Dear Prof. Solomon,

We sincerely thank you for a very careful reading, all the critical comments and fruitful suggestions from the point of an absolute expert in the field.

We rearranged the results section (more sections, no longer sub subsections) and in the revised version considered all your comments.

Our answers are in BLUE!

Note that manuscript text that was substantially changed or added, appears in BOLD in the revised version.

General comments: I enjoyed reading this paper and think it would make a useful contribution to the literature.

We are pleased to know that you like our manuscript. Thank you!

My comments in order of occurrence are:

1) I do think the paper could be made substantially shorter, with a tighter focus on the data itself and what is new (as opposed to review – while I would love to see a long review of wildfires by these authors published in another form, I think including so much background here is distracting and weakens the utility of their new material). In addition, the way wildfire smoke interacts with PSCs or volcanic sulfate is unknown and the speculation here may be right but we really don't know. I think the material on pages 1-8, and Figures 2 and 3, would be part of a great review paper.

We discussed this point in detail and after taking into account all the questions we get during numerous oral presentations in the last 6 months, we feel that the introductory figures 2 and 3 are simply a NEED to understand the full story. Especially if the audience of the ACP journal coming from multidisciplinary field.

We need to present the potential pathways on how smoke can be involved in all the processes that finally lead to ozone depletion. Since this is still one of the first papers on this new topic (impact of smoke on stratospheric ozone depletion), we need section 2. Currently we are still too far in thinking about review papers.

We do not see how we can skip the initial part (pages 4-8 and figures 2-3) and, at the same time, still provide a consistent and convincing explanation of our findings.

We did largely revised and reduced the text in Sect. 2.

2) Line 294. Please specify what dataset(s) was used for this averaging; several were mentioned.

We are now more specific: MOSAiC data (Ohneiser2021) and DACAPO PESO data (Ohneiser2022a)

3) Figure 4 is useful, but since a key focus of the paper is the changes at altitudes too low for normal PSC chemistry, I think it would be very valuable to add two more panels showing the same thing for the ozone in the 9-12 km region that is highlighted in the abstract and elsewhere. We really need to see how unusual the reported changes in that height range are, compared to

other years. This should be presented either here or elsewhere. Also, can 2021 be added to this plot?

Done. We added panels for the 10-12 km layer, and we included 2021.

4) Line 364. Near-complete reduction at what vertical level?

At 18 km height. It was noted in line 365 of the original version:

5) Line 371. Reference is needed for the statement that Raikoke contributed 10%, or say "(see below)".

Done. Ohneiser et al. (2021) is now given. We added a new section (Sect.6.2) to explain again why we think the Raikoke aerosol fraction was of the order of 10-20%.o

6) Can Figure 6 be hatched to show values that are the lowest in the record (if there are any)? Also, how can we relate this figure to statements made elsewhere regarding the 9-12 km layer?

We slightly improved Fig 6. by showing the 10-12 km layer (we switched from 9-12 km to 10-12 km in the revised version) and the PSC regime (now in the revised version from 14-23 km height). The message of the figure is to see the two ozone holes in 2020 and 2021 (very blue colors), and that the PSC evolution and ozone depletion occurred in smoke polluted air (by showing the base and top heights of the smoke layer). Figure 6 is an important introductory to Figure 7 (the main figure of the Antarctic ozone study).

7) It would be great if Figure 7 could delineate the degree to which the high aerosol amounts in the lower portion of the profile are unusual. Would it be possible to show the range of previous years as a shaded region rather than just the single line for the mean?

See our further response to point 8.

8) Again re Figure 7, how about showing the range of temperature anomaly in 2020 and 2021 from reanalysis? Single station data can sometimes be unrepresentative.

We revised and improved Fig. 7 significantly being inspired by all your fruitful suggestions (points 7,8,9).

We provide the shaded range for the background aerosol.

We show the year-by-year variability (ozone deviation and temperature deviation profiles for each individual year from 2010-2019) to better identify the natural variability.

We highlight the Calbuco year (dark gray lines, ozone and temperature anomaly profiles), and use the Calbuco impact as some kind of a (known) reference in the discussion of the importance of the smoke impact.

We use ERA5 temperature fields (70-90N and 70-90S) to obtain more representative temperature profiles.

The discussion is expanded accordingly.

9) Same issue for the low altitude ozone anomaly in Figure 7 – how unusual is it? A time series

like what I suggested in Figure 4 would best answer that question in my view; alternatively, a range of variability in other years could be shown here.

See our response to point 8.

