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 the anonymous reviewer for his/her time and constructive comments, and hope that we have responded satisfactorily to all the points raised.

Anonymous Referee #1 (RC1)

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General comment

The paper presents simulations with an aerosol climate model on forest fire smoke in the stratosphere using lidar observations and the satellite based GFAS inventory. It includes several sensitivity studies on injection height since a pyro-Cb module is still not available in the global model community. The paper demonstrates the importance of wild fires for stratospheric radiative properties and is suitable for ACP after revision since it might be a valuable contribution to a hot topic in atmospheric research.

Major comments (#MC)

At several places clarifications are needed, including missing definitions of acronyms.
Further explanations were added throughout the revised manuscript (see also the responses to the specific comments), and the acronyms and abbreviations are now all explained.

Figure 7 shows large discrepancies between model results and CALIOP satellite observations concerning the vertical distribution of aerosol extinction. The figure poorly displays the lower stratosphere as the region of interest because of selection of an unphysical linear pressure coordinate. Here log(p) or altitude should be used as in the other figures. As it is, the figure gives the impression that aerosol extinction calculated by the model is severely overestimated almost everywhere, despite averaging, in contrast to the text. The difference is larger than the value mentioned in section 2.3. There appears to be something inconsistent to the results presented in the other parts of section 3.1. It might be worth, to exclude the tropics here and/or look also for other satellite data. For example, OSIRIS sees extinction peaks at 12 and 18km for January to March 2020 averaged over the southern hemisphere.
A height axis is indeed better suited to show the relevant atmospheric altitude range. It was also found that the previous figure was incorrect due to an averaging error of the CALIOP data. This explains the impression that there was inconsistency with the other parts in Section 2.3. As suggested, we also excluded the tropics/subtropics (30°S – 60°S) and limited the longitude range to the area between Australia and Argentina (145°E – 70°W), which further improved the comparison. We have revised the figure and also added additional information to the text. Especially worth mentioning is the interesting fact that the January-

March 2020 period (brown line in Fig. 7) has almost an order of magnitude more aerosol retrievals in the UTLS than the averaging periods before. This is clearly a response to the Australian wildfires.

Figure 8 would be better with satellite observations for AOT included. There are datasets of several instruments available. At least refer to Fig.1 here and use similar color bars with the same units. It looks like that the model overestimates the perturbation at Antarctica. Concerning uncertainties, it should be also taken into account that in 2019 the stratosphere was perturbed by volcanic eruptions.

Figure 1b is shown here mainly to qualitatively illustrate the hemisphere-wide spread of the Australian smoke plume. As we also explain later in response to a specific comment, Fig. 1b and Fig. 8a are not directly comparable. Fig. 1b shows the anomaly from total long-term mean AOT. In the other hand, Fig. 8 is intended to show only the smoke AOT from the four pyroCb days.

The AVHRR instrument can only detect AOT over cloud-free, non-glint water surfaces. Therefore, there are no valid retrievals and subsequently daily means for each original 0.1°x0.1° pixel and day. For Fig. 1b, the original 0.1°x0.1° AVHRR data is compiled onto a grid with a spatial resolution of 1°x1° to account for sufficient samples in the temporal mean. 1°x1° pixels with less than 300 valid retrievals in January 2020 (i.e. approx. 10% of the potentially available 100 0.1°-pixels on each of the 31 days) are not considered in the spatial and temporal averaging for January 2020. This corresponds to a data coverage of approximately 10% of the 3100 potentially available retrievals for each 1°x1° grid cell (100 0.1°-pixels times 31 days).

In addition, we agree with the reviewer that other influencing factors, such as a stratospheric volcanic eruption, might have contributed to the AOT anomaly in Fig. 1b. This is another reason why a comparison with the model results in Fig. 8a, which only shows the AOT of the Australian smoke, is difficult. Note that no AVHRR observations are available over Antarctica, and even in January the instrument is rarely able to measure the AOT near 60°S. This information will be added in a brief manner in the figure caption and a more detailed explanation will be given in Section 2.3. The absence of robust mean observations is depicted as a white area in Fig. 1b.

Since the AVHRR instrument can only observe AOT over non-glint water surfaces, missing values over Antarctica, however, do not in any way imply a model overestimation, as shown by the comparison with AERONET AOTs in Fig. 3, which rather indicate an underestimation.

