Thank you to reviewer #1 for a positive and very useful review. The suggestion that we should explore model performance in previous dry years has led to more extensive revisions then we suspect the reviewer anticipated, and we have outlined the resultant change to the model 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.

This manuscript investigates the probability that the burned area anomaly in Amazonia in 2019 was caused by anomalous meteorological conditions. The presented approach is interesting and allows a quick evaluation of recent fire events. They estimate the probability that the event is caused by meteorological conditions using a model setup that includes the meteorological forcing, but not the changes in ignitions caused by humans. This is theoretically a valid approach, however, it remains unclear to me how well the model is able to capture extremes caused by meteorological conditions. I recommend to improve the discussion of the model performance by investigating the model performance for the investigated regions specifically for years that are known to have anomalies in observed fire occurrence caused by meteorological conditions.

In addition to the changes outlined in our separate author comment on changes in the model's error term description, we have included four new appendix figures (Fig. B1, B2, C1, C2, see figures at the end of this response) that demonstrate the model is able to capture levels of burning 2005 and 2010, years previously already highlighted (as discussed in the main text, lines 185-191) as driven by drier conditions. We have also included a new section 3.1 which describes burnt areas over our observational period, and the model's ability to capture inter-annual variability in burnt areas:

> **3.1 Burnt area from 2001-2018**
>
> The highest burnt areas are found in the Savanna regions of tropical South America (Fig. 3), though some burning still occurs in forested areas, particularly in areas which have experienced an increase in agriculture and decrease in tree cover since 2002 (Fig. S4). The model reproduces this spatial pattern, and the models full posterior encompass the full range of burnt areas (Fig. 3, and benchmarking SI). Burnt area starts to increase in May and dies out in October throughout most of the Area of Active Deforestation, though can start as late as July in more humid areas can continue through to December in drier Savanna (Fig S3). The bulk of the burnt area in August and September. September typically sees the highest burnt area in central Brazil, whereas fire peaks in August around Bolivia and Paraguay (Fig. 2F, S3). Our model reproduces this seasonal pattern in burning across all regions (see benchmarking SI), including onset and peak (Fig S3). As our model maintains constant human ignitions and suppression throughout the year, this suggests that the seasonal pattern can be largely reproduced from meteorological variations. Though a slight increase in

uncertainty in early fire season burning could point to increased human ignitions not captured in the model (Fig. S3).

Unusually high levels of burning occurred in 2004 in the Bolivian/Paraguayan dry forest (red line in Fig. 1E), 2005 in the eastern arc of deforestation (Fig. 1A) and Paraguay dry forest (Fig. 1E), 2007 in monsoonal coastal forests (Fig. 1D) and 2010 in Bolivia and Paraguay dry forests (Fig. 1D and E). 2005 and 2010 burning have previously been associated with droughts driven by a Tropical North Atlantic warming anomaly (Marengo and Espinoza, 2016). The model reproduced the spatial pattern of this increased burning (Fig C1, C2). In our different regions within the AAD, observed levels of buring fall within, although at the higher end, of our models posterior (Fig. B1, B2) with a high value of expectation (height of the posterior curve in Fig B1, B2) and high p-value (blue shaded area, B1 and B2). This, along with the model high spearman's rank performance (Fig S1 and S2) suggests that the model is able to capture the interannual variations driven by meteorological conditions.

Previous years of increased burning were 2004 in the Bolivian dry forest (red line in Fig. 1E), 2005 in the eastern arc of deforestation (Fig. 1A,B) and Paraguay dry forest (Fig. 1E), 2002 in Paraguay, 2007 in monsoonal coastal forests (Fig. 1D, 2F) and 2010 in Bolivia and Paraguay dry forests (Fig. 1E). Deforestation rates in 2004/05 were high (Marengo et al., 2018), and an increase in fire activity in 2007 has also been linked to deforestation across the Amazon (Morton et al., 2008). Additionally, in the early part of our observational record, much of the region has been shown to be less coupled to meteorological drivers and more heavily influenced by human fire and land management (Aragão et al., 2018). This is reflected by the improved performance of the model, which depends solely on changing population density and land use cover and not on changes in landscape management, during this later period in the AAD (Fig 2F, 2011 onwards), particularly in areas dominated by agriculture (Fig. 2H). On the whole, frameworks posterior is better able to encompass extremes in observations in humid regions (Fig 2G vs Fig 2H, I), particularly across the Brazilian arc of deforestation (Fig1A-C vs D and E). 19 out of 204 months up to 2018 for the AAD (~9%) fall outside the 90% confidence interval (tan in Fig 2F), suggesting that the frameworks posterior accurately describes the occurrence of more extreme months for the region as a whole. That only 13 months out of (204 months x 5 regions) 1020 months (~1%) fall outside the posterior for smaller regions (Fig 1) suggest that the posterior is wider than expected. Our assessments of mismatch between observations and model for these regions will, therefore, likely be conservative, particularly for humid regions B and C, with no months prior to 2019 falling outside the 90% confidence interval.

