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

This manuscript aims to account for the impacts of human development on fire activity in the INFERNO model through an implementation of linear relationships between the number of human-caused ignitions to the Human Development Index (HDI), and the amount of fire suppression to the HDI. The manner in which these relationships are implemented into the model leave important questions unanswered (a general overview is included here, with a more detailed discussion attached):

The authors thank the reviewer for constructive comments on our manuscript. We appreciate your feedback and will address the questions you have raised in the attached detailed discussion. We commit to revise our manuscript to provide more clarity on the implementation of Representing socio-economic factors in the INFERNO global fire model using HDI.

First: Does the HDI reflect the human impact on the number of ignitions and on the number of fires that are suppressed?

- **The authors provide no analysis and cite only a single previous paper in which the HDI was used as a predictor for fire activity. In this paper, Chuvieco et al. (2021), the HDI is used in a fundamentally different way, not in a relationship to the number of fires, as it is in this manuscript.**

Regarding the points the reviewer has made regarding HDI reflect the human impact on both the number of ignitions and on the number of fires that are suppressed. To represent the socio-economic factors impacting fire ignition and suppression, we include a Human Development Index (HDI) term (1-HDI) in our human ignition and suppression Eq. 2 and 3. This is highlighted in lines 103 to 105 of the manuscript.

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.

To the knowledge of the authors this work presents novel research. At the time this work was developed, this was the first attempt to represent socio-economic impacts on fire using HDI, there are no other works in the literature that could be referenced to support this implementation.

Although the HDI is used in a different way by the work of Chuvieco et al. (2021), in their study the authors show that HDI is one of the main drivers of burnt area interannual variability supporting the use of HDI in the way intended in this study. Paraphrasing the work of Chuvieco et al. (2021):

> *"Our results indicate that an improved representation of how humans impact fire occurrence, and how this is modulated by socio-economic conditions, as for example indicated by the HDI in our results, might improve the representation of year-to-year variation in fires within fire-vegetation models."*

"Since HDI can also be negatively related to people's dedication to agrarian activities (commonly also to more mechanization), higher HDI implies that population tends to rely less on fire activity for livestock grazing and agriculture than those in less developed areas. In other words, fires are more influenced by human agrarian activity (and therefore less fluctuating) when population is more dependent on agricultural resources (more crop and livestock density) and it has a lower level of development or income (HDI and GDP are highly correlated)."

Second: If such relationships exist, what form do they take?

- **The functional forms of the relationships to the HDI that the authors implement are not supported with sufficient evidence, apart from general conceptual arguments that more human development might reduce the number of fires. This neglects fire management practices in some developed countries where fire exclusion, i.e. the suppression of every observed fire as soon as possible, is no longer practiced, in favor of practices that include prescribed burns.**
- **Further, the relationships the authors include in their model impact the number of fires but do not reflect any relationship between fire management and reducing fire size, even though fighting fires that are actively spreading is a significant role of fire management.**

With reference to the functional forms of 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.

- **Finally, the relationships to the HDI that the authors impose involve simply multiplying existing equations in the model by 1-HDI. There is no evidence provided that the relationships between the number of fires and HDI should be linear or, if the relationships are linear, that these are the appropriate coefficients for this relationship.**

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 claim that their implementation results in an improved version of the INFERNO model, but the evidence for this is ambiguous at best, for the following reasons:

- **The relationships between the HDI and the number of fires are not validated. Rather, the validation is only performed on burned area, to which the number of fires**

contributes, but in combination with fire size. Because of this it is not possible to gauge the accuracy of their parametrizations explicitly.

The discussion and analysis of results of this study is focus on burnt area. INFERNO does not model number of fires. The spatial scales this model is applied to does not allow to model individual fires. In this work, we explore the use of the HDI to represent socio-economic impacts on fires in the Pechony and Shindell (2009) anthropogenic fire ignitions representation, building on the work of Mangeon et al. (2016), to represent socio-economic impacts on fires in INFERNO, and the analysis was based on the results diagnosed by INFERNO – burnt area

- **In terms of bias in the burned area, in a comparison between model versions after the implementation of the HDI and before, the new model version, that includes the HDI implementation, has a greater bias on a global scale as well as in 8 of the 14 regions that the authors analyze (this is shown in Table 2 in this manuscript)**

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 reginal bias compensate each other. Although this is highlighted in section 4 – Conclusions, in lines 330 and 331, the authors agree that improving this sentence would strengthen the manuscript.

