EGUsphere, author comment AC2
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Reply on RC2

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., EGUsphere, https://doi.org/10.5194/egusphere-2022-517-AC2, 2022

Answer to referee 1:

We thank the referee for his/her thorough review of our work and the numerous comments. There was initially a misunderstanding and our work has not been evaluated as a letter but as a standard submission to ACP. Therefore there are a number of requests made by the referee that could not be answered without infringing the constrains or getting away from the spirit of a letter format. However, we have been able to make a number of small modifications that meet most of the requests of the referee.

Major comments

The flow of the science discussion needs to be changed. Currently the authors discuss how the plume evolve in time from January to June in section 2. Then, they discuss the composition of the plume in section 3 but this section mostly uses data from the first days after the eruption. Lastly, they discuss the circumnavigation of the plume over one month and half.

I suggest the authors discusses (current) section 3 first, the (current) section 4 next, and the (current) section 2 last. I also suggest that the authors add in the introduction a small plan for the study, for example: In this study we first discuss the composition of the aerosol plume focusing on the days following the eruption, then we discuss how the aerosol plume circumnavigate the globe in the following days (months), and lastly, we discuss the zonal mean patterns.

Although another choice of presentation is possible, e.g. by discussing data as they appear in chronological order, we have chosen an order from general to more specific and local issues, which is to our opinion equally valid and fitting well within a letter format where the global pattern is presented before entering into the details. This choice allows to show in the first figure our major result on the separation between aerosols and water vapour, and it is also in the spirit of a letter not to delay the key results.

Through the study the authors refer to the plume meaning the sulfate aerosol plume but they also discuss the water vapor plume as well as the SO2 plume. They need to be explicit through-out the manuscript about which plume are they discussing, for example the title should probably be changed to: The evolution and dynamics of the Hunga Tonga sulfate aerosol plume in the stratosphere

We have tried to clarify when the confusion is possible. We are not restrained to the study of aerosol sulphates as recognized by the referee. We use water data and the quantitative discussion of the differences between the behaviours of the aerosol and water vapour plume is a main point of the paper. We also show both SO2 and sulphate charts. Therefore, we do think that the title deserves to be modified in the way suggested by the referee. However, the title has been modified according to the suggestion of the second referee.

The entire section A3 requires more explanation and more details, why 21-day window and not 11 or 31 etc, is there a difference? How was this value selected? The same goes for the 2 and 6 ppmv offsets.

Has this been done before? If so, how did it? Are they any citations to be used. Are the ERA5 fields interpolated to the measurements time and locations, are the authors using 12UT 0UT, an average of all synoptic times, etc.

The reply regarding the window width can be found below within the reply to the comment on the two bumps in the MLS water motion (comment to p3L60). The offsets are chosen to isolate the plume from the background. This has been added to appendix A3.

This method is a natural and straightforward approach which is similar, e.g., to the numerous studies of the stratospheric upwelling due to the Brewer Dobson circulation based on satellite observations. Figure 2a&c are, technically, nothing else than Hövmoller plots. A variety of filtering methods can be used and they are all good as long as the result is robust. We use daily means of ERA5 data calculated from the four synoptic times at 0, 6, 12 and 18 UT and zonally averaged.

P5L130-L135. All this is really interesting but the authors have not anything to support this. Is this shown in S2022? If it is please cite S2002 again so it is clearer. If this is not, please provided evidence to support these statements or delete this discussion.

These results are indeed established in S2022 and in Khaykin et al., 2022, both works being complementary to our own with a number of common authors.

All the data descriptions require more details, for example resolutions, footprints, validations, validity on the hunga tonga plume conditions, error estimates, etc. Please be consistent in the amount of details through all datasets.

We use only validated standard products and, in the specific case of this eruption, we follow the recommendation of the main investigators for each instrument when they are available. For instance Millan et al. (2022) recommend to use the version 4 data instead of the version 5 and to bypass the quality flag. Taha et al. (2022) states that the version 2.1 of the OMPS-LP extinction product can be used up to 36 km. We use L1 data from CALIOP in the version 3.4 since version 4 is only available with a delay of about 7 months. There is no reason to believe that any signal above 20 km is due to anything else than aerosols and that the lidar measurements are not performed at their nominal accuracy. We use the operational product of AEOLUS that is assimilated by ECMWF.

We have improved the description of data processing, in particular about the averaging procedures.

Minor comments:

P1L1 change stratospheric plume to stratospheric sulfate aerosol plume

We consider also the water vapour content and SO2 in the initial stage and therefore we are not restricted to sulphates although this is clearly the main topic of this work. The word limitation of the letter format does not allow to be fully descriptive in each statement, especially in the abstract.