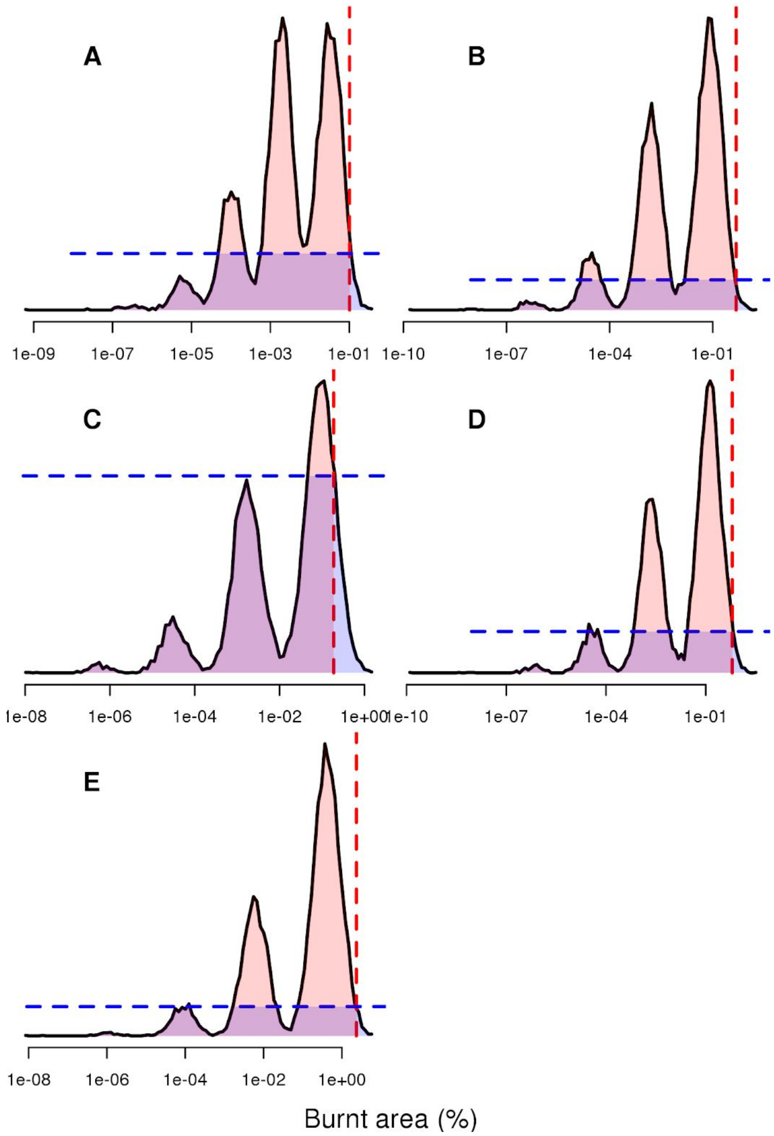

Figure B1: Full model posterior solution (black line) for August 2005 across each of the sub-regions compared to MODIS Collection 6 MCD64A1 burned area product (red dashed line). The red shaded area (posterior solution smaller than observed) shows the likelihood high burnt areas were influenced by factors external to the modelling framework. Blue shaded area is the area of the posterior which has less chance of occurrence than the observed burnt area (given by blue dashed line).

