



EGUsphere, author comment AC1
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Reply on RC3

Bernard Legras et al.

Author comment on "The evolution and dynamics of the Hunga Tonga–Hunga Ha'apai sulfate aerosol plume in the stratosphere" by Bernard Legras et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-517-AC1>, 2022

Answer to referee 2

We thank the referee for his/her thorough assessment of our work and the detailed comments. Although the initial comments of the referee need to be replied, we reply to others only to the comments that do not reappear later among the specific comments.

This manuscript presents an analysis of the progression of the volcanic aerosol cloud from the January 2022 Hunga-Tonga eruption. Other papers in review or recently published have explored other aspects of the Hunga-Tonga event with the Khaykin et al. study analysing the global-mean stratospheric AOD and water vapour from the eruption cloud, illustrating each are the strongest global perturbation in the post-Pinatubo period. But as far as I am aware this is the only paper to present a progression in the vertical profile and meridional extent of the aerosol cloud.

We are aware of another paper by Schoeberl et al., 2002, submitted by early July who also describe the separation of rising moisture from descending aerosols

I am conscious I have not been able to complete the review of this manuscript until now (12th August), and I am sorry for not being able to submit this review sooner.

In particular, I noticed that, in the period since this manuscript was submitted, another manuscript analysing the aerosol and water vapour within the volcanic cloud from the Hunga-Tonga eruption cloud (Khaykin et al., 2022) was submitted (on 31st July).

I understand that the authors must be keen for their results to appear as soon as possible, and the choice to submit a short ACP letter may have been partly motivated by an aim to achieve publication on a shorter timescale than would be the case for a regular ACP article.

So I apologise again for not being able to submit this review before now.

I am also aware another manuscript led by one of the co-authors of this study (Sellitto et al., 2022) has been submitted to another journal, which assesses the radiative effects from both the aerosol and the water vapour. That manuscript explains the unexpected strength of the radiative effects, both in terms of the aerosol forcing being higher than the modest 0.4Tg of SO₂ measured to have been present, but also in terms of the surface

warming effect from the >100 Tg of water vapour leading to the first well-observed case of an overall net warming eruption.

This paper is indeed complimentary to our work and we mention also Khaykin et al, 2022 that focuses more on the water vapour transport.

This Legras et al. manuscript has an "applied for MS type" set for "ACP Letter", and see that ACP has this type requiring an enhanced criteria of "particularly important results and major advances":

https://www.atmospheric-chemistry-and-physics.net/about/manuscript_types.html

Whereas the main focus of the Sellitto et al. study is in relation to the radiative effects, the present study, submitted for ACP letter is the first to present a multi-platform assessment of the longer timescale progression of the scattering-magnitude of the aerosol cloud, through to the end of May 2022, and to assess its progressing altitude and depth, Figures 2 presenting for example the descent rate within the initial weeks after, and the later phase showing slower descent.

The enhanced criteria for an ACP letter are also clearly achieved, with these results being important to understand how the aerosol cloud is continuing to progress, in relation also to potential impacts of the emitted water vapour and may still have on the ozone layer, and either the latter phase of this year's Antarctic ozone hole or potentially for next season.

One key aspect in this regard is the fact that Figure 2 illustrates that the 15-25S latitude band has a continuing unprecedented >10 ppmv water vapour concentration at ~24-28km, this very-strongly-enhanced level apparent throughout March, April and May. And furthermore whereas the lack of variation in the 15-25 South band shown in Figure 2c is remarkable.

Furthermore, the latter results from Figure 2c (from the first weeks of June) give (for the first time in published article I expect) an indication as to for how long the unprecedented high water vapour concentrations may continue for, with there being a first indication that the water vapour is now beginning to reduce.

In my specific comments (comment 4) I am suggesting the authors extend the x-axis for Figure 2 to include the results through to end of July (or even into the first week or so of August, to then be able to give a clearer indication of whether indeed there is a continuing decreasing signal from the extreme 10 ppmv values the eruption has caused.

Figure 2 has been extended to 26 July. The situation evolves very slowly in the following weeks and we have a two-week gap in the OMPS data after 26 July that would truncate some of the curves.

The manuscript's focus is on the aerosol portion of the cloud, with this important complimentary finding from the latest results re: the longevity of the high water vapour concentrations in the Southern Hemisphere mid-latitude stratosphere caused by the eruption, I am requesting in that comment 2 that the authors add a sentence to the Abstract, to provide this prominently within the Abstract after the aerosol results have first been summarised

Although the Figures illustrate very well the main results from the study, some parts of the text in the manuscript require improvement before the manuscript can proceed to publication as an ACP letter.

And I have provided below a list of specific suggested minor revisions which are mainly seeking to improve the wording where the main results are summarised in the Abstract and conclusions.