> "Furthermore, it should be highlighted that although JULES-INFERNO performs well at the global scale, as can be seen when comparing the annual mean burnt area against GFED4s in Table 2, this is due to compensating errors at the regional level".

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.

- **In terms of the burned area trends, the implementation of the HDI does show an improvement on the global scale, and in 9 of the 14 regions. However, there are no R2 values or confidence intervals for the trends provided, or confidence levels for assertions that one trend is more accurate than another. Without these, it isn't possible to say whether the trends are significant or with what certainty the errors in the trends have been reduced.**

The observed dataset (GFED 4s) shows that out of 14 regions, 4 have positive burnt area trend. From these JULES-INFERNO only presents a positive trend for TENA and SEAS. It

While JULES-INFERNO+HDI tends to enforce decreasing trends, this only happens in 4 regions out of 14, TENA, SHAF, MIDE, and SEAS. For the remaining 10 regions, JULES-INFERNO+HDI presents a similar trend to JULES-INFERNO or even an improved trend when compared to GFED 4s.

- **In terms of standard deviation, the model version that includes the HDI shows an improvement on the global scale, but only in 5 of the 14 regional scales, with the previous model version performing better in the 9 others. This is a particular problem as the authors state that their aim is "to improve the regional**

representation of human–environmental coupling for applications at large spatial scales”

Regarding the impacts this approach has in burnt area variability, the authors have focussed the analysis on the standard deviation. Standard deviation is the average amount of variability in your dataset. It tells you, on average, how far each value lies from the mean. The impacted of the use of socio-economic factors in INFERNO is discussed in section 4.2 - Model limitations and known issues.in this section it is discussed that the use of HDI reduces the ability of the model to represent the burnt area regions that are characterized by high interannual variability, namely, BONA, BOAS, AUST, CEAS, SHSA and NHSA. Although this is seen as a negative impact, it must be noted that the control model - JULES-INFERNO - despite having a larger inter-annual variability, also has a poor performance in this aspect compared to observations. Lines 392 to 396. Furthermore, in lines 410 to 417 discuss the fact that INFERNO does represent the mechanisms that are characteristic off regions with high interannual variability in burnt area. The authors agree that improving these sentence to ensure this is clear would strengthen the manuscript.

- **Overall, a majority of the metrics that the authors provide show worse performance at the regional scale after implementing the HDI than before.**

While the authors recognize that the analysis presented in this work would be more complete by highlighting the regions where the implementation of socioeconomic factors in fire suppression and ignitions in INFERNO have a degradation of performance. 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 (line 325). Although this 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.

A specific section of the paper that is worth highlighting in these general comments is the abstract, which is highly misleading, and gives a false impression to a casual reader of this paper who would rely on it. It mentions only regions in which the model was improved by implementation of the HDI, without mentioning that the global bias in burned area was increased and that many regions experience worse model performance. Two statements in the abstract in particular are false.

- **The first is that the bias in burned area is reduced in 4 regions in the model “without statistically significant impact to 10 other areas.” The statistical significance of these biases is not discussed in the manuscript at any point, the global bias is increased by implementing the HDI, and some regions show quite large increases in their bias. Australia, in particular, shows an increase in its relative bias from -21% to -82%. It is unclear what statistical test would define this as insignificant.**
- **The second statement is that the new model version “improves the representation of the burnt area trends, especially in Africa, Central Asia and Australia.” One part of Africa shows an improvement, while the other shows a worse performance. Combined, the trend over Africa in total is worse in the updated model version (details are provided in the specific comments). This statement is therefore highly misleading at best.**

For this manuscript to be publishable, the authors must fundamentally reframe it in a

manner that transparently reflects their results. This includes stating clearly that the functions implemented in their model are initial attempts at parametrization that are not based on previous analysis, clearly stating that the results of implementing these parametrizations are mixed, and that further work is required to derive and validate equations that reflect the impact of human development on fire activity at a global scale. This should be reflected in sections where results are summarized and discussed. The current framing overstates the results and is not supported by sufficient evidence.

The authors agree with the reviewer that the manuscript would benefit from increasing the emphasis on the regions that are negatively impacted by the approached presented, as well as the benefit that including a statistical test to objectively quantify significance of changes and will aim to improve this in a revised version of the manuscript.

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

Abstract: the abstract mentions only regions where the introduction of the HDI improved the INFERNO model, which is highly misleading. There are many areas in the model where the introduction of the HDI resulted in worse performance, including regions where observed positive trends that were previously reflected in the model are no longer present after the inclusion of the HDI.

