Dear Editor, Dear Reviewers!

Thank you for careful reading the manuscript and providing us with valuable comments to improve the manuscript. The revised version is given at the end of this reply letter.

This letter contains our step-by-step answers (in bold) to your comments and how we considered the recommendations. In the beginning, a short overview of the main points of changes and improvements.

- **We extended our analysis and include all January observations (6-31 Jan) in the revised version. Therefore we updated Figure 1 and updated and extended Figure 5 and Table 1.**
- **As a new point, we combined AERONET photometer and Polly lidar observations to increase the number of derived 355 nm lidar ratios, so more lidar ratio pairs are available now in Figure 5 and in Table 1.**
- **As strongly requested by Mike Fromm, we introduced a new Sect 3.1 (3 pages, a new Figure 2) and now present an extended discussion on the source-receptor link, i.e., on smoke long-range transport, pyroCB events, smoke ascent during travel (based on HYSPLIT computations, Himawari-8 observations, MODIS observations, CALIPSO and own lidar measurements). In the new Figure 2 (HYSPLIT trajectories) we included MODIS fire maps.**
- **We also included AOT observations with OMI southwest of Punta Arenas from 21-30 January 2020 and the recent ascent rate study presented by Torres et al. (2020) for the 2017 Canadian smoke.**
- **We better include recent CALIPSO observations (Andreas Foth performed this study), however without showing any CALIPSO result, because the goal of the paper is clear, and we try to write that now even more clearly: *In this rapid communication letter we present first measurements of smoke lidar ratios and depolarization ratios of the 2020 stratospheric Australian smoke.***

Reviewer #1 (Mike Fromm)

Ohneiser et al. (2020). (Hereafter "O20") report on a small set of very recent ground-based lidar observations of tropospheric and stratospheric wildfire smoke over southern South America, Puntas Arenas. They attribute the smoke to fires in Australia. The bulk of the paper consists of analysis of the lidar data and a nearby AERONET photometer. O20 also compare the POLLYNET lidar observations with stratospheric smoke observations over Europe in 2017 obtained with a similar lidar setup. To the extent that stratospheric smoke observations are now available from at least two different events with a similar instrument configuration, this is a useful study. The subject matter is well targeted with ACP. However, I find several weaknesses in the manuscript leading me to the assessment that the manuscript, in its present form, does not merit publication in ACP. I discuss these next, and follow that with a list of minor issues O20 that should consider. The Australia wildfire event to which O20 refer and attribute for their smoke observations occurred very recently (since November 2019). The manuscript's Introduction contains several statements giving facts regarding the seasonal set-up weather/climate conditions without citing any sources. In Section 3, O20 call out "Bureau of Meteorology" but provide no matching listing in the Reference section. Hence I was left with the impression that this manuscript, which became available just weeks after the fire events unfolded, is rushed and premature. The authors acknowledge that this is a "rapid" report. If there is a separate standard for evaluating such a "rapid" report I am unaware of it. Hence I applied the standard of scrutiny I use for all ACP-D manuscripts.

