Replies to reviewer comments

RC1

1. As I started reading through the manuscript, it was unclear to me if the manuscript was going to compare results from run with specified/prescribed and interactively simulated fire emissions. It was unclear if the prescribed/specified fire emissions had inter-annual variability or not. Please try to make this clear upfront.

The Introduction text was changed to reflect this - Lines 100-101

2. When discussing results, PLEASE mention the figure and its panels in parenthesis so that it's explicitly clear that the results and/or discussion follows from which figure and from which of its panel.

As the reviewer noted, the reference to figure and its panels can improve the understanding of the discussion and readability. Reference to figures and respective panels have been added to the manuscript - Lines 397, 401, 408, 409, 412, 416, 439, 443, 447, 449, 454, 455, 460, 463, 479, 480, 483,496, 501, 510, 519 and 521.

3. There are no sub-headings and panel identifications in several figures which made it really difficult. For example, in Figure 11 and 12 there are no sub headings on panels. The acronyms NHAF, NHSA, SHAF, SHSA threw me off several times. I missed the distinction between NHAF and NHSA even after reading the Figure 11 caption multiple times.

The authors thank the reviewer for raising this. The Figures 3 to 15, which contain several panels were changed to include sub-headings better identifying them.

4. The INFERNO fire model appears fairly simple. I am surprised that the average burnt area per PFT (in equation 1) is specified a priori as a model parameter. First, what are the units of this quantity? Table 1 says its units are km2 but it has to be km2 per unit SOMETHING? Is it per unit grid cell area (unlikely), per unit 1000 km2, per unit ignition? Second, and assuming this quantity represents average area burned per unit ignition, this seems to imply that area burned per unit ignition can never exceed this, assuming Fpft (the flammability) varies from 0 to 1 (please mention this). If true, it's not correct to call this quantity average area burned for a given PFT

The units for average burnt area are km^2 per ignition. This has been corrected in both Table 1 and the model description in line 127. INFERNO does assume this as the maximum burned area which is burnt in any given time, rescaling it according to fire activity, represented by flammability, and we think referring to it as “scaled average burned area” would be more appropriate. The manuscript has been changed to reflect this in Table 1, line 127 and 297.

5. In equation (1) and elsewhere the lack of units makes it difficult to understand things. Please mention units for all terms of all equations.

Units have been added throughout the manuscript to help with the understanding of equations. Changes have been made in lines 123, 125, 128, 132, 143, 144, 145 and 153.

6. My logic tells me that INFERNO should be a module of JULES. If this is the case then meteorological variables from the atmospheric component are passed to JULES which then provides quantities like soil moisture to INFERNO. As it reads, the manuscript seems to imply that INFERNO is a separate component.

As the reviewer noted, INFERNO is a module of JULES, however when JULES is coupled to the UM atmospheric model there is a set of conditional compiling statements that exclude INFERNO for being compiled, making INFERNO only available when JULES was used in standalone mode. Further work had to be developed to ensure an interface between INFERNO, JULES and the UM was available, passing on the atmospheric variables to INFERNO. In Section 2 of the manuscript the authors try to highlight this by describing the atmospheric coupling which also includes the coupling to the atmospheric composition model UKCA.

7. The part related to INFERNO’s description needs an equation for I_{N}, natural ignitions. As a reader, I was curious to know how natural ignitions are modelled as a function of lightning frequency.

INFERNO does not parameterize natural ignitions (from cloud to ground lightning). These need to be provided to INFERNO either as a constant value, based on ancillary data or modelled/parameterized externally through the atmosphere model as described in section 2.1 in lines 118 to 122. Considering this and the advantages of the coupling of INFERNO to the atmosphere model (UM) we have opted to provide the natural ignitions parameterized by the UM which follows the approached described in Price and Rind (1994), as described in the section 2.3 of the manuscript (lines 216 to 222).

8. In equation (2), it seems theta cannot be the soil moisture (which varies between 0 and porosity, typically around 0.4), it seems theta is more likely the soil wetness which varies between 0 and 1, as the soil moisture itself varies between 0 and porosity.

The authors thank the reviewer for their comment. The equation has been changed to reflect this.Theta is the unfrozen soil moisture as a fraction of saturation. This has now been corrected in lines 141 and 142.