I am conscious the authors are not native English speakers, and although there are a large number of suggested edits, these are mostly minor in nature.

Only 2 of the specific revisions might be considered major, the first being the request to indicate within the title the main focus on the manuscript being re: the aerosol properties, with suggestion to add text in the Abstract mentioning the slow but clear separation of the aerosol from the water vapour, 2 distinct plumes emerging during March and April (evident from Figures 1 and 2).

Whereas the Millan et al (2022) manuscript analyses the period up to the end of March, this manuscript is the first to analyse this longer timescale progression through April and May where the aerosol has a slow but steady descent, whilst the water vapour plume remains at the same altitude or with slight ascent.

Whereas the early-phase of the very high water is documented in the Millan et al. (2022) GRL paper (submitted 30th April, accepted 2nd June), the analysis in that paper extends only to the end of March.

The MLS results shown in the present manuscript's Figures 1 and 2 show the more situation in more recent months, and potentially indicate the scenario forward-projection model simulations shown in Zhu et al. (2022) may even have been an underestimate for the amount of water vapour in the winter mid-latitude Southern Hemisphere stratosphere.

The persistence of moisture is now mentioned in the abstract and commented in the discussion. However, we do not use any modelling use that allows to foresee the future and the long-range impact of the plume, in particular regarding the ozone hole, is beyond our scope.

The other more-substantial comment is in relation to the interpretation within the impressive analysis to assess the descent of the aerosol portion of the cloud in the initial weeks after the eruption, or the portion that dominated the backscatter signal at that particular time.

The approach is novel and interesting, enabling to make quantitative statements about the progression of the aerosol cloud, potentially revealing that there has been a systematic removal of some sub-population of the aerosol with differing characteristics to the others. This could potentially be as a results of particles at larger sizes sedimenting faster (such as a more-strongly-hydrated fraction, ash particles, or even potentially volcanically-detained marine aerosol).

Whilst this analysis is another excellent part of the analysis, using the term "aerosol motion" in the methodology section (line 215, Appendix A3) and also subsequently in the text, is too specific an attribution, in my opinion. The further analysis to derive an "aerosol radius" in Figure 2e is certainly too strong, and in the next para I request the authors revise this size-association to a be clear this is an "apparent size" or similar. The text also needs to be moderated to be consistent with this being indicative of a particle size, also in relation to the sizes dominating the backscatter signal at the wavelength observed.

My specific suggestion is to change these two terms instead to "apparent aerosol descent rate" or similar and "optically-derived aerosol radius" or similar. The term "effective radius" has a specific translation to the ratio of volume to surface area, so that term should not be used.

I recommend the term "apparent descent rate" is used, then communicating implicitly this is a derived quantity, the "optically-derived" being required for the derived-size, to remind that there may well be other smaller aerosol sub-populations present, that descend more slowly, considering that smaller-sized particles tend to be under-represented within the optical signal measured from the lidar detector.

The Figure 2b) and 2d) also label the descent of the CALIOP-derived mid-visible extinction as "vertical motion", but so certain a translation from the measured optical properties, although appropriate for the descent of that signal, I'm advising not be denote with the word "motion".

Suggest to change "Vertical motion" instead to "apparent descent rate" or similar. Whereas that is a more tentative suggestion that I leave it to the authors to decide, the further analysis to associate an "aerosol radius" from the apparent descent rate the request to change "aerosol radius" to "apparent particle size" fall speed the aerosol descent rate translates into, where I am requesting some changes in the interpretation.

These 2 more substantial changes are straightforward to implement however, and can be considered minor in character, and my overall assesment then is to recommend the manuscript be published as an ACP letter, once the set of specific revisions below have been made.

This is a main point which deserves a fairly detailed reply as we have some difficulties to understand the concern of the referee and we are reluctant about the systematic usage of the word "apparent" suggesting that our interpretation is, a priori, invalid. Most information on atmospheric aerosols is from optical measurements and inversion by various instruments and methods. There is a huge specialized literature that validates these measurements and provide confidence in the results albeit the uncertainty remains quite large in practice. In the case of this paper, we do not use any inversion but rely on the physically measured quantities, backscatter or extinction that can be directly related through the calibration of the instruments to the photon count of the sensors. Therefore we consider as granted that we see an aerosol layer and not an apparent aerosol layer and that its location is well determined from the CALIOP lidar as this is just a matter of time counting, something that can be done with high accuracy. That this layer exhibits a vertical descending motion cannot be disputed. It does not necessarily represent the whole of the aerosols. There might be other undetected much smaller aerosols but these latter will not fall, will be entrained in the Brewer Dobson upwelling and likely evaporate as noticed by Schoeberl et al. 2022. Anyway, we can only discuss and interpret what we see, not what we miss. The simplest interpretation is that we see falling particles and it is then reasonable to estimate a radius from this falling speed and see whether this is consistent with the other pieces of the puzzle. The main other piece is the "lidar ratio" calculated from OMPS-LP and CALIOP. The theory (Fig.2f) says that this ratio increases when the particle size increases and decreases when the particle size decreases. This is also what we find in the data (Fig.2g). We also see a first phase of the evolution where both the water vapour and the aerosols descend very fast and at the same speed. We explain it from the radiative cooling of the water vapour which is calculated in Sellitto et al. (2022).

