The paper describes a new aerosol dataset that is derived from the stellar occultation instrument, GOMOS. The authors use an un-conventual retrieval, which relied on zonal and monthly averaging of the level 1 transmittance, to reduce the noise levels and scintillation effect. They also compare the new product to SAGE II and SCIAMACHY aerosol profiles and GOMOS's previous retrieval. The authors could have done a better job enhancing the comparison of the new version with correlative measurements and providing some estimates of the accuracy and precision of their product. The authors should have also recommended the wavelengths that are the most accurate and most suitable for scientific studies. I find the paper suitable for publication in AMT, subject to major corrections.

Major points:

Figures 3, 4, 5, 6, and 7: The authors' extensive focus on the single averaged profile "September 2002, 10°-20° S" across five different figures, particularly since the profile primarily comprises background aerosol and clouds, seems puzzling to me. To improve the paper, I recommend consolidating these five figures into one figure with multiple panels. It might be beneficial if the authors incorporate additional cases, such as diverse volcanic eruptions and latitudes, to provide a better understanding of the new product's quality.

Figures 9, 10, and 11: Similarly, the figures are repetitive and need to say more about the quality of the FMI retrievals. The plots can easily combine into one figure with three panels. There is no need to show all 9 Wavelengths as it serves no purpose. Instead, the authors should select one reference wavelength for each retrieval, maybe 750 nm, and plot it for each zone. Additionally, displaying individual instruments rather than merged records could prevent potential distortion in the comparison, as each instrument carries its distinct biases.

Specific comments:

L73: “The proposed new algorithm for aerosol retrievals is based on the removal of extinctions due to the Rayleigh scattering and absorption by ozone and other trace gases from GOMOS transmission spectra.”

This is partly true. The new algorithm is also based on averaging the transmittance zonally and monthly. Please revise the above statement.
The paper only presents the data set's average band steps in the summary section. It would be useful to include this information in section 2 as well, providing an explanation regarding the rationale for selecting these specific average band steps.

L92: “The outlier filtering is performed using an algorithm based on …”

Can you explain the cause of the outliers and how often it exists in the data?

L94: “For each tangent altitude and each wavelength, the average transmittance is computed as the weighted median transmittance. This estimate is insensitive to outliers, … ”

Using a median instead of a mean may potentially exclude isolated events such as pyroCB or the early stages of a volcanic eruption, which are considered outliers by definition. The authors should address this concern within the text to acknowledge the inherent limitation of using a median for data analysis and its possible impact on capturing rare but significant events.

L102: "…increased aerosols after Soufrière Hills, Rabaul and Tavurvur volcanic eruptions"

I don't believe Rabaul and Tavurvur eruptions reached the stratosphere or anywhere near 20 km. Please check eruption altitudes and modify the text.

Figure 1: Can you clarify the start and end altitudes for each panel?

L103: "It is clearly seen that the transmittances are lower (optical depth is higher) for the volcanic aerosol conditions (compare transmittances at 20 km, thick red lines)."

The low transmittance is observable both above and below the red line. Please refine the sentence and corelate the observed changes in the transmittance with the volcanic eruption injection altitudes.

L114-L118: "The NO2 and NO3 optical depths are computed using retrieved GOMOS NO2 and NO3 profiles" "As in ALGOM2S retrievals (Sofieva et al., 2017), ozone optical depth is computed using a DOAS-type retrieval with the triplet in the Chappuis band"

It would be helpful to clarify the advantage of directly retrieving ozone while resorting to averaged NO_2 and NO_3 official retrievals, instead of either retrieving or averaging all species?

L119: "In the processing, we filtered out unreliable averaged transmittance …"

Why does the filtering process not involve individual unreliable transmittance instead of the average?

Figure 2: The selected wavelength symbol needs to be clarified. Can you choose different color and symbol?

L148: "One can notice the expected spectral dependence of aerosol extinction that is larger for shorter wavelengths."

It would be useful to provide an explanation supported by a reliable reference why this spectral dependence is expected. The 400 and 440 nm are short wavelengths, yet they show a stark difference near the tropopause.

L148-L158: The description of the profiles displayed in Figure 3 and the subsequent discussion of the Angstrom exponent lacks clarity. It would be beneficial to explicitly identify the type of aerosol profile depicted in the figure. Considering the observations, it appears that the figure shows a background aerosol layer spanning from 20-25 km and a cloud layer near the tropopause. There seems to be a discrepancy in the Angstrom exponent values, as typically, a

background aerosol layer would exhibit an Angstrom exponent range between 2 and 2.5, indicating smaller aerosol particles, rather than the values of 1 and 1.5 observed. Conversely, a near-zero Angstrom exponent would be expected for the cloud layer, contrasting with the reported values of 1.8 and 3. Addressing these discrepancies and aligning the observed values with the anticipated norms for different aerosol layers would significantly strengthen the analysis.
The repeated assertion in both the Abstract and Summary sections regarding the realistic wavelength dependence in the retrieved aerosol extinction profiles, which is based on this figure, requires additional substantiation. The authors need to provide further evidence or rationale supporting this claim.

L161: “on Earth Radiation Budget satellite, ..”

Add (ERBS). Also, to be consistent, spell out ODIN and ENVISAT.

