

ANSWER to RC1

Review of amt-2021-58

The manuscript studies the dynamics of the stratospheric injection from the Raikoke volcanic eruption. Nadir sensors measuring the SO₂ column were combined with a limb sensor measuring stratospheric aerosol extinction to obtain a more complete picture of the volcanic effects. A coherent circular cloud of SO₂ and aerosol was identified and studied with a trajectory model. Overall I found the paper well-written, interesting, and relevant for the scientific community as a whole. However I have some major concerns related to some of the analysis of the limb stratospheric aerosol data. I believe that these are fixable, and would recommend that the paper is published after these issues are corrected.

General Comments

My main concern is related to that of the “arch effect” correction. The specific comments below go into more detail on my concerns, but the primary one is that a 1D retrieval does not only introduce “arches”, it also underestimates the main plume. The underestimation of the main plume is not taken into account by the analysis performed here. I don’t believe the correction itself actually influences any of the main results of the paper, so this could easily be changed, but I feel like something should be done. I would suggest either removing it entirely, providing more justification that the correction is in fact something positive, or adjusting the manuscript so that the correction is presented as a potential source of error instead of an actual correction.

ANSWER 1: We agree that the density of the local cloud, calculated within the 1D RTM, may differ significantly from the real one. But at the same time it is clear that all values of the cloud density that are obtained below its real height are artifact and must be removed. We improved Figure 3 (now 3a) and made an additional Figure 3b. We have expanded our discussion of the arc effect considerably. We also added the following text to the article: “We consider this procedure of correction only as an estimate, which shows the possible significance of the arch effect”. “These estimates show the significance of the arch effect. For more accurate calculations, the arch effect should be investigated within a 2D RTM”.

Specific Comments

p.4 l.86-98: At this point various resolutions (wavelength and spatial) are introduced for TROPOMI and OMPS NM. The importance of the spatial resolution is obvious but the importance of the wavelength resolution is not. I would suggest that some characteristics of the retrieval are introduced either in addition or instead, i.e., any available bias/precision estimates for the SO₂ column.

ANSWER 2: We added next statement: “For large volcanic SO₂ signals like Raikoke, comparisons between TROPOMI and SNPP/OMPS so far (for several eruptions) show little bias, with the total SO₂ mass estimates from the two normally agreeing to within 5-10% (with the exception of the

very early stages of large eruptions, where the density of SO₂ and/or volcanic ash is too high to be fully accounted for in operational algorithms). For retrieval noise on a pixel-to-pixel basis, SNPP/OMPS SO₂ (for stratospheric clouds) is less than 0.1 DU. TROPOMI's noise on a pixel-by-pixel base is several times greater, but once TROPOMI pixels are averaged to OMPS footprints, the noise is reduced by ~30%."

Section 2.2: This section is missing some discussion of the microphysical assumptions necessary for the OMPS LP aerosol retrieval and how they differ from the CALIOP retrieval. I believe this is a gamma particle size distribution with fixed parameters?

ANSWER 3: In this paper, we only used images of CALIOP total attenuated backscatter signal as a reference to identify the Raikoke plume location and altitude. We didn't use any retrieved aerosol data. We have added the following to L101 (first line in 2.2): "The OMPS LP V1.5 aerosol retrieval algorithm is described Sects. 2 and 3 of Chen et al. (2018)." We also replaced "CALIOP aerosol data" in L286 (second line in 3.3) with "CALIOP backscatter data".

Figure 3: After staring at this figure for a while I could not reason out what is actually being shown. To demonstrate the relationship between H and h we would need to see a single cloud, with two different lines of sight/observer locations, but instead we see five different clouds and a few tangent heights? Is it intended that the "five clouds" A-E are not different clouds but the same cloud seen at different times? If so it is also confusing since OMPS LP is backwards looking the first observation is "E" instead of "A".

ANSWER 4: We have significantly improved Figure 3 and expanded the explanation for it. We consider the positions A-E as different positions of the same cloud. The order of the letters has changed.

p.4 l.106-108: Are all three slits from OMPS LP used in this analysis or just the central slit?

ANSWER 5: For Figs 4 and 11 only central slit was used, for other cases – all 3 slits.

p.4 l.118: "LP signal strength (e.g., extinction coefficient)" is confusing, extinction coefficient may be re-lated to signal strength but it is not an example of signal strength.

ANSWER 5: The text has been changed: "LP (e.g., extinction coefficient)"

p.5 l.136: You state that the displacement is approximately equal to a latitude displacement, I assume that is only for illustrative purposes since there is no need to make this approximation in the actual correction?

ANSWER 6: Correct.

1p.5 l.141: "We can therefore use Eq. (1) to calculate and apply a correction for determining the magnitude and position of an aerosol cloud." I understand how the equation can be used to calculation a correction for the position of the aerosol "cloud", but I don't see how it can be used to correct for its magnitude.

ANSWER 7: We added: “we believe that all parts of the arch below the real height are artifacts, so the value of the extinction coefficient for them should be equal to zero”.

p.6 l.157-161: I agree this algorithm will remove the “arch” however it is not convincing to me that this improves the aerosol extinction, in fact, I am not even convinced that a 1D retrieval always overestimates the total retrieved optical depth as would be suggested by the text. If you imagine an aerosol point source and do successive 1D retrievals along the orbit you will obtain an arch, just as the authors suggest. The arch is obviously unphysical, and for this reason the authors remove it. But, for the one 1D retrieval where the point source is located at the tangent point, the 1D retrieval will actually greatly underestimate the magnitude of the point source. The reason for this is that the 1D retrieval is assuming horizontal homogeneity, so it cannot add a point source, it must add aerosol with a greater extent. In addition the 1D retrieval will also underestimate below the point source because these altitudes in the 1D forward model contain extra aerosol scattering from assuming horizontal homogeneity. The underestimation effect is completely ignored by the authors and for this reason I do not believe the arch correction as presented is meaningful. I do not see any way to either remove the biases of a 1D retrieval or estimate its effect that does not involve full two-dimensional radiative transfer simulations.

