Response to RC1

We thank the reviewer for taking the time to review our manuscript and provide thoughtful and interesting comments. These points often merit further discussion to which we have tried to do justice, but have tried to make minimal changes to the existing text as some of the discussions are a little speculative and getting deep into them would lengthen the (already rather long) manuscript.

In the following point-by-point response we have kept the reviewer's comment verbatim in black text, our responses are in blue and proposed new text in green. Line numbers refer to the reviewed version of the manuscript. Text insertions into existing text are additionally denoted with underlining.

The authors of "Understanding and simulating cropland and non-cropland burning in Europe using the Base model" use generalized linear models to develop a fire model capable of predicting cropland and non-cropland burned area in Europe. This model is likely suitable for use in land surface and climate models. To my knowledge, few land surface models include the ability to model cropland fire. This work is timely, technically rigorous, and falls within the scope of bio-geosciences. I have several comments which are listed below.

- Intro: The lack of land surface models capable of representing cropland fire is mentioned in the discussion. I suggest discussing it in the intro as well as the motivation for this work.

Yes, it was a motivation and we agree we should mention it more in the Introduction. Some land surface models do include cropland fire (CLM, since v4.5 but it is not enabled in all configurations used in CMIP6, and JULES-INFERNO, but not yet in the main JULES release) but these are a definite minority of models. We propose to add the following text after line 110:

"Cropland burning as an explicit process is almost entirely neglected in fire-enabled DGVMs and the land surface models used in Earth System Models (ESMs); we are aware of only one such model in which it is simulated (Li et al., 2013), one in which it is prescribed from remote sensing data (Rabin et al. 2018), and one in which fires in croplands are simulated in the same manner as fires in grasslands (Burton et al. 2019)."

- Table 1: Adding the data source and citations could help better inform the reader

Yes, good point, we are happy to do this.

- L270: How were the data points sampled? Was anything done to uniformly distribute the sampling across space, or account for spatial autocorrelation

They were sampled completely randomly in space and time (i.e not the complete time series for a gridcell, we also sampled from the individual months). We did try some degree of stratification (to balance the burnt for non-burnt gridcells) but that degraded out results. Initially we also tried sampling every alternate year, but the interannual fluctuation seem to alternate for at least part of the time series so that introduced a bias depending on whether we took every first or every second year. In the end we found that completely random sampling gave the best results. We propose to clarify by modifying the sentence at line 271 to read:

"We considered every month and gridcell which had more than 10% of the LCT present as a data point, and used 80% of the data points (sampled randomly from all grid cell-months) for training and kept 20% for testing"

- L460: Some text comparing and contrasting these models with mechanistic models could be interesting. For example in mechanistic models, wildland fire is influenced by wind and terrain which impacts spread, whereas cropland fire appears to be a more complex phenomenon perhaps better suited to description using a statistical model.

Yes, very good point that we missed. We agree that statistical (or agent based) modelling of fire definitely makes sense, at least until we know more about the processes. Therefore, we propose to include the following in a new paragraph at line 46 6.

"These results imply that current mechanistic modelling approaches are likely not well suited to modelling cropland fires. Mechanistic models are typically based on biophysical relationships concerning flammability and rate of spread, and with the general assumption that higher flammability or faster rates of spread produce more burnt area. Our findings imply that this approach is not valid for cropland burning as more flammable conditions do not necessarily imply more burning in the croplands. Given this, and the complexities of human land management and other socioeconomic factors, the inclusions of statistical or agent-based (Perkins et al. 2024) approaches in future cropland modelling efforts may prove fruitful."

- L575: The analysis of the role GDP and HDI play in the model is interesting. Can the authors provide insight into whether this is corelative or causative? Do these relationships apply in time (i.e. moving into the future)? What if there were abrupt changes in these metrics due to for example a short-term financial crisis?

Good questions. Essentially, they are correlative, but we argue that there is a strong "indirect causation" rather than "just correlation". It cannot be directly causative – higher GDP (or the other components of HDI which are years of schooling and life expectancy) does not directly affect fire occurrence. Rather, these variables are acting as a proxy for "socioeconomic development" in a broad sense, and, as this "development" occurs, they capture changes in human behaviour, infrastructure, legislation, etc which result in less burnt area. These "indirect causations" are not difficult to imagine. Higher GDP leads to more investment in fire-fighting capability and more capital-intensive, mechanised agriculture which doesn't involve using fire. More education leads to more awareness of air pollution and less tolerance of it. There are many ways that a more "developed" society stops using fire and actively suppressed it, and has the means and incentivise to do so. However, these are (as of yet) unquantified, so we don't wish to speculate too much in the manuscript about mechanisms, so we propose to clarify the causative nature of the relationships be modifying the sentence at line 578 to read:

"As such, they have correlative rather than causative relationships with burnt area as neither explicitly captures the effectiveness of human fire management nor the tendency to utilise fire as a land management tool, and may be collinear with urbanisation and other infrastructural developments that may fragment landscapes and lead to declining burned area (Haas et al., 2022)."

