses only on the state of Florida in a revised version of the manuscript. Line 650.

l540: “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; Mour˜ao 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 have revised the text (lines 701 to 711) to clarify the specific contributions of these authors, explicitly outlining how each study shows that governance quality, coordinated public policies, institutional fire management capacity, and sustained investment in land and fire management can substantially reduce burnt area and moderate the relationship between human activity and fire occurrence, particularly in highly developed regions.

l552: “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; Mour˜ao 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 have revised the text (lines 721 to 729) to clarify the specific contributions of these authors, explicitly outlining how each study shows that improvements in fire suppression efficiency, coordinated prevention policies, land management changes, and sustained institutional investment have offset the effects of increasing fire weather severity, leading to declining burnt area trends despite a lengthening fire season across the Mediterranean.

l560: 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 have reflected this in a revised version of the manuscript. Lines 734 to 739.

l573: “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 have clarified this in the revised manuscript, lines 753 to 756 of the revised manuscript, also highlighting INFERNO’s limitation due to the use of BAPFT.

l595: “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 have include a reference to the work of Forkle et al. (2019) on line 778 of the revised version of the manuscript.

l603: “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. A reference to the work of Burton et al. (2019) have been added to support this statement in line 789 of the revised manuscript.

<u>Specific comments</u>

l1: “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 have clarified 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. Line 1.

l4: “formulation” should be plural.

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

l5: Specify period of the trend.

We agree with the suggestion and have included this in line 6 of the revised manuscript

l7: Change “reflects” to aims to reflect.

We agree with the suggestion and have reflect this the revised version of the manuscript

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

We agree with the suggestion and have reflected 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 have add specific text on the impact on bias in line 12 to 17 of the revised manuscript

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

We agree with the suggestion and have changed it in the revised manuscript. Line 22.

I18: Specify: climate change and variability.

We agree with the suggestion and have reflected this in line 24 of the 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. The notion of fire being used as a land management tool has now been introduce in the paragraph stating in line 30 of the revised manuscript.

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

We agree with the suggestion and have reflected this in line 104 of the revised version of the manuscript

I97: Comma is missing after “Section 4”.

We agree with the suggestion and have reflect this in the revised version of the manuscript. Line 143.

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

We agree with the suggestion and have revised the manuscript to present the correlation values as fraction rather than percentage. This has been changed in the manuscript text, figures, and tables.

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 have now implemented this correction in the revised version of the manuscript to ensure a more robust assessment of statistical significance. Figure 2 has been updated to reflect this.

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

We agree with the suggestion and have changed this in Figure 2 of 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 have reflected this in the revised version of the manuscript. Lines 227 to 235.

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

We agree with the suggestion and have reflected this in line 225 of the revised version of the manuscript

l114: Specify: a strong positive correlation with FWI.

We agree with the suggestion and have reflected this in line 234 of the revised version of the manuscript

l130: Typo: this method shows with an s.

We agree with the suggestion and have reflected this in line 256 of the revised version of the manuscript

l130: Specify: the observations show a log-linear decline.

We agree with the suggestion and have reflected this in line 256 of the revised version of the manuscript

l130: Specify: with a posterior mean slope of -6.57.

We agree with the suggestion and have reflected this in line 256 of the revised version of the manuscript

l140: If this is correct, specify: BAPFT is the average burnt area per fire for each PFT.

We agree with the suggestion and have reflected this in line 266 of the revised version of the manuscript

l142: “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 have clarify this in line 267 of the revised version of the manuscript.

l145: Please remove “significantly”.

We agree with the suggestion and have reflected this in line 273 of the revised version of the manuscript

l186: Typo: dataset

We agree with the suggestion and have reflected this in line 315 of the 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 have included a **second sub-panel showing fNS as a function of PD at different HDI levels in Figure 4 of the revised manuscript.**

l202: 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.

