Responses 1.

Dear Editor, dear Authors,

The manuscript "Transport of the Hunga Tonga volcanic aerosols inferred from Himawari-8 limb measurements" applies existing methodologies to derive qualitative information on the stratospheric aerosol load from limb observations at the peripheric field of view of Himawari observations to Hunga Tonga-Hunga Ha'apai (HTHH) eruption in January 2022. The topic of the manuscript is of interest for the AMT readership. The study itself is quite short. The main result, besides the fact that the HTHH eruption signature is visible from Himawari limb observations, is an estimation of the plume speed as it spreads zonally and meridionally. I would consider the possibility to add the words "plume speed" or similar in the title. The paper is relatively clear, even if some aspects should be clarified (see Specific Comments). More efforts are needed to compare Himawari observations with simultaneous CALIOP observations of the HTHH plume, and plume speed results with what is already present in the literature (again, see Specific Comments). Besides these points, I don't actually have major concerns, and, in my opinion, this manuscript should be published when the following Specific Comments are addressed.

My best regards

Thank you for the comments and the time you have taken to consider my paper.

I think the use of "plume" for the aerosol is arguable and the word "transport" conveys a better description of the westward movement and N-S spreading. I have removed the word "Tonga" from the title (for reasons explained in my response below). The Specific Comments are very helpful and I have accepted nearly all of them. My responses below.

Specific Comments:

1) L23: "persisting up to the present time (September 2023)" can you please add a reference for that? As far as I know, there are no long-term observations published on water vapour perturbation from HTHH eruption.

I've added a link to a NASA site that shows those observations and referred to some recent papers on this. Actually, this paper also provides those observations. I have added the text to reflect that it is predicted the aerosol will persist for some years – reference included. I have also added two new Figures which illustrate the spread of the aerosol through time and space up until the end of 2023.

2) L24: "...and converted... please add a statement on the rapid nature of this conversion (Zhu et al., 2022 or Sellitto et al., 2022 already cited in your manuscript)

Added the word “rapidly” and reference as suggested.

3) L25: "observed from the ground..." what about AERONET analyses of Boichu et al. (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2023JD039010?af=R)? For the satellite dispersion you might mention the IASI (already shown by Sellitto et al. 2022 and Legras et al., 2022 but also more systematically here: https://essopenarchive.org/users/527694/articles/656444-observing-the-so2-and-sulphate-aerosol-plumes-from-the-2022-hunga-tonga-hunga-ha-apai-eruption-with-iasi)

Thanks. I missed that reference but have now added it in.

4) L29: the phrasing "limb viewing *aspect*" should be clarified already at this point (even if this is clearer later in the text, i.e. in the methodology section)

I have clarified what is meant by “limb viewing” and placed this as a new sentence in the section “Limb geometry” – which I think is the best place.

5) L45-46: how much is the resolution in km?

AHI resolutions are now included in a new paragraph under “Methodology”. The resolutions vary depending on spectral channel.

6) L53-54: the sentence is not fully clear to me

The detail was deliberately excluded from this paper as it is elegantly explained by Horvath et al. (2021). The correction arises from the fact that the view is not strictly at 90 degrees so there is a very slight foreshortening in h. I feel this is well explained by Horvath and the point of mentioning this here was to alert the reader to the full detail in Horvath’s paper. However, I have added further explanatory sentences as I agree this part needed more information.

7) L56-58: "...which are assigned...limb...of a NaN." is it too technical for this kind of manuscript? This can probably be deleted.

I have retained this but defined the meaning of NaN. I like to keep this as detection of these values is part of the algorithm.

8) L59-60: this is also difficult to understand for me. Can you please clarify?

I have added an explanation in the revision. By setting a lower bound on the reflectance it is hoped that clear space is being detected with no atmosphere – so this provides an upper height value.

9) L60-on and Eq. 2.2: even if previous works are cited here adressing this issue, more details should be added to the reasons for the choice of the two wavelengths in the definition of the parameter R. Is this choice supported by RTM simulations? Why not other channels? E.g. the IR channel at 11.2 microns is located in a sensitive band for H_2SO_4 (see Sellitto and Legras 2016 or Sellitto et al. 2017), can any information on the HTHH plume be extracted using this channel?