The authors agree with the reviewer and commit to change abstract to address this in a revised manuscript, stating that that there are mix results, making the emphasis of the results more balanced.

Lines 7-8: It should also clarified that the reduction of the global burned area results in a greater global bias than was present in the model version without inclusion of the HDI. Without this clarification this statement creates the appearance that the reductions in burned area were always an improvement.

The authors thank the reviewer and commit to change the manuscript to address this in a revised version.

Line 9: "by more than 100%" is a confusing way of wording this, it implies that these positive biases became negative biases, which is true in some cases, but not all. Maybe the intent is to say that the relative bias is reduced by more than 100 percentage points?

The authors agree with the reviewer, the suggested change will make the wording clear and commit to make such changes in a revised manuscript.

Line 9: "without statistically significant impact to 10 other areas" statistical significance is never discussed in the manuscript when referring to biases in the burned area, so it unclear where this statement comes from. Many of the other areas do include substantial changes. Northern Hemisphere Africa and Southern Hemisphere Africa together have an increase in their biases, after inclusion of the HDI, of 26.86 Mha, which is more than the total burned area in all of the other regions individually, apart from Australia. Australia itself has an increase in its bias of 24.28 Mha, from -8.46 Mha to -32.74 Mha, relative to a total burned area of 39.88 Mha. An argument that this is not statistically significant is false and extremely misleading.

Including the statistical significance in the results would improve the clarity of these statement. In addition, linking the regions where there is a negative impact when the new parametrization is included to the model limitations and known issues would improve the clarity and understanding of where INFERNO is expected to perform well. The authors thank the reviewer and commit to change this in a revised manuscript.

Line 10: The observed burned area trend in Southern Hemisphere Africa, according to Table 2 in this manuscript, is -0.54 Mha/yr, the trend in the model version without the HDI is -0.14 Mha/yr and the trend in the model version with the HDI is -1.94 Mha/yr. Therefore, the model version without the HDI is closer to the correct trend. While it is true that the HDI version of the model shows a closer trend in Northern Hemisphere Africa, the total observed trend in Africa is -2.74 Mha/yr, the total trend in the model version without the HDI is -1.4 Mha/yr, and the total trend in the model version with the HDI is -4.65 Mha/yr. The model version before the introduction of the HDI therefore has a bias of 1.34 Mha/yr across Africa and the model version after the introduction of the HDI has a greater bias of -1.91 Mha/yr across Africa. I am leaving out the parts of the middle east region that are on the African continent, but since that region as a whole only has a burned area of 0.9 Mha over the time period under analysis it should not make a significant difference. Therefore, the statement that the introduction of the HDI "improves the representation of the burnt area trends, especially in Africa. Central Asia and Australia" is false, or at the very least highly misleading.

In addition, the abstract fails to mention that the trend is worse in several regions including Temperate North America and South East Asia, where observed positive trends incorrectly become negative trends. Without this clarification the abstract gives the impression that the introduction of the HDI resulted in a uniform improvement in modeled trends.

The authors thank the reviewer and commit to change the manuscript to address this in a revised version.

Lines 33-36: It is unclear whether this summary is supported by the Andela et al. (2017) paper that the authors cite. Andela et al. (2017) show contrasting relationships between population density and burned area in different regions that cannot be simply summarized by stating, as the authors do, that "areas with low population density are associated with lower burnt areas, and densely populated areas tend to be associated with increased burnt area." Even though the authors caveat this in the subsequent sentence, they only do so for prosperous regions, which is not supported by the literature. E.g. the findings of Archibald et al. (2009), whom the authors cite in this work.

It should be noted that lines 33 to 36 of the manuscript state:

> *"Furthermore, Andela et al. (2017) demonstrates that human population density and prosperity can significantly impact burnt areas. On the one hand, areas with low population density are associated with lower burnt areas, and densely populated areas tend to be associated with increased burnt area. On the other hand, for heavily populated and prosperous regions, burnt area decline is likely a function of perceived threats to highly valued infrastructure, prompting extensive fire suppression efforts, sometimes involving high monetary costs."*

This is supported by Andela (2017), where the following is stated:

> *"A shift toward more capital-intensive agriculture has led to fewer and smaller fires, driven by population increases, socioeconomic development, and demand for agricultural products from regional and global markets. Together, these factors influence fire use in predictable ways, with a strong inverse relationship between*

burned area and economic development."

