Tian et al 2nd review, Reviewer 2

The paper is greatly improved – it's at the minor revisions stage. There are still a few caveats needed in the text (a few sentences – sometimes this information appeared in the response to the Reviewer but did not make it into the manuscript text), and one figure that is in the response to the reviewer should also be placed in the revised manuscript or the SI.

With reference to the authors' Response to Reviewer 2 (page number, etc), noting that **'s mark the most important issues to should be addressed with a few more sentences of explanation/caveats on the work:

Page 8, re: revised figures: yes, the slashes work MUCH better, makes the presentation easier to read.

**Re: discussion on lines 36-37 and Figure R1: The figure, along with slanted marks for 90% confidence regions, should be included in the revised manuscript or its SI, and discussed in the text, and the text for lines 36 and 37 should be modified accordingly. The text for those lines could be interpreted to mean that positive correlations between precipitation and fire emitted carbon take place everywhere, whereas the figure shows that the effect is location-specific (and strongest in central Africa and Australia, and may be the opposite, elsewhere). An example of how it could be reworded: the original sentence for lines 36 and 37 is: "We find global fire aerosols cause a cooling of surface air temperature and an inhibition of precipitation. These climatic perturbations further reduce regional leaf area index and lightning ignitions, both of which are not beneficial for fire emissions. " A modified version (or words to this effect): "We find global fire aerosols cause a cooling of surface air temperature and an inhibition of precipitation. The response of fire emissions to these climatic changes varies with region: in central Africa and Australia, the reduction in precipitation leads to a reduction in regional leaf area index, reducing forest fire risk. However, in North America, Eurasia, and the Amazon Basin, precipitation is anti-correlated with forest fire emissions (reductions in precipitation lead to increases in forest fire emissions). These differences may reflect the seasonal variation of rainfall in the different regions; a reduction in precipitation during monsoon "wet" seasonal precipitation leading to less LAI in the subsequent dry season." Note that this last sentence is my interpretation of what may be causing the regional difference: Australia and parts of Africa have seasonal rainfall ("wet"), followed by seasonal "dry" periods. A decrease in precipitation during the wet thus leads to less LAI for burning in the subsequent dry, and hence the positive correlation between the amount of rainfall in those regions and subsequent smoke emissions (less rainfall in the wet means less leaf area added in the wet, so less fuel in the subsequent dry). The negative correlation in other regions (Amazon, North American and Eurasian boreal forests, where the precipitation perhaps is not as seasonally diverse) indicates that precipitation increases there reduces fires – since the precipitation is occurring during fire season. Note also that this is what I mean by "time to effect" – some of the authors' results may be explained by a delay between the change due to meteorology (e.g. LAI reductions due to precipitation reductions coupled with a strong seasonal dependence to precipitation occurrence).

So, the sentence needs to be modified to match the authors' additional analysis, and the figure needs to be included in the manuscript or SI. It would be good for the revised figure to include the confidence level slash marks as well.

Page 10/11 re: model species include sulfate, nitrate, sea-salt, dust, black carbon, and organic carbon. A small amount more detail is needed. The authors have not included particulate ammonium, base cations, and primary versus secondary organic carbon in this list. I infer from the absence of particulate ammonium and base cations that the model does not include particle inorganic heterogeneous thermodynamic calculations (e.g. Fountoukis, C., and A. Nenes (2007), ISORROPIA II: A computationally efficient thermodynamic equilibrium model for K+-Ca2+-Mg2+-NH4+-Na+-SO42-NO3--Cl-H2O aerosols, Atmos. Chem. Phys., 7(17), 4639-4659), and that the Bauer et al (2007) approach mentioned is a simplification that doesn't generate particle ammonium and doesn't take the base cation chemistry into account. The authors need to confirm this and include a caveat sentence in the text to the effect of "We note that the current model speciation does not include explicit inorganic heterogeneous chemistry (e.g. Fountoukis and Nenes 2007) due to the computational cost of these calculations being prohibitive in a climate model context, and this may affect model results." Similarly, its not clear from the description whether the model includes organic aerosol created by oxidation of organic gases – please include this in the description, mention its absence as a potential impact on the model results. The inorganic heterogeneous chemistry calculations tend to be computationally intensive – and hence this is a good reason why they might not be included in a climate model simulation (and can be mentioned as such). The aim here is to describe the model speciation and processes and the potential impact of their absence precisely – a couple more sentences should do it.

Page 12: "In the revised paper, we clarified as follows…". Ok, this assumes that the meteorological changes results in instantaneous changes in VPD and LAI, land surface water and energy fluxes. The precipitation had to be time-smoothed to avoid large fluctuations in flammability. Fair enough – but my concern here is related to the authors mention that decreases in precipitation can lead to decreases in flammability (earlier comment): I can see how this would work, in the context of monsoon rain areas: decreasing precip in the wet season leading to less foliage available for burning later, in the dry season. What's missing in their discussion here is a sentence explaining why the counterintuitive results occur in some regions but not in others. For example, a sentence to the effect of "It should be noted that the forest fire emissions response to meteorological changes that change VPD and LAI may not be instantaneous, but may occur over time - for example, a reduction in precipitation in one season at a given location may reduce foliage growth and hence reduce the fuel available for combustion in another season."

Page 12: "We do not consider…" ok, fine – the issue of plume height has been discussed."