Agreed. I have added an explanation and a new Figure that supports the use of the R ratio. I did look at one infrared channel (11.2 µm) but essentially this does not give better information than the ratio. The problem is that this is an emission measurement (with cold space) behind the aerosol so the sensitivity does not appear to be good enough.

10) Figure 2: not clear what red and green curves are. Is it the same as Fig. 3b? Please explain and add this information in the caption.

Agreed. I have added definitions in the caption.

11) Figure 2 caption: delta angle symbol is not consistent in the figure and caption

Thanks. Fixed.

12) L67: "in a sequence of three panels (Fig. 3)" --> "in Fig. 3"

Thanks. Changed.

13) L88-89: by the way, how can R be negative?

Yes my sentence is misleading. I have re-worded this to read >0 and degree of "positiveness".

14) L92-93: please have a look at this for satellite observations of the particle size distribution of HTHH plume: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL105076

Thanks. Reference added in two places (here and in Conclusions).

15) L118-119: please also discuss the many differences between Himawari and CALIOP observations in Fig 6

Agreed. I have added sentences describing the main differences and speculated on the reasons – mostly due to differences in viewing geometries. I have also added another reference.

16) Is there anything more that can be done in terms of comparison of limb Himawari and CALIOP observations? Please consider more systematic comparisons, which is basically the only “major” comment that I have for this manuscript.

Yes I am sure there is. Unfortunately, I think this would require a lot of work and a new paper. The problem is trying to find coincidences and accounting for the different viewing geometries. It would be a major work but is do-able and would also require some radiative transfer calculations to make the R-ratio more quantitative.

17) L121-122: why this choice of altitude intervals? How they represent what's mentioned at L123-124? What if different choices of altitudes ranges are chosen?

I did try different intervals but found only small differences. The ranges were informed by analysing numerous vertical profile plots at various latitude locations and also to look at different layers within the stratosphere.

18) L137-138: this is discussed in Legras et al., 2022; please use their work to discuss your results: are the vertical spread and different velocities pointed out by Legras et al., 2022 consistent with what you get?

Yes. The Legras et al. paper is referred to just above (L127) where it is mentioned that these results agree with Legras. So yes they are consistent.

19) Fig. 5 and others: can also a longitude information be added to this kind of figures - like done for CALIOP curtains e.g.?

Not really. The limb view covers a range of longitudes but I have included the two limits and stated the range in variation of longitude (somewhat similar to how Caliop samples the Earth below).

20) Fig 7: why not putting letters to identify panels (panels a, b, c and d)?

Yes. I have added letters (A), (B) etc as suggested.

21) Fig 7: it is somewhat surprising that the aerosol signal in Himawari limb observations shows up only starting from about 1 month after the eruption. A number of previous works have shown that the formation of secondary sulphate aerosols was very quick for this eruption. How can you explain this? Can you, e.g. a very quick apparation of large signal in the lower stratosphere in the western limb,

as expected considering the HTHH literature and, in particular, CALIOP, OMPS and IASI observations of Legras et al., 2022 and Sellitto et al., 2022?

The first detection is on 22 January (just 1 week after the eruption)-see Fig. 3. But the aerosol layer is moving, taking ~16 days to circumnavigate the globe (at 20-30 S). The limb detection has some lower sensitivity (which I have not estimated), so even if there is aerosol at all longitudes along these southern latitudes, it is only detected if sufficiently optically thick.

22) L147: "largest" considering which metrics?

Well there are several metrics and it's probably the largest according to many of them. I have changed "largest" to "most energetic" – and I think that is a defendable statement (I've added a reference).

23) Throughout the whole paper and also in the title: please use the full name of the volcano "Hunga Tonga-Hunga Ha'apai" or, after defining it, the abbreviation "HTHH".

