NEW ANSWERS to RC1 (07/30/2021)

Review 1 of amt-2021-58

The authors have addressed most of my initial concerns, however I still take issue with the implemented “arch effect” correction which I believe needs to be considered before the manuscript can be published.

General Comments

In my opinion the correction is not really necessary for the analysis that follows, but if it is to be included then statements made about it need to be justified which in many places they are not. To be clear, the authors use the correction to attempt to improve the resulting extinction profiles in two, somewhat coupled, ways: first to get a better sense of the locality of the Raikoke plume, and second to get a better extinction estimates from OMPS-LP. The first point is fine, I take no issue with, and might be important for the discussion for the CCC. The second point is the one that I have a problem with. There is no evidence presented that the correction is improving the extinction, and in fact I believe it is making it worse. As I stated previously my intuition is that a 1D retrieval is smoothing the true field into the shape of an arch, not simply introducing unphysical arches. This means that the overall extinction loading is relatively unaffected by the biases in a 1D retrieval, and yet the correction is significantly reducing the loading. Now, I fully admit that I might be wrong, but there is no justification at all in the manuscript for the efficacy of the correction and there are figures in the manuscript that suggest the correction is not working (see specific comments). If justification for the correction cannot be provided then my preference would be to remove it, or at least use it only to assess the locality of the plume and not the overall loading.

ANSWER 1: We have added to the new version of the article a new figure 4b and its discussion:

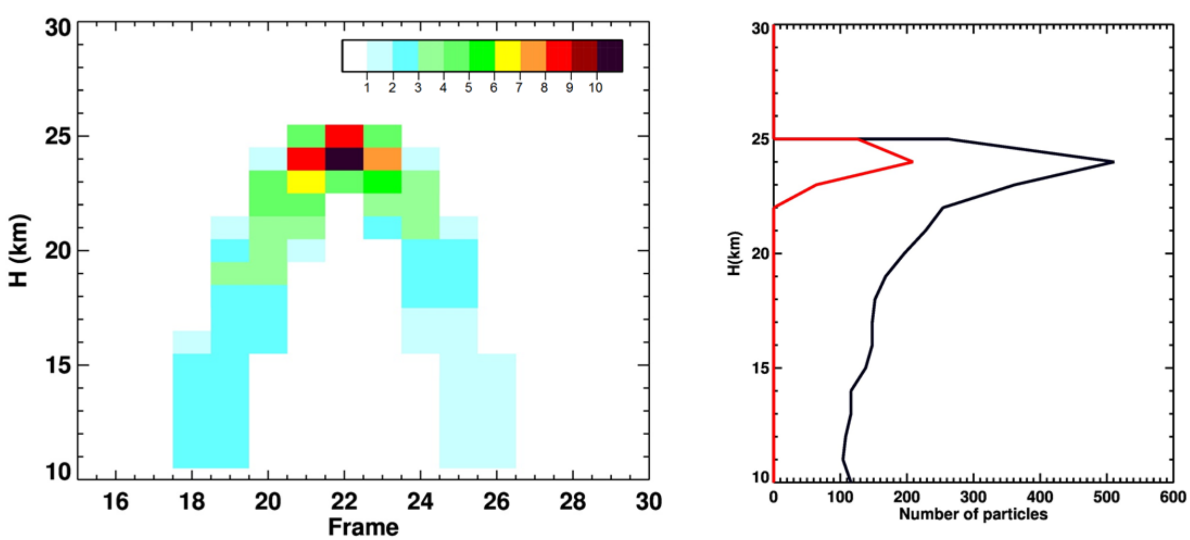

Fig 4b. (Left) An arch that appears during the limb observation of an aerosol cloud 2 km thick (H = 23-25 km) and 2° long. Each frame corresponds to a satellite orbital shift by 1°. Radiance from aerosol is proportional to the number of observed particles, taking into account the distance to them (the left part of the arch is slightly brighter than the right, because on frames 17-21 the cloud was closer to the satellite than on frames 23-27). The radiance units are arbitrary. (Right). The red line is the profile of a cloud of 400 particles, which is observed on frame 22 (middle of the arch in Fig. 4a). The black line is the number of visible particles, summed over all frames, that is, over the entire arch.