Page 13: "In the revised paper, we added following comparisons…" Ok, fair point. Though note that the Canadian Fire Weather Index does include the effects of meteorology and vegetation, but not the feedbacks between them, nor the ignition/surpression side of things.

Page 13: "In the revised paper we added:". Ok, all three points help describe the methodology used.

Page 14: In principle, the successful suppression of fires…" Ok. "The selection of constant values" Ok – though I note that some political jurisdictions suppress all fires (even in unpopulated areas). But the lack of global data upon which to improve on this is a good point. "Third, considering the complex nature of fire activities…" Ok, fair enough. For the information of the authors, there are a number of plume rise algorithms that are in use in the regional air-quality modelling context - a summary and references for a recent regional wildfire modelling intercomparison (Ye et al, ACP, 2021 https://acp.copernicus.org/articles/21/14427/2021/ is one place to start). Also fair to note that these algorithms are intended for forecasting smoke, and are therefore based on observation input data such as satellite retrievals of hotspots, and hence are less useful in their current form for climate applications where a climatological hotspot input would need to be constructed.

Page 15: "In this study, the annual total GFED emission is used…". Ok; spatiotemporal pattern is from the rest of the parameterization, GFED totals for the same are used to adjust the amounts; has been modified to fit GFED. This begs a question: in the event that the model is used to predict fires under future climate conditions, will the GFED totals still be used (if not, how much of a difference will it make)?" Add a sentence on the implication of using GFED normalization for current climate simulations versus future climate where it could not be used: how much difference does the renormalization make to the model results?

Page 16, "In the revised paper… Size-dependent optical parameters" Are the optical parameters such as the complex refractive index values also dependent on the chemical speciation of the aerosols, or was a "typical or climatological" complex refractive index used? Similarly, does the first AIE depend on aerosol speciation"?

Page 16, "In the revised paper, we clarified as follows: "In ModelE2-YIBS, fire emissions…". Ok. Might want to caveat "We note that the changes in the environmental factors at one point in time may result in changes to fire emissions later in the same year".

Page 17, "We do not apply GFED emissions…" Ah. Ok, so the implication is that the regional distribution in the model differs from the GFED regional distribution due to the inclusion of the local meteorological, etc., effects, hence changes like this come up. Ok makes sense.

**Page 17, "we clarified how the AAE is calculated in the model:…". The portion of the text the authors have added to the paper does not include the information that follows within the authors' response – which should be incorporated into the manuscript. That additional text "We performed long-term simulations…" makes it clear that the deposition of black carbon in the model is not a dynamic process resulting from the forest fire smoke carbon emitted by the model being deposited as a time-varying flux boundary condition to the surface (which would, for example, include the size dependence of aerosols on the deposition flux). Instead they have estimated an average BC deposition during a fire season (how this is estimated is not clear) which is redistributed (presumably equally, again, this is not clear) to each model day. The added text in the manuscript needs another sentence to the effect of "We note that average BC deposition to snow (estimated by ….) was used as a climatological proxy to the physical process of BC deposition. The latter involves size resolved and meteorologically dependent BC deposition fluxes, as would be found in a chemical transport model, but was not used here due to computational constraints." The "…" in the sentence needs to explain how the average BC deposition was derived (e.g. perhaps the assumption is that all of the emitted BC would be deposited?).

**Page 17, “In the model, LAI is calculated daily…”. The point I made has not been addressed. The authors have stated that the changes to drought conditions will affect plant photosynthesis instantaneously *and exert impacts on LAI in the coming days*”. Which says that impacts on LAI will lag the photosynthesis changes, the point I was making in my original review. However, the text they have added does not make clear whether the model as implemented incorporates this time lag of LAI changes, or whether the LAI is assumed to instantaneously decrease when drought takes place. This still needs to be addressed in the text, with another sentence actually stating the time scale over which the vegetation response to the meteorology is parameterized within the model. Does the LAI change in the model instantaneously in response to drought, or not?

**Page 18, “We acknowledge that there are non-linear…from the associated changes caused by precipitation and LAI.” Fair point, but doesn't address my original concern – that the number of factors which may locally influence the impacts will not have meaning if the magnitude of those impacts are not considered. Suppose one location is highly sensitive to a single factor but very insensitive to the other 3. In such a case, and leaving out the potential for complex feedbacks altogether, a region might be sensitive to one parameter to a disproportionate magnitude compared to the other 3 parameters. The text current reads as if the number of sensitivities will determine the magnitude of the effect, while my point here is that the local magnitude of the sensitivity may also be important. There may be only one controlling factor – but it may have a outsize impact compared to a location with moderate sensitivity to 3 parameters. The text needs a caveat to that effect; rather than “several complex feedbacks that may exert offsetting effects”, the caveat should be “several complex feedbacks that may exert offsetting effects, and the relative magnitude of individual factors may vary spatially (both the number of factors and the magnitude of their effects will determine the overall response).”

Minor issues section.

I note that the text included in “(5) The fire-emitted minerals/dust-like material …” has not been included in the text. It should be included in the text, in the same section as the other 4 points, since this is a significant portion of forest fire particulate matter mass. There should also be an explanation of why it was not included, given that the model does have a particulate matter dust variable.

All other responses to minor revisions in my original review are ok.