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.

***<u>Reviewer #2</u>***

*Overall I think this paper will be suitable for publication in Atmospheric Chemistry and Physics, after revisions I've described below. I'm calling these major revisions, since one of my main concerns is that insufficient detail on the key components of the modelling system are provided in the submitted manuscript – as a result, I am unable to review the methodologies used within the model in the manuscript's current state (and will need to do so in a second look, once that information is provided). Some additional caveats to the conclusions should also be provided in the revised draft.*

- Thank you for your positive evaluations. We have added as many details about model configurations as possible to increase the clarity of this study.

*Having said that, I think that authors results (once they have provided that additional background) are quite interesting and worthy of publication: the potential negative feedback between forest fire smoke presence and fire starting conditions is an example, as is the use of a vegetative feedback model. The caveats associated with these models need to be clarified, see my detailed comments below.*

- We clarified the caveats in the revised version as suggested.

*My comments are divided into main and minor issues, and are usually referenced by line number in the current manuscript.*

***<u>Main Issues:</u>***

*A general issue regarding the manuscript Figures: the use of dots to show regions where the p value is less than 0.1 does not work. The dots are difficult to see, and obscure the values below, and the reader is unable to determine the boundaries of the p<0.1 region in most cases. The dots are difficult to distinguish from features such as the coastal boundaries in the maps, etc. Instead, the authors should (1) remove the dots from the* *existing Figure panels, and (2) create additional panels which show just the p value < 0.1 regions as colour-filled areas. These new panels should be placed in either in revised Figures, side-by side (original panel minus the dots on the left hand column of panels, and the corresponding p<0.1 region panel on the right hand column of panels), or as additional Figures in the main body of the manuscript, with a p<0.1 Figure following each of the original Figures (redone without the dots). As it stands, it's too difficult for the reader to distinguish the p<0.1 areas with the corresponding underlying regions on the map.*

- Thank you for your suggestion. For all revised figures, we have extended the range of color-map and replaced dots with slashes to indicate significant changes. In the revised paper, the boundaries of the $p < 0.1$ region are now very easy to identify.

*Lines 36-37: the idea that a reduction in precipitation could lead to a decrease in fire emissions is counterintuitive, and should be approached with caution and more caveats than provided by the authors in the current draft, since these effects will depend critically both on how well the flammability index employed simulates the fire risk, and how well the modelling system simulates the inputs for that risk.*

- Figure R1 shows the correlations between annual mean precipitation and fire-emitted carbon from GFEDv4s datasets. The correlation coefficients are very spatially heterogeneous with more than 80% of fire-land grids insignificant at 90% level ($r_{90\%}$=0.353). Moreover, correlations are positive over 37% of fire-land grids, especially in central Africa and Australia. Therefore, the reduction in precipitation does not always lead to increased wildfire emissions. On the contrary, higher precipitation tends to increase vegetation amount (LAI), which provides ample fuel load for fires and partially offsets the negative effects of precipitation on fire activity.

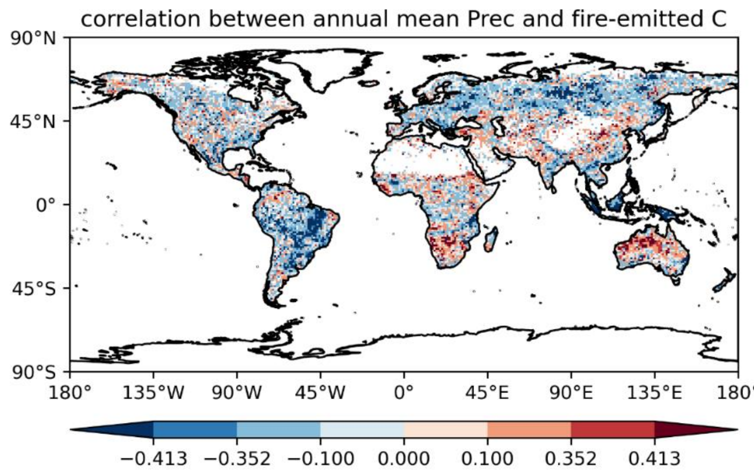


**Figure R1**. Correlations between annual mean precipitation and fire-emitted carbon between 1997-2019 from observations.