The referee makes two specific reservations about the presence of ashes and the possibility of aerosols of marine origin. The significant presence of ash is not supported as there is no depolarization, no detection by UV instruments or in situ spectrometer, and evidence that the ash and ice cloud sedimented very fast in the first hours following the eruption. We discuss below arguments that do not support the marine aerosol hypothesis. The fact that the aerosol plume thickness stays almost constant with time after the first month (see Figs 1 and 2) calls for a fairly compact size distribution of the scattering aerosols. Therefore, we do not see any support for the specific reservations made by the

referee and we cannot make reservation, based on Popper's principle, on such general statements as " *there has been a systematic removal of some sub-population of the aerosol with differing characteristics to the others*". If the referee has some specific support for his/her hypothesis, they need to be made available to us or published before we can discuss them.

We write "the apparent aerosol radius is estimated by interpreting the aerosol plume motion as a fall speed of the scattering particles using Eq. 9.42 of Seinfeld and Pandis, 2016." and we think that there is no need to associate the word "apparent" to each occurrence of the word radius.

List of specific revisions. -----

1) Title -- Re: the first of the two potentially-major comments, and my summary statements above, although I'm conscious it's possible other manuscripts are in review that I am unaware of, to my knowledge this manuscript is the first to present such a comprehensive assessment of the optical properties of the aerosol portion of the Hunga-Tonga volcanic cloud.

In my general comments above, and in specific revision 4) below, I'm suggesting to update the Figure 2 timeseries to complete through to the end of July, which would then have the first 6 months after the eruption. And then although the water vapour is not the main focus of the article, adding specific mention of the 6-month timescale that the article assesses will be of benefit to readers being able to refer to this paper also for this issue being able to assess the longevity of the unprecedented high water vapour concentrations, and even see first indications of a potential decline.

Given there will be interest in the water, my specific suggestion is to consider adding a new 2nd part of the title:

"and the progressing aerosol properties in the first 6 months post-eruption"

I wonder if there could even be the potential to refer to the result re: the separation from the water vapour portion of the plume, with a longer 2nd part, continuing after the current "in the stratosphere" with a colon ":" and then

": progressing aerosol properties and vertical separation from the water vapour in the first 6 months post-eruption".

We are, as a general rule, more in favour of short titles, easy to catch the attention of the reader and to memorize, even if they cannot be fully descriptive, but we see the point of the referee and we have changed the title in the suggested way.

2) Line 1, Abstract -- suggest to add 1 sentence re: the continuing high water, and also a 2nd extra sentence re: the distinct vertical-separation that has emerged in recent months between the altitude of the volcanic aerosol and the altitude of the volcanic water vapour.

This aligns with the suggestion above to add specific mention in the title, and then the Abstract can 1 sentence stating this finding re: the continuing high water beginning to decline in June (and July if that turns out to be the case when the Figure is updated).

I have provided a suggested wording this:

"We also show the unprecedented high water vapour concentrations shown in the MLS measurements in February and March, are in this study shown to have continued

throughout April and May, a > 10 ppbv in the altitude range 24-28km at 15-25S."

And in the 2nd sentence to add

"We also show that in these recent months, there has been a distinct "vertical separation" has progressed with the aerosol now at 22-24km, the water vapour remaining at ~25-28km altitude."

The Abstract is relatively short in the submitted manuscript and I feel there is definitely space for the 2 additional sentences actually.

Actually we do not have that much freedom as the submitted abstract fills exactly the ACP Letters 200 words limit There is a sentence saying "As sulphate particles grew through hydration and coagulation, they sediment and separate from the ascending moisture entrained in the Brewer-Dobson circulation." which accounts for the information required by the referee. We have updated the sentence describing the duration of the plume.

There could even be an opportunity to add a 3rd sentence: there being an apparent decline, and in relation to the impacts on the Antarctic ozone hole in the latter part of the 2022 season or whether high water vapour could reach the 2023 Antarctic polar vortex when it spins up in April 2023.

A suggested wording for this potential 3rd extra sentence is:

"We also see first indications for the rate of decline from these high concentrations, important given the potential for a continuing high water vapour shown to affect the Antarctic ozone hole."