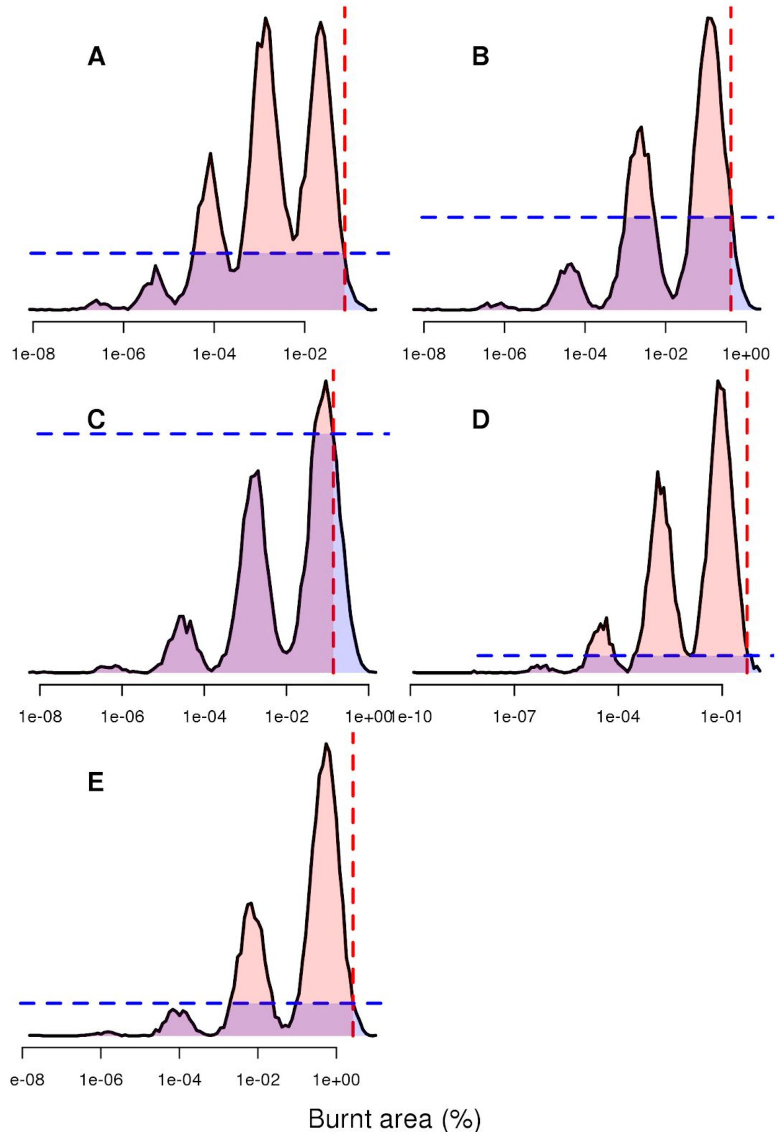

Figure B2: As Fig. B1 but for August 2010.

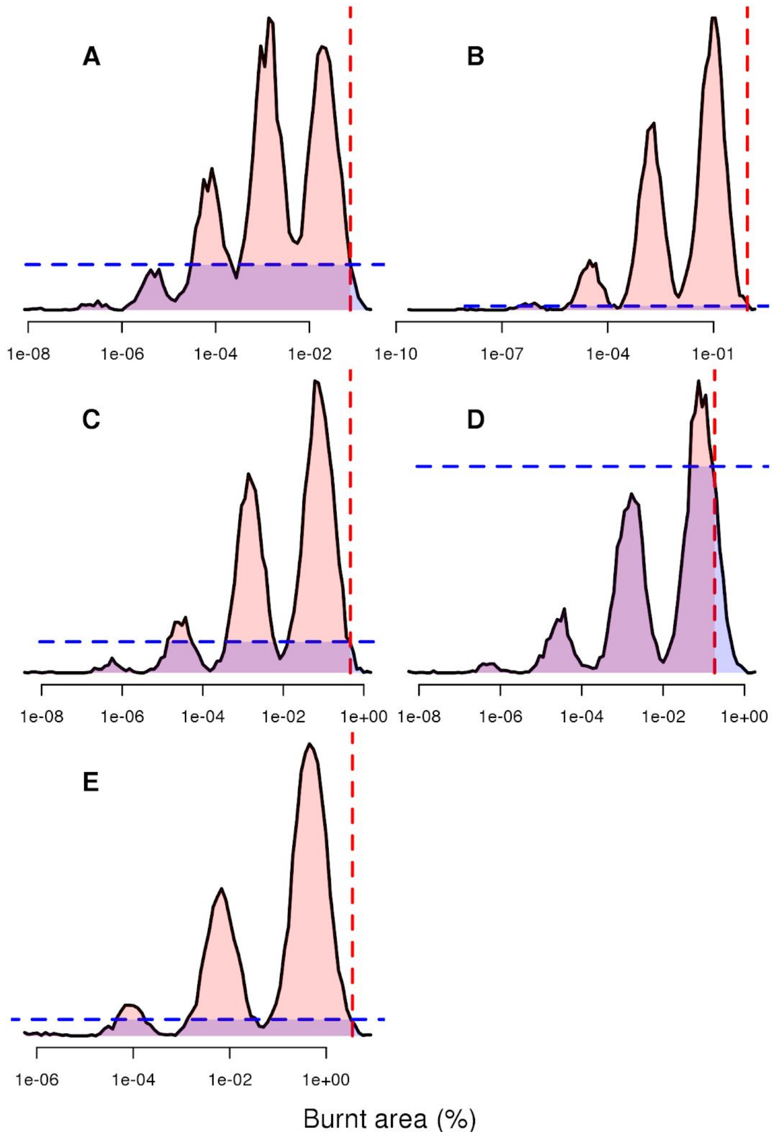

Figure B3: As Fig. B1 but for August 2019.