“The reality of the discussed "arch effect" is confirmed by simple modeling. Arch model in Fig. 4b was obtained by direct modeling of a cloud of 400 particles located at the nodes of a uniform grid (20 particles are distributed at a length of 2 degrees, and 20 rows of particles are uniformly distributed along the radius in the range of 23-25 km). The model does not use radiative transfer models. Fig. 4b (left) only the distance between the particles and the satellite is taken into account, while Fig. 4b (right) shows the simply visible (for the limb sensor) number of particles at a given altitude h (in 1 km step). The red line corresponds to a one-time observation of a cloud located at the tangent point at an altitude of h = 22-25 km (an increase in the apparent thickness of the cloud by 1 km is associated with the curvature of the Earth, which is why the cloud itself turns out to be curved - see Fig. 3). That is, it is the most realistic observation of the cloud at its optimal location. The black line shows the sum of the cloud particles observed at different times. As a result of this summation, the number of particles visible at a given observed height h (which differs from the real constant cloud height H = 23-25 km) turns out to be overestimated. Therefore, when analyzing the picture in Fig. 4, we must remember that it is composed of frames received at different times, so the one cloud will be registered many times. The same effect of multiple registration will be observed for clouds of any complexity and configuration, including a uniform aerosol layer, because any cloud can be divided into a large number of elementary pieces, similar to a simple compact cloud considered in Fig. 4b.

The considered model of the "arch effect" does not depend on the specific model of radiation transfer and the methods of retrieval of the spatial distribution of aerosol. Therefore, for specific limb sensors (OMPS-LP, SAGE III, OSIRIS), it is necessary to evaluate how accurately the available retrieval packages handle compact clouds. This is especially true for 1D retrieval methods, which assume spherical symmetry of the atmosphere and which are used to obtain aerosol extinction in the OMPS/LP. Obviously, the arch effect for a spherically uniform aerosol layer should be fully compensated by the 1D retrieval model. But the further the real system is from the spherical symmetry, the more difficult it will be to take into account the “arch effect””.

Discussion of the problems of various retrieval and RTM for limb sensors is beyond the scope of this article. We have shown the importance of the “arch effect” on a simple model and in one specific case (as a maximal estimation) and raised the question of how effectively the various retrieval methods deal with this problem. If we exclude the discussion and assessment of the "arch effect", then the problem of the efficiency of processing compact clouds is out of the field of view of researchers.

As I previously stated, I would also be fine if the correction was presented as a potential source of error instead of an actual correction. This is still okay, but it would mean that the correction is not applied and the differences are only used as uncertainty estimates. The authors additional statement that “We consider this procedure of correction only as an estimate” does not go far enough since they then continue to use the corrected extinction as gospel. I have a few specific comments below, all related to the arch effect.

ANSWER 2: We are grateful to the referee for this important discussion, which helped to clarify a lot about the "arch effect". We hope that the new edition of the article (see also Answers 1 and 2) has taken into account all the main comments of the reviewer.

Specific Comments

p.5 l.153: The statement “If we believe that these lower altitude values do not represent a true aerosol signal, we need to apply a correction in order to accurately determine overall aerosol loading.” neglects the fact that a 1D retrieval is probably smoothing the true field, reducing the extinction inside the plume in addition to creating arches. Performing the correction will do more harm than good if the 1d effect is more of a smoothing effect.

ANSWER 3: In the new version of the article, we have separated the "arch effect" from the possible problems of retrieval methods. The effectiveness of specific techniques in relation to the "arch effect" should be examined on a case-by-case basis (for different limb sensors and retrieval and RTM package).

p.8 l.210: The statement “The arch effect characteristic of the limb observations should be taken into account when calculating the optical thickness of aerosol clouds” simply is not justified with any of the information presented in the manuscript. If the effect is to be included there needs to be some evidence provided that it is improving the result.

ANSWER 4: See Answer 1 and new addition in the paper.

Figure 5: I have the same comment I had last time since I do not believe it was sufficiently answered. Why is the correction reducing the extinction by ~50% before the eruption even happens? To me this is evidence that the correction is not working as it should. Essentially this is saying that every limb sounder is overestimating aerosol extinction by upwards of 30-50% in clear conditions?

ANSWER 5: We update Fig.5 adding arrows as error bars. Also next statement added in the new version of the paper (see also Answer 1).:

“To estimate the possible retrieval uncertainty due to the "arch effect", we apply a simple compensation method for one specific case of the Raikoke aerosol cloud (see next section). This compensation method assumes that the considered aerosol clouds form compact clusters or a highly heterogeneous aerosol layer, and that the arch effect was not taken into account in retrieval. Thus, this example should be regarded as a maximal estimation for the "arch effect". Where this assumption is not valid, our correction will be overestimated, as, for example, happened with the correction of the background aerosol value (see Fig. 5), which was observed before the Raikoke eruption”.

Figure 10: This is maybe more of a comment. SAGE III is affected by the same 1D retrieval bias, the authors are suggesting that it could be biased by 30-40%, so why is it usable here? I realize

the same correction cannot be applied to the sparse sampling of SAGE III, but the fact that the manuscript does not mention this issue seems odd if it is as important as the authors claim.

ANSWER 6: We not rejected any OMPS-LP and SAGE III aerosol limb data: we used this data in seven figures in our paper. But we discuss “arch effect” and apply this effect for estimation maximal possible errors for one specific case (and for one graph on Fig 5). This estimation is show the problem which must be investigated in future (see Answer 2).