ANSWER 8: We improved Figure 3 and made an additional Figure 3b, as well as discussed in more detail the problems of limb observation. Also we added new statements (see ANSWER 1).

p.6 l.165-166: The wording here could give the impression that a tomographic retrieval has never been implemented for OMPS LP aerosol extinction, however it appears it was done in the Zawada et. al. reference on the same line.

ANSWER 9: The text has been changed: “One way to account for such effects is to use a two-dimensional (2D) radiative transfer model (RTM) that is able to account for such effects along with multiple observations in a tomographic retrieval (e.g., Livesey et al., 2006; Zawada et al., 2018; Loughman et al., 2018). Instead, we have developed an a posteriori adjustment method...”

Figure 5: When I look at this figure it tends to reinforce my belief that the “arch effect” is not doing what is expected. Should the correction not be close to 0 before the effects of the eruption? Here it looks like the presence of a plume has no effect on the “arch effect”.

ANSWER 10: See ANSWER 8.

p.7 l.188: Is there a reason to only include OMPS NM here and not TROPOMI?

ANSWER 11: TROPOMI, unlike OMPS, does not have a limb sensor.

Figure 5: What is the cause of the artifact in SO₂ at 35 days? If it is a sampling effect I would suggest to remove the datapoint.

ANSWER 12: We have corrected Figure 5.

p.9 I.228: “more and more pixels with SO₂ fall below the detection limit of the OMPS NM sensor” Does this mean that only pixels where SO₂ is detected are included in the analysis? Presumably if every pixel is included then this would only lead to poorer precision.

ANSWER 13: Correct. Only pixels where SO₂ is detected are taken into account in the analysis of SO₂ dynamics. Also we added in the text: “(OMPS in the stratosphere can typically detect 0.2-0.3 DU of SO₂)”

Figure 7: Specify which tropopause was used here for the integration.

ANSWER 14: We have changed Figure 7.

p.12 I.276: “The along-track field of view integrates over a distance of ~180 km for each 1 km vertical sample” While true that a 1 km shell ends up having a ~ 180 km horizontal extent geometrically, the actual horizontal resolution of a limb sounder is more complicated than this. See for example, von Clarmann et. al. 2009 (von Clarmann, T., De Clercq, C., Ridolfi, M., Höpfner, M., and Lambert, J.-C.: The horizontal resolution of MIPAS, Atmos. Meas. Tech., 2, 47–54, <https://doi.org/10.5194/amt-2-47-2009>).

ANSWER 15: Correct. See ANSWER 8.

Technical Corrections

Figure 4: In the caption “For accurately calculating” → “To accurately calculate”

p.6 I.165: Livesay → Livesey and Zawanda → Zawada

p.18 I.369: “Figure15” → “Figure 15a”

ANSWER 16: We have fixed all the mentioned errors.

ANSWER to RC1

Gorkavyi et al. (hereafter “Gteam”) have three primary thrusts in this manuscript on the Raikoke volcano plume of 2019, to 1. characterize the stratospheric plume SO₂ and sulfate evolution, 2. Introduce a limb-view retrieval artifact called the “arch effect,” and 3. Follow a compact plume element termed a

coherent circular cloud (CCC). These themes are carried out by invoking five different satellite-based measurement/retrieval products, three nadir-imaging and two vertical profiling.

The above themes, data, and methods are an appropriate fit for AMT. It is evident from the data presented by Gteam that the Raikoke volcanic cloud is scientifically important, even remarkable, and needs to be thoroughly characterized in the literature. Moreover, Gteam present a novel method for limb-view aerosol profile handling to deal with a known limitation in the limb approach to quantifying perturbations that are inhomogeneous and/or geographically small in the context of instrument resolution.

My summary assessment of GTeam is that each of the three thrusts need substantial revision to qualify as publishable. In each respect, the material has either been presented in prior literature and not fully recognized herein, or the current presentation lacks clarity, validity, or motivation. Details of these primary concerns are next. Following these is a list of minor concerns. Technical concerns and questions are handled by comments and annotations to the manuscript, provided separately.

