

## Authors' Response to Reviewers' Comments

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Title: Important role of stratospheric injection height for the distribution and radiative forcing of smoke aerosol from the 2019/2020 Australian wildfires

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*We would like to thank Pasquale Sellitto for his time and constructive comments that helped to improve the submission. We hope that we have responded satisfactorily to all his points.*

### Anonymous Referee #2 (RC2)

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#### General comment

The manuscript “Important role of stratospheric injection height for the distribution and radiative forcing of smoke aerosol from the 2019/2020 Australian wildfires”, Heinold et al., presents and discusses the aerosol spatiotemporal distribution and radiative forcing (RF) of ECHAM6.3-HAM2.3 model simulations of the pyro-convective paroxysmal phase of the record-breaking Australian bushfires in the fire season 2019/2020. The importance of representing pyro-convection and UTLS injection of smoke into models is discussed based on sensitivity analyses. The manuscript provides a new estimation of the RF and radiative heating of this Australian fire event, which is quite a hot topic. It certainly falls within the scopes of ACP and is very interesting for the aerosol/climate community, and is potentially suitable for publication. By the way, I have found that there are several aspects – some of them very important – that must be clarified before I can recommend publication of the manuscript. Thus, I kindly ask the Authors to reply to the following major and specific comments and to provide an amended version of the manuscript, that I would be glad to re-read when ready.

#### Major comments (#MC)

MC1) The temporal trend of the AOT obtained with ECHAM are contradictory with previously published observations. The trend shown in Fig. 2 (maximum AOT in January and then decreasing in February and March) is not consistent with, e.g., the stratospheric AOT from SAGE-III in Khaykin et al., 2020 (e.g. its Fig. 3) – paper which is cited in the present manuscript. In particular, Khaykin et al. show an increasing AOT from January to February, with a maximum in February. The Authors should discuss this marked difference. Is an increase in stratospheric-only AOT in February also present in ECHAM modelling (in Fig. 2 there's a total column AOT, I guess)? Is the possible coating of black carbon (BC) particles and evolving aerosol mixing state not well represented in ECHAM – which is quite a common feature in aerosol/climate models, as extensively discussed by Brown et al. (<https://www.nature.com/articles/s41467-020-20482-9>)?

*This is a central point of criticism throughout the entire review, but we have to strongly disagree with it. The argument that the temporal evolution of the smoke aerosol and*

*therefore the instantaneous radiative forcing estimates in the ECHAM-HAM simulations are supposedly misrepresented is incorrect.*

*In their figure caption of Figure 4, Khaykin et al. clearly state about the time lag between gases and aerosol particles that "The lagging increase of the aerosol mass is due to the fact that the OMPS-LP extinction retrieval saturates at extinction values above  $0.01 \text{ km}^{-1}$ . Profiles are, therefore, truncated below any altitude exceeding this value, which can lead to an underestimation of the early aerosol plume when it is at its thickest. This artifact, which explains the slower increase of aerosol mass than gases, persists until mid-February when the plume is sufficiently dispersed so that OMPS-LP extinction measurements no longer saturate." This clearly shows that extinction was too high to be not be measured in January and cannot be used for model comparison.*

*Peterson et al, 2021, which we now also include as a reference, provide emission strengths for the stratospheric smoke injection of 0.2-0.8 Tg for the pyroCb events on 29-31 December 2019 and 0.1-0.3 Tg for 4 January 2020. These values agree well with our simulation results. Secondary aerosol formation appears unlikely to be the explanation considering the required amount of smoke. However, it can be a source of model uncertainty, which is now also discussed in connection with the underestimation of modelled extinction profiles compared to lidar observations.*

*The study by Brown et al. (2020), mentioned by the reviewer, shows that aerosol-climate models (including ECHAM-HAM) may generally overestimate absorption by biomass burning aerosol due to an insufficient representation of the mixing state for fire aerosol. The revised version of the manuscript points out this uncertainty in more detail. For the extreme 2019/2020 wildfires, however, we can show in comparison with single scattering albedos (SSAs) derived from lidar measurements for this particular event that the assumptions about the optical properties of the smoke particles in the model are reasonable, with SSA values of 0.79–0.8 (550 nm) in the lidar inversions and slightly more reflecting values of 0.82–0.85 in the model.*