*There are several areas where there is insufficient description of the model and model components used: Section 2.2. To say that the methods are "well established" without providing further information is insufficient for publication. Needs more details (which could appear in a summary Table); e.g. number of gas-phase species and reactions, what secondary particle formation processes are included (and the methodology used – e.g. how is secondary organic aerosol formed?), what aerosol species are included in the model, how the aerosol handles aerosol mixing state and aerosol size distribution, how the radiative transfer routine makes use of the model's aerosol speciation and size distribution (e.g. Mie scattering with a homogeneous mixture? Some other form of mixing state and a heterogeneous mixture assumption?). In its current form, for example, the reader can't tell whether the model includes any other aerosol species aside from OC and BC, while forest fires emit gases such as SO2, NH3, and NOx, as well as some base cations in particulate form, all of which may result in the formation of secondary organic aerosol.*

- In the revised paper, we added following descriptions of the climate-chemistry model ModelE2 to clarify:

  "The gas-phase chemistry scheme considers 156 chemical reactions among 51 species, including $NO_x$-$HO_x$-$O_x$-CO-$CH_4$ chemistry and different species of volatile organic compounds. Aerosol species in ModelE2 include sulfate, nitrate, sea salt, dust, black carbon, and organic carbon, which are interactively calculated and tracked for both mass and number concentrations. The aerosol microphysical scheme is based on the quadrature method of moments, which incorporates nucleation, gas-particle mass transfer, new particle formation, particle emissions, aerosol phase chemistry, condensational growth, and coagulation (Bauer et al., 2008). The residence time of aerosol species varies greatly in space and time due to different removal rates. Turbulent dry deposition is determined by resistance-in-series scheme, which is closely coupled to the boundary layer scheme and implemented between the surface layer (10 m) and the ground (Koch et al., 2006). The wet deposition consists of several processes including scavenging within and below cloud, evaporation of falling rainout, transportation along convective plumes, and detrainment and evaporation from convective plumes (Koch et al., 2006; Shindell et al., 2006).

In ModelE2, gases can be converted to aerosols through chemical reactions, while aerosols affect photolysis and provide reaction surface for gases. For example, the formation of sulfate aerosols is driven by modeled oxidants (Bell et al., 2005), and the chemical production of nitrate aerosols is dependent on nitric acid and gaseous ammonia (Bauer et al., 2007). Moreover, the disturbances of aerosols on climate systems via direct, indirect, and albedo effects are considered in ModelE2. Size-dependent optical parameters of aerosols are calculated by the Mie scattering theory. The first AIE is estimated by the prognostic treatment of cloud droplet number concentration, which is a function of contact nucleation, auto-conversion, and immersion freezing (Menon et al., 2008; Menon et al., 2010). The AAE of BC is considered by estimating the decline of surface albedo as a function of aerosol concentrations at the top layer of snow or ice (Koch and Hansen, 2005). BC content in snow is determined by measurement-based average scavenging ratios (Hansen and Nazarenko, 2004). More detailed descriptions of ModelE2 can be found in Schmidt et al. (2014)." (Lines 121-145).