The LIS-OTD climatology provides total lightning flash density (including both intra-cloud and cloud-to-ground flashes); following Christian et al. (2003), total lightning flashes are converted to cloud-to-ground flash density using an empirical partitioning based on observed global relationships between total and cloud-to-ground lightning. This have been included in lines 341 to 344 of the revised manuscript to provide more clarity to the reader.

l211: “(...) 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 have reflected this in line 352 of the 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 have reflected this in Table 1 of the 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 have moved this section to section A1 – Burnt area evaluation of the Appendix A of the revised manuscript.

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

We thank the reviewer for this valuable observation and have reflect this in **Equations A4, A5, and A6 of the** revised version of the manuscript

l245: “additional noise” is inappropriate wording here, and should be replaced by: residual variability.

We agree with the suggestion and have reflected this in line 366 of the revised version of the manuscript

Reviewer 2

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

This is a detailed and extensive piece of work that attempts to use HDI to improve the JULES-INFERNO fire model.

I'm considered to be a SME and I'm somewhat confused by the approach. It is not clear why the authors have taken this approach, primarily because so many critical factors in wildfire propagation are not included. By taking a global data set, 'de-weathering', the assumption seems to be that the remaining correlation is between GFED and HDI, which patently isn't the case.

Although INFERNO necessarily requires a simplification of the complex fire processes, my concern is that the approach taken here does not include processes that are a critical part of the system.

I'm not a statistician, but the work largely neglects the physical process driving fire behaviour. I wonder if an EOF analysis would be more appropriate.

The study shows a 'de-weathered burnt area fraction'. It is unclear to me what the physical meaning of this metric is and how can it sensibly be interpreted with context to current fire regimes and understanding of fire weather and fire behaviour. I would be surprised if the meaning is clear to other readers.

The processes driving fire activity globally are extremely complex and the various contributing factors are not fully acknowledged in the study or incorporated into the approach. A primary driver of fire is fuel availability and there is significant uncertainty in future fuel regimes (including fuel structure, dryness and landscape continuity) in a changing climate. Shifting hydrological regimes influencing fuel availability are a factor driving global fire activity in recent years. The focus in this paper seems disproportionately towards HDI and imbalanced with the other ingredients in fire regimes.

This is a very detailed and methodical study, but the statistical approach does not reconcile with the physical process that drive global fire activity, which are highly heterogeneous. The approach assumes that by 'de-weathering' the GFED dataset the remaining fire mapping is dependent on HDI.

We thank the reviewer for the detailed review and thoughtful comments.

The simplifications in INFERNO reflect the necessary design choice for global-scale fire modelling within Earth System Models (ESMs), where spatial resolution and computational constraints restrict explicit representation of local-scale physical fire processes such as topographic effects, fine-scale fuel heterogeneity, or detailed fire spread dynamics.

In general, global fire models (e.g., SPITFIRE, LPJ-GUESS-SPITFIRE, CLM-fire, and INFERNO) represent fire behaviour through parameterized relationships that link fire occurrence, spread, and emissions to key environmental drivers, primarily climate, vegetation (fuel type and load), and human activity. This approach enables large-scale consistency and interaction with biogeochemical cycles, which is essential for coupled ESM applications. While such models cannot resolve local fire dynamics, they successfully capture first-order global patterns of fire activity and response to climate and human influence, making them valuable tools for understanding fire–climate–carbon feedback at regional to global scales.

Regarding the deweathering method, this approach is used to remove the influence of climate-driven weather variability from the burned area data. The intent is to isolate one dominant fire driver (e.g., socio-economic factors) from the remaining influences, facilitating analysis of their effect on fire activity. We do not assume that the residual variability after deweathering is solely due to socio-economic factors; other drivers, including fuel availability, topography, and local management practices, may also contribute. This is explicitly noted in the revised manuscript to avoid overinterpretation of the residuals.

Together, INFERNO and the deweathering approach provide a framework for examining broad-scale patterns and trends, while acknowledging that fine-scale, local fire processes and heterogeneity are not fully resolved.

Specific comments.