Primary Concerns

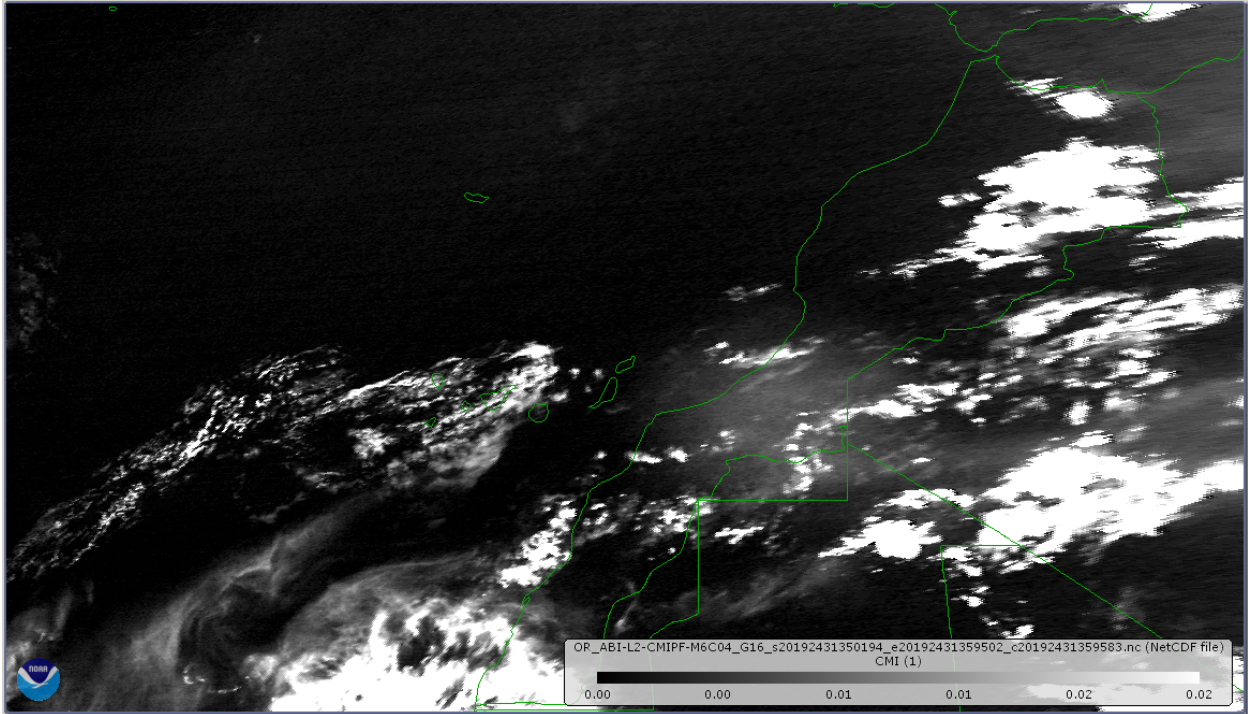
Gteam's foremost new content regards the "arch effect." Here they focus on a fundamental uncertainty with respect to limb-

view spectroscopy, that being the accuracy of retrieving extinction and altitude of an object that is small with respect to the instrument's (OMPS-LP in this case) line-of-sight field of view (FOV). The problem common to all such stratospheric limb viewers is that they peer through a roughly 200 km atmospheric path length (~180 km quoted by Gteam for OMPS-LP) composed of a wide altitude range in the Earth-centric reference frame. Generally, the assumption is made that any object in the view path occupies the entire FOV. To the extent that any object (like a meteorological cloud or aerosol plume) is smaller (vertically or horizontally) than the instrument's FOV, one or both of the retrieved extinction and feature altitude will be biased low. In highlighting the "arch effect," Gteam seem to be suggesting that the Raikoke sulfate plume presents a source of systematic error in OMPS-LP extinction and plume-height results. Hence the motivation for their focus on an adjustment to mitigate the "arch effect."

The "arch effect" argument, to be persuasive, requires at least one additional data item independent of OMPS LP to characterize cloud- or plume-object height, vertical thickness, and horizontal extent all within the OMPS-LP FOV. I.e. OMPS-LP on its own is under-constrained for such an assessment. Gteam present the arch effect argument by invoking only OMPS-LP profile data. Moreover, the illustrative example of the arch effect is for a scene that has a Raikoke plume object that is ~40

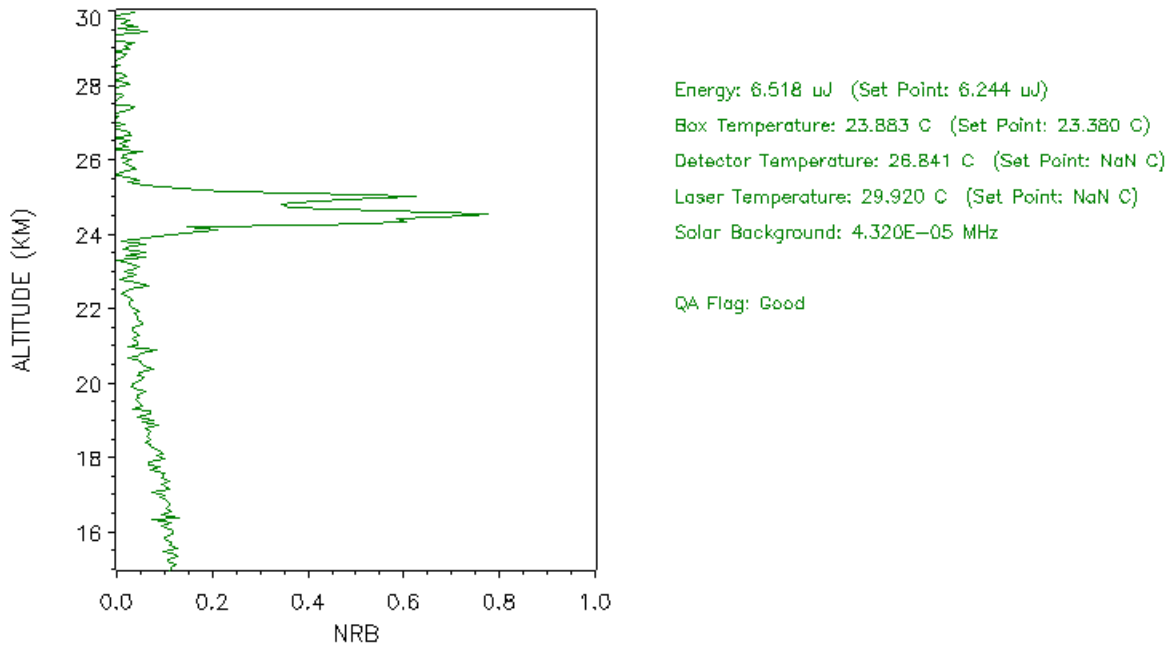
days old. Even though it is presumed to be compact with respect to the OMPS LP FOV, this (and most other) Raikoke plume elements have spread over distances greatly exceeding the OMPS LP FOV. With the data that Gteam present as an example of the arch effect—a consecutive sequence of OMPS LP aerosol profiles—it is not possible to know the true size of the Raikoke plume object precisely in the FOV. It is known, however, that the CCC the Gteam is following has horizontal dimensions as great as ~600 km, based on the CALIOP curtain one day later, shown in Figure 13. It is conceivable that the 31 August OMPS example given for the arch effect was a case of the limb view sampling a peripheral part of the CCC, presenting a much smaller horizontal distance. But insufficient independent information is provided to inform the reader.