P1L5 the phrase "The sulphate plume persisted until June" could be interpreted as if the plume flush out of the stratosphere in June please change to " the sulphate plume is mainly confined between 35S and 20N in June (due to the zonal symmetry of the summer stratospheric circulation at 24-25 km)". Or something along those lines.

The manuscript has been submitted on 20 June 2022 and this was the state of the available observations. The time axis has now been extended to 26 July as requested by the second referee. The statement is still basically true as the winter wave activity in the southern stratosphere has generated meridional dispersion and mixing from the middle of June. It has been adapted to mention the plume persistence over 6 months. Our charts are still bounded at 35S because we do not have room to extend the discussion towards the influence on mid and polar latitudes in this work.

P1L6. Swap the order of the latitudes, that is, "between 35S and 20N". (latitude ranges are typically expressed from the latitude further to the south to the latitude further to the north)

Done as suggested.

P1L17 Podglajen 2022 is not a good reference for this, use Carr et al 2022 and Proud et al 2022

Podglajen et al. show how acoustic wave emission corresponds to plume top height, demonstrating a complex injection sequence which is not mentioned in Carr et al. 2022. It is mentioned but not characterized in Proud et al. 2022 and discussed in Khaykin et al 2022. A further study by Taha et al. 2022 shows plume height based on OMPS-LP. We are now quoting all these works together.

P1L20. The SO2 injection of only 0.5Tg needs a citation, the authors could use: Millan et al 2022 and Carn et al 2022

Done as suggested although Millan et al. 2022 only says that the amount of SO2 is unexceptional and does not provide a quantitative estimate.

P2L21 the authors could add the reference Zhang et al 2022 to the Witze 2022

Done as suggested

P2L26 Move Figure A1 to the main body

Because of the letter format and because this figure contains information we consider as ancillary with respect to our topic, we prefer to leave it as an appendix figure.

The color bars in Figure A1 need to be changed to divergent color bars so the zero value is always white (or gray) for example and positive values are different shades of one color and negative values are different shades of another color, for example blues white reds.

We used a divergent color scale. It has been centred to meet the request of the reviewer.

The titles for subfigures d and e need to be changed to Angular speed in 15S-5S (degree/day)

The titles for subfigures f ang g need to be changed to Angular speed in 25S-15S (degree/day)

Done as suggested

P2L28 after "positive everywhere except a narrow region near 27km over the equator" add "(Figure A1-b)".

P2L28 change to: These conditions are stable during the whole ***January-March*** *period.*

P2L29 In April-June, the rotation weakens and changes sign ***(Fig A1-d and A1-f)*** *while the warming turns to cooling* ***(Fig A1-e and A1-f)***

Done as suggested

P2L30 .. the ***aerosol*** *plume stays mostly …. (or something similar)*

Done as suggested

P2L32 The sentence "By mid-February, the plume has already spread all around the Earth." is clearly wrong, the plume as the authors described in the abstract stays mostly confined between 35S and 20N, that is, it has not spread all around the Earth, I think the authors meant "By mid-February, the aerosol plume has already spread through all longitudes." In any case, there is no evidence of this, so I suggest either adding a figure showing the longitudinal spread or not mention it in this part of the text.

We mean of course a longitude spread and we argue that "around the Earth" is usually understood in this way in both English and French. The title of the Jules Vernes novel "Le tour du monde en 80 jours" is translated as "Around the World in 80 days" However, the sentence has been modified as suggested. We also refer here to Khaykin et al., 2022 who make a detailed study of the initial dispersion and to our figure 5 which displays such evidence. This is also in agreement with Fig. A1: at the maximal rotation speed of 30 degree per day, the world is travelled in 12 days.

P2L32. Delete However.

P2L34 and reaches and ***early*** *April maximum*

Done as suggested

P2L35 The sentence "This suggests particle growth" is ambiguous. Do the authors mean, that the OMPS and CALIOP result suggest particle growth or do they mean, that the CALIOP results suggest particle growth. It it is the latter, change the sentence to "Meanwhile, the Cloud-Aerosol Lidar with Orthogonal Polarisation (CALIOP) scattering ratio decreases, which suggests particle growth."

As CALIOP measures at 532 nm and OMPS measures at 745 nm, the opposite trends suggest particle growth and is shown quantitatively in Fig. 2F & g. We follow the suggestion.