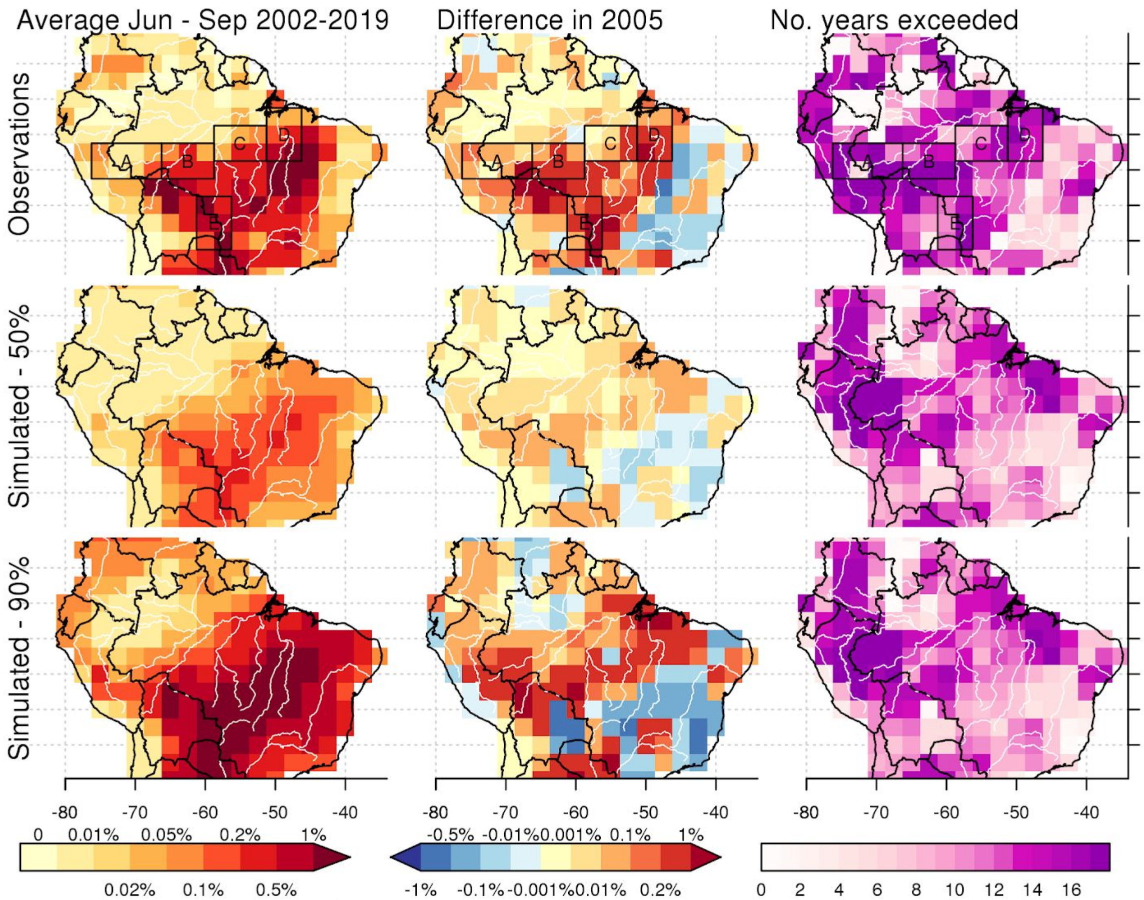

Figure C1: Same as Fig. 3 but for 2005.