Exploring complementary data sets for the Gteam 31 August 2019 case of the arch effect, it becomes apparent that indeed the OMPS center slit sampled CCC they are tracking. The GOES East visible reflectance image shown below illuminates the CCC over coastal northwest Africa at OMPS measurement time: ~13:50 UTC. The visible meridional extent of the CCC is ~3.4 deg. Latitude, approximately 380 km. This is roughly double the OMPS tangent path FOV.



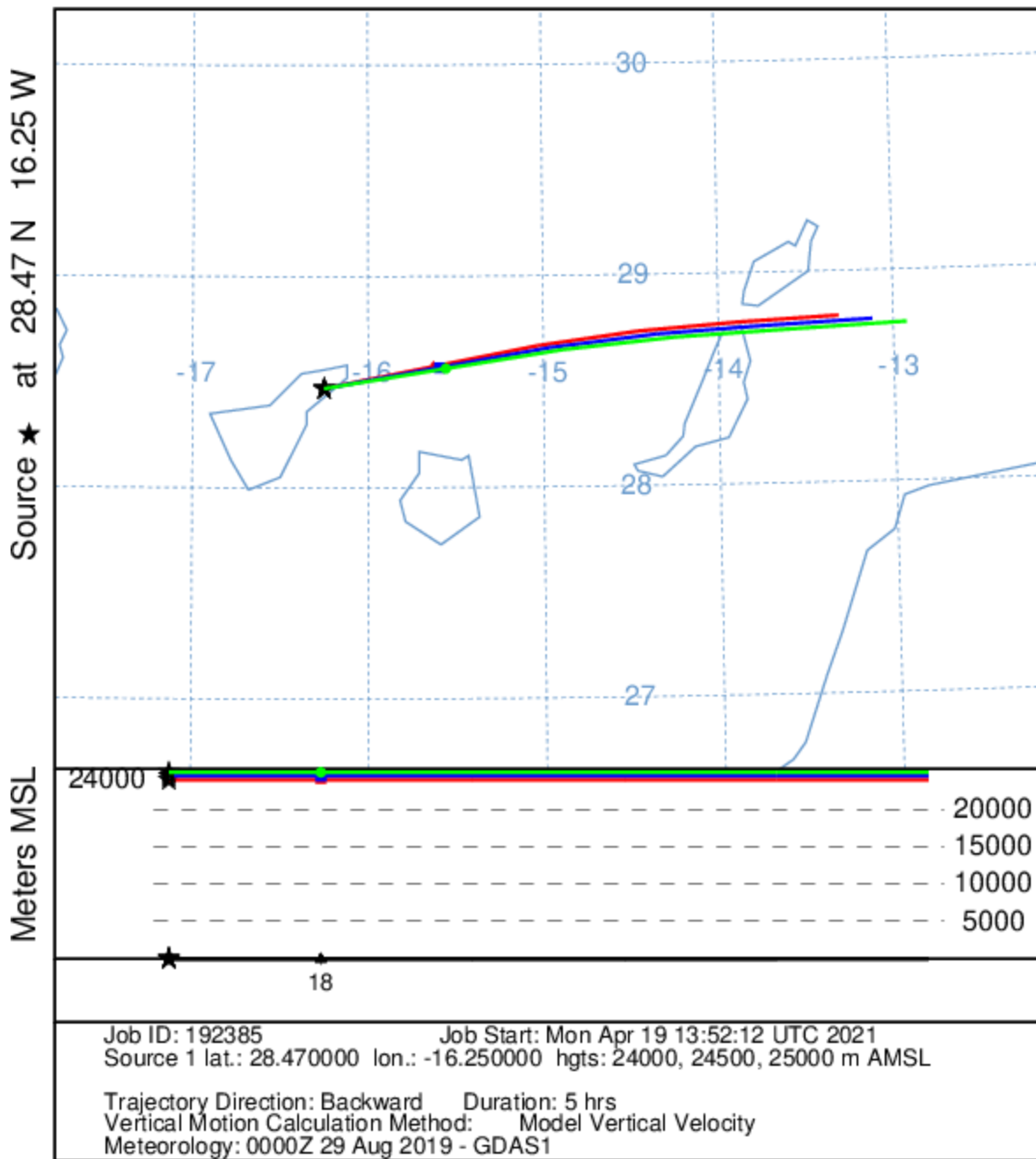
The NASA micropulse lidar (mplnet.gsfc.nasa.gov) at Santa Cruz, Tenerife, (due west of the OMPS extinction-profile curtain in Figure 4) measured the CCC as it blew west. A snapshot of the normalized backscatter ratio profile at 22UTC 31 August is below.

MPLNET Santa_Cruz_Tenerife 2019-08-31 22:10:30 V3_L1_NRB: NRB



Back trajectories to OMPS time 31 August show that the plume over Tenerife indeed passed over the OMPS curtain at OMPS measurement time.

NOAA HYSPLIT MODEL
 Backward trajectories ending at 1900 UTC 31 Aug 19
 GDAS Meteorological Data



The nadir GOES image and the precise lidar profile tightly constrain the plume geometry encountered by OMPS. Thus it appears that the CCC conditions encountered by OMPS are

vastly inconsistent with the limited horizontal cloud extent used to illustrate and motivate the arch effect principle. See the annotations to Gteam's schematic in the separate manuscript.