The authors acknowledge that this could be made clearer and propose changing lines 33 to 36 to read as follows:

> *"Furthermore, Andela et al. (2017) demonstrates that human population density and prosperity can significantly impact burnt areas. In their work the author shows that due to population growth, socioeconomic development, and the demand for agricultural products in regional and global markets, there has been a shift towards more capital-intensive agriculture, resulting in fewer and smaller fires. These factors have a predictable impact on fire use, with a strong inverse correlation between the area burned and economic development."*

The subsequent statement that "for heavily populated and prosperous regions, burnt area decline is likely a function of perceived threats to highly valued infrastructure, prompting extensive fire suppression efforts, sometimes involving high monetary costs." Is somewhat problematic as it implies that suppression efforts are based on strictly economic considerations, and that fire is not suppressed in heavily populated regions that are not prosperous. This neglects the impact of population density on fuel availability as even less prosperous urban areas contain significantly less fuel than non-populated areas. The impact of high population density on reducing burned area even without significant prosperity is visible in the strong negative correlation between population density and burned area in, e.g., Bangladesh in Figure 4 of Andela et al. (2017)

The authors thank the reviewer for the constructive comment and propose to **rephrase the sentence to**: *"~~for heavily populated and~~ prosperous regions, burnt area decline is likely a function of perceived threats to highly valued infrastructure, prompting extensive fire suppression efforts, sometimes involving high monetary costs."*

In this way the wording better reflects the approached presented in this work showing that population density is decoupled to the economic considerations, as rightly pointed by the reviewer. However, it should be noted that the impacts of population density on fuel availability are a separate mechanism that is reflected in the land use (urban versus plant cover) considered in the ignition functions.

Line 47: This is the only source the authors cite in which the HDI is used to predict fire activity. However, Chuvieco et al. (2021) use the HDI as a predictor of the coefficient of variation of burned area. This is in contrast to this manuscript where the HDI is directly implemented in equations that determine the modeled number of ignitions and amount of fire suppression and the aim is to improve burned area in an absolute sense rather than just its coefficient of variation. Without further analysis this is an insufficient theoretical basis for including the HDI in the INFERNO model in the manner that the authors do.

Although the HDI is used in a different way by the work of Chuvieco et al. (2021), it is shown that HDI is one of the main drivers of burnt area interannual variability supporting the use of HDI in the way intended in this study. Paraphrasing the work of Chuvieco et al. (2021):

> "Our results indicate that an improved representation of how humans impact fire occurrence, and how this is modulated by socio-economic conditions, as for example indicated by the HDI in our results, might improve the representation of year-to-year variation in fires within fire-vegetation models."
>
> "Since HDI can also be negatively related to people's dedication to agrarian activities

(commonly also to more mechanization), higher HDI implies that population tends to rely less on fire activity for livestock grazing and agriculture than those in less developed areas. In other words, fires are more influenced by human agrarian activity (and therefore less fluctuating) when population is more dependent on agricultural resources (more crop and livestock density) and it has a lower level of development or income (HDI and GDP are highly correlated)."

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.

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.

Line 57: The authors identify a "need for data collection to improve the quantification and modelling of fire activity and human populations." This statement is strongly supported by their discussion up to this point. However, many datasets are available, including ones that the authors use in this manuscript, that would allow for a data analysis to establish what the relationships between HDI and fire activity are, and to what extent they exist. Without such analysis the implementation of the HDI presented here is somewhat arbitrary.

As mentioned previously, 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. That would require data collection to improve the quantification and modelling of fire activity and human populations.

Line 87: While it is true that local meteorology and topography cannot be resolved by ESMs, is it sufficient to leave out these considerations entirely? For example, Haas et al., 2022 (DOI: 10.1088/1748-9326/ac6a69), show that topography and wind speed have an impact on fire size even when aggregated to a 0.5° grid cell scale.

The authors thank the reviewer for highlighting the interesting work of Haas et al. (2022), and we commit to mention this in revised version of the manuscript. However, it should be noted that the resolution of the model used in this study is approximately 1.75°. This is significantly coarser than the 0.5° grid cell scale analysed in the work of Haas et al. (2022).