1p.22 l.469: “We have shown that this effect is significant” the only way to show this effect is significant is to simulate 2D radiances and then retrieve in 1D. You have shown that a very specific correction technique, which may or may not work, introduces significant differences.

ANSWER 7: See Answer 1 and new addition in the paper

NEW ANSWERS to RC2

Review of GTeam response to reviewers and new manuscript. By Mike Fromm

My summary assessment is that this manuscript merits publication in AMT if GTeam strengthen their “arch effect” application to the very complex lower stratospheric, freshly perturbed volcanic aerosol condition that is the focus of this paper, in accord with suggestions detailed below.

GTeam have made substantial improvements and responded to every reviewer comment. The revised manuscript is greatly improved. In particular, GTeam have bolstered and clarified their treatment of the “arch effect,” which is the paper’s primary new contribution to the AMT literature in my assessment. Particularly helpful to the reviewer was their illustration of the arch effect for a mesospheric cloud. GTeam succeeded in clarifying how lower stratospheric clouds might produce the arch effect.

However, in doing so, GTeam's response to reviewers and the revised manuscript elucidate the reviewers' original concerns. By using the PMC as an example, GTeam nicely show how a simple cloud of limited vertical extent, horizontally flat, and singular in the view of OMPS-LP may be well represented by the model they construct in Figure 3. But this example is vastly different from the preponderance if scenes in the lower stratosphere post Raikoke.

ANSWER 8: We have added to the new version of the article a new figure 4b and its detailed discussion (see ANSWER 1). In the new version of the article, we examined a cloud in the stratosphere at an altitude of 23-25 km, corresponding to the Raikoke case (see Fig. 4b).

Based strictly on the CALIOP curtains shown by GTeam in Figures 12 and 14, it is obvious that multiple, stacked sulfate layers were the rule in the post-Raikoke stratosphere, unlike tie simple, single-layer geometry in the case of PMCs. That's not to say that the arch effect doesn't occur for lower stratospheric clouds, it is just to say that a convoluted, multi-layer condition (such as Fig's 12 and 14) either compromises the arch-effect interpretation or could even create a false arch effect if the cloud is itself arched or sloped in any manner. An example is the Raikoke sulfate layer sampled by CALIOP over northern Canada, seen here.
https://www-

calipso.larc.nasa.gov/products/lidar/browse_images/show_v4_detail.php?s=production&v=V4-10&browse_date=2019-07-06&orbit_time=10-50-31&page=4&granule_name=CAL_LID_L1-Standard-V4-10.2019-07-06T10-50-31ZD.hdf
This goes to my original question as to statements such as this on line 154: "If we believe that these lower altitude values do not represent a true aerosol signal…" There is no robust basis for relying on said belief when it is straightforward to test such suppositions with complementary (and regularly near coincident) data like CALIOP. Specific to the presumed OMPS-LP arch effect displayed in Fig. 4, CALIOP data in Fig. 14 make clear that indeed lower volcanic layers were north, south, and underneath the CCC that GTeam suggest was responsible for the arch effect in Fig. 4.

Given the clarification GTeam provide in Fig. 3 and the recognition that the example used herein (the 31 August CCC) for the arch effect is far from ideal (noting the large extinction of the CCC feature and the additional layers below), my suggestion is for GTeam to select another scene with which to make the arch-effect case. The ideal scene would be akin to the PMC example they trust: flat in real altitude and singular from the OMPS viewpoint. CALIOP would provide the independent standard, with an unambiguously single-layer, non-sloping cloud of horizontal extent consistent with the argument and model manifested in Fig. 3.

ANSWER 9: Discussion in new version of the paper covers these topics. The same “arch effect” will be observed for clouds of any complexity and configuration, including a uniform aerosol layer, because any cloud can be divided into a large number of elementary pieces, similar to a simple compact cloud considered in Fig. 4b. We plan to analyzed different cases from these reviewer’s comments in next paper (preliminary we did this for simple case: see below that for long cloud ~10° we have not “arch” but “mushroom”, but decide not overloaded current paper).