This is an interesting topic worth of investigation but we decided to stay away from it in this paper due to the format restrictions and because we cannot produce more than a speculation. In addition we do not think the abstract is the right place for a speculation that is not supported by any specific result. In the revised version we keep showing data in the 35S-20N range event if aerosols and water vapour have been obviously transported to lower latitudes in late spring and summer as the wave activity increased in the southern stratosphere. The issues related to water vapour are receiving a more detailed attention in a separate paper by Khaykin et al., (2022), with a number of common authors, which has been submitted after this one and is now quoted in the text

It is obviously a decision for the authors to make, to determine whether to add these sentences, but I think the paper's status as an ACP letter will be elevated with this broader context for the results presented.

t readers to the observational results presenting this indication of the continuing unprecedented high water vapour in the Southern Hemisphere stratosphere, I suggest the authors add an extra sentence to the Abstract flagging up this result.

I think the 2 or 3 suggested extra sentences would work as an extra final part of the Abstract, or be incorporated somewhere earlier within a longer 2nd half of the Abstract.

I am conscious that the ACP letter format requires only a short Abstract, and I am not sure whether the current Abstract is already close to the maximum allowed word limit.

But given the importance of the results, I wonder if the Editor could potentially give special permission for a modest extension to the reduced form usually required for an ACP letter. The article itself will still conform to the required length, so a minor adjustment to enable this information to be provided in the Abstract would be of benefit to the journal, in

terms of potentially raising awareness of there being this "ACP letter" manuscript format option when submitting to ACP.

We have been told that we should not take too much freedom from the rules of ACP letters even if we hope we will be permitted to do the minimal changes required to meet the legitimate requests of the referees.

3) Abstract line 3 -- The authors state with absolute certainty the initial plume was "without ashes", but on lines 64-65 refer to "the ash and ice plume". Whilst the focus of this article very usefully assesses the progressing composition of the plume, the statements need to be more nuanced to be clear the authors are arguing the ash was removed within initial days.

There was clearly injection of ash in the stratosphere but nothing suggests that it remains visible in the stratosphere after the first day but perhaps for a thin cloud at 35-40 km that is described in Kha

Also, the term "washed-out" is not correct, because the term "washout" refers to below-cloud scavenging of aerosol from precipitating rain drops, the term then not appropriate for the stratosphere.

The authors say "within the first hours", but whilst the very strong depolarisation seen in the 16th January CALIOP profiles are not seen after that time, there are moderate depolarisation seen in later CALIOP profiles which indicate at least some parts of the plume may still have ash, and the wording needs to be more precise here.

The only strong depolarisation seen on 16th January, about 50%, is on the thin cloud seen at 35-40 km, moving westward with a mean speed of about 40 m/s and submitted to a large shear. This cloud was seen a few days later from a lidar at La Reunion (A. Baron, personal communication, Khaykin et al, 2022) and then lost, at least from CALIOP data which show no trace after the observations resume on 27 January. We omitted these results due to space limitation and because they are described in Khaykin et al. 2022. This cloud is too thin to be seen from Himawari imager, unlike the initial plume just after the eruption, and is also totally separated from the two thick components of the plume which are described in this paper. The depolarisation remains always under 1% during the whole history of the aerosol plume, a value which is usually not considered as moderate but low. Larger values are only observed as noise where there is no aerosol. Although the presence of very thin ashes cannot be totally excluded, for instance inside the aerosol droplets, it does not show up optically and thus remains purely speculative.

I am not sure whether the authors are indicating the ash may have been encased within ice, and then removed by sedimentation more rapidly than in other cases, but the wording needs to be made more precise, and if they are advancing this suggestion that the ice sedimentation may have potentially provided an enhanced removal mechanism the ash case, the wording should at hint at this explicitly.

I am not sure if the "washed-out" and "washed-down" is actually the authors arguing the ice-sedimentation removal pathway, but if so the wording could be adjusted to hint this.

Please change "washed-out within the first hours" to "removed within the first days", and suggest to potentially add "possibly via sedimentation within larger ice particles" or similar wording.

The circumstances of this eruption are very unusual. The GPS-RO profiles in the close vicinity of the plume on the day of the eruption suggest that just after the eruption a saturated profile of water vapour was established up to the top of the umbrella as shown

in Khaykin et al., 2022. Therefore it is very likely that all the water in excess of the saturation has condensed in big ice crystals and has scavenged the ashes in the same way as precipitating rain does usually in a low cloud. We do not see any other mechanism that can explain the sudden collapse of the ash and ice cloud which was very fast compared to other recent well documented eruptions such as the Raikoke in 2019. Same analysis was made by Taha et al. (2022, in revision for GRL) who additionally mention that TROPOMI UV data show SO₂ but no ash on 17 January. Therefore, we keep "washed-out" in the abstract for the sake of compactness and we change the text sentence into "removed within the first day after the eruption likely via sedimentation within large ice particles". This is much more than a possibility.