*The issue of time-scale-to-effect needs to be discussed. The impression I have from the manuscript in its current state is that meteorological effects like the precipitation change* *go into the vegetation model, which then predicts a new LAI value for the given vegetation type, which is then used in the flammability calculation and hence in forest fire emissions. However, if this is the case, the changes in LAI are assumed to be instantaneous in the authors' model. An analogy: a "normal" springtime where the normal amount of foliage is added to the trees is followed by an extreme drought summer. While the vegetation may "dry up", the amount of LAI may not change, and the biomass available for combustion may not change – and the dryness of the vegetation may result in a greater fire risk, not a lower one. The authors describe the vegetation model as operating "dynamically", but not what that means in sufficient detail for the reader to understand whether changes are instantaneous or have an inherent time scale.*

- In the revised paper, we clarified as follows: "VPD and LAI in Eq. (1) are calculated in half-hourly and daily time step, respectively, while 30-day running average precipitation is employed to avoid unrealistically huge flammability fluctuations." (Lines 183-185) and "In the coupled model, ModelE2 provides meteorological drivers to YIBs, which feeds back to alter land surface water and energy fluxes through changes in stomatal conductance, surface albedo, and LAI." (Lines 160-162).

*The height to which forest fire plumes reach in the atmosphere has a critical impact on their downwind distribution, their radiative transfer impacts, etc. The paper has no description of the manner in which plume rise of the forest fire plumes is calculated, or how the emitted mass is added to the model grid. This description needs to be added to the manuscript. If plume rise is not used to redistribute the emitted mass in the model, this is potentially a significant limitation on the accuracy of the model results, and should be acknowledged as such in the text.*

- We do not consider the impacts of fire plume height on aerosol concentrations in the model. In the revised paper, we clarified as follows: "We consider only the fire emissions at surface due to the large uncertainties in depicting fire plume height (Sofiev et al., 2012; Ke et al., 2021)." (Lines 226-227).

  We acknowledged this limitation of ignoring plume rise in the discussion: "In addition, the simulations omit several factors influencing fire emissions (e.g., moist content of fuels) and aerosol radiative effects (e.g. fire plume height). For example, studies show significant impacts of plume rise on the vertical distribution of fire aerosols and the consequent radiative effects (Walter et al., 2016)." (Lines 396-399).

*The equation for flammability (eqn 1, line 142) seems rather simplified (for context, I'm more used to regional air-quality model smoke parameterizations, based on fire weather index). The authors should contrast this with some of the other indexes available for predicting fire conditions on a forecasting basis (e.g. FWI, Wagner et al, 1987: https://cfs.nrcan.gc.ca/publications/download-pdf/19927).*

- In the revised paper, we added following comparisons: "Compared to fire indexes, such as Canadian Fire Weather Index system (Wagner, 1987), this fire parameterization shows advantages in integrating the effects of meteorology, vegetation, natural ignition, and human activities (both ignition and suppression) on fires. Furthermore, it is physically straightforward and has been validated based on global observations (Pechony and Shindell, 2009)." (Lines 220-223).

*Note also that the units of precipitation (line 141) seem to be reversed – shouldn't that be mm day-1, not day mm-1?*

- Corrected.

*Line 156: The equations for natural and anthropogenic ignition sources are presented without discussion on the basis for the parameterization or how IN is used. Is there some threshold value of IN below which ignition is not assumed to occur, for example?*
*Some more discussion on the basis of the parameterization and how it is used should be added here. Similarly, what's the physical basis for equation 6 (line 159)?*

- In the revised paper, we added:
  (1) "Natural and anthropogenic ignition determines whether the fire can actually occur. If ignition is zero, the resulting fire emissions will be zero, regardless of flammability." (Lines 186-187)
  (2) "Humans influence fire activity by adding ignition sources and suppressing fire events, the rates of which increase with population and to some extent counteract each other. The number of anthropogenic ignition source $I_A$ (number km$^{-2}$ month$^{-}$

[1]) is calculated as follows (Venevsky et al., 2002)" (Lines 192-194)

(3) $k(PD) = 6.8 \times PD^{-0.6}$ stands for ignition potentials of human activity, assuming that people in scarcely populated areas interact more with the natural ecosystems and therefore produce more ignition potential. α is the number of potential ignitions per person per month and set to 0.03." (Lines 197-200)

