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

Teixeira et al. have integrated the Human Development Index (HDI) into the fire ignition and suppression parametrization within the INFERNO global fire model. Their findings indicate that incorporating socio-economic factors in INFERNO has led to a reduction in positive biases of burned area in specific regions and an enhanced representation of burned area trends in Central Africa. The methods proposed in this study are valuable for the scientific community, shedding light on the improved performance of the model and the potential interplay between socio-economic factors, climate, and vegetation.

While the paper is interesting and provides crucial insights into the human impacts on fire activity in the global fire model, there are notable areas that could benefit from further attention. The data and methodological approach employed seem robust, yet additional analyses are warranted to strengthen the support for the proposed approach. To enhance the manuscript, I suggest addressing the following major suggestions:

The authors thank the reviewer for the insightful comments on our paper. We appreciate your feedback and agree that there are areas that could benefit from further attention. We are glad to hear that you find our data and methodological approach to be robust, and we will certainly consider conducting additional analyses to strengthen the support for our proposed approach.

In response to your major suggestions, we will carefully review and address each point to enhance the manuscript. We value your input and will work to incorporate your suggestions to the best of our ability.

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

1. Page 2, line 49: When introducing HDI for the first time, it would be beneficial to provide more detailed information about HDI, including its calculation method, data sources, the range (0 to 1?), and spatial and temporal resolution.

The authors agree with the reviewer and commit to moving this to introduction where HDI is first referenced, in a revised version of the manuscript.

2. Page 2, line 53: Clarify how Zou et al. (2019)'s approach (based on Li et al. (2013), which includes region- and PFT-specific functions of population density and a global unified function of GDP, differs from the HDI approach in this study.

In the work of Li et al. (2013) a global unified function of GDP is only applied to the land used PFT (e.g., pasture and cropland) to represent the anthropogenic effects of managing this type of land. In this work HDI is applied independent of the PFT, as we consider that socio-economic factors will have an impact in all types of ignitions and suppression.

The authors thank the reviewer for the comment and commit to look into the work of Zou et al. (2019), as well as clarifying this approach in a revised version of the manuscript.

3. Page 3, Section 2: Introduce HDI in a dedicated section, covering details such as its calculation method, data sources, the range (0 to 1?), and spatial and temporal resolution.

The authors agree with the reviewer and commit to include a section dedicated to HDI in a revised version of the manuscript.

4. Page 4, line 95: Emphasize that fNS represents the fraction "not" suppressed by humans to avoid confusion regarding the decrease in suppression as HDI increases in equation 3.

The authors agree with the reviewer and thank the reviewer for highlighting that this can be a source of confusion, committing to improve this in a revised version of the manuscript.

5. Page 4, line 97: While the rationale behind the parameterization with 1-HDI is explained, additional analyses are crucial to support this parameterization, as the addition of 1-HDI may appear somewhat arbitrary.

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, such as the effects of industrialisation and climate change, land clearance, human population growth, replacement of indigenous and traditional fire management, and the subsequent development of large-scale firefighting and fuel management.

There is a need to better understand these impacts and the data available does not fully allow to an understanding of the impacts of fire management policies have. For example, how would one measure the number and extension of fires that never happened do to fire policies being in place?

The available literature includes works such as Curt and Frejaville (2017) and Pandey et al. (2023), but even those are limited to very specific regions of the world (e.g., at local level) or do not fully detail on the impact that fire management policies have throughout their implementation, and therefore present obstacles to derive an representation of the socio-economic factor on fire ignitions and suppression in the context of ESM.

The aim of this study is not to achieve that but rather to explore the use of the HDI to represent socio-economic impacts on fires at a global scale, aiming to improve the regional representation of human–environmental coupling for applications at large spatial scales within an ESM context, and should not be seen as an attempt to represent all the complexities inherent to these processes.

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.