- **Fig 3 shows the 'log transform 'deweathered' burnt area fraction % (of global fire emissions database)', (which is derived from satellite hot spot data and >12 years old). It is unclear how this should best be interpreted in context of fire regimes, or future emissions scenarios.**

We thank the reviewer for this comment. A log-transformation is applied to 'deweathered' burnt area fraction to meet the assumptions of linear regression and to improve the interpretability of the results. The burnt area fraction exhibited a right-skewed distribution, and log-transforming them reduced skewness and stabilized variance, helping to satisfy the assumptions of normality and homoscedasticity. In addition, because the logarithm is defined only for positive values, the transformation also ensured that all data points used in the analysis were non-negative.

To clarify the intent of this analysis, we note that this approach shows that the deweathered burnt area fraction declines with increasing HDI, with a mean slope of approximately –6.57 (%), based on the assumption of linearity. On this basis, we might expect future changes in HDI to influence fire emissions, in addition to changes in climate and other factors such as fuel load. We include this clarification here for completeness but do not intend to add these sentences to the revised manuscript.

- **GFED isn't explained in the text (it is in the appendix, but it's key to the main study and the Giglio study is from 2013, so a relatively old dataset (The Giglio reference date is 2013, but Figure 7 states 1997-2016). Giglio's paper shows a map with GFED regions; these are very heterogeneous (just one example is that Mediterranean Europe and Greenland are treated as homogeneous in the current study, while there are great differences in HDI and fire regimes across that spatial area).**

We thank the reviewer for these helpful comments. We have clarified the description of the Global Fire Emissions Database (GFED) in the main text, adding a new subsection from lines 177 to 186 of the revised manuscript, ensuring that its role in this study is clearly understood. GFED is a long-running, operationally updated dataset used globally for fire and emissions research. While the core methodological reference (Giglio et al., 2013) describes the development of the GFED4 framework, the dataset itself has been continuously updated to include fire emissions up to recent years (including 1997–2016 in our analysis). It is common practice in global environmental monitoring to cite the key methodological paper for such datasets even when newer data releases extend beyond the publication year.

Regarding the aggregation of GFED regions, we agree that they are spatially heterogeneous, and we note this as a limitation of using regionally averaged data in a global modelling framework. The focus of our analysis is on broad-scale relationships and trends between human development, climate, and fire activity, rather than capturing fine-scale spatial variability in fire regimes. We will add text to the revised manuscript to emphasize this point and to clarify how GFED data were used in the study.

- **The Canadian FWI has been used. The FWI is intended for use in forested areas of Canada. It was not designed to be used in Eurasia, tropical areas, grasslands or agricultural areas,**

rainforest vegetation or peat fuel. It captures short and medium term fuel drying. It does not capture seasonal grass growth or long-term drought. As it was intended to capture fuel availability in forest types, FWI does not readily translate to fuel availability across other landscapes, particularly on climate time scales.

The Canadian Fire Weather Index (FWI) is a component of the Canadian Forest Fire Danger Rating System (CFFDRS) and provides a numerical rating of fire intensity based solely on weather conditions (temperature, relative humidity, wind speed, and precipitation). Although it was originally developed for Canadian boreal forest conditions, the FWI system is not region-specific and has been successfully applied in diverse ecosystems worldwide, including in Europe, South America, Australia, and parts of Africa and Asia. Its broad adoption stems from its simplicity, weather-based formulation, and scalability. This has now been clarified from lines 190 to 192 of the revised manuscript.

- **The FWI is applied here globally outside it's intended aim. The calculation method (average time period used) is not explained clearly in the text. From Figure 1 it is unclear if this is averaged over all seasons and all years. Averaging at high latitudes will mask any extremes, which are typically the times when impactful fires occur.**

We thank the reviewer for this helpful comment and for pointing out the lack of clarity. In relation to the use of FWI, we have addressed this in response to the previous comment. To provide more clarity, Figure 1 presents the spatial distribution of the Fire Weather Index (FWI) over all seasons and years for the period 1997 to 2016 – as a climatology and trend - intended to illustrate the global pattern and long-term trend in fire-conducive conditions rather than to represent seasonal or interannual extremes. This averaged map is only used to provide contextual information on the broad-scale variability of FWI across regions.