Given these two additional views of the CCC encountered by OMPS LP, it is largely uncertain as to how to interpret the OMPS extinction profiles in Figures 10 and 4. This does not appear to be a candidate for the arch effect, and the maximum extinction in the CCC—according to the example of it shown in the 1 September CALIOP curtain (Figure 13)—may be as much as an order of magnitude greater than retrieved by OMPS (CALIOP backscatter x lidar ratio of 50 sr). Considering all these factors, Gteam is encouraged to either clarify and bolster their arguments for this arch effect example, or find another case where it can be independently shown that a sub-FOV-filling plume element is sampled by OMPS.

If such an example can be demonstrated, it still seems unlikely that the aging/spreading Raikoke plume as sampled by OMPS LP justifies wholistic application of the arch-effect adjustment. The sheer variety and complexity of plume presentations (e.g. the various CALIOP curtains illustrated by Gteam) to OMPS during the analysis time frame indicate that compact-to-the-point-of-sub-FOV plume elements are rare. This includes the CCC and the meridionally broader Raikoke layers at lower altitudes do not meet the size-limited view as depicted in Figure 3.

It is essential to characterize the schematic angles and "object" (also referred to as a "cloud") in terms of their geophysical horizontal and vertical dimensions. It is all important to know what size range of cloud in the along-track FOV direction creates the arch effect. It is difficult to know from the schematic how realistic the cloud object is, but it appears to be tiny in relation to the 180 km OMPS LP FOV.

To make a compelling argument for the arch effect, three things would have to be presented that are lacking. 1. A case study involving an independent plume-object physically characterized, 2. Such an element being located within the OMPS LP FOV (i.e. a space-time match), and 3. The plume element being demonstrably smaller than the FOV (as drawn schematically in Figure 3). An example of such a case study is provided in Penning de Vries et al. (2014) (cited by Gteam), who attempted to reconcile SCIAMACHY limb-scatter profiles of the Nabro volcano stratospheric plume with simultaneous nadir SO₂ imagery. It is notable that the Nabro example was when the plume was less than two days old; demonstrably compact.

How is an "arch" identified in the OMPS data? How do we know when an arch shape might be geophysically accurate vs. one that is an artifact of a tiny cloud? Gteam describe a wholesale processing of the OMPS LP extinction profiles, applying the arch effect correction to the whole set. At least that is how I understood the method description. If this is the case, does that suggest that Gteam considers the arch effect a global

vulnerability? Much more clarification of the correction application is needed.

ANSWER 1: The first 7 pages of the Primary Concerns are based on a misinterpretation of Figure 3. We agree with the reviewer that the CCC was much larger than 180 km (this is evident, for example, from the Figures 12-14). But the arch effect occurs if the horizontal length of the cloud is less than 1100 km, and not 180 km (for a cloud height of 25 km). We improved Figure 3 and made additional Figure 3b, which clearly demonstrate the effect of the arch or the observed decrease in the height of the cloud when it is displaced from the tangential point. We have added the following text to the article:

“The arch effect is observed when the length of the visible part of the cloud is less than ~ 1100 km (at a cloud height of 25 km). Figure 3b shows cloud F_0G_0 , 1 km thick and 226 km long, centered above tangential point T. Due to the curvature of the globe, such a cloud has an observed thickness of 2 km (see Figure 3b). If we take a cloud 226 km long and with a real thickness of N km, then the observed cloud thickness will be $N+1$ km. Thus, the real average height of a thin (1-2 km) cloud is underestimated by 0.5-1 km even under the most optimal observation conditions. Consider a cloud FG, the center of which is displaced from the tangential point by 273 km (or by $\varphi \sim 2.5$ degrees). The real height of the FG cloud is 24-25 km, but its observed height varies from 13 to 22 km. If we consider the F_0G cloud with a length of 499 km, then its real height above the earth's surface will be 24-25 km, and the observed height is 13-25 km. Let us take into account that the limb profiler assigns the latitude of the tangential point to any extended cloud. Therefore, a single cloud shown in Figure 3a in five different observed positions, instead of one real geographic latitude, receives several “observed” latitudes, which creates an arch effect. Let the region F_0G_0 be a gap in a continuous cloud. Then this gap, together with the arch effect, will lead to a decrease in the maximum observed height of the cloud layer by 1 km (see Figure 3b).”

I could find no discussion of the vulnerability of OMPS-LP extinction profiles to saturation in the presence of optically thick aerosol plumes. Given the likelihood that the Raikoke sulfates presented widespread scenes of such optically dense conditions, as did previous eruption plumes like Nabro, Sarychev Peak, and Kasatochi (Fomm et al., JGR, 2014; Lurton et al., <https://doi.org/10.5194/acp-18-3223-2018>) it is important for Gteam to directly address if/how this issue was dealt with in their various visualizations of OMPS LP extinction and SAOD.

ANSWER 2: Although Raikoke was the largest volcanic eruption seen by OMPS LP, it is still considered a mid-size eruption and nowhere near the size of Pinatubo or even El Chichon eruption. As noted by Rieger et al., (2019) (see below), the “saturation bias” cited by Fromm et al, 2014 and Lurton et al. 2018 was caused by the OSIRIS V5.0 algorithm conservative approach in masking any data when the extinction exceeded $2.5 \times 10^{-3} \text{ km}^{-1}$. OMPS LP algorithm has no such restriction, and we are not aware of any detector saturation caused by this volcanic plume.