Table 1: The title "Retrieval method/data" and the entries need rectification. I propose changing it to a more accurate label, such as "Measurement/data." I also suggest specifying the types of measurements, such as solar or stellar occultation and limb scattering, corresponding to each instrument.
The information provided for OSIRIS requires correction, and it could be revised to "Radiances in UV/VIS".
There is a need to clarify the specific version used for OSIRIS aerosol data. If the authors indeed used V6.0 and not V7.0, it's important to acknowledge that V6.0 is an older, discontinued retrieval, no longer accessible online and not extending to the present time. If this is the case, the text should be amended accordingly. Additionally, retrieval method was only included for SCIAMACHY. Either include the retrieval method for all instruments or remove it from the SCIAMACHY entry to maintain uniformity.
Regarding the use of two different SCIAMACHY products for intercomparison, it could potentially lead to confusion. It would be advantageous to either select the most accurate product or explicitly justify the use of two products, providing a clear rationale for their inclusion.

Figure 4: Improvements are needed in the figure and the subsequent discussion. To enhance clarity, I recommend creating a second panel displaying the percentage difference between the two profiles. This addition will help illustrate the agreement between the profiles, particularly in regions with low aerosol values above 20 km. The existing cloud contamination around ~16 km is noticeable and may divert attention from the primary purpose of the figure, which is to compare aerosol retrievals from both instruments. My suggestion is to display solely the stratospheric portion of the aerosol profiles, reducing the interference from clouds and eliminating the necessity to show an unfiltered SAGE profile. After making these adjustments, I recommend modifying the text to further discuss the differences between the two isntruments.

L174: “Although the spectral dependence of aerosol extinction profiles is similar above 20 km, it differs below 20 km.”

I don’t think the reader can reach the same conclusion by looking at Figure 5. Please add an Angstrom exponent or wavelength ratio plot for both instruments (similar to Figure 6) and modify the text accordingly. As previously suggested, it's advisable to refrain from displaying the tropospheric part of the profiles.

L207: “In general, GOMOS aerosol extinction profiles are very close to those of other instruments above 20 km.”

Again, I don’t see how the reader can form any opinion about the differences between the FMI retrieval and other instruments just by looking at Figure 7. The figure is dominated by clouds, which is not subject to this intercomparison. As suggested above, the authors should plot only the stratospheric part of the profile and adjust the x-axis scale accordingly. They should also include two more panels showing the percent difference for each wavelength and modify the text to comment on the differences compared to each instrument reported accuracy.

L226: "we filtered out the data points that have unrealistic values for the Ångström exponent (a>4 below 27 km) or are potentially affected by clouds (a<-0.2).”

The filtering criteria, while reasonable, heavily rely on the assumption that all wavelength retrievals are consistently accurate, which might not always hold true, especially when wavelengths are in proximity to strong absorbers. As shown in Figure 3, the cloud criterion (a<-0.2) failed to detect the cloud near the tropopause. Please provide further insight on how effective this filter is.

Figure 8: I don’t see the significance of plotting the whole record at different wavelengths rather than different altitudes, at least not by reading the text. I suggest plotting data at different altitudes instead. The discussion of the figure seems inadequate and could be enhanced. Given the extensive ten-year aerosol record, there should be interesting and noteworthy features worth highlighting.

“Table 2. The list of volcanic eruptions and strong wildfires.”

In the table, there is reference to a single fire instead of multiple fires, which might be misleading. Additionally, numerous eruptions mentioned in the table seem insignificant as they did not reach the stratosphere and consequently were not detected by GOMOS. It would be advisable for the authors to revise the table by specifying the eruption altitude instead of the Volcanic Explosivity Index (VEI). Moreover, removing any low-altitude eruptions, like Eyjafjallajokull, from the table could enhance its relevance.

L272: “In order to use GOMOS aerosol profile time series in the merged Climate Data of Stratospheric Aerosols, CREST (Sofieva et al: Climate Data Record of Stratospheric aerosols, 2023, in preparation)”.

If the utilization of GOMOS data in CREST records is one of the primary objectives of this paper, it should be clearly stated at the beginning of the paper for better alignment with the

study's aims. Also, remove the "in preparation" reference, as this type of reference might not be allowed.

L276: “for typical aerosol particle size distributions …”

Change to “for typical stratospheric aerosol particle size distributions …”

L281: “We found that the best agreement with the merged aerosol extinction time series is for FMI-GOMOSaero aerosol extinction converted from 672 nm.”

The paper lacks a clear justification for selecting 672 nm over 750 nm. The two wavelengths look similar in Figure 11 and are shown only at a single zone and altitude. Presenting an in-depth analysis of the accuracy, or lack thereof, for both retrieved wavelengths and recommending their use in scientific studies would significantly strengthen the discussion in section 3.

L284: In the Summary and Discussion section, the title should be adjusted to "Summary and Conclusion" to accurately reflect the content. As the section contains no discussion but rather a summarization and final insights.

L297: “In the future, the developed multi-wavelength dataset of aerosol extinction profiles can be used for retrievals of particle size distribution.”

The statement appears somewhat premature, particularly considering the authors lack of trust in the retrieved long wavelength, 750 nm. It would be advisable to either provide additional evidence to substantiate this statement or reconsider its inclusion.

It seems there is no section designated for Data Availability or a link provided for accessing all the data utilized in the study. A Data availability section should be included to confirm whether this data is publicly available, along with the corresponding link or repository details.