However, for the more in-depth analyses presented in this work, the monthly FWI is used, not the long-term average. The use of monthly data preserves important temporal variability and enables a more detailed assessment of fire–climate relationships, including those relevant to high-latitude regions where extremes are important. We have revised the Figure 1 caption and the methods section (line 211) of the revised manuscript to explicitly state this distinction and clarify the calculation procedure to avoid misunderstanding.

- **HDI is averaged 1997-2016. This time period may not be is appropriate as input to projections (China for example is changing rapidly). HDI values are applied over large areas (countries) with heterogeneous land use and population density, which is inconsistent with typical fire regimes, which can vary considerable over relatively small areas, particularly with topographic and vegetation structure and noting that topography is not included in the framework.**

Thank you for this comment, clearly indicating that the manuscript was not entirely clear. As indicated above, Figure 1 is solely used to provide contextual information on the broad-scale variability of different variables, including the Human Development Index (HDI) across regions, and their trends over the period 1996 to 2017.

However, for the remaining analyses in the manuscript, the HDI data were not averaged over 1997–2016. Instead, we used an annual HDI dataset, which provides year-by-year values at the national level. These annual HDI values were supplied as inputs to the JULES-INFERNO+HDI framework and were used directly in the analysis without temporal averaging. We agree that HDI represents country-scale socioeconomic conditions and does not capture subnational heterogeneity in population density, land use, or topography. However, our goal was to assess the influence of large-scale human development trends on fire activity, consistent with the global scope of the model. Incorporating finer-scale socioeconomic and topographic variability is an important avenue for future work as higher-resolution datasets become available.

We have revised the Figure 1 caption and the corresponding text to explicitly state this distinction between the purpose of Figure 1 and the subsequent analysis, so that the temporal frequency and the time period of the data used is more explicitly stated, thus providing more clarity and avoiding any misunderstanding.

- **My understanding is that an objective of the modelling is to capture emissions, but there is no distinction made between low intensity and high intensity fires, in either the GFED4 data or FWI, or whether the fire is an agricultural fire or wildfire (which will have varying fuel consumption and therefore produce varying emissions), the seasonal timing of the fire activity, or the vertical extent of the fire, which are important considerations for emissions. For emissions, it is typically the large intense fires that are critically important as these are the ones which produce upper-troposphere and stratospheric injection of particulates and are often associated with incomplete combustion. Although INFERNO necessarily requires a simplification of the complex fire processes, my concern is that the approach taken here does not include processes that are a critical part of the system.**

We thank the reviewer for highlighting these important points regarding fire intensity and emissions processes. We acknowledge that INFERNO represents a simplified framework and does not explicitly distinguish between low- and high-intensity fires, agricultural versus wildfires, or the vertical injection of emissions. Similarly, the GFED4 data and the Fire Weather Index (FWI) inputs provide aggregate fire activity information rather than detailed characterization of combustion completeness or plume injection height.

The rationale for this approach is that INFERNO is designed to capture broad-scale, integrated fire activity and associated emissions trends at global and regional scales, rather than reproducing individual fire behaviour at sub-grid or event-specific resolution. Consequently, the model is most effective at estimating relative changes in emissions driven by climate, weather, and human development factors, while absolute emissions from extreme, high-intensity fires may be underrepresented.

We note that the model outputs remain useful for trend analysis, scenario comparison, and large-scale atmospheric impact assessment, but agree that incorporating processes such as fire intensity, fuel type, seasonal timing, and vertical extent would be required for a more mechanistic representation of emission injection and atmospheric transport, and we have identified this as an important direction for future work in a revised version of the manuscript. Lines 827 to 829.