MC2) The Authors obtain a relatively large and positive RF at top of atmosphere (TOA), which is basically in contradiction with all estimations available at the moment (Khaykin et al., Yu et al., Hirsch and Koren, papers that are cited in the manuscript). The observational (Hirsch and Koren) and hybrid observational/modelling (Khaykin et al.) estimations agree on a relatively large negative TOA RF. The Authors interpret this disagreement with respect to these previous estimations as the result of all-sky calculations and the high surface reflectivity in the present manuscript, which, I agree, can partially explain that. By the way, the RF is very sensitive to optical properties of the aerosol layer (in particular, the absorption properties of the layer and its angular distribution of scattering, see discussion in SC56 and other SCs). In addition, also Yu et al. obtain a negative TOA RF but at all-sky conditions. This should be discussed more thoroughly in the text (see suggestions in several of the following specific comments) and the different statements supporting a positive RF must be smoothed a bit.

*The review of the above-mentioned papers shows that there is no reliable all-sky estimate of the aerosol radiative forcing for this event to be compared to our model results. They mostly refer to clear-sky conditions and predominantly to open ocean: (1) Khaykin et al. (2020) simply derive an all-sky forcing (RF) by assuming that "all-sky RF [are] reduced to about 50% of the clear-sky RF", which a priori excludes the possibility of a change in sign. (2) Hirsch and Koren present a clear-sky value from the CERES data that is at least significant at 20–60°S, but otherwise is not significant in the deviation from the CERES mean. However, they also find a*

*possibly significant positive forcing above clouds and Antarctica, but the low accuracy of the CERES data in these regions does not allow a more precise statement. (3) Yu et al. (2021) consider only clear-sky conditions but provide an effective forcing for shortwave and longwave radiation in contrast to the instantaneous forcing in this study. The atmospheric adjustments included in the effective forcing obviously reduce the instantaneous forcing considerably. Nevertheless, the instantaneous forcing we present is important as a measure of the energy added instantaneously to the atmosphere (here the stratosphere). Thermodynamical and dynamical adjustments are currently under research and will be treated more extensively in a future study.*

*Regarding the optical properties of the fire aerosol, the comparison with the lidar-based inversions of single scatter values was strengthened, and the asymmetry parameter was included in the discussion (see the reply to the specific comments). This analysis, however, further supports that the optical properties of the fire aerosol are reasonably realistic for this case, and thus the positive instantaneous solar radiative forcing at TOA.*

### **Specific comments (#SC)**

SC1) P1 L24-25: “Global...wildfires”: I would not call this “uncertainties” but rather “incomplete representation”, like said in the following line, or something similar *“show significant uncertainties” was replaced by “lack adequate descriptions of”.*

SC2) P1 L27: Its more “observation-based input to the simulations” than “observation based approach”

*Agreed. Changed accordingly.*

SC3) P1 L28: please add “Based on our simulations,...” before “The 2019-2020 Australian fires caused...”

*Done.*

SC4) P1 L32: “While at surface,...” is an awkward way to start a sentence, please rephrase.

*Done.*

SC5) P1 L34: “deep wildfire plumes...”, “deep” is a bit too generic here: do you mean “with high altitude injection” or just “extreme”?

*Exactly, high-altitude plumes were meant here. The previous wording was obviously inspired by deep pyro convection.*

SC6) P2 L5: “life”: do you mean “wildlife”?

*No, we had all life in mind here, not just wildlife but also people. Therefore, we would prefer to keep just “life”.*

SC7) P2 L6-7: “In addition...whether”, this is an awkward sentence, please rephrase

*Agreed. The sentence was rephrased.*

SC8) P2 L14-17: please add SAGE-III and TROPOMI observations (as shown by Khaykin et al., 2020) into the discussion of evidences of the fire with satellites

*SAGE-III and TROPOMI were added to list of satellite detections.*

SC9) P2 L18-19: The first estimation of the radiative forcing of Australian fires was provided by Khaykin et al., 2020, with hybrid observations/modelling approaches. Please mention this manuscript and the method.

