We are grateful to the referees for their time and energy in providing helpful comments and guidance that have improved the manuscript. In this document, we describe how we have addressed the reviewer's comments. Referee comments are shown in black italics and author responses are shown in blue regular text. A manuscript with tracking changes are attached at the end.

Reviewer #1

I am very grateful to the authors for their hard work in addressing both my and the second reviewer's comments from the previous revision. In particular the inclusion of uncertainty estimates throughout, and more precise description of the model configuration and experimental setup, have made the manuscript much more informative. I have a few wording suggestions on some of the revised text detailed below, where in a couple of places I feel the authors may not have completely addressed my original points. Subject to these minor additional changes being made though, I would be pleased to recommend the manuscript for publication in Atmospheric Chemistry and Physics.

(In the below comments, line numbers refer to the clean version of the manuscript)

- Thank you for your positive evaluations.

Minor comments:

L17-L18: "instantaneous environmental perturbations" – they're not instantaneous; they are rapid adjustments. In place of the first two sentences, the authors could maybe say something like: "Fire emissions influence radiation, climate, and ecosystems through aerosol radiative effects. These can drive rapid atmospheric and land surface adjustments which feed back to affect fire emissions" or something along those lines.

- In the revised paper, we modified the text as suggested: "These can drive rapid atmospheric and land surface adjustments which feed back to affect fire emissions." (Lines 18-19).

L19-L21: "Here, we quantify the impacts of fire aerosols on climate through direct, indirect, and albedo effects based on the two way simulations using a well-established chemistry- climate-vegetation model" – the authors have not completely addressed my main concern here that the abstract is misleading; once again I would assert that the current study does not exactly quantify the impacts on climate; it quantifies the aerosol radiative forcing and rapid adjustments/fast

response. The abstract needs to be clear and unambiguous about this throughout – readers should not have to dig in the main text to work this out. Instead of this sentence, the authors could maybe say something like: "Here, we quantify the impacts of fire aerosols on radiative forcing and the fast atmospheric response through direct, indirect, and albedo effects, based on two-way simulations using a well-established chemistry-climate-vegetation model". Or alternatively, "Here, we quantify the impacts of fire aerosols on climate through direct, indirect, and albedo effects based on atmosphere- only simulations using the GISS-E2 model coupled to an interactive vegetation and wildfire scheme". Or something similar.

- In the revised paper, we modified the text as suggested: "Here, we quantify the impacts of fire aerosols on radiative forcing and the fast atmospheric response through direct, indirect, and albedo effects based on the two-way simulations using a well-established chemistry-climate-vegetation model." (Lines 19-22).

L34-L39: Same goes for the short summary – currently there no changes to the short summary to indicate that the manuscript only reports the rapid adjustment/fast response from atmosphere-only simulations. It still talks about the impact of fire aerosols on climate and the impact on temperature and precipitation without providing the context that this is not the full coupled climate response. Like the abstract, the short summary should stand on its own and not be ambiguous or misleading without further context. As with the previous comment, I suggest modifying it to say something like: "We quantify the impacts of fire aerosols on climate through direct, indirect, and albedo effects. In atmosphere-only simulations we find global fire aerosols cause cooling of surface air temperature and inhibition of precipitation over many land regions. These fast atmospheric perturbations further reduce regional leaf area index and lightning ignitions…" etc.

- In the revised paper, we modified the text as suggested: "We quantify the impacts of fire aerosols on climate through direct, indirect, and albedo effects. In atmosphere-only simulations, we find global fire aerosols cause cooling of surface air temperature and an inhibition of precipitation over many land regions. These fast atmospheric perturbations further lead to a reduction in regional leaf area index and lightning activities" (Lines 35-38).

