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 sincerely thank the anonymous reviewer for his time and constructive comments also in the third round. With this revision, we think to have addressed all remaining comments.

Anonymous Referee #1 (RC1), 3rd round

I would recommend publication after minor corrections:

Minor comments

(line numbers refer to the revised manuscript with tracked changes ATC2)

Page 1, line 37: You may insert "which is consistent with our all-sky values" or something similar since the assumption for global average cloudy sky is rather artificial.
Our estimates of all-sky forcing are based on modelled but reasonably realistic land type and cloud distributions in the aerosol-climate model ECHAM-HAM. In contrast, Sellitto et al. (2022) assume a surface reflectivity of 0.5 globally to represent the cloud effect. It is therefore not fair to write that the values are consistent. In fact, their assumptions are rather artificial, which is why we use again the term equivalent in the sentence: "[…] calculated a global equivalent of TOA forcing as high as […]".

Page 1, line 38: You may insert after "1.1W/m2" for clarity "(i.e. negative forcing)"
Good point, done.

Page 6, line 8: The references Iacono et al., 2008 and Pincus and Stevens, 2013 are missing in the reference list (Page 18ff).
The missing publications were added to the reference list.

Page 21, line 25: Please include coauthors and doi in the Jolly-reference.
Done.

Referee #2 (RC2), 3rd round

I sincerely thank again the Authors for their revisions and the very interesting discussion that originates from it which, it is important to notice it here, is an integral part of the ACPD review process and of the general journal philosophy. I think that such discussions between modelers and experimentalists is quite necessary in our scientific domain.

In general, I'm very satisfied of how most of the strong statements have been smoothed in this version. I see that a few divergences still are there, between the Author's view and mine on the subjects of: 1) the aerosol extinction and absorptivity evolution during plume's dispersion, and 2) the representativity of the point LiDAR observations at Punta Arenas. For the sake of the discussion, I will list my further concerns in the following but leave it up to the Editor to see if more modification of the text is needed. I also add a list of specific corrections, which are more recommended.

Apart from that, I recommend the manuscript for publication on ACP and I warmly thank the Authors for the great paper.
Pasquale Sellitto

Again, we thank you for your availability also for the third round of review. We acknowledge the critical and fruitful discussion within the review process of this study, where we have definitely learned a couple of things from the perspective of the volcanic research.
We have tried to make further improvements and caveats as we could follow the arguments and hope to have found good counter-arguments for the others.

Outstanding divergences

1) The Authors insist on the fact that the ageing processes of the plume are over by end of January, also based on what discussed in Ohneiser et al., 2022. The buildup of secondary aerosols in the stratosphere can take several weeks (see also volcanic plumes in the stratosphere) and the ageing processes of biomass burning aerosols can be even more complicated and take longer. From a theoretical point of view, I don't see any reason to affirm that this is over by January. A marked increase in the AOD is observed and available in the literature for different biomass burning plumes at the temporal scale of up to some weeks. In Ohneiser et al., it can be seen that depolarisation can be significantly larger than a few percent (up to 10%) until end February/beginning March, which does not support the idea of a plume ageing already finished by January.

We strongly disagree. This study does not say that plume ageing will be completed by the end of January. Based on Ohneiser et al. (2022), we refer to the temporal evolution of the lidar depolarization ratio at Punta Arenas, which clearly indicates the ageing of the plume by mid-February already, with a decrease from 20% to 10%. We have further clarified this to mention also the further reduction of the depolarization ratio to 5% (and ongoing ageing) after the end of February. This, however, does not affect our reasoning.

There is a decline in AOD at Punta Arenas, this is true, but how sure are the Authors that this station is truly representative of the whole SH mid and high latitudes, as stated in the text? Thus, I would recommend (up to the Editor to see if this is truly necessary) to add a sentence at P8, after L10-11, to mention that in any case the present modelling results in terms of the

AOD trend, even if in agreement with a single LiDAR station, are in disagreement with limb satellite observations, like those shown in Khaykin et al. 2020 and Sellitto et al., 2022 (which show an AOD increase from January to February). Possibly, also the statements about the representativity of the single point LiDAR observations at Punta Arenas should be smoothed.

