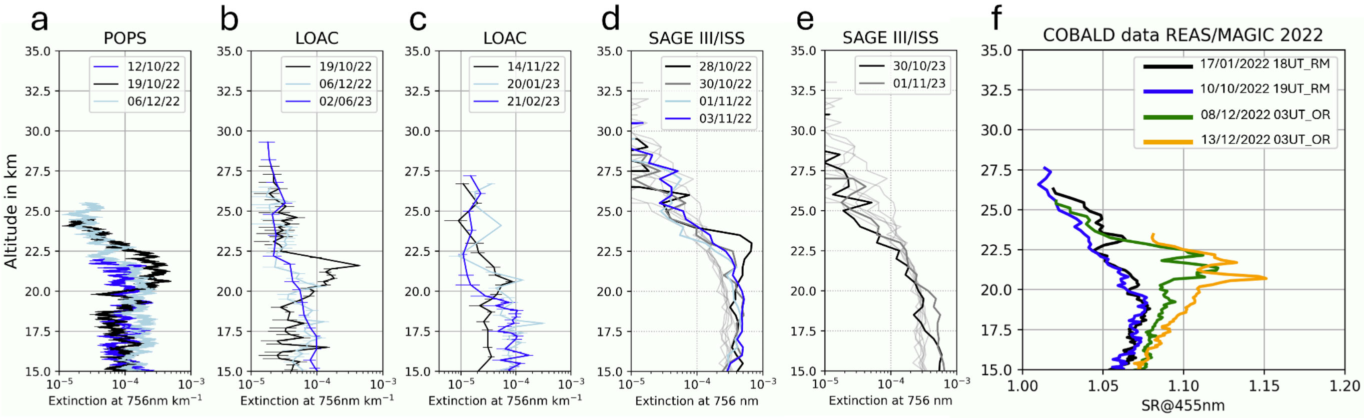
While we are appreciative to the reviewer for taking the time to go through the manuscript and give feedback, as a first thing we want to point out that we feel that the tone adopted in the review is less professional than desirable. This is a manuscript which has already passed many eyes, opinions, 1 ACP review and a lot of long work. The phrasing of the review below sounds to our ears - in parts - disrespectful towards us and the work that has already been done. This said, we have put another round of in depth work into this manuscript to improve it following the reviewers comments.

The authors do not do justice to one of their stated objectives, lines 114-116, “Additionally, SAGE III/ISS observations at 756 nm are employed to validate aerosol plume signatures detected by optical particle counters (OPCs), ensuring consistency across measurement techniques.” If the authors were serious about this statement, they would calculate OPC extinction coefficients at the wavelengths of the corresponding satellite observations. They don’t do this, but rather calculate OPC extinctions at 500 nm, use OMPS extinctions at 675 nm, and SAGE III extinction coefficients at 756 nm. A good compromise wavelength to allow the comparison of all three would be 675 nm. SAGE III has measurements at 676 nm. Is there a reason not to do that? Additionally if the authors were serious about their validation objective they could show a single plot with extinctions coefficients from OPCs and satellites in October 2022 when there is an impact from Hunga. Such a figure would be of great interest and would not detract from the point of this paper, but rather put the various extinction coefficient measurements in context.

Generally, channel 676 nm should be avoided in vertical regions with low aerosol concentrations (UTLS and above the main aerosol layer), please refer to Wang et al., 2020 in JGR. Hence, we prefer using the 756 nm channel by SAGE III/ISS. However, we agree that homogeneous wavelengths within Figure 2 would be beneficial and have recalculated the OPC wavelengths to match 756 nm from SAGE III. We also added the COBALD measurements to Figure 2, as suggested below.



We are not aiming at validating data but show confirmation of aerosol features from independent datasets. We changed the word *"validate" from line 115 to "confirm".*

We like the idea of showing POPS and LOAC observations from 19/10/22 within one plot and have added this to the Appendix Figure A2.

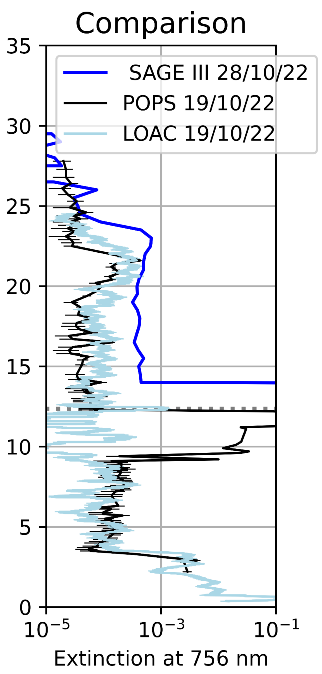


Figure A2: Comparison measurements by LOAC, POPS and closest SAGE III/ISS aerosol extinction observations, respective to Figure 2.