4) *Figure 2, line 14 -- as in the above comments, suggest to extend the x-axis of this Figure to continue through to end of July, to then be able to provide the full first 6 months after the eruption, and the latest information re: the indication from the early-June results in Figure 2c) of a potential decline in the unprecedented high water vapour of >10 ppmv measured by MLS.*

The axis has been extended until 26 July. We gathered data after this date but there is little change over the following weeks and since OMPS data are not available between 26 July and 12 August, this would have interrupted several of our data series and plots.

5) *Abstract, line 5 -- The word "While" is confusing in this context, I think the authors mean "Whilst". But suggest better to re-word "While SO₂ returned to" instead to*

"Whereas SO₂ had returned to...". Or alternatively change "While" to "Whilst".

Done as suggested.

6) *Abstract, lines 8-9 -- This sentence beginning "Sulphate aerosol optical depths" needs to be clear what timescale the organising into concentrated patches is referring to.*

Do the authors mean the initial days after the eruption -- please add specific mention of the timescale here -- it's only for the first few days or weeks this occurs, right?

The timescale is the first two months and the abstract has been corrected to mention it.

I also wonder if the authors are indicating some relationship between the strong radiative cooling from the emitted water vapour and the dynamical structures observed? Are these unusual compared to what has been observed for other eruptions?

I'm wondering if the unprecedented altitude at which the Hunga-Tonga eruptive plume detrained might unusually have preserved these structures when usually the closer proximity to the tropopause would see the plume dispersion more disrupted, the structures then not so apparent?

Please add a few words to this sentence of this Abstract to mention both the timescale the structures remain, and any specifics in relation to the higher altitude detrainment and/or the strong radiative effects from the emitted water vapour.

The abstract says that AEOLUS data suggests the structures are related to vorticity anomalies and that they are very similar to the product of shear-induced instabilities, that is active rather than passive dynamics. This is all we can say at the moment. The PV gradient shown in Fig. A2 suggests the need of local enhancement of the shear or a local vorticity source. This can be produced by radiative heating and has been shown to be very effective for forest fire plumes, even close to the tropopause (Khaykin et al., 2020; Lestrelin et al. 2021). In the present case, the effect is not detected by the ECMWF

analysis or reanalysis and needs further investigation coupling radiative calculation with dynamics. It is indeed possible that water vapour cooling played a role in generating vortical structure during the first two weeks. Meso-vortices are often detected after large volcanic eruptions but are short lived features for a few days only. The only case of long duration structure we are aware of concerning a volcanic eruption is the compact patch followed by Chouza et al. 2020 after the Raikoke eruption which has been surprisingly to our eye interpreted as a passive structure by Gorkavyi et al., 2021.

7) Introduction, line 14 -- The authors state the "explosive intensity is close to that of Mount Pinatubo in 1991", but the Wright et al. (2022) paper presents evidence the eruption was actually more explosive than Pinatubo, with explosive energy quantified to be comparable to 1883 Krakatau. Suggest to either change "to that of the eruption of Mount Pinatubo in 1991", or if that is a finding from another paper, then to add separately at the end of the sentence reference to the Wright et al. (2022) analysis suggesting on the scale of 1883 Krakatau.

This is based on the volcanic explosive index estimated by Poli and Shapiro, 2022 from seismic data and a model of the magmatic system. The estimate by Wright et al., 2022, is based on the emitted Lamb wave. The two estimates do not tell necessarily the same story about the eruption and it is not within our scope to reconcile these two approaches. We have modified the sentence about the Krakatau into "The induced atmospheric Lamb wave circled the globe at least 4 times with an amplitude comparable to that of the 1883 Krakatau eruption" and added Wright et al. 2022 to the references.

8) Introduction, line 19 -- change "the stratosphere" to "the upper stratosphere"

The second part of the sentence says "increasing its overall water vapour content by 10%" and is meant for the whole stratosphere. It would be misleading to change "the stratosphere" to "the upper stratosphere".

9) Introduction, line 25 -- Change "mean zonal pattern" to make the wording more specific to the pattern being described. Also the word "pattern" is a little non-scientific unless part of an analysis systematically applying a pattern-matching algorithm or so.

I think the word "zonal" is in relation to the plume's dispersion in the zonal direction, or maybe the authors mean the zonal variation of the plume in the initial days/weeks?

Please then change either to "The zonal dispersion of the Hunga-Tonga plume" or "Zonal variations within the Hunga-Tonga plume dispersion" or "Structures within the zonal dispersion of the Hunga-Tonga plume" or similar.

Thanks for pointing out that the header of this section might be confusing. This section does not deal with the zonal dispersion but describe the large scale evolution of the plume as seen from zonal averaging. It has been renamed "Six-month evolution of the zonal mean". In this work, we decided to proceed from the global impact view to the details rather than present a series of events in chronological orders.

