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

The authors aim to describe the representation of socio-economic factors in a global fire model using HDI. They describe applying a linear term to the human ignition parameterisations in INFERNO and argue that it improves the model performance in general as well as producing more accurate burnt area patterns.

Apart from a decrease in bias in some regions, performance decreases in other and especially the global values of burnt area are significantly worse than in the non-HDI version of INFERNO.

The authors are grateful for the insightful feedback provided by the reviewer on our manuscript. Your comments are valuable to us, and we will address the concerns you have raised in the attached detailed discussion. We pledge to revise our manuscript to enhance the clarity on the implementation of socio-economic factors in the INFERNO global fire model using HDI.

The approach presented in this work does improve the results from INFERNO by reducing the large bias produced by the model. JULES-INFERNO has large positive bias at a regional level, for example the bias off regions such as TENA (17.21), CEAM (4.4), SHSA (49.24), EURO (2.23), and MIDE (3.88) total to 76.96 Mha. All these biases are reduced in JULES-INFERNO+HDI. This alone shows that JULES-INFERNO performs well at the global scale as regional biases compensate each other.

Fig A1 and table 2 show that the HDI implementation doesn't seem to work at all in areas with low to very low population density and still high HDI like AUST and BOAS. This is not a model improvement, and it doesn't show the potential of including HDI in a global fire model. INFERNO is considered a global fire model and, therefore, an effort to add extra value to the model should aim at a general increase in performance.

Although there is as a negative impact in some of the regions, this is either small (e.g., the difference in the metric is in the order of the decimal place), or the negative impact is understood and discussed in Section 4.2 Model limitations and known issues. It should be highlighted that in the discussion model limitations and known issues we have identified that mechanisms that dominate the fire behaviour of some regions are not represented in INFERNO, the fact that JULES-INFERNO perform better in regions dominated by peat land fires and high interannual variability.

The improvements that JULES-INFERNO+HDI brings to some of the regions such as TENA, NHAF, and SHAF have a greater impact in the global standard deviation than the degradation of the standard deviation seen for regions such as CEAM, NHSA, SHSA, EURO, and MIDE. For regions such as BOAS, CEADS, SEAS, EQAS, and AUST, both model configurations perform poorly in terms of standard deviation and any differences between the STD/STDGFED4s are small when compared to the observed standard deviation (e.g., difference between the JULES-INFERNO and JULES-INFERNO+HDI STD/STDGFED4s smaller than 15%).

Furthermore, for some of these regions INFERNO is not expect to perform, especially in terms of variability. As discussed in Section 4, the fire behaviour of some of these regions is characterised by mechanisms that are not represented in INFERNO, therefore INFERNO is not expected to perform well in these regions.

The authors have updated the model by implementing revised per-PFT-BurntArea values that are independent of the implementation of HDI and added a (1-HDI) term to the ignition equations in the fire-model (equations 2&3).

Previous per-PFT-BurntArea values defined by Mangeon et al (2016) were heuristically determined, as referred in their work. These values account for the tunning towards reproducing the observed average burnt areas and may compensate for processes not represented in this implementation of INFERNO.

In this work we revised these parameters and used values that are supported by Andela et al. (2019).

The results presented in this study suggest that the straight-forward application of this dampening term (1-HDI) is not sufficient to improve the global performance. A look at Fig A2 and the regional burnt area for AUST and BOAS might suggest an application of a correction term that might weigh HDI itself by e.g. human population density, as it seems unlikely that a generally high HDI should still have its maximal effect in remote regions.

With regards to the approach taken to represent the relationships between the socio-economic effects on fire ignitions and suppression and HDI, it should be noted that socio-economic impacts on fire are complex and dependent on may factor that are difficult to model, depend on government policies, as well as cultural behaviour. The work by Pandey et al. (2023) is a good example of a study that highlights this complexity, as well as the different fire management policies around the world, showing that despite their differences they all result in a gradual reduction in fire occurrences and burned areas over time. In addition, the formulation of climate/ESM does not allow for representing these details.

The aim of this study is not to represent that complexity but rather to explore the use of the HDI to represent socio-economic impacts on fires, aiming to improve the regional representation of human–environmental coupling for applications at large spatial scales within an Earth System Model (ESM) context. As stated in the work of Mangeon et al. (2016), INFERNO is a simple physically based representation of fire activity aimed at representing fires in the ESM context. The current implementation of the HDI aims to follow that same philosophy.

Further, one could imagine that a retuning of the whole set of empirical parameters in equations 2-4 might help.