Specific comments (#SC)

Page 1, line 19: Is the amount of injected smoke from this study or from the literature?

This is the amount of stratospheric fire aerosol calculated by the model in this study. However, the value is also consistent with the estimates in the literature (Khaykin et al., 2020; Hirsch and Koren, 2021; Peterson et al., 2021), as we explain in detail in the Introduction (references are not allowed in the abstract).

Page 1, line 31: Also the global value should be provided in parentheses.

Done.

Page 1, line 32: Provide also the value for all-sky.

The value of +0.50 W m^{-2} in the line before is actually that for all-sky conditions.

Page 2, Figure 1 and line 10ff: Here only Fig 1a is mentioned, part 1b is mentioned first on page 6. More text is needed or the figure should be split. A definition for AOT is missing, including spellout and altitude range (Page 6 is too late). Why does AOT anomaly in Figure 1 differ from the one in Figure 8 by about a factor of 10?

Figure 1b and the significant increase in atmospheric opacity in the southern hemisphere shown by the AOT anomaly in the AVHRR imagery is now already mentioned in the Introduction. The abbreviation AOT is now explained at this point in the text and in the caption of Figure 1. Throughout the manuscript we refer to the total column AOT.

As already explained in response to the main comments, the discrepancies between Fig. 1b and Fig. 8a are due to the different parameters shown. Fig. 1b shows the anomaly from the long-term average of the total AOT of the AVHRR satellite instrument while Fig. 8a shows only the AOT due to the four pyroCb days calculated from two different model scenarios. Furthermore, discrepancies result from the sampling bias of the satellite, as described above, so that local values can be significantly larger than a total monthly mean if the number of sampled days is below the total number of days in the month. In addition, other influencing factors, such as a stratospheric volcanic eruption, might have contributed to the AOT anomaly in Fig. 1b. We point this out more clearly in the text.

Page 4, line 5: Why interpolation? The model output should be available at the time and the location of the measurements. Don't rely here on averages, especially not if the meteorology of the model is nudged to observations.

Model results are generally available in a discrete temporal and spatial distribution. In this study, for the global aerosol-climate model ECHAM-HAM, it is a 6-hourly model output on an approx. 1.875°x1.875° (180x180 km) latitude-longitude grid. Interpolation to the much higher temporally resolved but often irregular single-point observations from AERONET is mandatory. This is common practice.

Nudging ensures that the simulated weather patterns are close to reality. Only then are the model results directly comparable with the measurements.

Page 4, line 31: Vertical or horizontal resolution?

The text says averaging along the ground track of CALIPSO, which implies a horizontal resolution. The word 'horizontal' has been added to be clearer.

Page 4, line 36: Here something is missing. Which aerosol type? Which time? Model results need boundary conditions and cannot be a reference for observations.

Thank you for the questions. We have added the following to the manuscript:

'The CALIOP level 2 aerosol classification selection algorithm defines six aerosol types: clean marine, dust, polluted continental, clean continental, polluted dust, and smoke which is based on the extinction-to-backscatter ratio (i.e., lidar ratio). Comparison of the CALIOP backscatter with airborne measurements using a High Spectral Resolution Lidar (HSRL), conducted during the ORACLES campaign independently demonstrated the lack of detection of these aerosol types using the CALIOP lidar, and as such, have carried out the necessary steps to account for these biases as discussed in detail in Watson-Parris et al. 2018.'

As pointed out by the authors of Watson-Parris et al. (2018), the global model ECHAM-HAM was used as a basis of reference to which they compared the CALIOP products. Of course, like any model, this one has its own uncertainties and discrepancies, but this has no impact on the conclusions in general regarding the biases of the CALIOP retrievals in the middle and upper troposphere.

Page 5, line 12: Mention tropopause region and reason (Pyro-Cb) already here. Also it should be mentioned how many teragrams of smoke (carbon) were injected in each of the 4 events to enable comparisons with the values of other papers mentioned in the introduction (or the abstract?). More details on the relations between GFAS and estimated injected OC (organic carbon) and BC (black carbon) should be included (here or in an Appendix).

Thanks for the suggestion. The approach of the sensitivity experiments to inject the smoke in the tropopause region for pyroCb days is now briefly mentioned in Sect. 2.4.1. We also added the amount of smoke emitted in the model during these days.