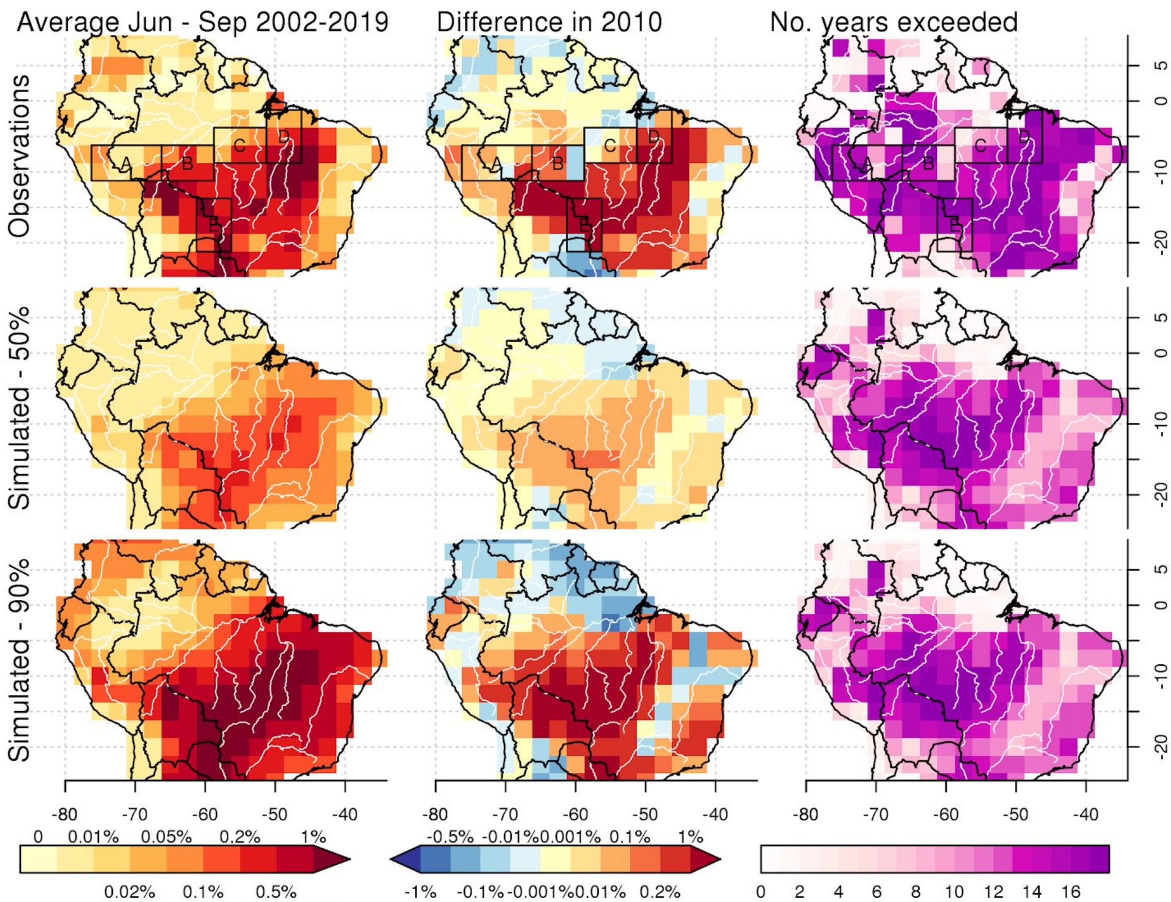

Figure C1: Same as Fig. 3 but for 2010.

Source code and data are made available as recommended in best practice guidelines. The manuscript is well written. The presentation requires some clarifications as indicated in the specific comments.

Specific comments:

p.1 l. 27: Last sentence of abstract should be a conclusion rather than another result.
Yes, this is a discussion point of the paper, which is why we have included in the abstract. We have included a result in that abstract that backs this statement up. The sentence now reads "*Burnt area for September in the arc of deforestation had a 31% probability of being caused by meteorological conditions, potentially coinciding with a shift in fire-related policy from South American governments.*"

P2. l. 65: what exactly is a loose attribution? I guess it comes down to interpreting correlations, which can easily be confounded due to the many drivers, as causations?
Yes, that's exactly right, and we have adapted the sentence so it now reads *"However, these do not consider the complex interaction of multiple drivers on fire and are therefore unable to go*

beyond a loose attribution of a particular forcing to fire, which can easily be confused due to the many drivers, as causations." to make this point.

P3 l.71: actually FDIs often make assumptions on the available fuel, for instance the difference in fuel drying between 1hr, 10hr, 100hr and 1000hr fuels in the fire danger index used in SPITFIRE. Isn't this the basis for Kelley and Harrison 2014 as well?
Models such as SPITFIRE do apply an FDI to describe the moisture content and drying of different fuel classes, but not the amount of fuel, and rely on some form of vegetation modelling to supply information on the abundance of fuel, which is the point we are making here. We have adapted the sentence to make it clear that FDIs do sometimes form part of vegetation-fire models. *"...such as burnt area or number of fires.* ***Some LSM fire schemes achieve this by modelling fuel moisture using FDIs*** *(Lenihan et al., 1998; Rabin et al., 2017; Venevsky et al., 2002)".*

It should be noted that the SPITFIRE variant in (Kelley and Harrison, 2014) does not use an FDI. In this model, the Nesterov FDI used in most other SPITFIRE based models was replaced by a more mechanistic description of fuel moisture based on equilibrium moisture content in relation to atmospheric drying potential driven by relative humidity, temperature and timing of rainfall events (see (Kelley et al., 2014) for more details). Additionally, (Kelley and Harrison, 2014) specifically makes the point that changes in FDI are not appropriate for assessing changes in observed fire measures such as burnt area because they lack information from other controls. We, therefore, feel it is appropriate to keep this reference.

p.3 l. 81, you could add Forkel et al. 2017 (GMD)
Included as suggested.

p.3 l. 77: I believe the main disadvantage of fire models embedded in vegetation models is the complexity of the whole model that makes it difficult to fuse the model quickly with most recent observations. Also the inputs from the vegetation model are limiting the model performance. Having a simple fire parameterization which is largely driven by observations clearly has an advantage here. But can not represent the feedbacks between vegetation and fire on the other hand or estimate impacts on the carbon cycle, hydrology etc.
This is an excellent point, and we have added a sentence on the problems of running rapid, near-time assessments at the end of this paragraph (line 81):

> Embedding fire within a complex vegetation model system also prevents rapid observation-model fusion, as iterative optimization techniques are too computationally expensive, instabilities arise from non-linear responses of fire to simulated vegetation and fuel dynamic. Many large scale vegetation-modelling projects, therefore, simulate up to a "present-day" that can be several months or years out of date (Friedlingstein et al., 2019; Hantson et al., 2020).