10) Introduction, line 31 -- The authors refer to Figure 1 showing an initial "fast latitudinal dispersion" but the extent of that latitudinal spread needs to be clear, and the word "meridional" is clearer than "latitudinal".

Suggest to change "fast" to "rapid", and add what timescale is intended here -- "in the first days after the eruption" or similar.

We made the changes "latitudinal" to "meridional" and fast to "rapid" and we refer to Khaykin et al. 2022 who provide a description of this initial dispersion.

11) Introduction, line 43 -- The authors explain the water vapour is descending, and note this being in opposite sense to the rising ERA5 motion, but the use of the word "against the rising ERA5 motion".

Suggest to simply change "against" to "in contrast to" and change "the rising ERA5 motion" to "the rising motion in ERA5".

Done as suggested.

12) Introduction, line 46 -- The authors explain a later second phase where the water vapour (then diluted to lower concentrations) begins to rise, and state this is "in agreement with ERA5 upwelling". Similarly to the word "against", better to change "in agreement with" to "ascending at the same rate as".

Done as suggested

13) Line 63, Section 3 sub-title -- Whilst this section presents an interesting discussion of the composition of the plume, since this is not measured directly, the word "Inferred" needs to be added at the start, the sub-title changed to "Inferred composition of the plume"

Done as suggested

14) Line 64, Section 3 -- Re-word "We now consider the history of the aerosol composition of the plume" instead to something more nuanced for the measurements being interpreted, such as "We now consider what the satellite measurements indicate for the early-phase composition of the Hunga-Tonga plume, and some initial cases where we infer the optical properties indicate that a change in the dominant scattering sub-population may have occurred".

Further to the comments above, since the aerosol particles are not being sampled, but inference from the optical properties, it's important to communicate to the reader the derived nature of the composition, at least in the initial sentence of the paragraph.

Most instruments measuring atmospheric composition in gas and aerosols, in situ or remotely, rely on optical properties and on the interpretation of the basic measurements with physics. We think that the reader of ACP is generally aware of that situation and does not need specific warning, especially in a letter format where the words are counted. We also use the in situ LOAC information, still based on optical properties, but it is not a satellite. Therefore we politely disagree here with referee on the need to change the introductory sentence of this section. See also our general comment above about "apparent" results.

15) Line 65, Section 3 -- Re-word "is rapidly washed down" (line 65) to "has a steeply descending altitude". The signature here may be indicative of another process, such as wind-shear or some dynamical aspect of the eruptive plume's detrainment. Whereas the descent within the longer-timescale variations in Figures 1 and 2 can be more certainly attributed, the daily-timescale vertical-shearing of the plume is clearly apparent within the CALIOP measurements (e.g. Sellitto et al., 2022), and from the ground-based lidar measurements at Reunion Island (e.g. Khaykin et al., 2022).

There are a range of possible causes in these initial hours after the eruption, and need to be cautious not to attribute too certainly a variation to a particular process.

Referring to our reply to comment 3 above, wind shear alone would not produce a vanishing of the initial ash signal on the time scale of a few hours or even a few days,

preserving SO₂, and we are not aware of any dynamical process that could do the job. Therefore we argue again that the fast fall of large ice+ash particles in the saturated stratosphere is the simplest and most plausible mechanism which is consistent with the observations. If there are other possibilities, they need to be defined and testable.

16) Line 69, Section 3 -- Re-word "hence made of" instead to "indicative of predominantly".

Done as suggested

17) Line 69, Section 3 (Inferred composition of the plume) -- Re-word "sub-micronic" to "sub-micron sized".

Done as suggested

18) Page 13, Figure 1 -- Whilst most expert readers will be aware of the difference between the observed quantity shown in the OMPS, CALIOP and MLS, and that information is provided in the Figure-caption, also considering the difference in wavelength between the OMPS and CALIOP aerosol extinction, suggest to add label on the far-right each row of the Figures to aim to ensure the reader can be mindful of this when comparing the different rows in each column.

A specific suggestion is for 3-line label in each case, the OMPS 3 lines being upper-line = "aerosol", with then "extinction-ratio" immediately below that, with then "(745nm)" as the 3rd line. Similarly, for the 2nd and 5th rows of sub-panels have the label at the far-right (for CALIOP) as "aerosol" "backscatter-ratio" and "(532nm)".

And then to have "water" "vapour" and "(ppmv)" as the label. Whilst this might seem quite a specific request, it will both help readers see the main result, and also ensure the inference in terms of the specific quantity and units shown is correct.

We are unsure this is needed as the information is already available twice on the header and the caption but, since it was easy, it has been done as suggested.