P2L38 The citation Gorkavyi et al 2021 is only for OMPS. The sentence could be change to something like: The limb instruments suggest larger vertical plume extension than CALIOP due to their coarser vertical and horizontal resolution (i.e., Schwartz et al 2020, Gorkavyi *et al., 2021).*

It is not only a matter of the resolution but also of the viewing geometry of limb instruments that generates the arch effect discussed in Gorkavyi et al., 2021. This depends on the curvature of the Earth and applies to all limb instruments in the same way. We do not see that Schwartz et al 2020 discuss this issue besides the general information about MLS. In the case of the plume formed from the Australian forest fires of 2019-2020, the comparison of MLS and CALIOP resolution is betted appreciated in Khaykin et al., 2020. We have changed into "effects of the Earth curvature in the viewing geometry". Such effects make difficult to see the bottom of the plume using limb instruments. We have quoted Schwartz et al 2020 in the Appendix that describes MLS

P2L40, either say Two separate descent regimes are identified from observations. For the first ***regime*** *…. Or say Two separate descent* ***phases*** *are identified from observations. For the first phase*

P2L46 In the second phase ***(after mid-February?) …***

Note that the dates in this discussion seems to be based in the results for the 15S-5S latitude band. For the 25S-15S latitude band the regimes are actually separated on March 1st. The authors need to specify which latitude band are they drawing their conclusions.

It is very difficult to define a precise date, with a one-day accuracy, for the transition between the two phases and even more to see how it varies with latitude. Therefore we give 20 February as a date about which the transition occurs and we choose the word "phase" over "regime". The transition is indicated by vertical bars on figure 2 as recommended below. We also write that "The descent of aerosols separates in two subsequent phases" which means it is not necessary to mention that the second phase follows the first.

P3L54, if the initial growth of the particles was by hydration until mid march, why is the clear separation of the two regimes in February 15 (for 15S-5S) or march 1 (for 25S-15S)

The separation of the two descent phases is attributed to the role of the water vapour cooling in the first phase. This effect is bounded by the dilution and by the lower effectiveness of water vapour cooling as the plume descend s towards the neutral radiative level (see Sellitto et al., 2022). Hydration is another process which is also bounded by dilution but does not need to follow the same curve as the radiative cooling. Therefore the progress of hydration until March or April does not contradict the separation of the two descent phases.

P3L58 Fig 2e does not show any plume information thus, the sentence "The extinction-to-backscatter ratio is also smaller on the periphery of the plume (Fig. 2e)." is not clear.

Fig. 2e should have been Fig. 2g where the average over the band 30°S-20°N encompassing the periphery of the aerosol plume is shown. This has been corrected.

P3L60 change to: The similarity of extinction-to-backscatter ***(Fig 2g)***

P3L60 Figure 2g shows data for 3 latitude slices, hence the sentence "The similarity of the extinction-to-backscatter in those two latitude slices" is ambiguous, please modify it.

The sentence has been modified.

There is no discussion of the two humps in the vertical motion infer using the water vapor data (Fig 2b), any guesses? Could these be an artifacts of the methodology? How accurate are they?

These bumps in the initial version are not meaningful and we were not happy with that. They are due to the low vertical resolution of the MLS data that generates jumps in the altitude of the mean. We initially used a 1-km smoothed discretization of the MLS data but this is not enough. We use now a 100 m smoothed version using the non oscillating Akima interpolator before applying the ascent rate estimation. Several values of the time filter have been tried (11, 21, 31 and 41 days). We retain 31 days as the shape of the oscillations does not change with 41 days. For consistency, the same filter is also used now for CALIOP data although not much change is observed with respect with to the 21-day filter. As a result, the bumps disappear and the main result that is the proximity of the water vapour ascent rate to the ERA5 ascent rate is preserved and improved. The two curves still oscillate one around the other and we cannot probably do better here as we compare two zonal means with fairly different sampling.

P3L65 If I remember correctly Carr et al (2022) only talks about the ashes, there is no mention of ice in that study, please clarify.

Carr et al. (2022) cannot distinguish ash from ice as they use a single infrared band of the geostationary imagers near 11.2 µm. Nevertheless the result shown in their Fig. 3 is consistent with what we see using the RGB Ash product.In any case thick ice and ash can hardly be separated with broadband channels. We have also added the reference to Khaykin et al. 2022 who discuss this further and demonstrate ice saturation at the altitude of the umbrella cloud on the day of the eruption.

P3L66. Please change to: What emerges on the west side are two greenish clouds ***(Fig 3d)*** *without any hint of ash* ***(ashes would appear as yellow/reddish)***

Done as suggested

P3L67 change The early CALIOP section -> The CALIOP cross section through these clouds show high scattering patches …

Done as suggested

P3L68 LOAC is not defined.