Page 5, line 14: The 47 level version does not have a QBO and has problems with the "H2O tape recorder", i.e. the vertical transport. It is better to use L90. This should be mentioned as a possible reason for discrepancies. Is nudging applied everywhere (may cause numerical problems) or only in the troposphere and lowermost stratosphere?

The model runs were done in nudged mode to reproduce the meteorology as closely as possible. The logarithmic ground pressure is nudged, as well as the divergence and vorticity in all model layers, with relaxation times of 24, 48 and 6 hours respectively. We are aware that this leads to bias at different levels, but only nudging allows comparability with observational data.

Possibly also because of the nudging we could identify the wind patterns typical for the QBO in the long model run.

Thank you for this advice. We will gladly take up the suggestion of a simulation with L90 for future model runs. Here, however, a rerun of the experiments is out of the scope.

However, it is important to emphasize that the uncertainties in the study of the Australian forest fire aerosol are mainly in the representation of the stratospheric smoke injection as we show in this paper.

Page 6, line 2 or 4: Does this refer to the 8% mentioned on the previous page or in BASE?

The experiments with and without interactive aerosol-radiation interaction were performed for the BASE, TP+1, and TP1_8020 case scenarios. The text was updated accordingly.

Page 6, line 9ff: This paragraph might be better moved to the introduction. Fig. 1b is inconsistent to Fig. 2, please explain why. Or is this just a problem with the range of the colors in the figures?

As replied to an earlier comment, Figure 1d is now already described in the Introduction.

The figures Fig. 1b and Figs. 2a,d are not directly comparable. Figure 1b presents AOT observations by AVHRR for January 2020. January 2020 was the month, in which the wildfire plumes where thick close to source in Southeastern Australia. In the weeks after the event, the smoke aerosol spread over a larger area and also the amount of smoke aerosol decreased. Figure 1b was therefore meant to show the apparent short-term direct effect of the wildfires and was shown only for January on purpose. In contrast, Figs. 2a,d show the differences in mean AOT for January – March 2020 between the model scenarios BASE (2a) and TP+1 (2d) against NoEmiss, respectively. Therefore, they exclusively show the contribution of the smoke-only AOT for the case where no smoke injection by pyroconvection is prescribed in the model (Fig. 2a) or for smoke injection into the model layer above the tropopause on the AOT for pyroCb days 29-31 December 2019 and 4 January 2020 in southeastern Australia (Fig. 2d). The *satellite-based AOT anomaly in Fig- 1b results from the difference against a long-term mean. Hence, many different aspects can contribute to the observed anomaly, e.g. wildfires in the region also beyond the 4 pyroCb days, general offset in AOT as mentioned by the reviewer in a previous general comment. Furthermore, due to the scarce observational data, the January 2020 mean of observed AOT is not directly comparable to the modeled mean AOT. For example, the AVHRR instrument can only observe AOT over cloud-free non-glint water surfaces. Therefore, there aren't valid retrievals and subsequently provided daily means for each pixel on each day. For the map in Fig. 1b, we only consider pixels with at least 5 days of valid retrievals in January 2020 to calculate the mean. This information will be added in a brief manner in the figure caption and a more detailed explanation will be given in section 2.3. The uncertainty of a single AOT retrieval is 0.2, which is in particular critical for low AOT values. The uncertainty of the January 2020 mean for a single pixel is therefore difficult to quantify. Therefore, a quantitative comparison between model and AVHRR observations was on purpose not included in the paper.*

However, by averaging over larger areas or longer time periods this uncertainty should average out to some extent. The figure below shows the average AOT in January 2020 between 20-60°S in 15° zonal sectors from Australia westwards for both the ECHAM TP+1 simulation and the AVHRR observation. Note that here still areas with fewer valid satellite observations (more frequent in the south of the 40x15° boxes) are underrepresented in the areal mean. Further, the AOT in the figure does not correct for the different wavelengths (550nm in ECHAM, and 630 nm by AVHRR). The figure indicates the general underestimation of AOT by the model simulation that was also seen in the comparison to the AERONET observations (Fig. 3).

Page 7, Figure 2 and line 11: Are these values out of the range of the color bar? Please adjust the color bar to accommodate this.

Done.