*Line 163, equation (7): This seems very simplified. The use of one set of coefficients implies that fire suppression activities are the same everywhere - they are not; different political jurisdictions within the same country can have different policies (e.g. no suppression unless the fire is within some distance of a population centre, versus "all fires are suppressed"). The authors need to explain why these coefficients are applicable over the entire globe, and the assumptions that were used in their creation. Note that what I am after here and in the above points is more description of the physical basis and the potential limitations they might have on the results: I'm aware of the need for simplifications in a climate modelling parameterization context – what's missing here is enough information on what's been done and the caveats that might affect the model results.*

- In the revised paper, we added:

  "In principle, the successful suppression of fires is dependent on early detection. It is reasonably assumed that fires are detected earlier and suppressed more effectively in highly populated areas." (Lines 201-202)

  "The selection of constant values in Eq. (7) is done in a heuristic way, due to lack of quantified data globally. It assumes that up to 95% of fires is suppressed in the densely populated regions but only 5% in unpopulated areas." (Lines 205-208).

  We acknowledge the limitations and uncertainties in the fire parameterization: "Third, considering the complex nature of fire activities, the fire parameterization in this study does not incorporate all fire-related processes (e.g., the influence of wind). In addition, the simulations omit several factors influencing fire emissions (e.g., moist content of fuels) and aerosol radiative effects (e.g. fire plume height). For example, studies show significant impacts of plume rise on the vertical distribution of fire aerosols and the consequent radiative effects (Walter et al., 2016). The impacts of human activity on fire emissions are calculated as a function of population density without considerations of differences in economy, education, and policies. These auxiliary factors may increase the spatial heterogeneity of fire aerosol radiative effects and deserve further explorations in the future studies." (Lines 395-403)

*Lines 173 to 175: So, really, this is a way of allocating GFED emissions. Unclear (needs a few more sentences of explanation): how is the temporal allocation of fire emissions simulated? Are the GFED emissions available as a function of monthly total, and these are divided up over each day within the month based on the daily average of the met inputs to the equations? Also, how are the emissions distributed in the vertical? No mention has been made of plume rise calculations. This has a crucial effect on the dispersion of the pollutants downwind, and their climate impacts. Or is the vertical distribution provided by GFED?*

- In this study, the annual total GFED emission is used to constrain the simulated fire emissions: "For each species, simulated gridded emissions are grouped by dominant PFT and compared to annual total emissions from GFED4.1s over the same grids." (Lines 215-216). "Such calibration adjusts only the global total amount of fire emissions without changing the spatiotemporal pattern predicted by the parameterization." (Lines 218-219).

  We do not evaluate daily fire emissions because GFED data are retrieved from satellite on the monthly basis: "The GFED4.1s provides monthly fire emission fluxes of various air pollutants based on satellite retrieval of area burned from the Moderate Resolution Imaging Spectroradiometer (MODIS) (van der Werf et al., 2017)." (Lines 101-103). In addition, we use prescribed monthly sea surface temperature to drive the climate model, and as a result it cannot reproduce the daily variability of meteorology that drives the variations of fire emissions. This method is applied to derive the long-term climatology of fire emissions.

  The GFED data do not provide fire emissions at vertical layers. We acknowledged the limitation of ignoring fire plume rise on the simulated aerosol concentrations in the discussion section: "In addition, the simulations omit several factors influencing fire emissions (e.g., moist content of fuels) and aerosol radiative effects (e.g. fire plume height). For example, studies show significant impacts of plume rise on the vertical distribution of fire aerosols and the consequent radiative effects (Walter et al., 2016).” (Lines 396-399).