As the reviewer mention, the equations used could be tuned to provide the best results. However, that could be masking compensating bias that are existent in the model. For example, the formulation of INFERNO described in Mangeon et al. (2016) overestimated the ignitions and suppression of fires. At the time this formulation was developed average burnt area values where heuristically determined, while posterior work by Andela et al. (2019) shows that these values can be ten times larger than the ones used by Mangeon et al. (2016). Including this HDI based parametrisation it is needed, when average burnt area values in INFERNO are used to match the ones reported by Andela et al. (2017). The authors commit to better explain this in a revise manuscript and increase the clarity of the reader.

Finally, a linear application of (1-HDI) seems arbitrary. A derivation of a factor depending on HDI for equations 2 and 3 is needed to justify the approach of choice.

Despite being a simple representation while trying to encompass, this approach does align with the few studies found in literature that looked at the impact governmental policies have on prevention of wildfires. For example, the work by Curt and Frejaville (2017) shows that, the wildfire policies implemented in in mediterranean France, resulted in the number of fires has decreased almost linearly since 1975, whereas the burned area changed more abruptly.

The authors thank the reviewer for promoting this constructive discussion and agree that there should be more detail analysis and explanation of this at an early stage in the manuscript, and commit to improve this, including this discussion in a revised version of the manuscript.

Reply to Specific Comments (original reviewer comment presented in bold)

General remarks:

- **I would like to see the temporal and spatial resolution of HDI described? (fig A2 only gives a hind)**

  The authors thank the reviewer for raising this and commit to include figures depicting the temporal and spatial properties of HDI.

- **GFED4s: I assume that it is actually GFED4.1s, right?**

  Yes, data used in this work and referred to as GFED4s corresponds to the Global Fire Emissions Database, Version 4.1

Comments by line:

8-9: Please describe what you mean by "reduces[..] positive biases[..] by more than 100%" Is it reducing a bias of 700% by 100% -> 600%?

In this sentence we want to reference that large bias were reduces for example, 700% by 100% -> 600%. The authors commit to revise this sentence as it can cause confusion to the reader.

48: Maybe replace "In this study" with "In their study". It is a little ambiguous.

The authors thank the reviewer for the suggestion and commit to make this change in a revised version of the manuscript.

102: It would be nice to mention that you revised the empirical parameters for INFERNO (as you did in lines 153ff) already here.

The authors thank the reviewer for the suggestion and commit to make this change in a revised version of the manuscript.

131: Please, explain the ES in JULES-ES (Earth System?).

In JULES-ES, ES stands for Earth System. The authors commit to clarify this in a revised manuscript

132: "*JULES simulates surface fluxes of water, energy, vegetation and carbon*" Vegetation is named as a flux here. Please, re-phrase the sentence.

The authors commit to rephrase this sentence to read as "*JULES simulates surface fluxes of water, energy, as well as vegetation and carbon*" in a revised version of the manuscript.

145: You write that the Analysis will be performed over the years 1997-2015 while you state in line 137 that you will analyse 1997-2016.

The authors thank the reviewer for highlighting this and will correct this in a revised version of the manuscript to reflect the period analysed – 1997 – 2016.

155 & Table1: The revised values differ substantially from the original ones and, since they have not been a result of model tuning towards the new algorithm, an explanation is needed as to why these are more suitable.

At the time INFERNO was developed by Mangeon et al. (2016) the average burnt area values where heuristically determined. Posterior work by Andela et al. (2019) shows that these values can larger than the ones used by Mangeon et al. (2016) and in this work the authors update these values according to the constraints available in the literature. The authors commit to better explain this in a revise manuscript and increase the clarity of the reader.

173: JULES-INFERNO+HDI seems to increase the negative bias the most in SHAF but it is not

mentioned here.

The SHAF bias is increased from -45.27% in JULES-INFERNO to -55.31% in JULES-INFERNO+HDI. Although this can be considered a small degradation to performance compared to the improvements seen for other regions, the authors agree that referencing this in the manuscript can increase the clarity on the impacts of introducing the use of HDI has in the model and commit to include this in a revised version of the manuscript.

Figure 4: I found it quite hard to read those two maps, especially where there is not much of a difference between the two simulations. It might help to only plot stippling where there is a significant relative decrease in bias. Slightly bigger maps might help as well. Maybe cut off Antarctica, for example.

The authors thank the reviewer for this feedback and agree to apply the suggested improvements in a revised version of the manuscript.

Figure 5: It would be nice to have colors here. The grey-scales are hard to distinguish. I would show all areas here, especially the Africas, as they are very important areas for fire and those, where Chuvieco et al. consider HDI the most important for interannual variability.

The authors thank the reviewer for this feedback and agree to apply the suggested improvements in a revised version of the manuscript.

175: This sentence implies that there is an improvement in some regions while it might deteriorate in others, when in fact it seems you added a negative correction globally which might improve at large positive biases but will make negative biases worse.

The authors agree with the reviewer and commit to rephrase this sentence to reflect that this has an impact in all regions, with the greater changed being applied to regions with high prosperity (HDI), as evident in Figure 1.