Due to the recentness of the Australian wild fire events (and there are several, dating back to spring 2019), very little citable information about the fire and fire-storm conditions is available. For instance, to my knowledge there are no details about the spatial-temporal nature of the fire emissions yet established in the citable literature. At this point the information is relegated to general media reports and blog posts. O20 attempt to describe the source conditions but generate/present no content on which to constrain the source term. The only reference they give is a NASA blog posting. Conceivably, this single callout might be sufficient to establish a plausible connection with the Puntas Arenas lidar data. However it fails to help O20's analysis, which consists only of a set of 20-day back trajectories, none of which make a connection to Australia at the times of claimed smoke initiation. In my assessment the trajectories offer little more than evidence of general westerly winds, adding little value over what one expects from meteorology. O20 recognize this yet proceed as if the trajectories prove a connection with the areas/time of Australia smoke emissions and pyroconvection.Given that O20 make an attempt to connect their observations to a very distant source,it is implicit that this connection is important to their thesis. Not only is the hypothesized source very distant from South America, the timing of Australia's pyroconvection is also unknown to the reader. An informal collaboration of scientists following pyroCb action observed pyroCbs in western and southeastern Australia on several dates since mid-November 2019. UTLS plumes were thus put in place over a two-month period sufficiently in advance of the Puntas Arenas observations that tighter constraints on the source of these reported observations are necessary. And given that there are available data sets and methods with which to establish the source-receptor relationship, data and methods they do not employ, this effort is deemed inadequate in its present form. An example of the above-mentioned method was employed by Foth et al. (ACP, 2019) who analyzed another stratospheric smoke plume observed by POLLYNET lidar at Puntas Arenas n 2010. In that case FLEXPART trajectories and upstream CALIPSO curtains together were critical in identifying a plausible candidate for the source of the smoke. Foth et al. also provided details on a pyroCb event in western Australia to support the end-to-end connection. Surprisingly O20 did not cite Foth et al., a critical weakness. Moreover, since O20 devote considerable attention to examination of the derived lidar properties in Puntas Arenas and a European POLLYNET lidar, it seems that an invaluable opportunity to compare the 2020 lidar observations with those examined by Foth et al. was not taken. O20 acknowledge that this is a "rapid" submission and that they are presently processing/analyzing the unfolding smoke event's data with an eye toward future papers. Given that acknowledgement and the paucity of essential information provided herein, my assessment is that this manuscript is unsatisfactorily weak in its present form. For the authors to meet their expressed intentions, and for the benefit of ACP*, ….*

….. I recommend that this manuscript be strengthened by adding more substance on the source term, a convincing analysis connecting the South America smoke with the source, and a direct comparison of the 2020 smoke observations with those of Foth et al.

Reply: We extended significantly the discussion on the link between southeastern Australia (source: fires , pyroCBs) and the lidar station at Punta Arenas (receptor: smoke layers in the stratosphere). We introduce a new section, Sect. 3.1 (three pages! See pages 4-6 in the revised version). We extended the trajectory analysis (we spent almost three days on this), but we do not show more than one backward trajectory scenario (snapshot) because trajectories alone do not help much. Instead of 9 January trajectories, we now present 10 January trajectories. To that time, the main air-flow conditions across the Pacific Ocean were better established than on 8 or 9 January, when the period with significant smoke layers over Punta Arenas just started. We checked the Himawari cloud observation for pyroCB developments and used also MODIS to determine cloud top heights. All this took almost another three days… We used in addition CALIPSO observation (Andreas Foth did that) to check whether the observations corroborate our hypothesis on the most likely transport or not. All this now discussed in large detail. A critical point in the source-receptor discussion arises from the fact that black-carbon-containing smoke layers strongly absorb sun light and ascent. In the case of 10 January, an ascent by 5-7 km within 10 days had to be assumed to bring our observations in consistency with trajectory analysis. So, the comparison with the smoke event described by Foth et al. (2019) does not really help because the smoke AOT was so low (0.002) over Punta Arenas in March 2010… , … there was no significant heating and ascent effect at all. At these conditions, the source-receptor analysis is much more easy and much more straight forward. Nevertheless, we mention this now in the revised manuscript (last paragraph in Sect. 3.1).

Before we continue with the step-by-step reply, it should mentioned that we checked all references (and missing citations…). We improved all this carefully along the suggestions of Mike Fromm. If we did not find proper references, we removed the statement.

Additional Substantial Concerns/Questions

Introduction, P2, L2-4. O20 report observations of CALIPSO-detected smoke at 20+ km on unspecified dates and provide a citation that leads to a generic CALIPSO web site. The reader cannot find the intended evidence at that link.

We removed the statement.

Introduction, P2, L8-9: "The stratospheric aerosol spreads across the southern hemispheric latitudes from about 20-30_S to the south pole within few weeks…" No evidence of this is produced in the manuscript and no citation is given.

We removed the statement!