*Section 2.2 also needs a description of the methodology used for each of the 3 aerosol effects (ADE, AIE, AAE); how they were parameterized in the model, perhaps as an additional table. How the aerosol speciation, mixing state, and size distribution was utilized by these parameterizations should be included in that description.*

- In the revised paper, we clarified as follows: “Moreover, the disturbances of aerosols on climate systems via direct, indirect, and albedo effects are considered in ModelE2. Size-dependent optical parameters of aerosols are calculated by the Mie scattering theory. The first AIE is estimated by the prognostic treatment of cloud droplet number concentration, which is a function of contact nucleation, auto-conversion, and immersion freezing (Menon et al., 2008; Menon et al., 2010). The AAE of BC is considered by estimating the decline of surface albedo as a function of aerosol concentrations at the top layer of snow or ice (Koch and Hansen, 2005). BC content in snow is determined by measurement-based average scavenging ratios (Hansen and Nazarenko, 2004). More detailed descriptions of ModelE2 can be found in Schmidt et al. (2014). It has been extensively evaluated for meteorological and chemical variables against observations, reanalysis products and other models, and widely used for studies of climate systems, atmospheric components, and their interactions (Schmidt et al., 2014).” (Lines 137-148).

*Lines 183-185: exactly what is meant by dynamically allocated needs to be described in a few more sentences (note timescale-to-effect issue noted above).*

- In the revised paper, we clarified as follows: “In ModelE2-YIBs, fire emissions are affected by environmental factors following above parameterizations. In turn, the radiative effects of fire-emitted aerosols feed back to affect those climatic and ecological factors.” (Lines 224-226).

*Lines 204-205: How is it possible that the model underestimates boreal fire emissions relative to GFED? The impression I had from section 2.2 was that the fire location procedure redistributes the GFED emitted mass, so should be equal. A few sentences explaining possible causes for the discrepancy are needed.*

- We do not apply the GFED emissions to constrain simulated fire emissions grid by grid. "For each species, simulated gridded emissions are grouped by dominant PFT and compared to annual total emissions from GFED4.1s over the same grids. 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." (Lines 215-219).

  Although the simulated global total fire emissions match GFED, the spatial pattern is derived by the fire parameterization and as a result may show regional biases (i.e. some regions overestimate but others underestimate fire emissions).

*Line 258: I question the AAE impact a bit: there is a question of the duration of time over which the layer of deposited particles exists at the surface before processes like additional precipitation remove the layer or cover it over. Deposited forest fire smoke does not last forever-how do the authors deal with, e.g., the deposited particles being removed by subsequent precipitation, covering by new vegetation, etc.? If the smoke layer deposition is assumed to never change post-deposition, then the albedo change impacts may be overestimated.*

- In the revised paper, we clarified how the AAE is calculated in the model: "The AAE of BC is considered by estimating the decline of surface albedo as a function of aerosol concentrations at the top layer of snow or ice (Koch and Hansen, 2005). BC content in snow is determined by measurement-based average scavenging ratios (Hansen and Nazarenko, 2004). " (Lines 142-145).

  We performed long-term climatological simulations in this study. It means that the BC deposition on snow/ice is estimated as an average amount that redistribute the fire-season total to each model day. It is different from the real world that fires occur with daily variability so that BC deposition is very high shortly after an event but will be removed some time later. The same strategy is applied for all climate simulations of fire aerosol radiative effects listed in Table 2. The day-to-day variations of fire BC should be estimated with chemical transport models driven with observed fire emissions and meteorology, instead of the long-term mean SST/SIC prescribed as boundary conditions for climate models.

*Lines 268 to 273: Please explain the reasoning here in more detail; this is unclear. The methodology apparently assumes an instantaneous change in the LAI as a result of the fires (if this is not the case, please describe timescale in the 2.2 methodology). However, a more likely outcome of drought is that the LAI in the short term will remain unchanged, while the foliage will become drier. The issue of time scale of the change in LAI to take place needs to be discussed in more detail, as well as the underlying foundation for the LAI dependence. I think this is a potential confounding factor to the work.*

- In the model, LAI is calculated daily based on the accumulation of carbon uptake. The drought conditions will affect plant photosynthesis instantaneously and exert impacts on LAI in the coming days. We clarified as follows: "Dynamic daily leaf area index (LAI) is estimated based on carbon allocation and prognostic phenology which is dependent on temperature and drought conditions." (Lines 154-156) and "In the coupled model, ModelE2 provides meteorological drivers to YIBs, which feeds back to alter land surface water and energy fluxes through changes in stomatal conductance, surface albedo, and LAI." (Lines 160-162).