Added text within the manuscript: *Extinction profiles from POPS, LOAC on 19 October 2022 and the closest SAGE III/ISS profile are shown within one Figure respectively in Fig. A2. The measurements show good comparability, with the presence of an enhancement above 20 km. SAGEIII observations tend to show higher values considering that the extinctions from the OPCs were calculated from the limited size range of the instrument. The comparison is also limited by measurement uncertainties, place and time deviations.*

Further detailed comments on these points and others follow here by line/figure number. Some of these comments are redundant to what was just said about instrument comparison since they were written as the manuscript was read.

Abstract: It would be helpful to know the wavelength assumed for the AOD estimates.

We added: *'… across the SAGE III/ISS wavelengths.'* for explanation.

62 "… measurements of aerosol size distributions from 200 nm to 50 μm and partial aerosol concentrations corresponding to the measured size range …" The authors do not explain either here or in the response to review what they mean by partial aerosol concentrations, nor how that is different than aerosol size distributions, which can either be cumulative or differential. Some instruments measure the cumulative and some the differential, but with either one, the other can be derived. What does one do with a partial aerosol concentration, and what is it? The word partial implies there are parts of the aerosol concentration which are not measured. Clearly that part of the distribution outside of the size range covered would be one. But the authors do not state that this is implied, and anyway that is already clear from the statement about the size range of the instrument.

We changed this accordingly and removed the word 'partial'.

Same question applies to the text on line 81. But at least on line 82 we find that the authors actually mean differential distribution, which is well known in the community, and thus not sometimes, but always, called such. Why use language not familiar to the aerosol community and thereby obscure what is really measured by these instruments ?
If an instrument measures the cumulative size distribution it can easily be used to derive the differential size distribution and vice versa. Instruments don't measure one and also the other at the same time. Depending on how they are designed they measure one or the other. It doesn't make sense to state they measure both.

We changed the text accordingly: *POPS measures aerosol properties in the atmosphere at a wavelength of 405 nm and weighs around 1 kg, including batteries (Todt et al., 2023). The instrument provides data on aerosol size distributions from 140 nm to about 2.5 µm and the differential aerosol concentration, corresponding to the respective size range.*

69 The time sampling … to end of paragraph is redundant to the statements just above where it was stated much more clearly. There is no need for this text, which is a holdover from the first version.

This is true and was changed accordingly. Thank you.

114-116 The OPC data are used to calculate extinction at 500 nm, according to their introduction, yet here the SAGE III data to be used are at 756 nm, even though the authors state, "Additionally, SAGE III/ISS observations at 756 nm are employed to validate aerosol plume signatures detected by optical particle counters (OPCs), ensuring consistency across measurement techniques." Right good idea. So why don't the authors used 521 nm from SAGE III so that these two data sets can be more carefully compared? In fact why don't the authors calculate extinction coefficient at the SAGE III wavelengths they are going to use for the comparison? There several choices, 521, 676, 756 nm. If 676 was used it would allow OMPS to be included in the comparison. While such a comparison may be a little off the main point of this paper, it would add additional interest and confidence in the data sets used, which would enhance the points of this paper, and add to the stated objective above.

We calculated extinction coefficients according to the SAGE III wavelength 756 nm, see above. The other mentioned wavelengths are not the most recommended for scientific purposes, please refer to Wang et al. (2020). We cannot really include individual profiles from OMPS in Fig 2 because of noise for a limb-viewing satellite (requiring averaging over time and space) and for the readability of the figure.

In the text:

Aerosol counting observations have been converted to extinction coefficients at 756 nm using a Mie scattering model assuming sulphuric acid droplets

POPS size distributions have also been converted to extinction at 756 nm.

122 Supplementary figure A3 is called out before A1 and A2. Why aren't the supplementary figures organized according to their call outs in the manuscript?

The order of the Appendix Figures has been changed.

Figure A1. How is this figure useful for this manuscript? What is the reason to expand the extinction coefficient to 4 orders of magnitude and include the profile from the surface? The discussion of this figure only focuses on the large extinction coefficients below the tropopause which are attributed to a cirrus cloud. Is the reason so that the authors can show the LOAC is sensitive to cirrus clouds? But this is off the paper's topic. The interest here is in the stratosphere. There is no additional information provided for the satellite measurements in this figure, and panel F show measurements in December 2021 before the Hunga eruption, and what does MAGIC at the top of this profile and in Figure A3 mean?

For the second time, why are the authors trying to squeeze measurements from an unknown instrument into this manuscript, particularly as the measurements have nothing to do with the Hunga plume in the NH. Figure A1 should be removed.

We have removed POPC observations from this manuscript. They were useful to show how well POPS observations did match observations with a different measurement technique during the Hunga phase, for a relevant measurement profile, as shown in Figure 2a. This purpose was explained during the first review round.