Rieger, L. A., Zawada, D. J., Bourassa, A. E., and Degenstein, D. A.: A multi-wavelength retrieval approach for improved OSIRIS aerosol extinction retrievals, *J. Geophys. Res.-Atmos.*, 124, 7286–7307, <https://doi.org/10.1029/2018JD029897>

Figure 14 and related discussion of CCC: Chouza et al (2020) <https://doi.org/10.5194/acp-20-6821-2020> do a very similar tracking of the CCC (although they do not describe in CCC terms). It is not cited by Gteam. There appears to be a similarity in how the CCC is tracked but then a divergence occurs in late August. Because of the similarity and relevance to this paper, Gteam are encouraged to read Chouza et al. and evaluate the

similarities and discrepancies between the two treatments of the CCC.

ANSWER 3: We have added the following text to the article:

“Figure 15 was published in December 2019 at AGU-2019 (Gorkavyi et al, 2019). On September 24, 2019 the CCC was observed by lidars in Hawaii (Chouza et al, 2020). Chouza et al (2020) traced the trajectory of this cloud back to July 17, 2019. Although the two studies were done independently, they came up with very similar results. Chouza et al (2020) consider this cloud as a Raikoke plume, but we prefer to call it CCC because it is a very small part of the Raikoke plume.”

Kloss et al. (2021) present a SAOD analysis very similar to Figure 7. In fact, the same version of OMPS LP retrieval is used for both. I did not see in GTeam what I expected, an acknowledgement of this previously published analysis and a motivation for presenting the analysis anew. The only substantial differences in the Gteam plot are an addition of two markers for pyroCb sources and a slightly later endpoint. Neither of these visual differences are taken advantage of, from my reading. If I missed it, please advise. Regardless, it would be important for Gteam to assess whether the figure should stay and also cite Kloss et al. for the earlier rendition.

ANSWER 4: We have replaced Figure 7 with plots of OMPS LP aerosol extinction at three different altitudes, 14.5, 18.5, and 20.5 km.

Minor Concerns

Note: Text below in quotation marks is directly from Gteam.

P4, L118: The schematic (Figure 3) shows a "feature" that is limited both horizontally and vertically. Why does this statement call out only the vertically limited aspect? What's the relative importance of limitations in the vertical versus horizontal?

ANSWER 5: See Answer 1

P4, L122: "If we believe that these lower altitude values do not represent a true aerosol signal..." How is this belief ascertained?

ANSWER 6: If we compare the lidar observations of the CCC (Figure 14) and the limb observations of this cloud (Figure 4), it becomes clear that all parts of the arch below 23 km are artifacts. There are many such examples. For example, PMCs are always 80 to 85 km high. According to the data of the limb sensor (see below, unpublished), these clouds have apparent heights of up to 25 km. All parts of the PMS observed below 80 km are artifacts from the arch effect.

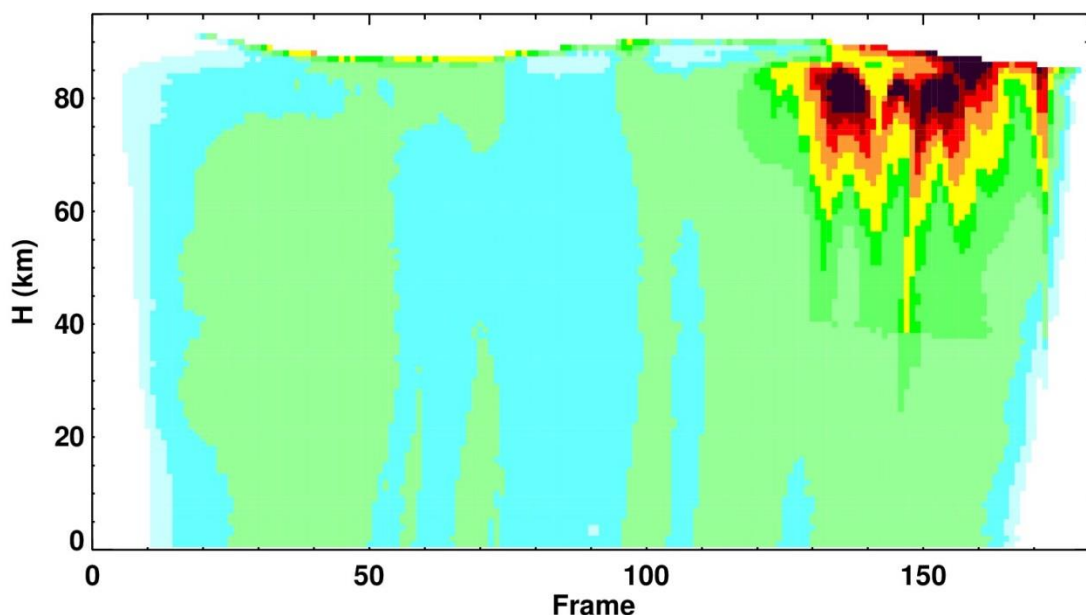


Figure 3 caption: The caption needs to describe the meaning of the horizontal solid and dash-dot line. Presumably these show

the OMPS instrument vertical FOV. But if so, why are the line styles different?

ANSWER 7: Now all lines are similar.