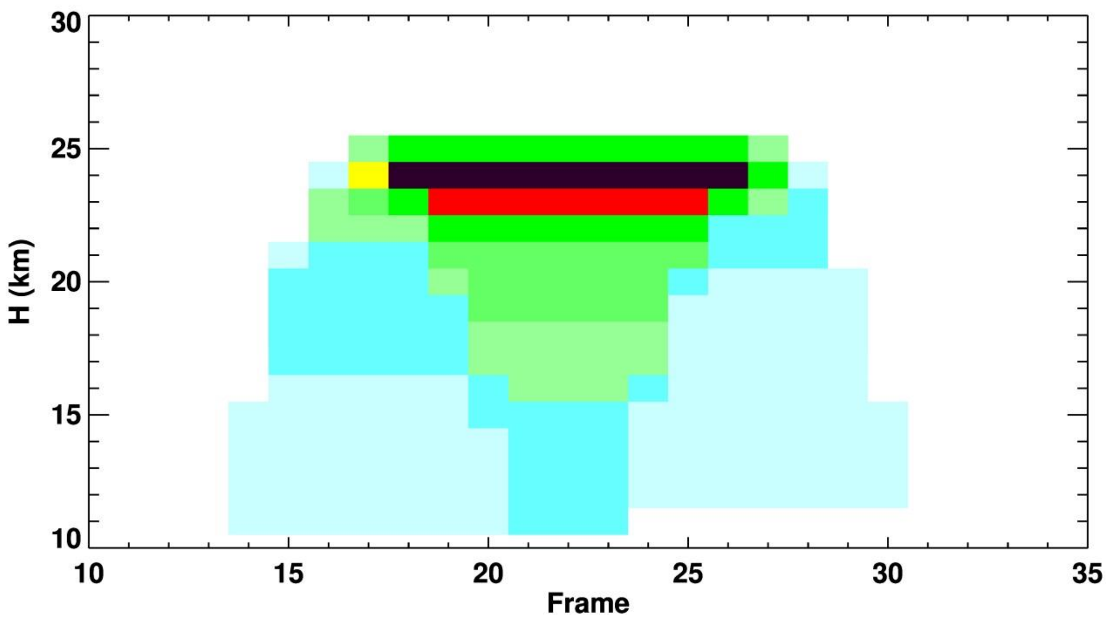

Fig. Arch effect for long $\sim 10^\circ$ cloud (some conditions as on Fig.4b)

Regarding my question in the original review of saturation effect on OMPS extinction, I recognize that the OSIRIS situation is technically different from OMPS, but it is still apparent that OMPS (and all other historical limb-view sensors) cannot faithfully characterize extinctions across full range in the landscape of the Raikoke plume. The 31cAugust, 1 September 2019 CCC case was mentioned in the first review. According to CALIOP backscatter, the actual maximum extinction presented to OMPS in scenes such as this far exceeds the maximum OMPS extinction in the OMPS record. The OMPS data I have at hand (about a year's worth of global profiles) doesn't represent the full OMPS record, but it appears that extinctions do not exceed values of about .005/km at 675 nm. Given that CALIOP-based evidence speaks to much larger values being commonplace in the Raikoke plume, some discussion of OMPS low bias, for whatever the correct technical reason, should be presented.

ANSWER 10: While there is no hard cutoff limit in the OMPS LP retrieval algorithm, there is a restriction on how much the retrieved aerosol extinction is allowed to grow per iteration at each altitude relative to the first guess, (see Loughman et al., 2018); Taha et al., 2021), which "may" cause an underestimation of the retrieved aerosol if the aerosol is too large. This constrain was further relaxed in the V2.0 algorithm (Taha et al., 2020). The CCC case was an interesting one given that the plume was a mixture of SO_2 and aerosol (as confirmed by TROPOMI and OMPS NM), which raises questions about the ability of limb scattering instruments to accurately measure SO_2. In any case, more detailed work is needed to validate the algorithm performance during large eruptions and pyroCbs, which is subject to a separate study and beyond the scope of this work.

The following sentence is now added after Fig.10 “Given that this plume is mostly composed of SO_2 and aerosol (Fig 12), it is likely that OMPS LP is underestimating its magnitude”

Regarding my concerns with the Junge layer attribution in parts of the manuscript, I remain unconvinced by GTeam’s explanation. By definition, the Junge layer is a background condition. The features that GTeam attribute to the Junge layer are definitely enhancement to the background (The figure they present in the author responses is quantifiably vague.).

ANSWER 11: We have changed the term “Junge layer” to more general “aerosol layer” or “background aerosol layer”.

GTeam claim that there were no volcanic aerosols south of about 20N, but clearly there were, likely attributable to Ulawun, which is clearly shown in the paper. Hence in the period analyzed herein, there were simultaneous stratospheric sulfate aerosol layers from northern tropics to high latitudes.

ANSWER 12: We deleted the statement “During this period, only the usual Junge aerosol layer was located south of latitude 20°N”.

Upon this re-review, I noticed that GTeam stated that due to instrument sensitivities, the Raikoke aerosols were only detected for 3 months. This is incompatible with several figures shown herein. If this assessment is to remain, it must be attributed to specific visualizations herein and not contradicted by any other.

ANSWER 13: This statement was corrected: “The sensitivity of the satellite data (OMPS LP, CALIOP) we examined is such that the aerosol cloud from the Raikoke volcano was observable for many months following the eruption.”