- **Fuel availability is not considered. Fire processes are different in different landscapes and a global approach to describing fire regimes is problematic due to the variation in fuel type and fuel availability..**

We thank the reviewer for this comment. In INFERNO, fuel availability is represented through its coupling with the Joint UK Land Environment Simulator (JULES) land surface model, which simulates vegetation type, litter, and biomass carbon pools that determine the amount of combustible material within each grid cell. Although the model does not explicitly resolve fine-scale variations in fuel structure or composition, this approach allows fuel availability to vary dynamically with vegetation productivity and climate. We acknowledge that this represents a simplification of real-world fire processes and that differences in fuel type and landscape structure are not fully captured. This has been clarified in the paragraph starting in line 334 of the revised manuscript.

- **Fuel type and structure consumed is not discriminated, although this will be a significant factor in calculating emissions.**

We thank the reviewer for this valuable observation. In the INFERNO model, fuel availability is represented implicitly through the coupling with the JULES land surface model, which provides estimates of vegetation type, litter, and biomass carbon pools. These variables determine the potential fuel load available for combustion and thus influence fire occurrence and emissions. While INFERNO does not explicitly model fuel moisture or structural heterogeneity at sub-grid scales, this coupling enables the model to capture broad spatial and temporal variations in fuel availability consistent with the underlying vegetation dynamics and weather.

It is also worth noting that due to resolution and computational constraints, global fire modelling frameworks inevitably are required to simplify the diversity and complexity of fire processes across ecosystems, and that differences in fuel composition, structure, and continuity are not fully resolved at the model's spatial scale.

In addition, while other studies may use INFERNO for generating fire-related emissions, in this manuscript, the authors are solely investigating the potential use of the HDI in influencing fire activity and on the performance of the model in representing burned area fractions. In particular, this work does not include any analysis on the ability of INFERNO to represent fire emissions.

To clarify this a paragraph was added, line 361 of the revised manuscript to highlight that, although INFERNO has been used in other studies to estimate fire-related emissions, this manuscript focuses exclusively on assessing the influence of HDI on fire activity and on evaluating model performance in simulating burned area fractions. Fire emissions are therefore not analysed in this study.

- **Fuel availability is a key factor in landscape fires. Fire trends can and do vary hugely with different vegetation structure (eg grasslands often burn annually). The consideration of vegetation type and structure and fuel availability is rather superficial in this study, however in many parts of the world, this is likely to be a key ingredient in changing fire-climate regimes.**

We thank the reviewer for these detailed observations. In INFERNO, vegetation type, litter, and biomass carbon pools from the JULES land surface model provide an implicit representation of fuel availability, allowing broad-scale variation in fire activity to reflect vegetation productivity and climate. This approach does not explicitly capture fine-scale fuel structure, fuel continuity, or landscape fragmentation, which can influence fire size and spread in specific regions. To acknowledge this we have included a paragraph starting in line 46 of the revised manuscript.

- **Fuel continuity is a key factor influencing fire size. This can be (indirectly) related to HDI due to landscape features such as highways, railway lines, which often act as barriers to fire spread or can be used to support containment lines. Natural boundaries such as watercourses and topographic features can also constrain fire spread. Population density will also contribute to fragmentation of landscapes.**

We appreciate the reviewer's thoughtful observation. The primary focus of this study is to assess the influence of socio-economic factors (via HDI) on broad-scale fire activity, rather than to resolve sub-grid processes such as fuel continuity, natural or anthropogenic barriers, or topographic constraints. While we agree with the reviewer that these factors are important for local fire behaviour, global-scale models like INFERNO necessarily simplify these processes to maintain computational feasibility and large-scale consistency. We have clarified these limitations in the revised manuscript. Lines 335 to 338.

- **The term 'suppression', is used several times but is not clearly defined. Many landscape fires are not actively suppressed, or are only suppressed or contained in proximity to infrastructure and assets. Suppression and containment will vary considerably geographically and methods such as hand tools, backburning, fuel reduction will vary considerably dependent on resources, assets and fuel loads. Mitigation efforts such as planned burns and fuel management will also vary with HDI (and resources). In addition, many fires extinguish overnight, dependent on favourable weather conditions, fuel availability and fuel continuity, but these processes are not captured in the current study.**

We thank the reviewer for this comment, and we have provided a definition of suppression in the context of this work in line 135 of the revised manuscript.