Introduction, P2, L11-12: "Stratospheric aerosol perturbations influence radiative transfer, ice cloud formation in the upper troposphere, and chemical processes in the lower stratosphere over months." This statement lists smoke-aerosol effects in the lower stratosphere, a subject that to my knowledge is unexplored. No evidence is produced herein and no citations are given. If published results along these lines are available they need to be cited. Otherwise, O20 should characterize the statement as suggestive or speculative.

We rephrased the respective sentence on page 2 (lines 25-19). But we think it is necessary to say why smoke is potentially important (radiation, clouds, chemistry). So, we leave in what we wanted to say concerning the *potential* importance of the smoke in the stratosphere. We provide more references now.

Introduction, P2, L13-14: "The height-resolved documentation of this unique event including the decay of the stratospheric perturbation will be mainly based on observations with…" O20 go on to list a subset of satellite-based aerosol profile data but do not include several important ones, e.g. OSIRIS, OMPS/LP, SAGE III ISS, ACE-Imager.

You are right… and we used many of the very useful passive-remote sensing products in the case of the Canadian smoke event in 2017. Nevertheless, we avoid to mention the respective tools and techniques in the revised version but provide references where these techniques were used and descriptions can be found. That should be sufficient. This is a lidar paper and the focus is on lidar stuff!

P4, L6-7, discussion of Fig. 1a: O20 describe smoke between 4-10 km, but it is not obvious to me. I see mostly cloud. The data represented in Fig. 1 is raw, unccalibrated range-corrected signal, and no depolarization ratio image is provided. Hence it is very difficult to determine cloud-aerosol differences. O20 go on to discuss the clouds that dominate the curtain, label them all as ice clouds, and make inferences regarding smoke nucleation. The raw information provided in this figure is not sufficient to support their description or inferences.

Aerosol-cloud interaction is not the main topic of the paper, so…. because of this comment, we were close to the point to skip all this. But then we left it in, because it is just a nice example for tropospheric smoke in the usually very clean free troposphere over Punta Arenas. And so many clouds formed in the presence of so different aerosol conditions… it is just exciting to see that. So at the end, we shortened the discussion, we just try to describe it a bit better in the figure caption…, and that's it. Not more! In the conclusion and outlook part

we come back to this point and state that we plan to work on aerosol-cloud interaction and that we will contrast seasons without and with smoke impact to evaluate the impact of smoke on the occurrence, development, and microphysical properties of liquid-water, mixed-phase, and ice clouds over the usually pristine and clean southern part od South America.

P4, L14, discussion of Fig. 2: Twenty days is a very long interval. None of the trajectories pass over southeast Australia. Two of the three don't even get to Australian longitudes by 27 Dec. On its own this figure provides little more information than that the winds are general westerly, which is not illuminating.

We removed the old trajectory figure (Figure2). By the way, we showed 13-day backward trajectories (this is the maximum HYSPLIT trajectory length), but were believing that we showed three-week trajectories. We selected a new case (10 January instead of 9 January). This day (10 Jan) is more in the center of the first intense smoke period, so the air mass transport pattern were better established and thus robust. These new trajectories facilitate the discussion, they are in quite satisfying consistency with the satellite observations (Himawari-8, MODIS, CALIPSO) and our Punta Arenas lidar observatio. We downloaded fire maps (as requested by the second reviewer) and integrated them into the new trajectory figure. Because the discussion on long range smoke transport became very long, we introduced a new Section (Sect 3.1, three pages).

P4, L15-17, more discussion of Fig. 2: "As can be seen, air masses obviously started over Australia at heights of 15-16 km…" This is not only not obvious, it's not even true. None of the trajectories pass over Australia.

As mentioned there is new Section 3.1 on smoke long range transport, inluding a long discussion of the new Figure 2.

P5, L9: "Typical values.." How does the reader know what is "typical" linear depolarization for stratospheric smoke? This implies a sufficiently large data set exists and is available to the reader. Such a data set is not presented here, nor citation given. Double-digit depolarization ratios are certainly unusual for tropospheric smoke. The literature on stratospheric smoke depolarization exists but is quite limited. Substantiating the claim made here requires a fuller overview.