Now, following the reviewers suggestion, Figure A2 only consists of a comparison plot between POPS, LOAC and SAGEIII/ISS observations.

Then there is this statement, "The good comparability between the POPS and POPC profiles gives confidence in the reliability of the POPS and LOAC measurements." How does it do that? LOAC is not compared in panel F. What could really help establishing reliability of the LOAC and POPS would be for them to be plotted together on the same graph. Didn't they make measurements on the same day from the same location? And adding to that would be a SAGE III profile on the same graph to get a better sense of the correspondence of these three different instruments' estimates of extinction coefficient, which would strongly test one of the authors' stated goals of establishing "…the reliability and confidence in the POPS and LOAC measurements." Certainly they won't agree perfectly, but perhaps within known uncertainties in the measurements.

This LOAC instrument launched on 12/10/2022 did not work properly because of too much use in harsh atmospheric conditions damaging the (sensitive) electronics. That is why we have a series of spare instruments. We replace a defective instrument by a new one for the next flights.

As stated above, we have added a comparison Figure A2 with SAGE III, POPS and LOAC observations, as suggested by the reviewer.

Figure A2. For the second time, why isn't this figure added to Figure 2? It's a sounding measurement at the same location as the POPS and LOAC and the timing is very interesting, showing no impact of Hunga on 10/10/22, one week before the Hunga plume was observed by POPS and LOAC at the same location. And then COBALD shows that the Hunga plume was still observable with in situ instruments two months later. This timing is also consistent the the CALIPSO data shown in Figure 5, where a dotted line could be added to show the COBALD sounding.

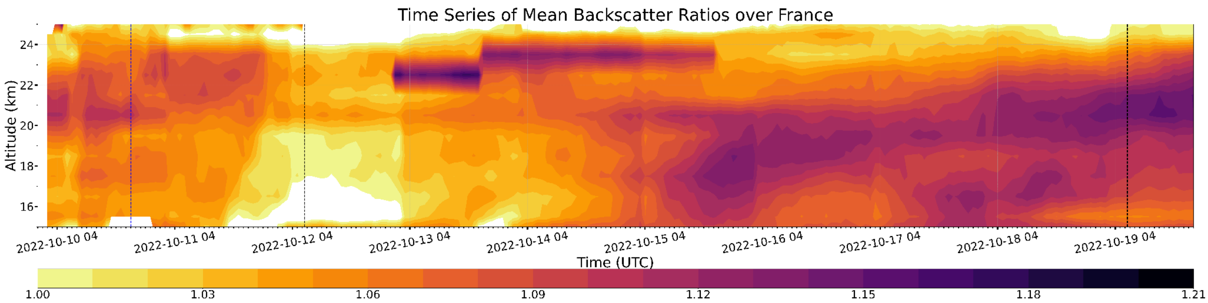
We added COBALD data to Figure 2, as requested by the reviewer. We added a methods section and some discussion accordingly. A vertical dashed line was added to Fig. 5.

2.4 Compact Optical Back- scatter Aerosol Detector, COBALD

COBALD is a lightweight (540 g) instrument that consists of two high-power light-emitting diodes (LEDs) that emit about 500 mW of optical power, at wavelengths of 455 and 940 nm, respectively. The backscattered light from the molecules, aerosols, or ice particles is recorded by a silicon photodiode using phase-sensitive detection. The precision along the backscatter ratio profile is better than 1% in the UTLS region (Brabec et al., 2012; Vernier et al., 2015). COBALD measurements within this work were obtained during the REAS/MAGIC campaign 2022 (Dumelié et al., 2024).

Respective COBALD observations are shown in Fig.2f. The first flight took place on 17 January 2022 just after the Hunga eruption but before the plume could be transported in the NH. The transport of the Hunga is evident on 8 December and 13 December where scattering ratios are observed between 20 and 23 km with values reaching 1.15 in contrast with earlier measurements in October when the plume had not yet been transported over France. COBALD measurements in Fig.2f confirm that no significant aerosol plume signal was observed on 10 October 2022, one week before POPS and LOAC show the plume signal in Fig.2a and b. COBALD observations on 8 and 13 December show an enhanced aerosol signal compared to January and October 2022.

Figure 5:



199-206 Why is there a call out to Figure 5 when Figures 3 and 4 have not been discussed? Reorder the figures and call them out in order for the sake of the reader if not for a logical progression of the manuscript! Or do the authors mean Figure 3 which is the back trajectories referenced in the discussion here, but the discussion also says that backscatter ratio was calculated along the back trajectory, but that is not shown. Backscatter ratios from CALIPSO are shown in Figure 5, but the discussion here does not mention CALIPSO. This paragraph needs to be straightened out. At this point it is inconsistent with the text, discussing data which are not shown.

We have rechecked this paragraph and found that the Figures are discussed in their proper order. Figure 3 and 4 are discussed prior to Figure 5 and the paragraph is not inconsistent with the text.