Short comments

- **The fire-science terminology could be used more precisely. Examples include 'fire models' and 'fire simulators'.**

In this context, we use the term fire model as a mathematical representation of a real-world system that simulates fire behaviour under different conditions. The term *"fire model"* is used consistently to describe approaches that estimate fire occurrence, spread, and emissions at large spatial and temporal scales. We agree that stating this will improve clarity and have included this in line 54 of the revised version of the manuscript. Please note that the term *"simulator"* is only used as part of the name of the land surface model JULES (Joint UK Land Environment Simulator).

- **L40 A discussion on how the processes are included in CMIP frameworks would be useful**

We thank the reviewer for this comment and we have included a brief description on how different CMIP models represent fire in line 47 of the revised version of the manuscript.

- **L105 FWI is not the influence of climate**

To account for climate effects on fire activity without implying that FWI represents climate itself, we use the FWI as a weather-driven indicator of fire conditions. Specifically, we apply a linear regression of normalized burnt area against FWI to capture the component of fire activity attributable to short-term weather variations. The predicted effect of FWI from this regression is then subtracted from the normalized burnt area, producing a “deweathered” dataset in which the influence of daily weather variability is removed. This approach allows us to isolate and analyse the influence of socio-economic factors, such as HDI, on fire activity while controlling for climate-driven variability.

The deweathering process have been further detailed in lines 211 to 224 of the revised version of the manuscript.

- **L140 topography is a key factor in fire spread as is local meteorology.**

We thank the reviewer for this important observation. In INFERNO, the burnt area calculation is decoupled from local meteorology and topography, as these processes are not typically resolved at the coarse spatial scales used in Earth System Models (ESMs). Instead, INFERNO relies on plant functional type (PFT)-specific flammability (including meteorological components) and fire occurrence metrics, including ignitions and average burnt area per fire, which capture broad-scale patterns of fire activity. While this approach does not explicitly represent topographic effects or fine-scale weather variability, it allows the model to simulate large-scale, climate- and vegetation-driven fire dynamics. We acknowledge this as a limitation and will better reflect this in a revised version of the manuscript noting that sub-grid heterogeneity in topography and local meteorology is not resolved, which may influence local fire spread.

This have been clarified from lines 267 to 271 of the revised manuscript.

- **L180 It is not clear to me how the 1860 -2016 dataset is applied**

We thank the reviewer for this comment. The JULES model was run from 1860 to allow for an appropriate spin-up period and to ensure that the model’s biogeophysical states (e.g., soil carbon pools, vegetation structure) reached equilibrium before the main analysis period. Running the model from this historical start date provides a physically consistent baseline for simulating subsequent changes.

In addition, a time series of HDI values was prescribed for each model grid cell throughout the simulation period. This allowed the model to represent spatial and temporal variations in human development over time and to evaluate how these changes interact with environmental drivers in shaping fire dynamics.

We provide this clarification here for completeness but do not propose to add these details to the revised manuscript.

- **L200 What are the origins of this approach for global vegetation structure, particularly its relevance to fire prone environments. 40% seems high for many trees. The vegetation types described aren’t reflective of many fire regimes.**

The origin of this approach is documented in Burton et al. (2019) and Burton et al. (2020), this work describes the methodology for including fire mortality in INFERNO and includes a comprehensive evaluation of the model performance in representing the evolution of vegetation within the context of JULES model. The mortality parameter is specified in Burton et al. (2019) and is derived from current literature. We thank the reviewer for the comment and have clarified this from lines 327 to 331 of the revised version of the manuscript.