We rephrased it (see the measurement section , Sect.3.2). Now it should by very clear: Sphercial particles produce almost zero depolarization. But already very small deviations from the ideal spherical shape causes strong depolarization according to the recent modeling study of the Greek team. And the strong spectral slope of the depol ratio is caused by the well defined size distribution (single accumulation mode, no coarse mode). And there are already papers with measurements on this topic (Burton 2015, Haarig 2018, Hu 2019). We state that.

P5, L20-26, discussion of depolarization/particle shape: This argument is confusing. To first order, depolarization increases with asphericity. Double-digit depolarization ratio then would suggest a more irregular shape than "almost spherical." Please clarify.

As mentioned, we improved the discussion to reduce (remove) confusion (see improved explanations in Sect. 3.2).

P5, L27, discussion of what Aeolus lidar would report: How does the reader know what the ALADIN lidar ratio would be? Is it appropriate to invoke unreported Aeolus measurements? If so, how is the reader to assess these Aeolus data?

We skipped the Aeolus technical and data analysis details, and also removed the star (Aeolus lidar ratio in Figure 4). It is simply too complicated here, and is not needed in this manuscript. We will come back to that point in detail in follow-up papers.

Technical issues: See the accompanying pdf of the manuscript with comment:

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2020-96/acp-2020-96-RC1-supplement.pdf

All points in this supplement were carefully checked and are considered now! Thank you!

Reviewer #2

The paper provides very useful and unique information for aged smoke properties, vertically resolved, using ground based lidar measurements. The Australian bushfires in 2020 injected smoke in the stratosphere and this was transported eastward to South America. This data set is extremely useful as input for the satellite algorithms of CALIPSO and AEOLUS, which are the instruments that can truly follow the evolution of such events. The paper is well written and structured and should be accepted for publication as a rapid communication in ACP.

Some minor comments should be considered by the authors before publication.

In order to put this event in perspective, it would be useful to insert a table which will summarize smoke properties from other stratospheric aged smoke events that have appeared in the literature. This will allow to the reader to interpret the intensive parameters found.

Yes, this is probably a good idea. But as mentioned in the conclusion section, we work already on a paper in which the record-breaking Canadian and record-breaking Australian smoke events will be compared in terms of geometrical (layer base, top, depth statistics) microphysical, optical and even cloud-relevant parameters. The study will be mainly based on lidar (ground, CALIPSO, Aeolus) and accompanying AERONET observations (Antarctica, South America, Canada).

There are not so many published lidar studies on stratospheric smoke, and in the few papers the lidar results are typically presented as integrated backscatter and backscatter coefficient profiles. So at the end, the two extreme events are (more or less) left for comparison. But this is already an exciting story….

As a compromise, we included the Canadian smoke lidar ratios and depolarization ratios in Table 1 (last line).

The backtrajectory analysis as presented in Figure 2 could be substantially improved,by including the hot spots from the fires. The authors actually should show how the smoke was raised at high altitudes over Tasmania and New Zealand and then followed the westerly winds. As it is written a lot of facts are speculated.

As mentioned in the beginning, because of this comment, we show a new Figure 2 (with trajectories and fire information). We extended the discussion and introduced a new Sect 3.1 (pages 4-6) and we use a 'better' trajectory day (10 January instead of 9 Januray) now. We keep the amount of assumptions (… sounds better than speculation) as low as possible. But we need to assume and estimate the ascent of the smoke layers by absorption of solar radiation and heating of the smoke layers, because these diabatic processes are not covered by trajectory analysis. And the smoke layers ascended by 5-7 km during the 10 day travel (in the case of the 10 January case, Figure 2), and later on (end of January) we discuss cases with ascent rates of the order of 1 k per day… We compare all this with literature values, modeling studies, and even observations (CALIPSO, MODIS).