It has been defined

P3L71 Please clarify how Figure 3g and h shows that the conversion to sulphates started immediately after the eruption.

We see the sulphates formed with an infrared AOD of about 0.1 just one day after the eruption. That is quite a sign. Furthermore, there is more sulphate where there is less SO_2. We have added “with an SA/OD reaching 0.1 one day after the eruption” to make the point.

P3L75 I think this sentence will be better of changed to: Four days later (Fig. 4b), the two clouds are still separated but …

Done as suggested

P3L77 highest cloud? There is no information of height on these plots. Do the authors mean further to the north?

The two strips have been produced from the two clouds in Fig.3e. This is now clarified in the text.

P3L80 How was the altitude determined? Please explain in the text.

This can be inferred from the low angular speed of the cloud at such latitude (see Fig. A1). This is also clarified.

P4L88 Which supplement movie B? Do you mean the movie described in appendix B. The reader should need to see a movie to understand the paper. Please include snapshots to reach the conclusions discussed in this paragraph.

The animation is not strictly necessary and Fig. 5 is enough. We recommend however to look at the animation as it is the best way in our opinion to appreciate the dynamics of the patches, much better that a series of snapshot unless it is done on the daily basis which is clearly not possible in a letter. It does not appear so eccentric in 2022 to provide a movie in support of a research paper and most journals recognize this possibility. We provide a mp4 animation which allows image per image viewing with most readers (unlike GIF animations). If the paper is accepted, the supplement movie will be archived and available in the same way as the text.

P4L84 "The sulphates persist for several months". This could be interpreted as if they have already return to background levels. Please change

This has been corrected. We now say they persist at least 6 months (from Fig.1).

P4L92 I don't see patches in c and d do the authors mean Figure 5a and b shows a number of localized concentrated patches …

If that is the case please mark the patches with circles or a contour of a distinct color. In particular the patches studied in Figure 6.

Perhaps we are too used to look at these images but we clearly see patches of higher concentration with oval shapes in panels c and d, albeit diluted with respect to panels. The colour contrast on a printed version depends on the printer and might not as good as on a screen. As said above, the best way to track patches is looking at the animation

Except for 28 January, the cases shown in Fig. 6 correspond to other days than Fig. 5. Figure 5 cases were chosen to show a number of patches that were not covered or split by the blank bands where observations are missing (the product is made from only one of the IASI instruments) Figure 6 cases have been selected from some of the best matches between IASI orbits and CALIOP, and as illustrations of specific features. It happens often that CALIOP misses the patches, makes an overpass over the blank domain of IASI or that the matches are too distant in time. We have many other cases that will be exploited and presented in further studies.

L95 due this western strip refers to the strips discussed in the previous section if it is please make it clearer.

Yes it is, this has been clarified.

P4L96 please add a patch near 11E and ***XXS*** *(so that the reader identifies the patch easily).*

Done as suggested. However the patch is that crossed by the two orbit tracks on Fig. 5a, as indicated in the caption of Fig. 6, so it was already well indicated.

P4L96 same question as before "the eastern strip" refers to the previous section? If yes please make it clearer if not please elaborate or add arrows to the figure.

Yes it is again. It is not possible to attribute all the patches to one of the two original strips at this stage in the IMS images. There is a significant amount of overlap due to differential rotation and mixing. We would have put marks on the figures if it was that easy. We leave for a further study a more detailed description of the interaction between the two strips and the observed physical properties.

P4L97 how can we be sure of "along an arc of same angular speed (Fig A1a)" if Fig A1a is an average over 3 months are Figure 6 are snapshots in time.

We cannot be 100% sure but the stratospheric circulation near 25 km has been boringly zonal during the first month and half after the eruption and we have a long sequence of CALIOP cross sections similar to Fig. 6c. Hence we are almost sure that the explanation in terms of the zonal mean flow is relevant. Again the letter format does not allow to expand all the arguments.

P4L100. What is a hairy pattern

This is an analogy for the filamentary structure above the head of the "jelly fish". Perhaps it was not a good idea. It has been changed into "filamentary halo".

P5L120 Not all around the Earth but rather "dispersed the plume through all longitudes".

Corrected as suggested.

P5L120 how many weeks is a few weeks? 2,3, 10. Please be specific. Further, this has not been shown.

Less than a month. It is visible in Figure 5.

P5L122 "about 2km thick" This has not been discussed (shown) previously please specify on which plot are you basing this conclusion.

This is easily seen from CALIOP measurements in Figs 1 & 2. We refer to these two figures.