*The method and the estimates of the radiative forcing of the Australian fire aerosol by Khaykin et al. (2020) were added as requested.*

SC10) P3 L1-3: please break this very long sentence

*Done.*

SC11) P3 L5-6: Radiative-heating-induced self-rising can occur in fires but this is not the norm, so please smooth this sentence

*Agreed, the statement has been softened.*

SC12) P3 L9: “effects”: Please specify which specific effects

*In order to be more specific, the sentence was adapted and now reads: “Such extreme wildfires and associated deep pyroconvection, for which injection of biomass burning smoke into the stratosphere has been observed, can have similar effects as volcanic eruptions in terms of stratospheric aerosol injection and radiative impact.”*

SC13) P3 L10-11 “which is considered to be the strongest warming short-lived radiative forcing agent.”: please add a reference for this statement

*In an earlier version of the manuscript, this referred to particulate climate forcers. As the current formulation is more general, we have corrected the statement and added references.*

SC14) P3 L11-12: “In addition...emitted”: also precursors of secondary organic aerosols can be emitted, please mention

*Now also precursors of secondary organic aerosol are mentioned.*

SC15) P3 L12: “...radiative properties...”: you might mean “optical properties”

*“radiative properties” replaced “optical properties”.*

SC16) P3 L13-14: “...as well...altitude.”: not clear, what do you mean?

*As explained by Ban-Weiss et al. (2012), absorbing aerosol like black carbon (BC) in lower atmospheric layers heats the surface due to diabatic heating. If located at higher altitudes, instead, it has a cooling effect, as the increased solar absorption is compensated by stronger outgoing long-wave radiation. Furthermore, Ban-Weiss et al. show that BC at high altitudes reduces high-altitude cloud cover, which also results in a surface cooling.*

SC17) P3 L16: “...the recent accumulation of extreme wildfires...”: you mean “aggregated effect”?

*What was meant is “the recent series of extreme wildfires”. The wording was changed accordingly.*

SC18) P3 L21: please remove “more”

*We believe “most” was meant, which we removed.*

SC19) P3 L27: not sure that “to capture” is the right verb here

*The verb “capture was replaced by “address”.*

SC20) P4 L18: “height profiles”: do you mean “vertical profiles”?

*Replaced.*

SC21) P5 L10-12: which altitudes for these vertical layers in the model?

*Note that these are not fixed injection heights. The PBL height is largely driven by the surface fluxes and associated vertical mixing. It therefore varies strongly in time and space. For this reason, no explicit altitude values can be given here.*

SC22) P5 L14: “47 levels”: which approximate vertical resolution?

*The vertical resolution is variable across, but approximate values are now given for relevant altitudes: “In the vertical, the model is set up with 47 levels with increasing layer thickness from the ground to 0.01 hPa (~80 km). The vertical resolution ranges from approximately 70 m at surface to 500 m at 2.5 km and 1100 m at 15 km height and coarsens accordingly thereabove.”*

SC23) P5 L18-19: “AOT and vertical profiles of extinction”: This looks redundant as the AOT is just the vertical integration of the aerosol extinction.

*This may have looked redundant in the way it was written. However, the aerosol extinction profiles are calculated by an online lidar simulator that was implemented especially for such comparisons with CALIOP and ground-based lidar measurements. We have expanded the description.*

SC24) P5 L28: “Since no direct information was available on the actual pyroconvective injection heights...”: This is not completely true as Khaykin et al. give an upper bound for the injection altitude at circa 17 km using CALIOP, and it is also shown at approximately this altitude for Hirsch and Koren, 2021 (as you mention later in the text). Please correct the sentence.

*This statement is meant literally. There was in fact no direct information at or above the wildfire sites, which can be typically used in models. There was no reliable radiative power information from satellites (otherwise it would have been included in the GFAS data) or other, possibly in-situ observations due to cloud cover (and heavy smoke). All approximate values for the injection heights in the studies mentioned are of course reasonable but based on satellite observations at some distance from the Australian continent or model assumptions from which the emission height was inferred. This is what we have also tested within the sensitivity study.*

SC25) P5 L37-38: The results of Hirsch and Koren (2021), seems more to show that smoke injection is injected at altitudes >16 km. Why do you say “14 km”?