Equations 2 and 3: What is the justification for this particular functional form? Beyond general arguments that greater development should lead to fewer ignitions being caused and greater resources that can be applied to suppression, the authors do not provide a justification for why the relationships should be linear in particular, and why, in these linear relationships the HDI should be

multiplied by -1, rather than any other negative number (e.g. multiplying the equations by 0.5- 0.5HDI). Because the authors also do not provide any evidence that the HDI can act as a predictor for the number of ignitions or amount of suppression, these equations do not have a sufficient theoretical basis.

An additional concern is that these equations strictly reduce the number of ignitions and increase the amount of suppression from the previous values, since the HDI is always greater than 0. This implies that, previously, ignitions were always overcounted and suppression was always underrepresented. Please clarify how these equations were derived in the first place and why this derivation may have led to these biases.

In addition, these equations imply that greater development always results in increased fire suppression. This neglects key aspects of fire management and changing practices in many regions, for example in British Columbia as described by Nikolakis and Roberts (2020), whom the authors cite on several occasions. These changing practices in many cases involve fire management policies that do not rely on complete fire exclusion but involve prescribed burning, which is itself a source of ignitions. There is a significant body of evidence that extreme fire exclusion results in fuel buildup that can cause subsequent fires to become larger (e.g. summarized by Plucinski 2019, DOI: 10.1007/s40725-019-00085-4). Therefore, many agencies are moving away from a focus on fire exclusion, and this is in direct contrast to the parametrization in this manuscript.

Finally, the authors' implementation of the HDI to account for fire management impacts the number of fires but does not account for the ability of fire suppression to reduce the final size of spreading fires. By only including the impact of HDI on the number of fires, the authors fail to reflect the findings in Andela et al. (2017), whom they cite, that show a decrease in fire size as well as the number of fires over a similar time period to this study, with this decrease in the number of fires having a significant impact in regions including northern Africa.

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. The data available does not fully allow to an understanding of the impacts of fire management policies nor derive the statistical relation to fully represent these processes within a fire model.

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 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 reginal bias compensate each other. However, we do acknowledge that it can have a detrimental impact on other regions. We commit to make this clearer in a revised version of the manuscript.

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, the authors are grateful for the detail provided by the reviewer on the of fire management practices, such as the work of Nikolakis and Roberts (2020) and Plucinski (2019). These studies provide a great understanding of the impacts of fire management policies and the need to adapt to a changing climate. However, the authors would like to highlight that these are based on mechanisms and processes that are not modelled by INFEERNO or JULES, and cannot be directly model at a global scale.

Line 153: please provide more details on how these PFT-specific average burned area values are derived from the values in Andela et al. (2019). There is quite a stark contrast between the previous values and the new ones, in some cases the new ones are over triple the previous values.

The PFT-specific values were derived by matching the PFT reported by Andela et al. (2019) in Tables 1 and 2 into the PFT categories represented in JULES as much as possible. These are not direct comparison and a balance between the PFT representation of the region in JULES was used to estimate a reasonable average burnt area for INFERNO. The authors thank the reviewer for raising the comment and acknowledge that this can be made more clear in a revised manuscript.

Line 171: please clarify how this spatial correlation metric is calculated, is it based on the mean burned fractions per grid cell across the time period in question?

The 2-D cross-correlation was used to determined what is referred to as spatial correlation between the model experiments and the observation data. The authors thank the reviewer for raising that this is not clear in the current version of the manuscript and commit to improve this in a revised version.

Figure 3: the introduction of the HDI appears to cause some sharp boundaries between countries such as Canada and the United States despite this not being visible in the satellite data. This could be worth commenting on, and the figure descriptions in the captions should be expanded in general.

Also, please clarify in the caption that this figure uses a color axis in which the difference in colors does not correspond linearly to differences in burned fraction, and include maps with a continuous, perceptually uniform color scale for better comparisons between different regions in addition to the current plots.

The authors thank the reviewer for the constructive comment and commit to include a reference to HDI causing sharp boundaries between countries and regions in a reviewed manuscript, as well as including a figure showing the spatial values of HDI for a given year.

In addition, the authors recognise that clarifying in the caption that this figure uses a colour axis in which the difference in colours does not correspond linearly to differences in burned fraction improves the reader understanding the figure and commit to include this in a revies version of the manuscript, as well as colour maps with a continuous, perceptually uniform colour scale.

In general, the maps in this paper would be improved by using an equal area map projection.

Because the purpose of these maps is in part to compare how much of the world's surface each

model version performs well in, a projection such as the Equal Earth projection (Šavrič el al., 2018,DOI: 10.1080/13658816.2018.1504949) would allow for this, whereas the current projection appears to inflate areas farther from the equator.