*Lines 278-279: This is not correct: the number of factors contributing does not take into account their magnitude or the potential for non-linear interactions. Relative magnitudes could potentially be compared in maps, but the number of factors is meaningless, when the magnitude of each of their effects may be quite different. Figure 5d should be removed, in favour of, e.g., maps of relative contributions of the four factors to the total change as an additional multi-panel figure in the SI.*

- We acknowledge that there are non-linear interactions among factors, and that is a key reason why the relative contributions are difficult to quantify. For a climate model, we cannot isolate the impact of individual factors on fires without the indirect perturbations by other factors. For example, if we fix lightning ignition, then the rainfall rate will be changed, and then LAI will be affected. In this case, we cannot isolate the direct impacts of lightning on fire emissions from the associated changes caused by precipitation and LAI.

Instead of showing relative changes/contributions, we present the number of factors that showing the same direction of impacts on fire emissions. We emphasize that the responses of different environmental factors to fire aerosol radiative effects are spatially heterogeneous and may counteract each other: "several complex feedbacks that may exert offsetting effects" (Lines 410-412). As a result, Figure 5d is a key result distinguishing our study from previous ones which usually ignore the feedbacks from multiple factors especially those in ecosystems.

***Minor issues:***

*Line 26: the change in average land precipitation should be stated as a relative change in percent as well as the absolute value, to give the reader an idea of the significance of a 0.018 mm month$^{-1}$ change in the average.*

- We are sorry for the incorrect number. It should be 0.180 mm month$^{-1}$ as we shown in Table 1 and Fig. 3b. In the revised paper, we modified as follows: "Land precipitation decreases by <u>0.180 ± 0.966 mm month$^{-1}$ (1.78 ± 9.56%)</u> mainly due to the inhibition in central Africa by AIE." (Lines 26-28)

*Lines 30-31 vs lines 28 to 29: the last and second to last sentence in the abstract apparently contradict each other, the 2nd to last line implies less fires due to feedbacks, the last implies more fires. Please clarify this in the abstract.*

- In the revised paper, we modified as follows: "The fire aerosol radiative effects may cause larger perturbations to climate systems with likely more fires under global warming." (Lines 30-32).

*Line 71, line 88: IPCC should be capitalized.*

- Corrected.

*Line 105: Reference is population dataset, but how was it used to get human-induced ignition approximation? Needs a couple of sentences describing the methodology used.*

- In the revised paper, we modified as follows: "To compute anthropogenic ignition and suppression effects (see section 2.3), we use a downscaled population density dataset from Gao (2017, 2020)." (Lines 107-108).

*Line 123: "dynamically" should be "that dynamically"*

- Corrected.

*Line 141: units on precipitation are apparently inverted?*

- Corrected.

*Line 153: "ignitions determine" should be "ignition determines"*

- Corrected.

*Line 170: the abbreviation PFT is used here without definition, also SIC line 191.*

- In the revised paper, we added the definitions of PFT (Line 150) and SIC (Line 109).

*Lines 193-194: "are analyzed." I wasn't sure whether you meant "were calculated" or if there was some additional analysis being done.*

- In the revised paper, we modified as follows: "Each simulation is integrated for 25 years with the first 5 years spinning up and the last 20-year averaged." (Lines 253-254).

*Lines 205-206: Also, forest fires release large amounts of NH3 and NOx (the latter makes nitric acid, which can combine with the NH3 to make particle nitrate). Also some SO2 which can react to make sulphate. Also some minerals/dust-like material. How are these included (are they included?) in the model?*

- In the revised paper, we clarified as follows:

  (1) "Here, EF is the PFT-specific emission factor of an air pollutant such as black carbon (BC), organic carbon (OC), $NO_x$, $NH_3$, $SO_2$, CO, Alkenes and Paraffin"

(Lines 214-215).