- **L250 this could be due to fuel availability and continuity, but these factors haven't been included.**

We agree that fuel availability and continuity are important determinants of fire activity and may influence trends in burnt area (BA) after deweathering. In our analysis, we make the simplifying assumption that, once the influence of weather is removed, residual variations or trends in BA can be primarily attributed to socioeconomic factors, represented here by HDI. We then use the model simulations to test this assumption and to evaluate how including the HDI–BA relationship affects model performance.

We acknowledge that fuel availability and continuity can influence BA trends. These processes are represented in INFERNO via its coupling to the land surface scheme, however, they are not included in the empirical relationship used to derive the HDI dependence

To address this, a new paragraph was added, lines 220 to 224, in the revised version of the manuscript.

- **How are large and small fire sizes categorised (0.2, 0.6 etc is not a fire size)**

We thank the reviewer for this comment. The values 0.2 and 0.6 refer to burnt area fractions, not individual fire sizes. These fractions represent the proportion of each grid cell that is burned, which we use to categorise "small" and "large" fire events in the analysis. We have made this clearer in a revised version of the manuscript(e.g., lines 417, 418, and 434 of the revised manuscript).

- **Maps of the regions are not shown until the appendix and the regions are highly internally heterogeneous with regard to HDI and fire regimes.**

The regions used in our analysis are defined according to GFED4 (Giglio et al., 2013) and have been widely adopted in global fire studies. While we acknowledge that these regions are internally heterogeneous with respect to HDI and fire regimes, they provide a standardized framework for comparing fire activity and emissions at large spatial scales.

Given the widespread use of the GFED4 regions in previous studies, as exemplified by the following list, we respectfully suggest that this figure remains in the Appendix.

Burton, Chantelle A., et al. "Fire weakens land carbon sinks before 1.5° C." Nature Geoscience 17.11 (2024): 1108-1114.

Chen, Yang, et al. "Multi-decadal trends and variability in burned area from the 5th version of the Global Fire Emissions Database (GFED5)." Earth System Science Data Discussions 2023 (2023): 1-52.

Giglio, Louis, James T. Randerson, and Guido R. Van Der Werf. "Analysis of daily, monthly, and annual burned area using the fourth‐generation global fire emissions database (GFED4)." Journal of Geophysical Research: Biogeosciences 118.1 (2013): 317-328.

Jones, Matthew W., et al. "Global and regional trends and drivers of fire under climate change." Reviews of Geophysics 60.3 (2022): e2020RG000726.

van Wees, Dave, et al. "Global biomass burning fuel consumption and emissions at 500-m spatial resolution based on the Global Fire Emissions Database (GFED)." Geoscientific Model Development Discussions 2022 (2022): 1-46.

- **What is 'burnt area fraction' a fraction of? Giglio 2012 specifies this as the fraction of each grid cell that burns each year but that is not clear in the current study.**

We thank the reviewer for this comment and have ensure this is clearer in lines 225 and 226 of the revised version of the manuscript.

- **Fig 10. It is hard to compare these plots due to the varying y axes. The interannual variability seen in the observations is interesting, but is not well captured in the smoothed JuELS-INFERNO-HDI, which is a concern.**

The varying y axes are necessary to reflect the wide range of burnt area magnitudes across different fire regions, allowing each region's variability to be visualized clearly. Regarding the interannual variability, this limitation is addressed in the manuscript (Section 4.2, lines 580–585 of the original manuscript), where we note that the incorporation of socio-economic factors in INFERNO via HDI reduces interannual variability for most regions. While this smoothing improves model performance in some regions (e.g., TENA and CEAM), it reduces the model's ability to capture high variability in regions such as BONA, BOAS, AUST, CEAS, SHSA, and NHSA. Importantly, even the control model (JULES-INFERNO) with larger interannual variability still underperforms relative to observations in these regions. This is clarified in the paragraph starting in line 486 of the revised version of the manuscript.

- **There are many acronyms, some of which are not expanded (eg GFED4). Also several small typos.**

We thank the reviewer for this comment and will ensure these are addressed in the revised version of the manuscript (e.g., line 178).