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

Figure 4: This point is also addressed elsewhere, but these maps do not appear to show a clear improvement in the model by including the HDI. Especially considering that many of the regions where the bias is lower in the non-HDI model run, such as northern Australia and many parts of Africa have very high burned areas, so biases there can have strong impacts on global fire.

Although there are regions where the non-HDI is lower there are many others where JULES-INFERNO+HDI is better (non-boreal north America, Europe, south hemisphere s South America, south Australia, southern Asia). Although some regions are not improved and the bias is increased, the bias exists for both JULES-INFERNO and JULES-INFERNO+HDI. The aim of this work is not to reduce all the model bias, but to target the regions that have large bias where that is dominated by the socio-economic factors.

Table 2: For the sake of easier interpretability please highlight which metrics show better performance than their counterpart in the other experiment, e.g., by coloring the cells in the table

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.

In general, more of the metrics in this table, where the two model version show a difference, 72 vs 58 by a cursory count (not double counting mean BA and bias), are in favor of the model version without HDI. This is 4 vs 5 on a global scale, and 68 vs 53 on the regional scales for the non-HDI version vs the HDI version. The authors state on line 72 that they “aim to improve the regional representation of human–environmental coupling for applications at large spatial scales within an Earth System Model (ESM) context.” These metrics appear to show that this aim is not met. Please also include confidence intervals and R^2 values for the trends in this table

As the authors cite Chuvieco et al. (2021) where the HDI is shown to have an impact on the coefficient of variation of burned area, the coefficient of variation should also be included in this table and discussed in the text.

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. This is mentioned in the paragraph in line 325. Although this 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 authors agree that this could be highlighted in the results section of the manuscript, as well as increasing the emphasis in the regions where there is a negative impact of modelled results, committing to address this in a revised manuscript.

Figure 5: The legend states that GFEDv4 is being used for this plot, whereas GFED 4s was used in the rest of this paper. Is this a typo? Please clarify

Also, please clarify what the -clim experiments are. Do these correspond to the sensitivity analysis described later? If so, why are these shown rather than the others? And is it true that, as these plots appear to show, the burned area in the HDI experiments drops nearly to 0 if only the atmospheric drivers are transient and all other drivers are kept at their 1990 values?

The authors thank the reviewer for raising this and would like to clarify that in Figurer 5 where it reads “GFEDv4” it should read “GFED 4s”. 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.

Line 225: these are global numbers, please clarify how they correspond to a regional improvement. The statement that the improvement in global standard deviation is reflective of a regional improvement is extremely problematic since STD/STDGFED4s is better for the non-HDI model version in 9 out of the 14 regions and only improved in 5, in some cases only marginally.

The results presented stating in line 225 correspond to the global values of several metrics that are analysed through the manuscript. This paragraph aims to summarize the discussion focused on regional metrics mentioned in previous paragraphs and highlights that the regional improvements lead to improvements at a global level. They correspond to regional improvements as the global metrics are made by the contributions from all regions.

The improvements that JULES-INFERNO+HDI 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 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.

Line 226: "reducing the RMSE" the RMSE is significantly lower for the non-HDI version, only the RMSEUE is reduced.

The authors thank the reviewer for highlighting this and commit to changing the sentence to refer to RMSEUE in a revised version of the manuscript

Figure 6: The inclusion of the HDI appears to exacerbate the negative bias in the trends in many parts of Africa, and, given the values in Table 2, imposes almost uniformly negative trends on the burned area, even in regions where this is not the case in the observations. This is to be expected, given that the HDI shows an almost global increase over this time period, as shown in Figure A2 and Equations 2 and 3 impose a negative correlation between HDI and ignitions as well as the number of unsuppressed fires. Given the lack of theoretical backing and explicit validation of those equations, this imposition of a negative trend in a uniform manner is somewhat arbitrary, and does not sufficiently support the statement in line 243 that "Overall, including socio-economic factors in INFERNO results in an improvement in burnt area trends."

The observed dataset (GFED 4s) shows that out of 14 regions, 4 have positive burnt area trend. From these JULES-INFERNO only presents a positive trend for TENA and SEAS. It

While JULES-INFERNO+HDI tends to enforce decreasing trends, this only happens in 4 regions out of 14, TENA, SHAF, MIDE, and SEAS. For the remaining 10 regions, JULES-INFERNO+HDI presents a similar trend to JULES-INFERNO or even an improved trend when compared to GFED 4s.