*Hirsch and Koren (2021, suppl.) found “smoke fragments [...] located on the lower stratosphere below 17 km [...] during the fire emissions.”*

*We find that the vertical spread in the CALIOP imagery justifies the “14 km” assumption, in particular, since due to the vertical resolution in the model in the tropopause region a larger altitude range is directly affected. We point this out more strongly in the text now.*

SC26) P5 L38-39: “In addition...the original biomass burning injection...”: The original injection is what is described above (P5 L10-12)? Please clarify in the text.

*Yes. The details of the original fire injection are given here again to make this clear.*

SC27) Table 1: in the NoEmiss lines, when you say “1 April” you mean “4 January”?  
*It is in fact 4 January. Many thanks for spotting this obvious mistake.*

SC28) More in general on the NoEmiss scenario: In the NoEmiss scenario, how are the previous emissions from Australian fires (i.e. the Australian fire season prior to 29/12/19) considered?

*All other days except the mentioned pyroCb days are treated as in the original configuration. This was added to the text for clarification.*

SC29) P6 L11: please suppress “significantly”  
*Deleted.*

SC30) P6 L12: The long-term mean (“which implies...”) is not shown in Fig. 1b, so please explain how it is calculated and rephrase the sentence?

*Please also note our response to reviewer #1. Now, Fig. 1b is already referred to in the introduction to show the hemispheric spread of Australian Smoke. Here, only the AVHRR AOT mean values for January are presented. The calculation is described in the Methods section.*

SC31) P6 L13: “AOT”: please mention wavelength  
*Done.*

SC32) P6 L14: “compared...half a year”: this is not clear at all, please rephrase  
*Deleted.*

SC33) P6 L15: “...relative to 2019”: You mean wrt the monthly mean AOT for January 2019? Please clarify in the text  
*Done.*

SC34) Figure 2: wrt MC1, the trend (maximum AOT in January and then decreasing) is not consistent with observations, e.g. the SAOD from SAGE-III in Khaykin et al., 2020 (Fig. 3). Please explain why  
*Again, note that the peak AOT in Khaykin et al. (2020) is actually likely delayed. The authors point to saturation effects in the satellite retrievals in the caption of their Fig. 4 as an explanation.*

SC35) P7 L8: “The emissions...are reproduced...”: The emissions are not “reproduced” by ECHAM but are “an input” to ECHAM: please rephrase  
*We agree. The sentence was rephrased to read now: “The dispersal of this smoke plume is reproduced using the global aerosol-climate model ECHAM6.3-HAM2.3 with the pyroconvective injection heights prescribed.”*

SC36) P7 L 10: “...provide an insight...due to wildfire smoke...”: If NoEmis has only the smoke emissions of 29-31 December and 4 January switched off, then this comparison does not provide “the AOT distribution due to wildfire smoke” but rather “the AOT distribution due to pyro-convective events of 29-31/12 and 04/01”. Please verify and possibly correct.

*Correct, this was well spotted and was corrected in the manuscript.*

SC37) P7 L16: "AOT differences": Differences with respect to what?

*This again refers to the difference between TP+1 and NoEmiss simulation results, which is now included in the text.*

SC38) P8 L15: "...is clearly better": Is it "clearly" better? Not to my eyes: the comparisons with different injection altitudes looks quite similar to me. This is not so surprising because the effect of the fires on the column AOT is not as strong as the one at selected UTLS altitudes (as visible in Fig. 4). Please, based on that, smooth these statements, and reconsider this discussion.

*The comparison of modeled and observed AOT in Fig. 3 may give this impression at first glance. However, considering the actual change in modeled AOT against the background of the in general very low levels of AOT at the Southern Hemisphere sites, the UTLS injection heights in fact lead to a substantial improvement. Now, we explicitly point out this fact.*

SC39) P8 L16-17: please suppress "using...above" (this is already clear from scenarios descriptions above, so is redundant)

*Deleted.*

SC40) P8 L18-19: "...indicating that the modeled effect...stratospheric smoke": This statement is not true because the solar absorption depends not only on the aerosol load but also on the optical properties of the aerosols - and then, for your estimations, on the assumptions made in the model on composition and atmospheric evolution of the smoke plume. Please smooth the statement.