179: Here, you state "JULES-INFERNO+HDI has a smaller bias than JULES-INFERNO globally, except for savanna regions in Africa, Australia, and central Eurasia." Globally the bias has significantly worsened when, just as in the previous comment, it improves the positive biases while it worsens the negative ones. Maybe choose a more neutral wording.

The authors thank the reviewer for highlighting this and commit to rephrase this sentence to reflect suggestions made.

188ff: Please describe in more detail what e.g. The STD is. Is it every-grid point or annual totals, what is STD/STDgfed4s supposed to show etc….

STD corresponds to S defined in equation 10, the authors commit to correct the references to standard deviation in a revised version of the manuscript.

STD/STDgfed4s shows the ratio between the modelled and observed standard deviation. In a perfect model forecast STD/STDgfed4s = 1.

Mention $\ddot{\emptyset}_x''''$ as constant bias to be removed.

The authors thank the reviewer for highlighting this and commit to include this in a revised version of the manuscript.

Equations 9 & 11: I suppose you generate the observation-bias from different types of burnt area observations as you state in line 146, but in the context of Figure 5 you only mention GFED4. I would

In equations 9 and 11 the metrics were calculated based on the temporal dimension of the observed data, as described in line 200. Only GFED4 was used line 145 refer should refer to the singular – observation. The authors thanks the reviewer for highlighting this and commit to correct this a revised manuscript.

A HDI map for e.g. 2016 would be nice get an impression of what the dataset actually looks like.

The authors thank the reviewer for the suggestion and agree that this should be included in a future revised version of the manuscript.

Table 2: I find it very difficult to look at. Please, do not use separators between each cell. Maybe only have separators between models and GFED data blocks. Further, it is just pure numbers, it might be more educative to have bold numbers for better performing model or even a colour code ranking them. Overall, I think this table should not be in the main article, because it is not a "product" of the article that others might later use, I would put it into the Appendix, but up to you.

The authors thank the reviewer and agree that this suggestion will significantly improve the readability of Table 2. The authors commit to improve this in a revised manuscript, while being mindful that the journal guidelines discourage the use of colours in tables.

Line 226: It is "RMSE_UB", I suppose.

Yes, the authors thank the reviewer for highlighting this and commit to make this correction in a revised version of the manuscript.

Section 3.2: I think, it would be good to add uncertainties to the trends presented in this section, both modeled and observed. Due It is hard to figure out the actual information in Figure 6.

The authors thank the reviewer for the suggestion and agree that this should be included in a future revised version of the manuscript.

317: Unfortunately, I do not share the authors opinion that this study has shown that an inclusion of HDI is necessary to improve the model. The model itself seems to need an update to be able to deal with the information added.

The authors would like to note that the focus of this work is not on a specific analysis of how HDI is used as a predictor for fire activity. Socio-economic impacts on fire are complex and dependent on may factor that are difficult to represent in Earth System Models (ESM). These factors depend on policies implemented at government level, as well as cultural behaviour which varies widely across the world. In addition, it needs to be highlighted the formulation of Climate and ESM does not allow for representing these details.

The aim of this study is not to represent that complexity achieve that but rather to explore the use of the HDI to represent socio-economic impacts on fires, aiming to improve the regional representation of human–environmental coupling for applications at large spatial scales within an Earth System Model (ESM) context.

The results presented in this study show that including socio-economic factors in the fire ignition and suppression parametrisation within INFERNO leads to improved performance in regions that were affected by large biases in the JULES-INFERNO configuration, providing evidence a missing mechanism in JULES-INFERNO was the main driver of such bias. This also aligns with the available literature where several authors have shown that declines in burnt area (e.g., in the Mediterranean) have occurred irrespective of increases in fire weather 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).

The authors agree with the reviewer that the model itself seems to need an update. This works presents a significant step in this direction.

320: Remove "Discussion & Conclusion"

The authors thank the reviewer for the suggestion and agree to rename this section in a revised version of the manuscript.

321: Pechony and Shindell (2009) fire ignitions aim to reproduce anthropogenic ignitions including any circumstances (like HDI). The empirical values therein might not hold anymore when applying a term for HDI.

The fire ignitions parametrisation proposed by Pechony and Shindell (2009) only account for urban versus rural differentiation in terms out human behaviour impacting ignitions and suppression. This relation continues to withhold even when the HDI term is introduced.