6. Page 5, line 115: Include a map of HDI to help readers visualize the global distribution of HDI.

The authors thank the reviewer for raising this and commit to include figures depicting the temporal and spatial properties of HDI in a revised version of the manuscript

7. Page 8, line 189: Consider moving the evaluation metrics to the methods section for better organization.

The authors agree with the reviewer and commit to moving the evaluation metrics to the methods section in a revised version of the manuscript

8. Page 10, Table 2: Consider marking the numbers with improvement in bold to enhance readability.

The authors agree with the reviewer and commit to improve Table 2 to enhance its readability.

9. Page 11, Figure 5: (1) Add colors to differentiate JULES-INFERNO and JULES-INFERNO+HDI. (2) Provide explanations for -Clim simulations in the main text to avoid confusion.

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

In addition, the plot lines labelled "–clim" do not represent the same sensitivity experiments described later in the manuscript. These are a separate set of experiments, and their including was an error that was not noticed during the development of the manuscript. The authors commit to address this in a revised version of the manuscript.

10. Page 11, line 221: It's very interesting that including HDI in the parameterization would reduce the interannual variability of modeled burned area. Is it because the HDI reduces the contributions from climate drivers?

Including HDI suppresses the ignitions, this results in dampening the variability that comes from interannual variability from weather drivers. It should be noted that INFERNO burnt area is never greater than the average burnt area per PFT, as defined in equation 1, and it has no mechanism to represent large fires.

11. Page 12, line 240: Is it because the trends over these regions are driven by climate rather than human activities? Like in southern Africa the increasing trend is mainly driven by ENSO (Andela & van der Werf, 2014).

The authors thank the reviewer for the comment, as well as for promoting this discussion. As highlighted by the reviewer this region trends are dominated by ENSO variability. Andela & van der Werf (2014) estimated that 51% to the upward trend over southern Africa can be attributed to ENSO, while there is also evidence that socio-economic developments can be responsible for a decline. The relation between ENSO and annual burned area depends both on the effect of ENSO on precipitation and on the antecedent precipitation-burned area response.

While the model setup is able to capture ENSO variability, as its weather is driven by reanalysis, there is no mechanism that allows INFERNO to represent the antecedent precipitation-burned area effects. This is a limitation of the model and the authors do not expect the model to perform well in regions where the precipitation-burned area response can be dominant.

12. Page 13, lines 278-281: (1) Specify the criteria used to determine dominant drivers in the experiments, e.g., based on differences in trend_1990 control minus trend_clim larger than a certain value or statistical significance. (2) Consider presenting the information in a figure rather than a table to enhance clarity. For example, the x-axis can be the regions and y-axis are the differences of trend between 1990 control and climate, with two bars representing JULES- INFERNO and JULES- INFERNO+HDF. A horizontal line representing the criteria mentioned above.

The authors thank the reviewer for the suggestion and will considering representing the data as suggested by the reviewer in a revised version of the manuscript.

13. Page 17, line 352: Page 17, line 352: Please clarify that, despite consistent results, it might be confusing to state this since the HDI index does not encompass the impacts of fire management policies.

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

14. Page 18, lines 397-398: I think that is a very important information which should be brought up at the beginning when introducing HDI. The fact that HDI based at a national level can explain several biases when HDI is implemented in the algorithms, e.g., negative biases in northern Australia.

The authors thank the reviewer for the constructive comment and commit to clarify this in a revised version of the manuscript.

Technical comments:

1. **Page 9, line 196: $RMSE_{UB}$ should be $RMSE_{UE}$?**

   The authors commit to address these in a revised version of the manuscript.

2. **Page 16, line 321: "Discussion & Conclusion" should be placed in the section title.**

   The authors commit to address these in a revised version of the manuscript.

References

Andela, N., & Van Der Werf, G. R. (2014). Recent trends in African fires driven by cropland expansion and El Niño to La Niña transition. Nature Climate Change, 4(9), 791-795.

Curt, T., & Frejaville, T. (2018). Wildfire policy in Mediterranean France: how far is it efficient and sustainable?. Risk analysis, 38(3), 472-488.

Pandey, P., Huidobro, G., Lopes, L. F., Ganteaume, A., Ascoli, D., Colaco, C., ... & Dossi, S. (2023). A global outlook on increasing wildfire risk: Current policy situation and future pathways. Trees, Forests and People, 14, 100431.