*This statement was modified in the manuscript.*

SC41) P8 L19: "is larger" --> "is slightly larger"

*We disagree. The bias of the BASE case is on average about 30% larger than for the other cases using UTLS smoke injection. This quantitative information was added to the discussion.*

SC42) P8 L20: "...correlation is also lower": From Tab. 2 it looks like BASE R is quite comparable wrt TP(+1) and the others setups.

*Replaced by "slightly lower".*

SC43) P8 L25-26: "which is also consistent with the CALIPSO satellite lidar observations": Which CALIPSO observations? (they're neither in Fig. 4 nor 5)

*The CALIOP comparison was deleted at this point.*

SC44) P8 L28: "reflect" is not a good choice here as a term, as it also has an optical meaning: please change term

*Replaced by "evident".*

SC45) P8 L28: "These remarkable values...": what do you mean?

*As now written, it is meant: "This remarkable wildfire smoke layering...shown in Fig. 4..."*

SC46) P9 L9-10: "again...apparent": You refer to Fig. 4 I guess: please mention this in the text.

*A reference to Fig. 4 was added.*

SC47) P9 L12-13: “The model results...smoke layer”: This is not peculiar of the simulations but only empirically visible in Fig. 4 and 5 (for model as well as in the lidar observations): please rephrase.

*Agreed. We included a reference to the discussion of Figure 6 here.*

SC48) Figure 4: Please spell "coeff." and not "cf."; please use “km<sup>-1</sup>” as aerosol extinction units

*Corrected.*

SC49) Figure 5: here as well, please use km<sup>-1</sup> as aerosol extinction units. Also, for the sake of visual clarity of the comparison, why not suppressing the pressure vertical axis and just put the height on the left, for your simulations results?

*The Figure was revised accordingly.*

SC50) P10 L10: “For other model scenarios”: Please specify which scenarios.

*Thank you for pointing out this inaccuracy. Meant are of course only model scenarios with prescribed pyroCb smoke injection. This we added to the sentence.*

SC51): Your estimation of the heating rate: The heating rate is very sensitive to the aerosol optical properties, please mention this in the text as a reason for large uncertainties in your estimations. Also: are these shortwave-only or shortwave+longwave heating rates?

*Again, all estimates of smoke radiative effects in this study are for the solar wavelength band, which was made clear here also for the heating rates. The uncertainties due to the aerosol optical properties in the model are discussed in Sec. 3.2.*

SC52) Figure 6: are these “monthly averages” for January? Please mention this in the caption.

*Done.*

SC53) Figure 7: Would it be possible to have a altitude vertical axis as well?

*Figure 7 was completely revised. It now has a height axis. In addition, the geographical area was limited to the longitudes between Australia and Argentina (145°E - 70°W) and the tropics/subtropics (30°S - 60°S) were excluded as suggested by Reviewer #1. Furthermore, an averaging error in the previous version was corrected.*

SC54) P12 L6: “greenhouse forcing”: You mean “greenhouse gases forcing”? Black carbon also can produce greenhouse effect (but it's particle, so better to be more specific)

*Correct, what is meant is the greenhouse effect caused by anthropogenic aerosols and gases, as it is now written.*

SC55) Your RF estimations: same question as for the heating rates: are these estimation for SW-only or LW+SW?

*All estimates of smoke aerosol forcing in this study are for the shortwave radiation. This was made clear in several places in the text.*

SC56) With reference to MC2: the RF of aerosols depends very strongly on the optical properties of the aerosol layer, which in turns, and this is very important for the complex



smoke emissions by fires, depend on the atmospheric evolution of the plumes. In particular, the aerosol RF depends quite strongly on both absorption properties (summarised by SSA) and the angular distribution of scattering (the phase function, summarised by the asymmetry parameter). Examples of such variability of RF on these two integral optical parameters (for volcanic aerosols, but it applies more in general) can be found here;- Sellitto et al., 2020 (<https://www.nature.com/articles/s41598-020-71635-1>), see their Fig. 5 - Kloss et al., 2021 (<https://acp.copernicus.org/articles/21/535/2021/>), see their Fig. 9. The situation can be even more complex for fire emissions, where the optical properties of the emitted and secondary formed aerosols, as well as their evolution in a complex environment of high humidity, many gaseous emission and locally high temperatures. Thus, your estimation depends strongly on the somewhat arbitrary assumptions of your simulations. This must be critically discussed in the text.