L65-L67: "Aerosol radiative effect is the instantaneous radiative impact on energy balance of climate system, representing the fast adjustment or response before changing global mean surface air temperature (TAS)" - Remove the word 'instantaneous'; the authors should be precise with

language and stick to the accepted terminology. It's not instantaneous, it's the fast/rapid response and the radiative forcing being diagnosed is the effective radiative forcing (if I have understood correctly, please tell me if I'm wrong though!). This is not the same as the instantaneous radiative forcing, which has a specific meaning. References to the 'instantaneous' radiative impact are therefore potentially confusing. I would also say "fast atmospheric adjustment…" in L66 (rather than just 'fast adjustment') just to be even clearer for readers that are less familiar with the literature, what is being allowed to adjust whilst GMST is kept fixed.

- In the revised paper, we modified the text as suggested: "Aerosol radiative effect represents the fast atmospheric adjustment or response before changing global mean surface air temperature (TAS)." (Lines 66-67).

L85-L86: "Impact of fire-induced instantaneous climatic perturbations to fire activities on the global scale have not been fully assessed" – again, it's not instantaneous. The standard terminology is either rapid adjustments or fast (atmosphere-only) response. Change to: "The impacts of fire-induced rapid adjustments on fire activity at the global scale have not been fully assessed" or something similar.

- In the revised paper, we modified the text as suggested: "The impacts of fire-induced rapid adjustments on fire activity at the global scale have not been fully assessed." (Lines 85-86).

L96: "quantify the feedback of fire-induced instantaneous climate effects to fire emissions" –same thing again, this is not the usual terminology and it's not actually instantaneous. Replace 'instantaneous climate effects' with 'rapid adjustments' or 'fast/atmosphere-only climate responses'.

- In the revised paper, we modified the text as suggested: "The main objectives are (1) to isolate the radiative effects of fire aerosols through ADE, AIE, and AAE processes and (2) to quantify the feedback of fire-induced rapid adjustments to fire emissions." (Lines 94-96).

(From author response to a previous comment) "The differences between YF and NF include the emissions of both primary aerosols and aerosol precursor gases (such as NOx, SO2, NH3). In the revised paper, we clarified as follows: "The fire emissions include both primary aerosols and trace gases, the latter of which react with other species to form the secondary aerosols. These particles could be transported across the globe by the three-dimensional atmospheric circulation and

eventually removed through either dry or wet deposition." (Lines 227-230) and "For YF simulations, fire-induced aerosols including primarily emitted and secondarily formed are dynamically calculated based on fire parameterization (see section 2.3) and atmospheric transport." (Lines 238-240)." – this hasn't entirely answered my original question, which was: what about trace gas emissions which aren't aerosol precursors? The model description mentions other gas-phase chemistry including NOx, CO, and CH4, all of which have radiative impacts either directly themselves, or via impacts on tropospheric ozone. I'm still unclear on whether these emissions were also perturbed, and whether there are therefore also impacts on radiative forcing from these gas-phase species and ozone perturbations. I agree they will likely be small compared to the aerosol forcing and so I don't have a problem with the rest of the paper focusing on aerosol radiative effects, but I would still like to clarify whether these gas species are perturbed and in principle contribute to the radiative forcing that is diagnosed directly and/or via feedbacks on ozone, or is it only the aerosol changes which affect the radiation scheme?

- These gas species can cause additional radiative effects which are ignored due to the small magnitude. In the revised paper, we clarified as follow: "Note that fire-emitted gas-phase species also perturb radiation via atmospheric absorption and/or feedback from rapid adjustment; these perturbations are far less than aerosol forcing and could be ignored." (Lines 265-267).

L186 – L187: "Natural and anthropogenic ignition determines whether the fire can actually occur. If ignition is zero, the resulting fire emissions will be zero…". To me, this reads quite oddly now. You could say 'ignition rate' (singular), or else I would put it back to 'ignitions' (plural) like it was before (and then make sure the surrounding sentences are consistent with this). I.e. either: "Natural and anthropogenic ignition rate determines whether fires can actually occur. If the ignition rate is zero, the resulting fire emissions will be zero…", or else: "Natural and anthropogenic ignitions determine whether a fire can actually occur. If there are zero ignitions, the resulting fire emissions will be zero…" or something along these lines.