Figure 3 caption. Don't the authors mean that the air parcels culminate in a box 48-50 N? The air parcels in the figure originate between 24 and 28 N.

We changed the respective sentence in the Figure caption to: *'The backward trajectories are initialized from a box spanning 48°–50°N and 3°–5°E at 20.6 km altitude.'*

208 After the paragraph above the discussion shifts to Figure 6, skipping any discussion of Figure 4 and either Figure 3 or 5, depending on what figure the previous paragraph was referring to.

We do not see any problem in the organisation of the figure's discussion and the order of appearance in the text.

Fig. 6 caption states the OMPS data are from January 2022 – 2024, but what is shown only extends to September 2024. Why the discrepancy?

We changed the caption to *'from January 2022 to September 2023'*. This is also consistent with the text.

Table 1. It would be interesting to include in Table 1 the equivalent quantities from the lognormal fit to the LOAC data, even though there would be only one mode for LOAC. Equally interesting would be effective radius and aerosol extinction from both instruments and from the two POPS modes. Even though the two distributions are somewhat different such a comparison would provide an indication of how important these differences are.

Please refer to the paragraph within the manuscript, which explains why we cannot quantitatively compare LOAC and POPS effective radius and mode radius: *'Based on a lognormal fit, LOAC data yield an effective radius of 304 nm, consistent with POPS. A second mode seems possible in LOAC observations but shifted towards smaller particle sizes. However, unlike POPS, the bimodal distribution does not significantly improve the fit compared to a unimodal one. Because LOAC observations are limited to particles larger than the observed peak concentration (at around 100 nm) within the Hunga plume, lognormal fit analysis cannot work as reliably as for POPS. Therefore, parameters such as the effective radius (304 nm) and mode radius (41.7 nm), calculated with LOAC observations here, should be interpreted with caution.'*

280-284 Why do the authors express the range of a quantity as high to low rather than low to high, which is more customary? High to low forces the reader to reread a sentence to insure it is understood, an unnecessary complication.

We changed this accordingly.

287 For the second time, what is a theoretical Angstrom exponent? An Angstrom exponent

just describes the relationship between extinction coefficients at two wavelengths, to allow extinction coefficient at an intermediate wavelength to be estimated. So does a theoretical Angstrom exponent imply extinctions from a theory, or are they from a model, which is quite different than theoretical? There are many ways models can provide an aerosol size distribution which is required to obtain an extinction coefficient. And, again for the second time, what two wavelengths are used to obtain the Angstrom exponent quoted?

The spectral variability of the AOD is a continuous function of the wavelength (see the 1964 paper by Anders Angström: https://onlinelibrary.wiley.com/doi/epdf/10.1111/j.2153-3490.1964.tb00144.x), and produces a continuous power law, sometimes referred to as the Angström law or formula. As already stated in the previous review stage, we obtain the Angström exponent with a power law regression using all the available wavelengths and not just two. Using only two wavelengths might be practical when just two wavelengths are available but there is no reason to not exploit the full information content of all the wavelengths, with the Angström law, when possible, like in our case.

289 What satellite observations provide these size distributions? They should be mentioned here.

Yes, we added the information.

"...and size distributions obtained with satellite observations of SAGE III/ISS (Duchamp et al., 2023), ..."

Table 2 has problems. The extinction coefficient has a range from 1.1 e-3 to 8.5 e3. That's a range of 6 orders of magnitude. The Angstrom exponent listed does not indicate the wavelengths involved.

Yes, the table was misleading. We worked on the table and corrected mistakes. Thank you for noticing this.

'Different optical and radiative properties of the Hunga plume transported to the NH are summarized as follows: spectral aerosol optical depth values at 384, 756, and 1020 nm are reported for the background stratosphere (November 2017–February 2018),..'

304 Where is this 50% increase in extinction observed? According to Table 2, comparing row 3 with row 1, the increase is more like 30%, e.g. 1.7/6.9 at 384 nm, 1.1/3.1 at 756 nm, and 0.7/1.6 at 1020 nm.

The reviewer is here comparing "Hunga only" with "Background" while "Hunga+Background" should be compared with "Background only" because this is what actually occurs in the real world and is observed by instruments. Also, and in particular, our "~50%" increase refers to Fig 8.

To avoid confusion, we changed the phrasing of the 50% increase in multiple places, because the effect is rather a doubling of the extinction and this doesn't sound so clear:

“The Hunga plume transported to the NH produced up to a doubling of the aerosol extinction coefficient… "

“During this period, the stratospheric aerosol extinction coefficient is approximately doubled at 16-24 km altitude, compared to background conditions within the 30–50◦N latitude band. “

“Aerosol extinction coefficients in the mid-latitudes (30–50◦N) were approximately doubled…"