*The assumptions in this study are not arbitrary. The ECHAM-HAM model is a widely used community model that has been thoroughly evaluated for aerosol processes and aerosol-climate interactions. The only assumption that differs from the default configuration is the adjustment of the smoke injection heights for the 4 pyroCb days. This is also not arbitrary but based on ground-based and satellite observations and, moreover, is shown to be reasonably realistic by the evaluation performed.*

*In addition, the comparison of single scattering values in the model with the lidar-based inversions as well as the newly included mention of the asymmetry parameter shows that the particle optical properties are adequate for this fire event and thus the positive instantaneous solar radiative forcing at TOA.*

SC57) P12 L13-17: Yu et al. (2021) also obtain a slightly negative TOA RF using a model and theirs are all-sky estimations. Please notice that their estimations are SW+LW (personal communication). As a matter of fact, your RF being positive is quite in contradiction with all previous observations, simulations and hybrid estimations of the TOA RF for Australian fires 2019/20, clear- and all-sky, and this must be mentioned in the text.

*Yu et al. (2021) only look at clear-sky conditions. However, in contrast to the instantaneous forcing estimated presented in this study, they provide an effective forcing for shortwave and longwave radiation. The atmospheric adjustments included in the effective forcing reduces the instantaneous forcing, especially due to the high altitude of the smoke aerosol layer, resulting in an overall negative TOA forcing.*

*Note that our clear-sky instantaneous TOA forcing actually agrees well with the clear-sky effective TOA forcing from Yu et al. (2021) for solar radiation, for which the rapid adjustments are of minor importance.*

SC58) P12 L20-21: This is another reference that can be helpful in comparing your results with volcanic eruptions: <https://www.nature.com/articles/ncomms8692?proof=t>  
*Many thanks for this additional reference.*

SC59) P12 L22-24: No mention to the stratospheric vortex driven by rapid vertical transport + plume heating seen for this fire event? (Khaykin et al., 2020; Kablick et al., 2020). *This was implicitly meant by the “responses in atmospheric dynamics”. However, this is now explicitly mentioned: “Khaykin et al. (2020) actually showed that a self-sustained 1000-km anticyclonic vortex formed as a result, which traveled through the stratosphere for weeks, accompanied by a local ozone reduction”.*

SC60) P13 L7-8: “uncertainties in AOT; in particular...single scattering albedo...”: This should be rephrased: the AOT representation and single scattering albedo are only in part inter-dependent. In addition, the angular scattering properties of the aerosol layer is also (or, at some conditions, even more) important for RF estimations (see Sellitto et al., 2020; Kloss et al. 2021, mentioned at SC56), and this should be mentioned.

*This sentence now includes the asymmetry parameter as a source of uncertainty, which is additionally addressed later in this section. See also the response below.*

SC61) P13 L9: “SSA lies between 0.82-0.85...”. Single scattering albedo (and asymmetry parameter of soot aerosols, see SC60) may be significantly affected by their mixing states, and coating of BC. SSA can be significantly larger (up to ~0.95 at 550 nm) if BC is coated by aqueous secondary aerosols (organic or sulphate) - e.g.

<https://www.nature.com/articles/s41467-020-20482-9> . Also, and importantly, it looks like smoke aerosols are too absorbing in models due to a generally incomplete representation of the aerosol mixing state for biomass burning aerosols:

<https://www.nature.com/articles/s41467-020-20482-9> . This must be mentioned and discussed in the text, as this might be a large source of uncertainties for your RF estimations (as well as heating rates estimations and, even more at the basis, AOT fields).