19) Page 13, Figure 1 -- The grid-lines shown in each sub-panel are very useful for the reader, whereas the gold/orange coloured lines are good for the darker background colours in the OMPS and CALIOP sub-panels, the gridlines cannot be seen in the MLS panels when overlaid on the aqua coloured contouring for the background (or the green colours). Please use a darker colour for the gridlines for the MLS Figure sub-panels shown in the 3rd and 6th of the rows of sub-panels in the Figure.

Done as suggested

20) Page 14, Figure 2 -- As explained in main comments, in Figures 2b) and 2d) change "vertical motion" to "vertical motion / apparent descent rate" and in Figure 2e) change "Aerosol radius" to "optically-derived aerosol radius", also revising the caption text accordingly.

See our general comment at the beginning of this reply. The aerosol radius is derived from the fall speed formula that does not involve optical properties and differs from the effective radius which is related to the optical properties. Following the suggestion of the referee would generate confusion.

21) Page 15, Figures 3 and 4 -- The panel h) of Figure 3 and panels b) and e) of Figure 4 are important new constraints on the sulphate component of the volcanic aerosol, in relation to the hypothesis whether the higher-than-expected strat-AOD could potentially

be from non-sulphate aerosol, for example marine aerosol detrained within the volcanic plume.

To our knowledge, NaCl has been detected once in a stratospheric volcanic plume after the eruption of El Chichon. It was not a marine volcano and produced alkalid magma that could contain halite crystals that were lofted in the plumes (Mechelangeli et al., 1991). In the case of the andesitic Hunga Tonga-Hunga Ha'apai, dissolved marine salt should have been hydrolysed by high temperature water to generate NaOH which reacts with silicium and aluminium to form silicates ($\text{Na}_2\text{Si}_2\text{O}_5$, $\text{NaAlSi}_3\text{O}_8$, ...) in the magma (G. Carazzo, personal communication). Chlorine is released as HCl which is highly soluble. Millan et al, 2022, mention that the HCl perturbation seen by MLS is unexceptional. The presence of halites in the aerosols would have produced significant depolarization of the lidar signal which is not seen. In the absence of any manifestation of NaCl in the plume and any need to consider this possibility, we are not inclined to take this hypothesis as plausible.

Readers will be interested then in the specific maximum value of the sulphate AOD shown in these 5 sub-panels of the 2 Figures. In each case however, the legend for the colour plots shown is not apparent currently, with the legend contour-labels shown only for the pale green colouring showing 0.01. Whilst I realise that may well be the intention to enable the reader to infer the aqua colours are above that 0.01 AOD value in Figure 3h), and the same aqua colour has that achieves that $\text{AOD} > 0.01$ criteria in Figure 4b, it is not easy to recognise the values for the upper end of the legend contour-scale. I think these are 0.08 in each case, and it would help the reader cross-check these, also with other Figures of the total AOD, in relation to assessing what the measurements are indicating in terms of the proportion of the total AOD that could potentially be non-sulphate.

For this reason, please add the 0.08 values to Figure 3h and Figure 4b, or if the value is 0.09 or 0.07 in one case, please set that accordingly.

Similarly for Figures 3g, 4a and 4d, please add the upper legend-scale contour label for the value shown for the column SO_2 Dobson Unit value shown, and Figure 4f) for the CALIOP integrated scattering.

The color scale have been homogenized, simplified and properly labelled. See also the corresponding reply to referee 1.

22) Page 15, Figures 3 and 4 -- The panel h) of Figure 3 and panels b) and e) of Figure 4

The unit used for the integrated scattering is shown as "str" with superscript -1, which I think the authors are using as a shorthand for "per steradian". However, the recognised shorthand is to denote steradian as "sr" rather than "str". I can see that in this specific case, the readers may have chosen to abbreviate the unit as "str" rather than "sr", to avoid confusion with the SR abbreviation for Scattering Ratio.

And then I agree that's better in that case, to retain that non-standard three-letter abbreviation of the steradian unit. However, given this non-standard abbreviation, and considering some readers may not be so familiar with the "per steradian" unit, I'm requesting the authors give the name of the unit in full in the Figure caption.

The only two panels where a steradian unit is used are Figs. 2g and 3f, not the one listed here. Nevertheless, we made the suggested modification.

23) Abbreviation for the sulphate component of the stratospheric AOD in the labels and captions of Figures 3 (panel h) and 4 (panels b and e) -- page 15 -- and in Figure 5 on page 16 and Figure 6 on page 17 (panels a, e and g).

The authors have used the abbreviation "SA OD" in the titles of the sub-panel Figures and "SA/OD" in the caption.

I have encountered occasional misunderstandings within some communities re: the SAD abbreviation for "Surface Area Density" sometimes being mistakenly stated to be "Sulphate Aerosol Density", similarly to the way the acronym GCM sometimes is sometimes referred to mistakenly as Global Climate Models.

I'm further aware that in some published papers the four-letter acronym "SAOD" is used for Stratospheric Aerosol Optical Depth.

