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. 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.

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.

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.). 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.

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.