In some of these regions INFERNO does not model all the processes that represent fire behaviour. This as an impact on overall model results. For example, due to the nature of model resolutions and timescales in Earth System Modelling, INFERNO was not designed to model the processes that are needed to represent large and severe fires which dominate the trends and fire regime characteristics of these regions. Therefore, it is expected that regions where fire regimes are dominated by large and severe fires may be affected by a negative bias in burnt areas and fire emissions, as well as on their response to a changing climate.

This is address on Section 4.1 of the manuscript and the authors would like to highlight that a holistic view of model performance needs to be considered. Faced with these results the authors believe that the use of HDI improved the trends in INFERNO when compared to observations.

Line 256: This section is interesting in general. The only part missing is a discussion of how the drivers that are compared individually correlate to each other. From the standpoint of strictly testing model behavior, varying these drivers individually is instructive, but if broader conclusions are intended it is relevant to discuss if, e.g., a change in population density also corresponds to land use changes in general and, therefore, whether it is realistic to decouple them.

The authors thank the reviewer comment, and although the authors feel this is considered out of scope for the aim of this work, it is recognised that this would provide important understanding for the fire community and will consider this in future work.

Table 3: please also include mean burned areas as in Table 2, and include R^2 values for the trends.

The authors commit to include R2 values for the trends in a revised version of the manuscript

Line 320: Conclusions is not the most appropriate title for this section, discussion would be more fitting.

The authors agree with the reviewer and commit to rename this section in a revised version of the manuscript

Line 325: This statement is not supported by the evidence the authors provide, for reasons discussed previously, particularly given that the non-HDI version shows better performance in many areas, most by some metrics

The authors thank the reviewer and agree to remove the reference "without having a negative impact in regions that perform well when compared to GFED4s" in a revised version of the manuscript.

Line 332: Given that the trends are so uniformly negative upon implementation of the HDI, whether this is an improvement is unclear.

The observed dataset (GFED 4s) shows that out of 14 regions, 4 have positive burnt area trend. From these JULES-INFERNO only presents a positive trend for TENA and SEAS. It

While JULES-INFERNO+HDI can tends to enforce decreasing trends, this only happens in 4 regions out of 14, TENA, SHAF, MIDE, and SEAS. For the remaining 10 regions, JULES-INFERNO+HDI presents a similar trend to JULES-INFERNO or even an improved trend when compared to GFED 4s.

In some of these regions INFERNO does not model all the processes that represent fire behaviour. This as an impact on overall model results. For example, due to the nature of model resolutions and timescales in Earth System Modelling, INFERNO was not designed to model the processes that are needed to represent large and severe fires which dominate the trends and fire regime characteristics of these regions. Therefore, it is expected that regions where fire regimes are dominated by large and severe fires may be affected by a negative bias in burnt areas and fire emissions, as well as on their response to a changing climate.

This is explicitly address on Section 4.1 of the manuscript and the authors would like to highlight that a holistic view of model performance needs to be considered. Faced with these results the authors believe that the use of HDI improved the trends in INFERNO when compared to observations.

Line 337: Same comment as above

The authors thank the reviewer and agree to remove the reference that it allows to be confident on the results and reference that improves the representation of the in a revised version of the manuscript.

Line 367: Is this sentence referring to an impact on the effect that the anthropogenic drivers have? Somewhat unclear, please rephrase

The authors thank the reviewer and agree to rephrase this in a revised version of the manuscript.

Line 376: it is good that the authors address this issue here, but it is somewhat hidden among the statements claiming that the model has been improved. Given the evidence shown, the issues in this approach should be emphasized to a greater extent.

The authors agree with the reviewer and commit to highlight this an earlier stage of the manuscript in a revised version.

Line 408-409: please provide a citation, or clarify that this is based on the authors' experience

As mentioned in the manuscript, this is addressed in Teixeira et al. (2021) and Burton et al. (2019). The authors commit to improve this in a revised version of the manuscript by explicitly referring to these limitations in the manuscript.

Line 434: Given the issues in this paper, and the fact that the HDI version appears to enforce strong decreasing trends in many parts of the model, even where there are none observed, it is somewhat concerning that calculations for such future scenarios may be biased towards less fire activity, leading to an underestimation of the dangers that fire poses under climate change.