(2) "The fire emissions include both primary aerosols and trace gases, the latter of which react with other species to form the secondary aerosols." (Lines 227-229).

(3) "The aerosol microphysical scheme is based on the quadrature method of moments, which incorporates nucleation, gas-particle mass transfer, new particle formation, particle emissions, aerosol phase chemistry, condensational growth, and coagulation (Bauer et al., 2008)." (Lines 124-127).

(4) "In ModelE2, gases can be converted to aerosols through chemical reactions, while aerosols affect photolysis and provide reaction surface for gases. For example, the formation of sulfate aerosols is driven by modeled oxidants (Bell et al., 2005), and the chemical production of nitrate aerosols is dependent on nitric acid and gaseous ammonia (Bauer et al., 2007)." (Lines 134-137).

(5) The fire-emitted minerals/dust-like material is not implemented in the current model.

*Line 213: "enhance surface aerosols": What about aerosols aloft? Wondering about the model's vertical distribution algorithm for smoke emissions.*

➢ In the revised paper, we clarified as follows: "We consider only the fire emissions at surface due to the large uncertainties in depicting fire plume height (Sofiev et al., 2012; Ke et al., 2021). 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 226-230).

*Line 226: Maybe this should read "net shortwave radiation reaching the surface"?*

➢ Corrected.

*Line 234: I found the result for Australia counter-intuitive: is the albedo of Australia starting off high due to high albedo land surface (not much snow and ice in Australia)? There may be an underlying assumption here of the smoke particle deposition not being* *disturbed, post-deposition. Lots of wind-blown dust in Australia; a surface layer of deposited smoke particles would be expected to get mixed with the local dust over time, reducing the impact of the AAE. Or is that sort of effect included in the model? Same question on line 258: how long would a surface layer of smoke particles last, given processes like erosion, new vegetation growth, mixing with wind-blown dust (potential for coagulation there), etc?*

- The statement of "AAE reduces surface albedo and increase shortwave radiation over … Australia" is a mistake and we removed "Australia" in the revised paper.
  In the revised paper, we added: "BC content in snow is determined by measurement-based average scavenging ratios (Hansen and Nazarenko, 2004)." (Lines 143-145).

*Lines 240 – 241: Change in shortwave is not balanced by change in longwave if I've understood the numbers correctly; rather, 1.23 W/m2 reduction in downward shortwave, and reduction of 0.83 W/m2 in upward longwave, which implies a net decrease in energy and cooling. Please comment on the significance of this level of change (e.g. with respect to the standard IPCC global average "contributions of different aspects of the radiative balance to global radiation budget"): how significant are the changes relative to global net radiative transfer.*

- Changes in surface shortwave flux ($-1.227 \pm 0.216$ W $m^{-2}$) is partly offset by the changes of longwave ($0.281 \pm 0.371$ W $m^{-2}$) and heat fluxes (sensible flux + latent flux, $0.826 \pm 0.311$W $m^{-2}$), leading to a negative changes in vertical heat flux below the surface.

*Figure 5d: Note that in Figure 5, the caption reads, "Only grids with fire OC larger than 1x10-21 kg $s^{-1}$ $m^{-2}$ are shown in (d)." Why choose this particular number, as opposed to, for example, showing the grids where $p<0.1$?*

- This threshold was chosen to be consistent with Fig. S1b, to better represent the spatial distribution of the feedback of each environmental factor to the fire emissions. We clarified as follows: "Only grids with fire-emitted OC larger than $1\times10^{-12}$ kg $s^{-1}$ $m^{-2}$ <u>(colored domain in Fig. S1b)</u> are shown in (d)."(Lines 638-640).