Thank you for this positive review. As with reviewer 1, the suggestion that we should explore model performance in previous years has led to extensive revisions, outlined in more detail in a separate author comment. Here, we have made detailed responses to all these major points raised, specific comments and technical corrections, below each comment. We also outline revisions made to the manuscript where appropriate. Reviewer comments are in normal text, responses are in blue, and text quoted from the main text or SI are in *italics* or, for extensive quotes, indented in Times New Roman. Line numbers quoted below are for the original text unless otherwise stated.

Kelly et al. apply a simple numerical model to assess whether or not the 2019 fire season in the Amazonas region was cause by climatic anomalies or other causes. Based on an ensemble of simulations they suggest that the increase in areas of high deforestation was unlikely to be the consequence of climate anomalies and are more likely related to forestry activities. The manuscript is well written, clearly structured and the documentation guidelines for code are followed. I agree with reviewer #1 that while this is a valid and interesting approach, some discussion should be devoted to the ability of the model to robustly capture the observed interannual variability and specifically the extremes.

In addition to the appendix figures included in response to reviewer 1, we have also incorporated into the new section 3.1 a description of historic annual, seasonal and inter-annual burning in tropical South America, and discussed how well our model reproduces these fire regime measures. This includes dry years and years of increased human fire activity. We show the model captures years of increased burning driven by meteorological conditions and, as expected given the model set up, the increased uncertainty associated with human driven-year season fires and years of increased deforestation. See response to reviewer 1 for changes.

Minor comments:

P1L16: Can you be specific here based on what type of information the model is optimised?
We have adapted the sentence (line 16) to read *"To answer this, we take advantage of a recently developed modelling framework which optimises a simple fire model against observations of burnt area, and whose outputs are described as probability densities."*

P1L18: This makes it sound as if the model redicted an increase in burnt area in these regions, whereas as far as I understand this is actually based on observations. Please clarify.
We replace *"We show…" at the start of this sentence* with *"Observations show…"* to clarify.

P3L74ff: Please see comment by reviewer #1.
Please see the response to reviewer #1

P3L85: As far as I understand the set-up, the method tracks uncertainty in the parameter set of the model, but does not address alternative model structures or uncertainty in observations. We
We have clarified this in response to reviewer #1's second comment on line 85

P4L132: Please provide a motivation for this change in approach, and clearify that you mean to say you included all data points in the assimilation procedure?

Kelley et al. 2019, a global study on a 0.5-degree grid, and made the pragmatic choice of sampling 10% of the observations due to computational demands. We did not have the same demands in this study because of our coarser resolution and sample study area. We have now made this clear in the text. We also rephrased "data point" with "grid cells" to match terminology typically used for this type of fire modelling and made it clear that we are talking about the assimilation procedure. The sentence now reads *"We used all of the 44750 grid cells on our 2.5° grid and monthly time step for 16 years* **in our assimilation procedure, This is a departure from Kelley et al.** *(2019)****, where only 10% of grid cells were used, as our sample size was much smaller and we did not face the same computational demand."***

P5L144: Please used evaluated or similar in stead of validated. Please provide a succint description of the evaluation result here so ease of reading the paper. P6L168: Hard to follow, please rephrase

We have replaced "validated" with "evaluated" and provide a little bit more detail of the benchmarking methods implemented. By adding the following sentence to line 144:

> … parameter uncertainty of our model, corresponding to the yellow areas in time series in Fig. 1. The mean burnt area for a particular parameter combination ($\overline{BA}_\text{ß}$), was obtained from:
>
> $$\overline{BA}_\text{ß} \ = \ \int_0^1 P(BA|\text{ß}) \times BA \ dBA \qquad\qquad (4)$$
>
> $\overline{BA}_\text{ß}$ was evaluated using the implementation of the fireMIP benchmarking metrics (Rabin et al. 2017; Hantson et al. 2020) as per Kelley et al. (2019).

A summary of evaluation results is now provided in the new section 3.1, as outlined in the response from both reviewers regarding model performance.

P6L174: How is this different from 1. Please also check language of the sentence "Calculated as". What has been calculated?

We have expanded our description of the 3 measures. See response to reviewer #1s comment in line 174 of the original text.

P6L217: Visually, this isn't true for all the areas and years, can you please provide a quantitative assessment to back up this point?

With clarification of the three different measures of likelihood, and the change in figure caption in response to a similar comment by reviewer 1, we hope it is now clear we refer to the scatter points on the left of figure 1, and not the time series.

P8L246: "Novel" makes this read is if the model had been developed in this paper, whereas indeed you have "simply" (correctly and usefully) applied a model published in 2019. Please rephrase and emphasise the novel aspects.

While the model has been developed in this paper (especially in the revised version of the m/s), it is the "approach" that is novel, not the model itself. By applying a Bayesian framework, we are able to the test observations as opposed to the model. For Bayesian statisticians, we foresee this to be particularly exciting as it mirrors the inverse framing of the question associated with Bayes theorem. I.e what is the chance of the observations given the modelling framework. That might be to technical for a conclusion in an interdisciplinary journal, so we have chosen to simply add *"... by testing the likelihood of observed extremes in fire again inferred historic relations*" to this sentence (line 246)

 Fig. 2 In order to have the paper readable without the SI, would it be possible to add the regions A-F and ADD to the Figure 2?
A-F have been added to observed maps of fire in Figs 3,4, C1 and C2. We have included a map of AAD as Fig. A2 (see reponse to reviewer #1s comment online 179).

Hantson, Stijn, Douglas I. Kelley, Almut Arneth, Sandy P. Harrison, Sally Archibald, Dominique Bachelet, Matthew Forrest, et al. 2020. "Quantitative Assessment of Fire and Vegetation Properties in Historical Simulations with Fire-Enabled Vegetation Models from the Fire Model Intercomparison Project." *Geoscientific Model Development Discussions*. https://doi.org/10.5194/gmd-2019-261.
Kelley, Douglas I., Ioannis Bistinas, Rhys Whitley, Chantelle Burton, Toby R. Marthews, and Ning Dong. 2019. "How Contemporary Bioclimatic and Human Controls Change Global Fire Regimes." *Nature Climate Change* 9 (9): 690–96.
Rabin, Sam S., Joe R. Melton, Gitta Lasslop, Dominique Bachelet, Matthew Forrest, Stijn Hantson, Fang Li, et al. 2017. "The Fire Modeling Intercomparison Project (FireMIP), Phase 1: Experimental and Analytical Protocols." *Geoscientific Model Development* 20: 1175–97.