The authors thank the reviewer for prompting this discussion. While JULES-INFERNO+HDI can enforce strong decreasing trends, this only happens in 4 regions out of 14, TENA, SHAF, MIDE, and SEAS. For BONA and EQAS both JULES-INFERNO and JULES-INFERNO+HDI present equal trends. For the remaining 9 regions, JULES-INFERNO+HDI presents an improved trend when compared to GFED 4s. From these 9 regions only SHSA and BOAS have a strong decreasing trend when compared to GFED 4s.

In some of these regions INFERNO does not model all the processes that represent fire behaviour. This as an impact on overall model results. For example, due to the nature of model resolutions and timescales in Earth System Modelling, INFERNO was not designed to model the processes that are needed to represent large and severe fires which dominate the trends and fire regime characteristics of these regions. Therefore, it is expected that regions where fire regimes are dominated by large and severe fires may be affected by a negative bias in burnt areas and fire emissions, as well as on their response to a changing climate.

This is explicitly address on Section 4.1 of the manuscript and the authors would like to highlight that a holistic view of model performance needs to be considered.

Technical Corrections (original reviewer comment presented in bold)

Line 5: should read "the Human Development Index" rather than "a Human Development Index"

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

Line 10: should be a comma after Africa rather than a period

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

Line 55: "due to the effects of reflecting the effects" redundant, please rephrase

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

Line 315: There appears to be a typo in this sentence as the "JULES-INFERNO+HDI (pop and lu)" experiments are referred to as containing both a burned area decrease and increase.

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

Line 321: Stray section header at the start of this sentence

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

Line 346: "the inclusion of socio-economic factors reduce the role of temperature in driving trends by reducing the role of temperature in driving trends" redundant, please rephrase

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

Line 348: should be "that climate drivers" rather than "of climate drivers"

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

Line 405: remove the word "on" from "impact on the modelled burnt area"

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

Figure A1: some subplots have y labels while others do not. Since what is represented on the y-axis is the same for all plots, it would be better to just leave them out

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

Figure A1: the legend obscures too much of the timeseries, please move it for clarity

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

Figure A2a: labels on colorbar are cut off

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

Figure A2e: title says "Pupulation" rather than Population

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

Line 505: This reference appears to be repeated twice

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

References

Andela, N., Morton, D. C., Giglio, L., Chen, Y., van der Werf, G. R., Kasibhatla, P. S., ... & Randerson, J. T. (2017). A human-driven decline in global burned area. Science, 356(6345), 1356-1362.

Andela, N., Morton, D. C., Giglio, L., Paugam, R., Chen, Y., Hantson, S., ... & Randerson, J. T. (2019). The Global Fire Atlas of individual fire size, duration, speed and direction. Earth System Science Data, 11(2), 529-552.

Burton, C., Betts, R., Cardoso, M., Feldpausch, T. R., Harper, A., Jones, C. D., ... & Wiltshire, A. (2019). Representation of fire, land-use change and vegetation dynamics in the Joint UK Land Environment Simulator vn4. 9 (JULES). Geoscientific Model Development, 12(1), 179-193.

Chuvieco, E., Pettinari, M. L., Koutsias, N., Forkel, M., Hantson, S., & Turco, M. (2021). Human and climate drivers of global biomass burning variability. Science of the Total Environment, 779, 146361.

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

Haas, O., Prentice, I. C., & Harrison, S. P. (2022). Global environmental controls on wildfire burnt area, size, and intensity. Environmental Research Letters, 17(6), 065004.

Mangeon, S., Voulgarakis, A., Gilham, R., Harper, A., Sitch, S., & Folberth, G. (2016). INFERNO: A fire and emissions scheme for the UK Met Office's Unified Model. Geoscientific Model Development, 9(8), 2685-2700.

Nikolakis, W. D., & Roberts, E. (2020). Indigenous fire management: a conceptual model from literature. Ecology & Society, 25(4).

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.

Pechony, O., & Shindell, D. T. (2009). Fire parameterization on a global scale. Journal of Geophysical Research: Atmospheres, 114(D16).

Plucinski, M. P. (2019). Fighting flames and forging firelines: Wildfire suppression effectiveness at the fire edge. Current Forestry Reports, 5, 1-19.

Teixeira, J. C. M., Burton, C., Kelly, D. I., Folberth, G. A., O'Connor, F. M., Betts, R. A., & Voulgarakis, A. (2023). Representing socio-economic factors in the INFERNO global fire model using the Human Development Index. Biogeosciences Discussions, 2023, 1-27.