From Pechony and Shindell (2009) starting at paragraph 13:

> *"Humans actively suppress both anthropogenic and natural fires. Firefighting policies and their effectiveness depend on cultural, economical, and other factors. In general, success of fire suppression depends on early fire detection. We assume that in highly populated areas fires are detected earlier and suppressed more effectively than in scarcely populated areas, and the fraction of suppressed fires increases with increasing population density. Assuming exponential dependence, we can formulate the fraction of nonsuppressed fires, fNS as:*
>
> $$f_{NS} = c_1 + c_2 \exp(-\omega PD) \qquad (6)$$
>
> *The fraction of fires that remain unsuppressed at the most populated areas is expressed by c1. The maximum number of fires that remain unsuppressed at the distant, unpopulated regions is defined by the sum of c1 and c2, and the rate at which the number of unsuppressed fires decreases with increasing population density is determined by ω. Owing to the lack of global quantitative data, constant values are selected in a rather heuristic manner: c1 = 0.05, c2 = 0.9, ω = 0.05. Thus, up to 95% of fires are assumed to be suppressed in the densely populated regions, and 95% are assumed to remain unsuppressed in unpopulated regions. <u>When appropriate global data becomes available, these constants can be determined more accurately and can also vary across the globe, and with time to reflect different fire suppression capabilities in different socio-economic conditions</u>."*

Considering this, it should be noted that the HDI implementation scales both c1 and c2 according to HDI.

As the reviewer mention, the equations used could be tuned to provide the best results. However, that could be masking compensating bias that are existent in the model, not allowing to have a good understanding of the limitations that are still present in the model allowing for the discussion available in Section 4.2 Model limitations and known issues.

326: I think it shows that performance is improve in regions with large positive(!) biases. This is somewhat expected when dampening terms are included.

As the reviewer mentions, a dampening term results in a reduction of positive bias. However, what this work shows is that the authors include a dampening term (1 - HDI) in the ignitions and suppression functions of INFERNO, and this results in a reduction of the large positive bias that are seen for JULES-INFERNO, while having a smaller impact in other regions.

The authors thank the reviewer for this comment and commit to rephrase this sentence.

327: I think it can be stated that at least in AUST and SEAS the performance has been "well" before and has deteriorated significantly when introducing HDI. Not to mention the global performance.

The authors thank the review and agree that this sentence should be rephrase to acknowledge that there is a degradation of performance in some regions (e.g., AUST and SEAS), committing to improve this in a revised version of the manuscript.

Regarding the global performance, it should be noted that JULES-INFERNO has large positive bias at a regional level, for example the bias off regions such as TENA (17.21), CEAM (4.4), SHSA (49.24), EURO (2.23), and MIDE (3.88) total to 76.96 Mha. All these biases are reduced in JULES-INFERNO+HDI, and are no other positive bias are introduced. This alone shows that JULES-INFERNO performs well at the global scale as reginal bias compensate each other. This is highlighted in section 4 – Conclusions, in lines 330 and 331, the authors agree that improving this sentence would strengthen the manuscript.

330: In INFERNO+HDI these compensating errors are even bigger. The pattern might be better, but it is even more skewed than INFERNO.

The improvements that JULES-INFERNO+HDI to some of the regions such as TENA, NHAF, and SHAF have a greater impact in the reduction the errors for these regions than the degradation of errors seen for other regions. For example, in regions such as BONA, NHAF, SHAF, and EQAS, although the bias is larger in JULES-INFERNO+HDI, the difference between this model configuration bias and the bias for JULEES-INFERNO is less than 15%.

Furthermore, for some of the regions INFERNO is not expect to perform, especially in terms of variability. As discussed in Section 4, the fire behaviour of some of these regions is characterised by mechanisms that are not represented in INFERNO, therefore INFERNO is not expected to perform well in these regions.

The authors thank the reviewer for promoting this constructive discussion and agree that there should be more detail analysis and explanation of this at an early stage in the manuscript, and commit to improve this, including this discussion in a revised version of the manuscript.

337ff: Trends of 20 years of fire data are to be taken with a grain of salt, I think. As mentioned earlier, if we take the uncertainties of these trends into account, there might even be no improvement.

The authors thank the reviewer for the comment and agree with the reviewer that there are uncertainties regarding fire data and that this as an impact on the evaluation.

429: Doesn't it in fact add new compensating biases?

The improvements that JULES-INFERNO+HDI to some of the regions such as TENA, NHAF, and SHAF have a greater impact in the reduction the errors for these regions than the degradation of errors seen for other regions. For example, in regions such as BONA, NHAF, SHAF, and EQAS, although the bias is larger in JULES-INFERNO+HDI, the difference between this model configuration bias and the bias for JULEES-INFERNO is less than 15%.

For regions such as NHSA, BOAS, CEAS, and AUST there is an increase in the bias introduced. However, it should be noted that these model limitations and known issues are discussed in Section 4.2 where the limitations of the model are highlighted and related to fire mechanisms that are not represented in INFERNO, or bias in the underlaying vegetation model causing impacts on the modelling of fires.

References

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