- In the revised paper, we modified the text as suggested: "Natural and anthropogenic ignition rate determines whether the fire can actually occur. If the ignition rate is zero, the resulting fire emissions will be zero, regardless of flammability." (Lines 198-199).

L187-L188 and L193-L194: "Natural ignition source IN" and "The number of anthropogenic

ignition source IA" – these should probably be "The natural ignition rate IN" and "The anthropogenic ignition rate IA" respectively.

- Corrected as suggested.

L223: "has been validated based on global observations (Pechony and Shindell, 2009)" – could potentially also reference the Hantson et al. (2020) FireMIP paper (https://doi.org/10.5194/gmd-13-3299-2020) here, since this intercompares and validates multiple global fire models including the INFERNO model which is essentially another implementation of the Pechony and Shindell (2009) scheme.

- In the revised paper, we cited the *Hantson et al. (2020)* FireMIP paper as suggested.

L240-L241: "These fire emissions cause radiative perturbations and the consequent changes in climatic variables, which feedback to influence fire emissions" – change to "These fire emissions cause radiative perturbations and consequent fast atmospheric adjustments, which feedback to influence fire emissions" or something similar.

- In the revised paper, we modified the text as suggested: "These fire emissions cause radiative perturbations and the consequent fast atmospheric adjustments, which feed back to influence fire emissions." (Lines 256-257).

L254: "last 20-year averaged" -> 'last 20 years averaged' or 'last 20-year average being analysed'

- Corrected as suggested.

L254: "Two-tail student t-test is performed" -> 'A two-tailed Student's t-test is performed'

- Corrected as suggested.

Section 2.4: I am grateful to the authors for now including ± standard deviation values after all the global mean or sum values. However the methods section also needs to be updated so that the reader knows what the ± values correspond to (apologies if it does say somewhere and I've missed it; I couldn't see in obviously Section 2.4 though). Without stating this, readers will likely assume that it corresponds to the same 90% confidence interval as is used for shading in the figures.

- In the revised paper, we added: "The global mean or sum value is depicted in the form of mean

value ± standard deviation" (Lines 273-274).

L331: "3.4 Climate feedback to fire aerosol radiative effect" – suggest changing this to: "3.4 Fast response feedback on fire emissions" or something similar, since as far as I can tell this section doesn't actually discuss the feedback on aerosol radiative forcing, rather it discusses the feedback on fire emissions.

- In the revised paper, we modified the subtitle as suggested: "3.4 Fast response feedback on fire emissions" (Line 350).

L340: "the joint the impacts of fire-aerosol-induced instantaneous climatic change" – c.f. previous comment about L85; replace 'instantaneous climate effects' with 'rapid adjustments' or 'fast/atmosphere-only climate responses'.

- In the revised paper, we modified: "To illustrate the joint the impacts of fire-aerosol-induced fast climate responses, we count the number out of the four factors contributing positive effects to fire emissions over land grids (Fig. 5d)." (Lines 359-361).

Data availability (L430-L434): No information is given on the availability of the simulation output being analysed here; please add this.

- In the revised paper, we added: "Model data from this study are available from the corresponding author upon request." (Lines 461-462).

L643: "through fire-climate interactions" – maybe change this to "due to the fast response feedback" or something similar, so that the caption is easy to interpret even without reading the main text for additional context

- In the revised paper, we modified: "Changes in fire emissions of (a) BC and (b) OC due to the fast response feedback" (Line 683).

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.

- Thank you for your positive evaluations. We have added as many details as possible to further improve this study.

*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.