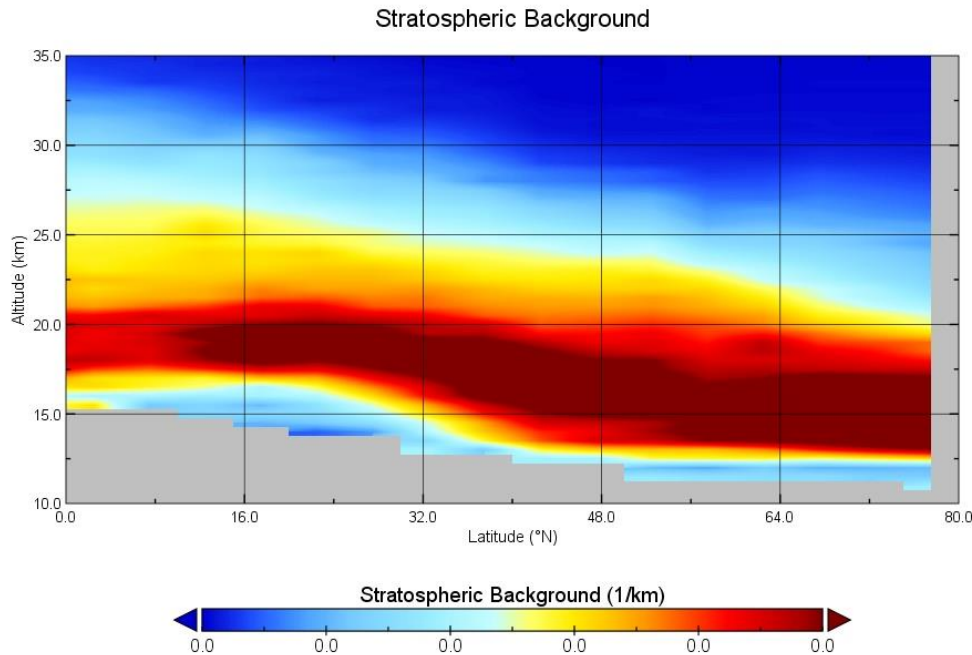
Figure 4. Please add an x-axis marking/labeling in deg. latitude, to give the reader a useful geographic reference frame.

ANSWER 8: Done

P6, L153: The Junge layer peaks much higher according to Kremser <https://doi.org/10.1002/2015RG000511> citing Junge. An examination of CALIOP data shows lots of Raikoke aerosols widespread, lower down, on both sides of the CCC.

ANSWER 9: Our statement “The aerosol layer at an altitude of 18-20 km with a decrease to the north to 15-16 km is the Junge layer” (P6, L153)” is in full accordance with LARC/NASA data (see figure below) and does not contradict the classical works: “stratospheric aerosol occurs in a distinct layer between 15 and 25 km altitude with a peak near 20 km [Junge et al., 1961]” (Kremser et al, <https://doi.org/10.1002/2015RG000511>). Also we added in text next statement after Fig.4:

“Note that CCC has been the southernmost part of the Raikoke plume since late July 2019 (see section 3.3). During this period, only the usual Junge aerosol layer was located south of latitude 20°N.”



P6, L158: Please define/describe "artifact density." Why is the term "density" used? Why not "extinction?"

ANSWER 10: "Density" was replaced by "extinction".

P6, L157-161: This paragraph has several vague or unintuitive terms, such as "real," "artifact density," "cleaned." Please replace these terms or define them.

ANSWER 11: "real" was replaced by "true"; "density" -> "extinction"; "cleaned" -> "corrected".

P6, L158: How is an "isolated" feature determined from OMPS data alone? There is no discussion of complementary data (e.g. OMPS nadir) informing the limb data.

ANSWER 12: We use CALIOP (and SAGE – see new Fig. 10) as an additional source of information.

P7, L185: “The sensitivity of the satellite data we examined...”
What satellite data? OMPS NM? According to Figures 5 and 7,
the aerosols are detected much longer.

ANSWER 13: We improve our statement: “The sensitivity of the satellite data (OMPS LP, CALIOP) we examined is such that the CCC from the Raikoke volcano was observable for 3 months following the eruption and the increase in the Junge aerosol layer was observed even longer.”

Figure 5 caption: “5: The daily zonal mean (45-85N) SO₂ mass (assuming a cloud height of 13 km) and the aerosol extinction coefficient at 675 nm (13-18 km.)” This is 6 OMPS LP altitude bins. Which extinction is it? Max, min, average?

ANSWER 14: We have clarified the caption for Fig. 5: “the average aerosol extinction coefficient at 675 nm (summed up over 13-18 km and divided by 6 (km) to get the average extinction coefficient).”

Figure 5 caption: “ash fallout” How does the reader know how much ash fallout is observed? What does ash fallout have to do with SO₂? Please explain.

ANSWER 15: The statement about “Ash fallout” was deleted.

Figure 5 caption: “...extinction starts to decrease due to gravitational sedimentation, but very slowly (from OMPS LP and NM data)” What does this mean? How do these inform about sedimentation? Sedimentation is one of several processes that can diminish extinction. How is sedimentation demonstrated herein?

ANSWER 16: We have clarified: “decrease due to gravitational sedimentation and other processes”

Figure 6 caption: "main plume" How is the "main plume" defined?" This is the first invocation of "main plume."

ANSWER 17: We have clarified: "the main plume (45-85°N)"

Figure 6 caption: "south branch" Is "south branch" synonymous with CCC, or is it more general? Perhaps just stick with "CCC" if they are one and the same.

ANSWER 18: "south branch" was replaced by CCC.

P9, L227: "After that, the detection limit of the instrument..." What is the detection limit? Please state the value.

ANSWER 19: We added next statement: "OMPS in the stratosphere can typically detect 0.2-0.3 DU of SO₂".

P9, L239: No pyroCb's are mentioned by Kloss et al. as far as I can tell. If they are, please provide some more detail. If not, what is the justification for adding these symbols to the figure? There is no evident increase of SAOD clearly attributable to these symbols in the plot.

ANSWER 20: We are fixing Fig. 7 with a caption and have clarified:

"In addition, Figures 7b and c also show the aerosol transport to subtropics and tropics at higher altitudes. Increased aerosol loading in the lower stratosphere can also be attributed to two pyroCumulonimbus (pyroCb) events that took place before and after the eruption, Alberta fires (June 18) and Siberian fires (July 2) (Kloss et al., 2021). OMPS LP detected both plumes in the stratosphere at 12-13 km, although it became difficult to separate them from Raikoke plume once it spread around the NH."