With regards to the relationships in time, yes, we believe the relationships do capture the effects of temporal changes in GDP/HDI as they are essential for capturing the declining trend in NCV burning (supp. Fig F3 in the manuscript). This is driven by a solid increase in HDI across the study region,

especially in regions such as the Balkans (although spatial differences are larger than the temporal ones). For moving to the future, we become sensitive to broad issues such as the universality of the trajectory of societal development and potentially more "developed states" than we have today (maybe where prescribed burning becomes commonplace or maybe suppression of extreme fires becomes even more effective). And also the specific caveat that applies to all models that they might not be reliable outside of the regime in which they are trained and tested. However, in practical terms for BASE and the state-of-the-art scenario modelling (i.e. SSPs), we believe that the model's response to GDP and HDI will likely to be reasonable, i.e. a general decrease in fire activity as the development indices increase (although there is a saturation at high values) and a difference in development between the SSP scenarios with different socioeconomic trajectories. This is apparent in some on-going work where we are looking at future projections.

Regarding abrupt or short-term changes. We are not aware of any studies relating changes in fire regime to abrupt socioeconomic changes, although that is an intriguing idea. However, as mentioned, HDI and GDP are correlative rather than causative indicators which represent slowly evolving factors such as infrastructure, legislation, and public awareness. As such, in reality we wouldn't expect an immediate short-term response to say, a financial crash. On the other hand, in the model we would see such a response if, for example, GDP dropped sharply and significantly. This would probably be unrealistic, although it is possible that immediate cuts to public services may resulting less effective fire fighting that year. For this reason, HDI (which is less sensitive to short term financial events) may be a more robust metric.

We propose to remove the existing sentences from line 604 to 607 and replace them with the following:

"There are other issues that might arise with using GDP/HDI. We note that there is greater variation of HDI and GDP in space than in time, and it is likely that the spatial variation dominates the fitted model response. However, introducing HDI does allow the model to capture the declining rend in NCV burning (Fig. F3) so the temporal response seems to be reasonable. Another potential issue is that annual GDP is sensitive to short term financial crises or other abrupt changes. Such a drop in GDP would have an immediate effect in the model and this is likely not entirely realistic (although we are unaware of any studies attempting to quantify this). HDI is likely a better indicator in this regard as economic activity comprises only one third of its value, the other two factors (life expectancy and years in education) are not so immediately susceptible to short term changes in economic circumstances."

- L655: Did the authors consider other remote-sensed burned products that might include small fires like GFED4s?

Actually, ESA FireCCI51 also includes enhanced sensitivity to small fires and results in a similar burnt area to GFED4s (~450 Mkm^2, Lizundia-Loiola et al. 2020). We considered using GFED5, but by the time it was released our study was fairly advanced and we also became aware of an issue whereby the product overpredicts significantly in Sweden, which would be very problematic for our study domain. As we already discuss the small fires issues in broad terms, so we propose to simply mention the enhanced FireCCI51 small fire sensitivity by modifying the sentence at line 654 to read:

"Remote sensing products based on MODIS (including the ESA FireCCI51 data used here) are known to struggle with detecting small fires and have high omissions errors, particularly in the

Mediterranean (Katagis and Gitas, 2021), although FireCCI51 does feature improved sensitivity to small fires (Lizundia-Loiola et al., 2020). "

- L690: Finally, can the authors address if this model is specific to this region or could be transferred to other regions of the world? How involved do they believe the process of doing this would be?

We believe the actual model is very specific to Europe but the methodology and many outcomes can be taken to other regions. We have some preliminary work for NCV fires globally which shows promise. Cropland fires will likely be trickier and may need to take into account cropland specific management practices, and the grassland fires may also need to be handled explicitly (but that is also not confirmed at this stage). Doing regional studies would likely be far more tractable than global, especially for agricultural fires. We propose to add a new closing sentence to the Conclusions:

"In addition, the scientific outcomes and methodology developed here can facilitate the development of similar models for other regions."

Minor comments:

- L26 here and elsewhere rephrase meteorological fire danger for clarity

We actually only use that phrase in the abstract and suggest changing it to: "fire weather danger".

- L34 remove "of" just before "state of the art"

Yes, but actually we changed it to "to", because we do need the appropriate preposition there.

- L39-40 suggest rephrasing these sentences

We suggest to change it to:

"The strong model skill of BASE when reproducing seasonal and interannual dynamics of NCV burning and the novel inclusion of cropland burning indicate that BASE is well suited for integration in land surface models."

- L49-52 split and shorten this sentence

Yes, it is a bit long. We suggest:

"It interacts with many components of the Earth system, with notable effects on biogeochemical cycles, surface energy budgets, and vegetation dynamics and composition (Archibald et al., 2018; Bowman et al., 2009). Through these effects, fire alters the chemical composition of the atmosphere and the physical properties of the land surface, thereby influencing regional and global climate (Archibald et al., 2018; Bowman et al., 2009; Jones et al., 2022)."

- L58 rephrase "coherent political level" for clarity

We have replaced "coherent political level over a broad spatial extent." With:

"a local, national and transnational levels."

- L210 revise "artefacts"

Replaced with “anomalous values”

- Figure 1: Here and through the figures would be clearer if the acronyms were defined in the axis labels and figure captions

Yes, we added explanations of the acronyms to the captions, but believe that also adding them also to the axis labels isn’t practical given the length of the full names.

- Figure 5: Here and elsewhere the single shared legend and brief caption could be clearer if they provided information about the mean lines, uncertainty regions, etc.

Ah right, the trend lines, sorry for the omission. We have modified the caption labels and added appropriate variants of:

“The trend (calculated with linear regression) is plotted as a straight line with the 95% confidence interval shown as coloured shading.”

References:

Lizundia-Loiola, J., Otón, G., Ramo, R., & Chuvieco, E. (2020). A spatio-temporal active-fire clustering approach for global burned area mapping at 250 m from MODIS data. *Remote Sensing of Environment*, *236*, 111493.