9. On page 6, line 186-187, I am troubled by the fact that emission factors for aerosols are doubled. Does it mean that the standard emission factors based on Andreae (2019) are too low?

The factor of 2 scaling applied to biomass burning aerosols is a common practice applied in Earth System Models and it is applied in the standard configuration of UKESM1 where biomass burning emissions are prescribed. This is used to improve the agreement between observed and simulated aerosol optical depth (AOD) across the three evaluated wave lengths (440, 550, 700 nm) when compared to observations. As stated in the manuscript, this has previously been described by Johnson et al. (2016) and Kaiser et al. (2012). In these studies, the authors acknowledge that the discrepancy between modelled and observed AOD (prior to emission scaling) could be due to other biases or missing processes in the models.

For this study, and to make a comparison between the simulations with prescribed and interactive fire emissions more comparable, we decided to follow suit and applied the same scaling factor when coupling INFERNO emissions to the atmospheric composition component of UKESM1.

10. In section 3.2.1 changes to land cover result in several differences in regions where the land cover is not changed due to teleconnections. Clearly, these are primarily due to land-atmosphere interactions and not due to fire-atmosphere interactions. Please make this clear

The authors thank the reviewer for their comment. Detailing the mechanism that leads to impacts at a global scale cause either the land cover change or the fire-composition-atmosphere feedbacks, as well as the primary forcings, can lead to extensive work that is out of the scope for this manuscript. The authors have decided to remove this statement (lines 366 to 369) and added a sentence to clarify that effects could have a contribution from both the land cover change as well as the fire-atmosphere-composition feedbacks and would prefer to revisit this in the future on a manuscript focused on this topic..

11. Figure 10. Please make it clear on y-axis that the quantity being shown is CO. Also, please check there are units and quantity name on y-axis of all similar plots.
The y-axis of Figure 10 was updated to clearly identify the data being shown

12. Page 8, line 224, “Aerosol emissions are distributed vertically following an exponential increasing function . . .”. Does this mean there are more emissions at the surface and less up in the atmosphere or the other way round?
This means that there are higher values for biomass burning aerosols emissions at the higher levels than at the surface. The authors have now changed the text to make this clear in the document - line 230.

RC2

Comment 1: Line 34: black carbon is missing a letter

Response: Changed to fix the missing letter

Comment 2: Line 90 introduce the significance of peat fires. The authors should note that both GFED4s and GFAS rely on MODIS products, which are less capable of detecting low temperature smouldering peat fires than VIIRS and other moderate resolution sensors. By line 320, it is noted that peatland fires are not included. It would be good to clarify this at the beginning of the manuscript

Response: The main INFERNO model limitations have now been added to the last paragraph of section 2.1 - lines 161 and 162.

"Furthermore, it should be highlighted that in this configuration of INFERNO, there are no interactions between fire and vegetation and it does not include a peat burning capability."

Furthermore, the lack of efficiency in detecting low temperature smouldering peat fires in the observation datasets is also mentioned in lines 330-332 to highlight possible observational bias.

Comment 3: Lines 235-237: Some extra spaces in the text. Further GFED4s is a multi-sensor satellite dataset that uses a statistical model to predict small fires. The small fires are not observed directly from active fire data.

Response: The authors thank the reviewer for raising this. In line 245 we now highlight that small fires are statistically modelled in GFED4s. The extra spaces were removed from the text.

Comment 4: Section 3.1: The model's poor performance in the boreal means a significant underestimation of burned area in forest and peat areas that are often the dominant source regions of emissions for the Northern Hemisphere - as well as large impacts on the Arctic. If the point of INFERNO is to develop a coupled fire-climate-composition Earth system model, leaving out much of the boreal does not mean the model estimates burned area fraction well. Why is this happening? The authors have a good explanation for why north Africa is underestimated. Can the authors explain why the overestimation of tree fraction in the SHSA produces smaller fires? Recent Amazonia fires have shown smaller fires in grasslands turning into large understory fire complexes that dry out the system for large canopy fires. Is this fire behaviour of rainforests well represented in the model?