Unfortunately, there was a misunderstanding. We wrote in plural that the ground observations peaked around or after mid-January. This was not limited to Punta Arenas but included the other southern hemisphere AERONET stations. We have therefore added 'AERONET' here. By the way, this does not refer to the Polly lidar, but to the AERONET sun photometer measurements in Punta Arenas.
Because of the possibly unclear saturation issue of the mentioned OMPS LP satellite instrument and the related potential delay in smoke detection (see Khaykin et al., 2020) we would prefer not to include the sentence as suggested.

2) The Authors bring the consistently large values of LiDAR ratio (>70 sr) as an indication of the fact that aerosols in the plume stay absorbing until March. The LiDAR ratio depends on the aerosol composition but also on the size of the particles. While BC aerosols have usually a larger LiDAR ratio value with respect to other aerosol types, the LiDAR ratio has also tendency to increase with increasing aerosols mean size. **As aerosols are probably increasing in size due to ageing, the fact that the LiDAR ratio stays large until March simply cannot be used as an argument to infer an aerosol large absorptivity. As a matter of fact, LiDAR ratios as large as >80 sr have been found even for the purely scattering sulphate aerosols of some extreme volcanic eruptions** (see e.g. Prata, A. T., Young, S. A., Siems, S. T., and Manton, M. J.: Lidar ratios of stratospheric volcanic ash and sulfate aerosols retrieved from CALIOP measurements, Atmos. Chem. Phys., 17, 8599–8618, https://doi.org/10.5194/acp-17-8599-2017, 2017). Thus, I would recommend (up to the Editor to see if this is truly necessary) to **modify or even suppress the sentence at P15 L1-3: “while high lidar ratios…Ohneiser et al., 2022)**”.

The lidar ratio is greatly affected by the particle properties. However, the analysis by Cao et al. (2019) shows that for the particle size range here, there is only an unnoticeable increase in the lidar ratio with particle radius.
On the other hand, the mentioned study by Prata et al. (2017), argues that the partially quite high lidar ratios for some volcanic ash plumes can rather be explained by different particle populations or types (sulphate-rich versus ash-rich aerosol layers) that they have been able to discriminate.
Therefore, we keep considering our argument that more or less constant high lidar ratios of approximately 91 sr on average (!) point to particles that continue to be absorbing.

References:

Cao, N., Yang, S., Cao, S. et al. Accuracy calculation for lidar ratio and aerosol size distribution by dual-wavelength lidar. Appl. Phys. A 125, 590 (2019). https://doi.org/10.1007/s00339-019-2819-y.

Prata, A. T., Young, S. A., Siems, S. T., and Manton, M. J.: Lidar ratios of stratospheric volcanic ash and sulfate aerosols retrieved from CALIOP measurements, Atmos. Chem. Phys., 17, 8599–8618, https://doi.org/10.5194/acp-17-8599-2017, 2017

Specific corrections

1) L33-34: "…and subject to the model uncertainties in the smoke optical properties": this is said to apply to the surface RF but in fact this also applies to top-of-atmosphere RF (discussed at L30-32). Please correct the phrasing to account for that.
Done.

2) P5, L5-6: "In addition, values…data inversion (Ohneiser et al., 2022)": please specify that the single scattering albedo is specifically retrieved in that paper only for January
Done.

3) P15, L10-11: "In terms of single scattering albedo…": you cannot use SSA observations with this LiDAR to judge your modelling estimations quantitatively because you strictly have SSA retrievals only for January. Please correct.
The statement was limited to January 2020, for which the retrieval is actually available. Below in the text, we had already pointed out during the last revision that a larger contribution to stratospheric fire aerosol cannot be ruled out and that dilution effects on the spatio-temporal evolution of the smoke plume may not or only insufficiently be represented.