Page 8, line 17: This scenario should be also in section 2.4.1, maybe in parentheses. Or refer at least to Table 1.

Here, the half-sentence explaining the scenario again was misleading. It is in fact the BASE case. This half-sentence was removed.

Page 9, line 1ff: Caption too short, spell out RMS, normalized against what average(s)?

The missing information was added to the figure caption accordingly.

Figures 4 and 5: Standard units for extinction are "km-1", please convert axes, also to be consistent with Figure 7. I suppose the authors mean 1e-06 with Mm.

The figures were revised accordingly.

Figure 5: Please adjust the heights of the panels. It would be also nice to have additional panels with consistent palettes where the lidar ratio is applied for conversion.

Figure 5 is primarily meant to give a qualitative impression of the layering of the Australian smoke plume. The quantitative comparison is already shown in Fig. 4. The height axes of the panels were unified and the color tables adjusted. However, the high temporal resolution of this lidar imagery does not allow a conversion to extinction, since a clean cloud-aerosol discrimination is not possible and thus no lidar ratio can be assumed.

Page 11, line 8: Add "(with interaction between radiation and dynamics)"

There seems to be a misunderstanding about the calculation of the radiative forcing. See also the reply to the comment on page 12, line 7.
The instantaneous forcing was actually calculated by a double call of the radiation scheme in the model, so that no dynamic influences are included in the aerosol radiative forcing and heating rates. These are in fact instantaneous estimates. Since we obviously missed to mention this, the following sentence was added to the model description: "The instantaneous direct radiative forcing from the modeled wildfire aerosol is calculated by double calling the radiation routine in ECHAM6.3-HAM2.3 in order to diagnose the radiative forcing avoiding any impact on the atmospheric conditions such as dynamics, moisture fields and clouds, but including the net solar and thermal fluxes at the bottom and top of the atmosphere."

Page 11, line 11ff and Figure 7: As shown, the agreement is poor (not "well"). The figure has to be improved as mentioned above and more explanation is needed. The disagreement cannot be explained by sampling issues alone (section 2.3). These results are in a strong contrast to Figure 4 where the model at least follows the observed vertical patterns.
See the reply to the corresponding main comment. Figure 7 was completely revised and additional information was added to the text.

Page 12, line 7 and later: Taking the difference of radiative fluxes from 2 simulations is not exactly "instantaneous radiative forcing" since convection or other non-radiative processes might be different. It is, however, an estimate.
We believe there is a misunderstanding regarding the calculation of the radiative forcing. Please note that here actually the instantaneous forcing was calculated by a double call of the radiation scheme. This means that there is no response from clouds, humidity, etc. Nevertheless, there may be negligible model deviations over the Australian area due to non-linear effects in the aerosol microphysics of the total particle population. However, we already explicitly refer to 'estimates' in the text, e.g. in the heading of Section 3.2 and therein, as well as in the Abstract and in the caption of Table 3. In addition, "estimates of solar radiative forcing" was added to caption of Fig. 8.

Page 12, line 22: Is this number local or some kind of average?
As mentioned earlier in the text, this is a monthly average for January 2020. The sentence was revised to be clear: "On the other hand, according to the model, the smoke-containing air layer itself experienced significant absorptive heating with maximum heating rates of 1.7 K day^{-1} on average in January 2020 for the TP+1 case."

Page 13, line 9: The particle SSA depends strongly on the partitioning between BC and OC. More information on this would be useful here, at least some typical number of the ratio with a range.
Fair point. Together with further edits on the uncertainties of the forcing estimates in response also to Reviewer #2 we have added: "A recent comprehensive analysis of aircraft data indicates that model parameterizations may overestimate absorption by biomass burning aerosol (Brown et al., 2021). In our model, at the height of maximum extinction of the smoke plume, the ratio of black to total carbon (BC/(BC+OC) mixing ratio) is approximately 0.05 – 0.08, corresponding to a particle SSA between 0.82–0.85 at 550 nm."

Page 13, line 14: This is in contradiction to the large high bias in Figure 7.

Please see the reply to previous comments on Figure 7.

Technical corrections

Page 16, line 19: Check abbreviation for journal.
Corrected.

Page 18, line 14: Please separate the 2 references.
Done.

Page 19, line 5: Check abbreviation for journal.
Checked, correct as is.