*We are aware of this study on the apparently widespread overestimation of absorption of biomass burning aerosol in aerosol-climate models.*

*The revised version of the manuscript now discusses this point in more detail, including a reference to the study of Brown et al. (2020). In addition, the comparison with current lidar-based derivations of SSA values for other intense fire events and for this one in particular was made more concrete. These values, however, support our previous conclusion that smoke absorption is even slightly underestimated in the model for this extreme wildfire case.*

*Regarding the asymmetry parameter, it is difficult to make an evaluation because the exact morphology of the smoke particles is not known. For the smoke particles, only the asymmetry parameter of the fine mode fraction is relevant here. In our model, the asymmetry parameter for the Australian smoke is about 0.6 at 550 nm, which however is in good agreement with typical values for wildfire aerosol in the literature (see e.g., Reid et al., ACP, 2005).*

*In the case of the volcanic ash, mentioned by the Reviewer, the asymmetry factor is even more uncertain due to the more irregular particle shapes (depending on the eruption type) and the uncertainties in radiative forcing may not be directly comparable.*

SC62) P13 L11-12: “For the 2019-2020 Australian fires, first inversion results even point towards SSA values just below 0.8, as calculated using the method of Veselovskii et al. (2002).”: more details are called here.

*The sentence was rephrased to include more details as well as a reference for the lidar-derived SSA value.*

SC63) P13 L14-15: “Thus, together with the low bias of the modeled smoke AOT, we argue that our results illustrate a conservative estimate for the positive TOA forcing of this event.”: This statement is definitely too strong, due to the large uncertainties discussed in my previous comments and to the fact that all previous RF estimations (Khaykin et al., Hirsch and Koren, Yu et al.) indicate rather a negative RF at TOA.

*Based on the revised discussion on the uncertainty factors of the model estimates of the radiative forcing for the Australian smoke, we see us actually more confirmed in our statement (see the response to SC61). It should also be emphasized again that this study, unlike those*

*mentioned, provides an estimate of the radiative forcing of smoke for all-sky conditions as well as for snow and ice-covered areas over Antarctica, not only predominantly over open ocean. The assumption for all-sky conditions in Khaykin et al. is an approximation rather than a modeled estimate (“all-sky RF reduced to about 50% of the clear-sky RF”). Yu et al. consider only clear-sky conditions. And Hirsch and Koren find that “the SW all-sky [...] deviations from the [CERES] mean are not consistent throughout the whole period”.*

SC64) Table 3. As already said for the AOT trend, in Khaykin et al. the strongest RF is in February and here is in January. As already mentioned in previous comments, this might be linked to an insufficient representation of secondary aerosols formation in your model and mixing state. Please mention and comment in the text.

*As already stated in reply to comments MC2 and SC56, this comment originates from overlooking the saturation effect in the satellite measurements mentioned by Khaykin in the figure caption of Fig. 4, which explains the delayed aerosol maximum in the observations. Nevertheless, it is possible that in this study the model underestimated the secondary aerosol formation, however, this would not have such large extent. We mention this as a further potential source of underestimated extinction in the revised manuscript.*

SC65) P14 L15: “While this is appropriate...pyroCb clouds”: Thus, why not using the satellite observations of the plumes themselves as proposed by Kloss et al. (2021, Fig. A1-2), see also SC56?

*It is certainly a good idea to further explore the potential of satellite observation for initializing the injection heights of vegetation fires. However, bush or forest fires are more complex than volcanic eruptions as described in the mentioned paper. They are spatially variable and can extend over larger areas, which is also difficult to predict precisely. For modelling applications, satellite-based radiative power has proven to be a good measure of the height of smoke injection into the atmosphere, which can be well attributed to individual fire sites. However, it has been shown that, just as in this case, heavy smoke and cloud development strongly hamper detection.*

SC66) P14 L20-21: “Consequently, aerosol-climate models underestimate the wildfire aerosol impacts on the energy balance, as the vertical location of the smoke relative to clouds is fundamental to its radiative impact.”: This might be true for pyro-convective fires while it is demonstrated that the biomass burning RF is overestimated in general, in models, again available at the following link:

<https://www.nature.com/articles/s41467-020-20482-9>

*Agreed. The statement was made more precise.*

SC67) P14 L29: please put references in chronological order.

*Done.*