P10, L241: "The top panel in Fig. 8 shows that the maximum altitudes of the plume are around 25 km, when the plume

penetrates the tropics.” How is “when” shown? Figure 7 seems to show that Raikoke material only got to the subtropics.

ANSWER 21: We have replaced Figure 7 with plots of OMPS LP aerosol extinction at three different altitudes, 14.5, 18.5, and 20.5 km. The new figure clearly shows the plume transport to the tropics at altitudes 18.5 and 20.5 km.

Figure 8(a): Please explain the two apparent outliers of ~25 km layers over Northern Europe. They seem to be all by themselves; no indication of a ramp up to those altitudes from nearby layer observations.

ANSWER 22: We agree with the reviewer that the original figure was confusing. We have now modified the figure to show the day number instead of the maximum altitude since the reader can match the altitude in Fig8b with the location and day number in Fig 8a. In addition, we changed the maximum altitude period to only show the first four months of the plume altitude, which is 150 days since June. The maximum altitude estimate uncertainties increases as the plume move around and subside following the first 150 days. Fig 8a period now matches Fig8b white line, which wasn't the case in the previous version. The only difference is that Fig8b shows the location of the plume every two days for 150 days, while Fig8b is plotted every day for the same period. We have now modified the text accordingly.

Figure 8(a) caption: What is the number of days shown? Span of time? Are all the reported dates consecutive, or are there gaps? If there are significant gaps, these dates should be mentioned.

ANSWER 23: See our reply to the previous comment. The new caption and figure are clearly showing the period, which is 150 days since June, plotted every other day.

Figure 8 caption: The sAOD panel is not described.

ANSWER 24: We have added the following to the figure's caption "Figure 8(b): (top) Stratospheric aerosol optical depth ($\times 10^3$, sAOD) at 675 nm for latitudes and period similar to Figure 8b. The sAOD is derived by integrating aerosol extinction profiles above the tropopause to 35 km"

Figure 8(b) caption: How are the data comprising the white line determined? The white line doesn't conform closely to the color shading. I.e. it crosses extinction contours in time. Please explain fully.

ANSWER 25: The plume altitude is derived using the OMPS LP cloud algorithm, which can identify enhanced aerosol layers in the stratosphere. We have now added this to the text. The reason for the mismatch between the white line and the extinction contour lines is the color table used, which favors larger aerosol extinction values. We have now changed the color table to better show small extinction values.

Figure 9 caption: The Junge layer is higher than the range indicated here.

ANSWER 26: see Answer 9.

P16, L309: Specifically, how were the CCC detections done out to 22 September? I could not find a definitive explanation.

ANSWER 27: We added after Fig.13 (new): "Using the data of CALIOP, OMPS LP and NM as well as OMI"

Figure 14 caption: "OMI" is mentioned here for the first time as part of this analysis, but it is not mentioned in the Data and Methods section. This should be done, unless OMI is not part of the present analysis.

ANSWER 28: See ANSWER 27, also we added to end 2.1: "We also used OMI data as an additional source."

P18, L349: “A carbonaceous aerosol plume associated with wild fires in British Columbia in August 2017 reached the stratosphere a few days following the initial injection above the tropopause...” Confusing. It is stated that the plume was injected above the tropopause, but only reached the stratosphere a few days later. Both clauses are attributed to Torres et al. Please clarify.

ANSWER 29: The text has been changed: “A carbonaceous aerosol plume associated with wild fires in British Columbia in August 2017 reached the stratosphere a few days following the initial injection and resulting from self-lofting triggered by solar heating (Torres et al., 2020; see similar effect for Australian fires - Khaykin et al., 2020).”

P18, L352: “...19 to 26 kilometers is observed for a cloud consisting of sulfate aerosol.” From the material presented to this point, how does the reader know it is sulfate? If compositional information has not been provided, a citation is needed. Alternatively, the paper could just state that sulfate is assumed.

ANSWER 30: The text has been changed: “observed for a cloud presumably consisting of sulfate aerosol”

P18, L376: What are "large-scale mixing events?" Please explain using a meteorological perspective.

ANSWER 31: The text has been changed: "large-scale mixing events" -> “turbulent mixing”

P18, L279: “SO₂ reduction” is vague and general. Provide more detail to this description, such as "e-folding time"

ANSWER 32: The text has been changed: “SO₂ reduction” -> “e-folding time”

P18, L383: "...the CCC circled the globe almost three times at the latitude of 30oN..." The CCC ended up almost 10 deg. south of this according to figures herein. Perhaps elaborate a little more on the CCC's slow movement even south of 30.

ANSWER 33: We added: "(during September, the CCC shifted south to 15-20°N)"

P18, L383: "...increased its height from 19 to 28 km." "28" is higher than shown in Fig. 14. The text in the body of the paper only gives pressure and theta. Suggestion: enhance the description of the endpoint altitude in the main text and state "26 km" or whatever actual number the authors think is represented by the figure.

ANSWER 34: The text has been changed: "28 km" -> "26 km"

P18, L387-389: This sentence is confusing, combining 3 ideas: arch effect, 45 km background, and aerosol that can pollute the background. Please flesh out these points more exhaustively.

ANSWER 35: We transfer this sentence to end 2.2 with clarification.

More generally, the Conclusion section is too thin, and doesn't flow logically from points made in the body. A thorough rework of this section is called for.

ANSWER 36: The conclusion has been improved and expanded.

ANSWER 37: We have corrected all errors and taken into account all the comments of the reviewer made in the comments to the text of the article.