Furthermore, we used the Hofmann et al. (Nature, 1987) approach and show times of the ozone mixing ratio for Antarctica (Neumayer+South Pole) from mid-August to mid-October 2020 and the same for 2021, for the height layers from 10-12 km, 14-16 km, and 18-20 km, to see the potential impact of ozone transport. The smooth and monotonic decrease indicates the dominance of heterogeneous chemical processes. This is now presented in the new Fig.8.

10) Lines 460-463. This is a very important point – can it be expanded and substantiated?

The text in lines 460-463 was: In the lower height range (9-12 km height, below the main PSC layer), an expected clear correlation between the smoke SA concentration and an additional ozone loss was, however, not found in 2020. The reason may be related to specific meridional ozone transport and tropospheric-stratospheric exchange processes in 2020. A clear relationship between smoke occurrence and negative ozone anomaly at PSC-free conditions (9-12 km) was found in 2021, only (Fig. 7d-f).

We rearranged the text, but we still do not have a better answer (now for the 10-12 km height layer). There was certainly a smoke-related ozone loss by chemical processes (in this region the aerosol perturbation showed a maximum), but obviously this reduction was compensated by ozone transport in 2020. This is now a bit better discussed based on the new Fig.8.

11) The discussion of how much extra ozone loss occurs in the PSC region (lines 479-484) is pretty rough.   Factors such as temperature and transport can induce variability in this region from year to year.   This discussion is not very convincing as a result.   Can you put this figure in context relative to variability from other factors? If not, I suggest deleting it.

The text in the lines 479-484 was: To evaluate the importance of an apparent smoke-related ozone reduction of 0.4-1.2 mPa (9-12 km) and 1-1.5 mPa (15-20 km) the following numbers are useful. Under volcanic widely quiescent stratospheric aerosol conditions (2010-2019) the ozone partial pressure in the central 15-20 km PSC height range dropped from values of 11.5-14.5 mPa (May-June 2010-2019 mean, Neumayer + South Pole stations) to values around 4 mPa (September-October 2010-2019 mean), and thus by 7.5-10.5 mPa (i.e., on average by 65-72%). According to this, a smoke-related further ozone loss of 1-1.5 mPa as found in September-October 2020 corresponds to a relative additional ozone reduction by 10-20%.

We deleted this paragraph.

12) Line 505. Please clarify how you identify the PSCs only about 18 km. How can you be sure?

This was just a comment to the shown lidar observations. We changed this misleading sentence.

We provide much more information on PSCs now. PSCs can be appear even at heights close to the tropopause over Antarctica (Pitts et a., 2018, Tritscher et al., 2021). Over the Arctic the PSCs were exclusively found above 14 km height (Deland et al., 2020).

13) Line 521.   I see your broad points here but I don't follow how you get the fraction volcanic –

yes, the ratio would likely have been about 45 for volcanic but was measured to be 70. But please explain how you get 10%.

We introduce a new subsection on the potential Raikoke impact (Sect. 6.2). In that subsection, we present an alternative way regarding the estimation of the Raikoke fraction. This alternative estimation is in good agreement with the more sophisticated one presented by Ohneiser et al. (2021).

14) Figure 10. Same comment as above re. the corresponding Antarctic plot, Fig 4.

10-12 km height layer is now highlighted by horizontal lines.

15) Line 543. Same problem with variability as made above re the Antarctic; I don't think you can reliably infer the extra ozone loss in this way. For example, cold years also imply very little downwelling, which will also lead to low ozone values, and I think you can't be confident from such a simple way of looking at it.

We rephrased and shortened the section. We avoid such quantitative statements.

16) The low altitude anomalies are key in Figure 11. Same comments as above for the Antarctic – we need to better understand how unusual these are.

We present Fig. 11 (now Fig. 12) in the same way as the improved Fig. 7 (showing year-by-year ozone and temperature deviation profiles, and using ERA5 temp data).

17) In closing, I want to make one more major comment. There is evidence from OMPS data and presentation on that which I have seen that the aerosols from the Soufriere eruption in 2021 may have contributed to the aerosol load in the Antarctic in that year. It would on the other hand, be surprising if so much of the 2020 Australian fire aerosol lasted into 2021. You may want to make the same kind of comparison that you did for the Arctic in Figure 9 to probe the extinction and what that can tell you. Or you may want to at least discuss the possibility.

Thank you. We introduce a new subsection (Sect. 5.3) on the potential impact of La Soufriere volcanic sulfate aerosol. However, our Punta Arena observations (Ohneiser et al., 2022a) do not support the hypothesis that La Soufriere volcanic aerosol was responsible for the ozone hole in 2021.