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 SO 2 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 SO 2 column.

ANSWER 2: We added next statement: "For large volcanic SO_2 signals like Raikoke, comparisons between TROPOMI and SNPP/OMPS so far (for several eruptions) show little bias, with the total SO_2 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 SO_2 (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 related 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 SO 2 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 l.228: "more and more pixels with SO2 fall below the detection limit of the OMPS NM sensor" Does this mean that only pixels where SO 2 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 l.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., H¨opfner, 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 l.165: Livesay → Livesey and Zawanda → Zawada

p.18 l.369: “Figure15” → “Figure 15a”

ANSWER 16: We have fixed all the mentioned errors.