P5L126 "Our estimation of fall speed and extinction-to-backscatter ratio trends is consistent with a growth up to 1.5-2.0 μm and then a decrease in mean size". This is only after the unrealistic values. Please also mention those here.

There is a misunderstanding here. It is readily explained in the text that the radius calculation should not be considered during the fall speed due to the water vapour cooling that induces a similar descent rate of both water vapour and aerosols. This does not generate per se a doubt on the estimates made during the second phase. It is not unrealistic, it is just invalid. Perhaps, the confusion was due to the word "discrepancy" that we used on p2L43. There is no discrepancy if we explain the observed behaviour. This has been modified.

P5L129 delete strongly. There is no calculation in this study to suggest a strongly underestimation, there is only evidence of an underestimation but not of its magnitude.

Corrected

P5L139 add i.e., before the Solomon citation (other studies have shown the potential of water vapor as well)

Perhaps the referee means e.g.

P5L139 add Millan et al 2022 to the S2022 reference.

Millan et al. 2022 make a general comment that H20 cooling can increase the Brewer-Dobson circulation but do not consider at all the possibility on an intense cooling within the concentrated plume that can induce a fast descent of the plume. They comment about the TOA and surface radiative forcing by quoting S2022. We consider the descent due to cooling as an original contribution of S2022. It is not mentioned either in Schoeberl et al 2022 who nevertheless correctly describe and interpret the second phase of the separation between aerosols and water vapour.

P6L161. Why are you using the near real time data? there should be a more accurate product. Near real time data are normally only used for situations where near real time data is needed. The data discuss in this study is from 8 months ago.

The IMS product is provided in near real time like a number of other satellite products which have applications in numerical weather and air quality forecasts. There is so far no other version for this product.

P6L165. Do you mean SO2?

Yes, this was a LaTeX error.

P6L175. How was the conversion from pressure to altitude done for MLS data? Please specify in the manuscript.

We use the ERA5 data for this conversion. The conversion is performed daily in the zonal average framework. In the considered altitude range, the levels of the ECMWF model are pressure levels. This is now mentioned in the appendix.

P8L218. This details about the python based code should be in the code availability section. Here just say: The theoretical extinction-to-backscatter ratio for the plume has been calculated using Mie calculations. The extinction and backscatter coefficients have been …

We think it is important to draw the attention of the reader who might be interested in conducting such calculations as this tool is very useful and handy.

Figure 2

For panels b, d, and e,   please add a vertical line indicating the separation of the two regimes. In the caption you could say something like: The vertical line separates the two regimes (or phases) as discussed in section X.

Done as suggested.

The title for panel a and c need to be changed to "… band 15S-5S" and to ".. band 25S-15S"

The title for panel b need to be changed to Vertical motion in 15S-5S

The title for panel d need to be changed to Vertical motion in 25S-15S

The labels in e need to be changed to 15S-5S and 25S-15S (similarly the labels in g need to be changed)

Modified as suggested

Figure 3

Please add the location of the volcano in panels a-d.

Done as suggested.

Please repeat the color information in the caption. That is, in the caption include This product allows to qualitatively distinguish thick ash plumes or ice clouds (brown), thin ice clouds (dark blue) and sulphur-containing plumes (green). Mixed ash/sulphur-containing volcanic species would appear in reddish and yellow shades.

Done as suggested.

The (g-h) caption is wrong. It should say 16/01/2022 instead of 16/02/2022.

Corrected

Also, I strongly recommend using the same format for all dates through-out the study. For example, use the format 28 Jan 2022 as used in Figure 1.

Done as suggested

Figure 4 The color bars for SA OD through out the sub figures should match so the reader could easily intercompare the values. The same for the SO2 colorbars.

That said, all SO2 plots and all SA OD should have the same color bar ranges so that the reader could easily intercompare them.

We have homogenized the color scales but there is no reason to use the same one in an instantaneous lat x lon chart and in a zonal mean. We need also to accommodate the dilution with time which is particularly strong for SO2. Nevertheless, the color scale is common in all panels of Fig. 5 and we use always the same minimum value. The color scale of Fig. 5 is used in two panels of Fig.6, a different maximum being used in a third panel to better exhibit the tripolar structure.

Figure 5 Either the D and N labels are wrong ore the caption is wrong. Please double check and fix.

The caption has been corrected.

Figure 6 All other figures al labeled (a,b,c,d,etc) from left to right and top to bottom. Please change the labels to be consistent.

The figure has been reshaped into a vertical two-column format that improves the reading and complies to the standard labelling convention.

Figure A2. Add latitude label. The color bar for this figure should also use a divergent colorbar.

Done as suggested

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