I am assuming that's why SA is specified separately there, to aim to ensure readers realise the AOD in this case is actually for the sulphate aerosol component of the optical depth.

I'm aware that in some manuscripts (e.g. Dhomse et al., 2014, 2020), a lower-case "s" is used for "stratospheric", which is partly in relation to some readers potentially translating the SA characters within SAOD as sulphate aerosol rather than the shown unit for optical depth of the "stratospheric aerosol".

I agree with the point that the abbreviation AOD is the recognised acronym, and prefer then that manuscripts use a lower case s for an sAOD abbreviation for stratospheric AOD.

In this case, given that the IMS metric is retrieving a measurement of the sulphate component of the aerosol optical depth, it is important that readers correctly interpret the stated acronym.

It is important however also to be clear that, at least to my understanding, the optical depth retrieved from the IMS is also that the optical depth is for the sulphate aerosol in the stratosphere.

What I am recommending in my review is for the authors to use the abbreviation "SO4" for sulphate, and separate this from the A for aerosol, so then the recognised acronym AOD can be retained within the abbreviation.

Given there are two words beginning with S being abbreviated, I'd recommend best to use the lower-case s for sAOD, to abbreviate the retrieved quantity with "SO4-sAOD".

If the authors prefer the 4-letter SAOD acronym, this approach also works with "SO4-SAOD", although my preference is for the "SO4-sAOD", on the basis that this is "most easily scanned" or recognised when viewing the Figures.

Please use either "SO4-sAOD" or "SO4-SAOD" consistently rather than "SA/OD" or "SA OD", to ensure readers can immediately see it is the sulphate component of the aerosol (in the stratosphere) that the IMS metric is measuring.

We are a little bit confused about where is the problem and what to do. It should be clear the paper deals with the stratosphere since no data is shown below 18 km, hence it is perhaps not necessary to overload all quantities with a "s" prefix. Then we do not see what it really wrong here with SA/OD. SA means Sulphate Aerosol which is material entity and OD means Optical Depth which is a physical property. It is then legitimate to separate them by a / or an hyphen. SO4 is a poor descriptor of the sulphates and of the assumed conditions in the IASI/IMS products which are based on the spectroscopic properties of H2SO4 linked with water, which is acceptable as long as water activity is above 1%. SO4 has different spectroscopic properties (Sellitto & Legras, 2016). It is quite common that the same symbol or acronym bears different meanings in sometimes close domains. Besides using Chinese characters or/and adopting the somewhat cryptic notations of

biology, we do not see a general solution to this problem that maintains compactness. Within the context of our paper, we do not see where any confusion is possible and SA/OD appears only 6 times in the main text. The figure titles and captions that were using SA OD without / have been put in line with the text.

24) Please add the wavelength (in subscript) in abbreviations of the AOD

Considering the difference in wavelength between the aerosol extinction from OMPS (745nm) and that of the CALIOP lidar (532nm), please add the wavelength

Where practical to do so, please include the wavelength either as subscript in the labels for the Figure. Since the right-hand labels are added in Figure 1, this is not essential in that case, but in other Figures it helps cross-comparisons to be aware of the >200nm wavelength difference between the AOD values derived from the OMPS and CALIOP instrument.

Related to this issue, also the wavelength of the retrieved stratospheric sulphate AOD should also be given, to ensure then the proportion of the AOD can be interpreted consistently.

With changing the acronym to SO₄-sAOD, the wavelength can be added with subscript to then ensure the specific measurement of SO₄-sAOD can be interpreted to be able to infer the proportion of the stratospheric AOD that is sulphate at the correct wavelength for the IMS retrieval.

I must admit I am not sure of the precise wavelength, but am assuming this must be at the mid-visible, at 532nm or 550nm.

But please add this wavelength to the caption of the corresponding Figures, and, given the relevance to the origin of the Hunga Tonga aerosol, please also add this to the labels in the Figures as well. Thanks.

The wavelength of the backscatter and extinction are mentioned in all figures showing data from CALIOP and OMPS-LP. A few were missing and have been added. Regarding SA/OD, there is here a misunderstanding in spite of the description of IASI/IMS retrieval provided in Appendix A1.2. The SA/OD retrieved from IASI is obtained from an inversion of the infrared spectrum, so it is not associated to a particular wavenumber. Of course the information comes mainly from a few absorbing bands, mainly near 8.5 μm for concentrated sulfuric acid (see Sellitto & Legras, 2016) which are spectrally resolved by IASI and compared to a full spectrum without sulphate. So this is not at all as if the measurement was done by applying a narrow filter at a given wavenumber. It would be therefore very confusing on the origin of the data to overload the IMS SA/OD product with a given wavenumber, certainly not in the visible range anyway, and we cannot see any example of such usage among IASI products in the literature.

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