We had made the point that vegetation-fire models were designed for a different purpose (carbon, hydrology etc) in a previous sentence. (line 74)

p. 3 l.85: again I think the main advantage is that it is largely driven by observations and it is optimized using observations.
We agree, and therefore now start the sentence on with *"**The main advantage** of this system is that it can assess…"*

p. 3 l. 85: track uncertainty in the model? Uncertainty in the model suggests that you may refer to the uncertainty related to the model structure, e.g. the shape of functions you implement and the choice of drivers. Usually such bayesian frameworks capture uncertainties of the optimized parameters and can propagate these uncertainties to the output variables. (which is also possible with other optimization techniques.) Please be more precise.
We have clarified that we mean model parameters by changing the sentence to read *"The main advantage of this system is that it can assess the contribution of different fire drivers directly from observations and track uncertainty in the model parameters and the model's ability to reproduce observations.".* Although "*model's ability to reproduce observations.*" is technically represented by two error terms that are included in the modelling framework, we separate out the performance terms as they are not part of the model that represents a physical process. They are also important in translating the more traditional style model output into a useful model posterior that can be used for attributing specific months.

L .109: something wrong with the sentence. Which variable has such coarse resolution? You might reconsider interpolating that variable to higher resolution and therefore being able to maintain the information of the subgrid heterogeneity of other variables could be advantegeous.
It was only land-use data and burnt area that was on a higher-resolution grid. As burnt area was not used to drive the model, we decided that the advantages of higher-resolution for just a small set of drivers were not worth the computation expense of optimising the model at the higher resolution grid. We have now made this clear by replacing the first sentence in the paragraph starting line 109 to:

> All variables were resampled and, where necessary, interpolated to a monthly time-step as per Kelley et al. (2019). All driving variables were provided on a resolution of 2.5° except land use, provided at 0.5°. We, therefore, choose to regrid all datasets to a resolution of 2.5°, as interpolating to a finer resolution would provide no new information about the meteorological drivers tested.

l. 155 based on what information did you choose these areas?
In the original version of the m/s, these were pragmatic choices of areas of definite mismatch. With the new error term of the model, we have been able to focus in on specific areas and transects of historic deforestation activity, and have made this clear by replacing lines 154-165 with:

> We chose five regions (marked A-E in Fig. 1, 2. See Fig. A2 for locations) to represent forest areas already under pressure from deforestation. Regions A-C form a transect (west to east) across the

agricultural-humid tropical forest interface in Brazil's arc of deforestation, often associated with deforestation (Fig. S4), whereas D and E regions are found in agricultural regions embedded in savanna and grassland regions that experience regular fires:

A. Acre, Southern Amazonas States and Brazilian/Peruvian border
B. Rondônia and Northern Mato Grosso, Brazil.
C. Tocantins, Brazil,
D. Maranhão and Piau in coastal deforestation regions
E. Brazilian, Bolivian and Paraguayan border

We also assessed an overall Area of Active Deforestation (AAD) in the Amazon region (Fig. A2). This area is defined as the parts of South American southern tropics with significant decreasing tree cover trends, as seen in VCF (Dimiceli et al., 2015) and increasing agricultural fractions in the HYDEv3.1 dataset (Klein Goldewijk et al., 2010). Trend analysis used the same technique described in Kelley et al. (2019), where we took significance as $p<0.05$ on the linear trend for each month in the year on logit transformed variables, using the greenbrown R package (Forkel and Wutzler, 2015). AAD was additionally assessed over 3 sub-regions areas, primarily to evaluate the models' historic performance and assess the increase in 2019 fires across the humidity gradient:

F. Area of Active Deforestation
G. The Southern end of agricultural-**humid tropical forest** interface in Brazil's central states, often associated with arc of deforestation in Brazil's central states
H. **Drier** savanna and woodland in Cerrado and Caatinga in the eastern Basin were land has already been heavily converted to agriculture
I. Southern **dry-deciduous** Chiquitano and Gran Chaco forests, mainly along the Amazon, La Plata watersheds.

l. 165: maybe possible to describe the technique briefly to not make the reader go back to Kelley again?