- Due to the word limit (500-character including spaces), we modified the *short summary* as follows: "We quantify the impacts of fire aerosols on climate through direct, indirect, and albedo effects. In atmosphere-only simulations, we find global fire aerosols cause surface cooling and rainfall inhibition over many land regions. These fast atmospheric perturbations further lead to a reduction in regional leaf area index and lightning activities. By considering the feedback of fire aerosols on humidity, lightning, and leaf area index, we predict a slight reduction in fire emissions." (Lines 35-39)

  We added the following discussion: "It may seem counter-intuitive that reduced precipitation would decrease wildfire emissions, while the observation-based data show that the fire-precipitation correlations are not negative in all regions (Fig. S6). In this study, the inhibition of precipitation in central Africa (Fig. 3b) reduces regional LAI (Fig. 5c) and decreases fuel availability for fire occurrence, resulting in a positive correlation between fire and precipitation that matches the observed relationship in Africa (Fig. S6). However, in North America, Eurasia, and the Amazon Basin, precipitation is anti-correlated with fire emissions. These differences may reflect the seasonal variation of rainfall in the different regions." (Lines 382-389).

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.

- The climate model includes the heterogeneous chemistry for particulate ammonium and base cations, and considers the formation of secondary organic aerosols. In the revised paper, we clarified as follows: "The thermodynamic gas-aerosol equilibrium module is used to calculate the phase partitioning of the $H_2SO_4/HSO_4^-/SO_4^{-2}-HNO_3/NO_3^--NH_3/NH_4^+-HCL/CL^--Na^+-Ca^{2+}-Mg^{2+}-K^+-H_2O$ system (Metzger et al., 2006; Bauer et al., 2007a)." (Lines 127-129) and "Heterogeneous chemistry on dust surfaces and NO_x-dependent secondary organic aerosol production from isoprene and terpenes is included in the model (Bauer et al., 2007b; Tsigaridis and Kanakidou, 2007)." (Lines 125-127).

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."

- In the revised paper, we added: "It should be noted that the response of flammability to abovementioned factors 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" (Lines 194-197).

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

- Done.

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/suppression side of things.

- Done.

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

- Done.

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.

- Done.

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?

- The GFED total is used only to calibrate EF for constraining the present-day prediction. In the revised paper, we clarified as follows: "The EF is then calibrated to minimize the root-mean-square error between the simulated and GFED data for all land grids. Such calibration adjusts only the global total amount of fire emissions without changing the spatiotemporal pattern predicted by the parameterization. The EF is the intrinsic attribution of wildfire emissions that should not vary greatly with climatic conditions." (Lines 227-231).

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"?

- In the revised paper, we clarified as follows: "Aerosol optical parameters are calculated by the Mie scattering theory using complex refractive index depending on chemical speciation and particle size." (Lines 144-145) and "The first AIE is estimated by the prognostic treatment of cloud droplet number concentration, which is a function of species-dependent contact nucleation, auto-conversion, and immersion freezing" (Lines 145-147).

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".

- In the revised paper, we added: "Note that the changes in the environmental factors may result in changes to fire emissions later." (Lines 240-241).

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.

- Done.

***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?).*

- In the revised paper, we added following text as suggested: "We note that average BC deposition to snow estimated by measurement-based average scavenging ratios is used as a climatological proxy to the physical process of BC deposition (Hansen and Nazarenko, 2004). The latter involves size resolved and meteorologically dependent BC deposition fluxes, as would be found in a chemical transport model, but is not used here due to computational constraints." (Lines 149-154).

***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?

- In the revised paper, we clarified: "Dynamic daily leaf area index (LAI) is estimated based on carbon allocation which is updated every 10 days and prognostic phenology which is dependent instantaneously on temperature and drought conditions" (Lines 163-165).

***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)."*

- In the revised paper, we clarified as follows: "Although the reduction of 2% to 3% in fire emissions by the fire-climate interaction through aerosol radiative effect seems limited, such change is a result of 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." (Lines 434-438).

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.

- In the revised paper, we added: "The fire-emitted minerals or dust-like materials are not implemented in the current model, given that these species is not included in the current GFED4.1s." (Lines 231-233).

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