Response: Achieving a good performance modelling fires at the Earth System Model spatial and time scales is a state of the art challenge. The work developed by Li et al. (2019), in the context of FireMIP, provides a global multi-model estimate of fire emissions for the historical

(1700–2012) period - it shows that there is a tendency for fire models to underestimate biomass burning emission in the boreal regions and that there is also a large spatial variability between models. This happens mainly due to the different treatment of the land surface between models (e.g. not including peatland fires in INFERNO) and the way anthropogenic fire behaviour is modelled, for example, the treatment of crop fires seasonality.

Despite INFERNO's limitations for this region, interactively modeling fire in Earth System Models provides benefits and several advantages at a global scale.

An explanation for the reason why an overestimation of tree fraction results in smaller fires in SHSA is provided in lines 291-294 and follows below:

"In addition, there is an overestimation of tree fraction in savanna biomes, such as the southern Africa region (SHAF) and the southern edge of the Amazon forest region (SHSA). The differences in the specified average burnt areas for these biomes – smaller for trees than grasses – causes an underestimation of fire size in these regions."

With regards to the fire behaviours of rainforests, unfortunately INFERNO does not represent these regional scale effects which are specific to certain biomes. INFERNO is a simple fire model that was designed for Earth System Model applications, and thus it focuses on the large-scale occurrence of fires and the large-scale aspects of fire behaviour.

Li, Fang, et al. "Historical (1700–2012) global multi-model estimates of the fire emissions from the Fire Modeling Intercomparison Project (FireMIP)." Atmospheric Chemistry and Physics 19.19 (2019): 12545-12567.

Commnet 5: Figure 1 and the dominant PFT: since the model was found to be sensitive to underlying vegetation, do that authors have an uncertainty analysis of the PFT used in UKESM1UKESM1-AMIP configuration with other global land use products, like the MCD12C1 0.05 degree MODIS land cover product for climate modeling? The PFT ignores the Cerrado and established croplands in eastern Amazonia, as well as overestimating C4 grasses in northern Australia.

Response: A description and evaluation of the vegetation modelling in UKESM1 is provided in Sellar et al. (2019). In their work the authors compared modelled vegetation results to the IGBP-LUCC and CCI-LC land cover data sets and highlight notable bias which include an excess of C3 grass in tundra regions which the observations indicate should contain more bare ground. The southern extension of the Saharan bare soil causing a deficit of grassland in the Sahel, caused by biases result from precipitation deficits in these regions, associated with errors in the position and intensity of monsoon rains. And a small overestimation of tree fraction in savanna biomes, most notably on the southern edge of the Amazon forest attributed due to the lack of fire disturbance, the inclusion of which would be expected to improve vegetation structure in these regions.

Comment 6: Figure 3. Burnt area fraction is underestimated in the boreal and all of Australia, as noted by the authors, but also in the Indo-Gangetic Plain, the southeastern U.S., much of central American and extending into Ecuador, Venezuela, and Colombia, eastern China, and Indonesia. In terms of climate, nor representing emissions from peat fires in southeast Asia and near the Himalayas and Andes calls into question the performance of the fire-composition-climate coupling. Further, many of these locations of human dominated fire regimes - whereby lightning strikes are not the main drivers of fire. So how well is the HDI performing?

Response: Although the model presents an underestimation of biomass burning emissions for a myriad of regions which are important at the local to regional scales, these represent a relatively small contribution to the overall biomass emission budget, with regions such as South America and Africa dominating at a global scale. Therefore, the impact in the atmospheric composition and consequent feedback in the Earth System context is relatively small. This can be seen by the comparison of modelled atmospheric composition fields (aerosols and carbon monoxide) with observed datasets. However, this doesn't mean that those regions where we find negative biases are of least importance. They provide a contribution at regional scales and these biases need to be taken into account and their inherent limitations when the model results are analysed.

As noted, in the regions pointed by the reviewer, the main driver of fire ignition are human activities. It is known that humans can change background levels of natural fire activity and that different cultural and political influences in the management of fire can shape fire regimes at a regional level. Moreover, due to their nature, cultural and political influences on fire management have a high spatial and temporal variability. For these reasons, it is difficult to include these detailed processes in the model. Introducing the HDI dependence in ignitions represents an attempt to include these cultural and political influences in the model. The authors acknowledge that this is not an ideal representation of these effects but it provides a significant improvement in regional model results. However, discussion of the impact of these is out of scope in this document, as it would significantly increase the length and complexity of this paper. A separate paper is under preparation where the details and performance of the introduction of this parameterization will be presented.