We have included a brief description of the trend analysis and, perhaps more importantly, acknowledged the software used to perform it by adding the sentence this sentence (see the response to the last comment): "Trend analysis used the same technique described in Kelley et al. (2019), where we took significance as $p<0.05$ on the linear trend for each month in the year on logit transformed variables, using the greenbrown R package (Forkel and Wutzler, 2015)."

l. 168: unclear, please rephrase

We have tried to clarify each measure with a brief description in bold, and have also provided much more detail as to how each measure was constructed. Lines 168-175 have been replaced with:

1. The **likelihood of observed monthly burnt area** based on the information provided to our model. In the predictive model, the probability of a burnt area y (where y can be outside training data Y_s, as is the case for our year 2019 analysis) being explained by our model ($Pred(y)$ - full model uncertainty, or model error, in tan areas on time series in Fig. 1, 2) is proportional to the probability of y given a parameter set, β, weighted by $P(\beta|Y_s)$:

$$Pred(y) \propto \int_{PS} P(\beta|Y_s) \times P(y|\beta)\, d\beta \tag{5}$$

Where the observed burnt area, y falls within the model's full posterior ($L(y)$) is then the sum of all probabilities greater than y,

$$L(y) = \int_{y}^{1} Pred(y)\, dBA \tag{6}$$

As our posterior solution is not normally distributed, observations can fall at the extremes of the posterior (i.e when there is no burnt area, $y = 0$ and by definition $D(y) = 0$), and still have a high likelihood (i.e if $P(Y_s = 0|\, ß)$ in equation (3) is much greater than 0. See Fig B2 as an example). We, therefore, define the significance of $D(y)$ as the probability of y occurring by chance ($pv(y)$) from the sum of all probabilities below $P(y)$ (Fig. A2):

$$pv(y) = 1 - \sum_{i \varepsilon 2Z}^{i \le n} \left[\int_{y_{i-1}}^{y_i} P(x)\, dx - P(y) \times (y_i - y_{i-1}) \right] \tag{7}$$

where $\{y_1, ..., y_n\}$ is the set of solutions to $P(y_i) = P(y)$

Whenever $D(y)$ and $pv(y)$ is low indicates burning significantly higher than expected than suggested by the model in that month.

2. The **likelihood burnt area would have been higher than the annual average**, i.e the fraction of the model's full posterior greater than the model's annual average climatological posterior (the point where the vertical lines cross 1 in right-hand columns in Fig. 1,2). A climatological burnt area $clim$ for a given month, m, in the year (i.e January, February, etc) can be calculated from the convolution of each year's posterior solutions, $\beta_{yr,\, m}$. Not that it's the model inputs, incorporated in β_m, and not the model parameters that vary with time:

$$P(BA \mid clim_m) = P\left(BA \mid \beta_{2001,\, m} \cap \beta_{2002,\, m} \cdots \cap \beta_{2019,\, m}\right)$$

Where $P\left(BA/2 \mid \beta_i \cap \beta_j\right) = \int_0^{BA} P\left(BA - x \mid \beta_j\right) \times P\,(BA \mid \beta_i)\, dx$ and

$$P\left(BA/3 \mid \beta_i \cap \beta_j \cap \beta_k\right) = \int_0^{BA} P\,(BA - x \mid \beta_k) \times P\left(x/2 \mid \beta_i \cap \beta_j\right) dx \qquad (8)$$

The probability of an anomaly A in a given year, yr, for month m is, therefore:

$$P\left(A \mid \beta_{yr,\,m} \cap clim_m\right) = \int_0^1 P(A \times BA \mid \beta_{yr,\,m}) \times P(BA \mid clim)\, dBA \qquad (9)$$

The likelihood of a year having a higher anomalous, A, is the sum of probabilities of $A < 1$:

$$L\left(A \mid \beta_{yr,\,m} \cap clim_m\right) = \int_0^A P\left(A \mid \beta_{yr,\,m} \cap clim_m\right) dA \qquad (10)$$

And the likelihood of the year an average burnt area is given $L\left(1 \mid \beta_{yr,\,m} \cap clim_m\right)$

3. The **likelihood of the observed anomalous year** occurring is given by :

$$L\left(\frac{y_{m,\,yr}}{\overline{y_m}} / \mid \beta_{yr,\,m} \cap clim_m\right) \qquad (11)$$

where $y_{m,\,yr}$ is the burnt area for the month, m, and year, yr, in question, and $\overline{y_m}$ is the climatological average of that month

l. 174: so 1 and 3 are basically the same? 3 for annual 1 for monthly burned area?
This should now be clearer in the revised text, outlined above, 3 is roughly equivalent to 1, but on climatological anomaly rather than raw values.

l. 179: how do you define areas of recent deforestation? Please indicate these areas.
Areas of decline are areas showing a decreasing trend in MODIS VCF tree cover (Dimiceli et al., 2015) and increasing agriculture in HYDE (Klein Goldewijk et al., 2010) (see the response to comment on line 165). The areas were shown in Supplementary Fig. S4, but we have also included a map of regions as a new appendix Fig A2, which we refer to at appropriate points in the text.

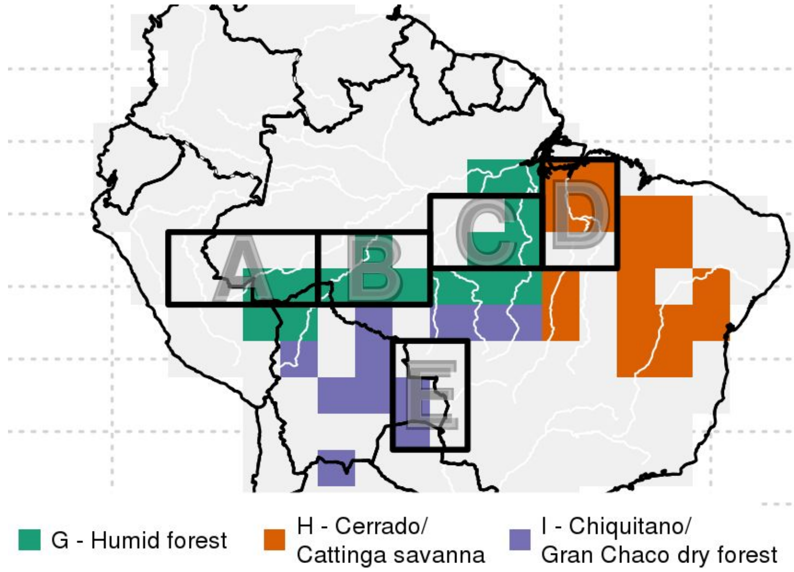

Figure A2: Study regions. Boxes mark areas used for time series in Fig 1 and rows A-E in Table 1. Coloured areas for time series if Fig 2 and F-I in Table 1, with the entire coloured region being used for F AAD. See Fig. S4 for construction of AAD and areas G-I.

l. 217: the authors write that in no region the observed anomaly has been that far outside the model range as in 2019. If I look at figure 1 I get a different impression: Region C: 2019 within full posterior of the model, 2007,2010, 2012, 2017 not. Region d: 2010 and 2004 seem much more outside the model range than 2019.

The "anomaly" is in reference to the scatter plots on the right of Figure 1, and not the time series. We have added a brief discussion on the months where observations fall outside the time series posterior in section 3.1 (see the response to the reviewer's main comment), and have made it clear, whenever appropriate in the main text, that anomaly refers to the scatter plots in Fig 1 and (new) Fig. 2 (see additional author comment 3). The discussion on observed vs modelled anomalous years at the end of (now) section 3.3, "***Climatic conditions in 2019***" reads:

> The observed anomaly for August 2019 is higher than the model across all regions except. This is particularly prominent in regions B and C where observations show that burnt area was 196% and 138% greater than the August average (Table 1). Whereas the model suggests that meteorological conditions alone should have resulted in a fire season with a 16-122% (based on 5-95% parameter uncertainty range for parameter uncertainty) increase in burnt area in B and 2% reduction to 4% increase for C compared to the August average, with only a 57% and 53% chance of a greater burnt area than the average for B and C respectively. The likely occurrence of the observed anomaly was 7 and 10% for B and C respectively (Table 1) - much greater than any previous year (Fig 1B and C, August column).
>
> The higher observed anomaly vs the model extends over much of the AAD (Fig. 2 "August" column, red points). The model suggests a 4-6% reduction for the AAD, with a 49% probability of greater than the annual average burnt area (Table 1). By comparison, the observed burnt area was 45% greater than the annual average, with a 20% likelihood. Again, the observed anomaly seems to be least likely in more humid regions. For our humid area, G, the model suggests a small (10-14%) increase in burnt area, with only a 12% probability of 107% increase seen in observations, whereas the 5% observed reduction in drier savanna regions in H seems to be in line with the model (at 30% likelihood).

l. 246: what is the novelty in your bayesian approach?

See the response to reviewer #2

l. 249: explain the setup of your model: it underpredicts burned area when taking into account meteorological conditions but keeping land use and population density constant.

We have added *"... when taking into account meteorological conditions but keeping land use, population density and human-fire interactions constant"* to this sentence, which now reads:

> The model predicts a lower burnt area than we see in the observations for Amazonia during June-August 2019, **when taking into account meteorological conditions but keeping land use, population density**

and human-fire interactions constant. This indicates that from observed meteorological data alone, we would not expect 2019 to be a high-fire year.

l. 445: Figure 1 caption, please explain the color of the columns on the left.
We have added *"The colour indicates the year, with 2019 in red."* to the caption.

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