Not agreed. As explained by Van Eaton et al (2023), the islands Hunga Tonga and Hunga Ha'apai are part of a much larger (submerged) caldera belonging to the Hunga volcano. So I have changed "Hunga Tonga" to "Hunga", added the Van Eaton et al (2023) reference and included "Hunga Tonga-Hunga Ha'apai" in two places as this seems now to have become common usage.

Responses 2.

This paper presents an interesting study on the ability of Himawari-8 limb observations to provide information on the vertical profile of aerosols injected into the atmosphere by volcanic eruptions, such as that of Hunga Tonga in early 2022. The results are convincing about the possibility to use geostationary satellite measurements for such purpose.

Thank you for the comments and your time to consider my paper.

However, I have the following reservations before publications in Atmospheric Measurement Techniques:

- Other aerosol products based on Himawari-8 measurements are available, e.g. the RGB-Ash product (https://navigator.eumetsat.int/product/EO:EUM:DAT:MSG:VOLCANO/print) that has been used to analyze Hunga Tonga aerosols. Such products should be mentioned in the introduction and contrasted to the aerosol product presented in this study, which focuses on the aerosol vertical distribution.

  I have added a sentence about aerosol products from geostationary satellite instruments. I don't think the RGB ash product is particularly relevant for this study.

- The paper is loosely written and often lacks details that would help better understand some aspects of the study. For example, the description of the Advanced Himawari Imager (AHI) instrument is lacking and there is few information on the measured radiances and their characteristics. Only the limb geometry is explained in section 2, which relies largely on a study by Horvath et al. (2021) without a summary of the corresponding methodology. As an example, the parameter "h" in equation 2.1 is not precisely defined and it is not clear also what is considered to be the Earth limb. In satellite limb measurements the limb corresponds to the whole atmosphere viewed by the instrument, which does not seem to be the case here. A more thorough definition of the various parameters and notions used in the study is thus needed.

  Yes it was an oversight not to include some discussion of AHI in the main text (it is in the Abstract). I have rectified that by adding two sentences (in Methodology) and a reference to the Himawari Users Guide.

  I have also added more text, 3 new Figures which I hope explain the analyses and results more clearly.

I have defined h and the meaning of “limb”. Horvath et al. (2021) provide a very detailed analysis of the geometry of limb viewing using geostationary instruments and I do not see a need to repeat that in this paper.

- Some characteristics of the study are given without explanation. For example, in page 3, line 59, it is not clear why the edge of the atmosphere corresponds to pixels with 0.45 μm values <1%. Similarly, why only 7 of the spectral channels are used and what is behind the choice of the mentioned channels?

Yes. I have added an explanation of the use of <1% and added a reference. Only 7 channels were considered as the other 9 channels are either redundant or not useful. I have added a sentence to provide a reason for this selection.

- Even if the longitude of the points considered in the analysis is undefined a possible range of longitudes could be indicated by taking into account the viewing geometry of the satellite instrument.

Yes. I have included the eastern and western longitude limits as suggested and added a range for the eastern and western look directions.

- More information is needed on the limits of the methodology described in the paper for the determination of aerosol vertical profile. In which altitude range is it the most efficient? What is the accuracy of the R product introduced in Equation 2.2? More information is also required on the choice of the reflectance at 1.61 and 2.25 μm. Some references are mentioned but overall the explanations are not sufficient.

I have added a Figure (new Figure 4) which explains the principle for using channels at 1.61 and 2.25 μm and explanatory sentences. It’s not clear to me what is being asked about the efficiency of the altitude range? Essentially the altitude resolution is determined by the inherent pixel size, the angular sampling and the viewing geometry. It is difficult to assess the accuracy of the R product without having something to compare it against. Caliop is a good candidate for this but the differences in viewing geometry between Caliop and Himawari make this difficult to interpret in a quantitative manner. I have added sentences which explain this. I guess some careful radiative transfer calculations would help determine an accuracy for R but I feel this is beyond the scope of this short paper, which was simply to introduce the use of limb measurements from an unlikely source.

- Since the aerosol cloud takes some time to form from the SO2 cloud, despite more rapid formation linked to increased water vapor amounts due to the

Hunga Tonga eruption, results of the study should be analyzed in this context. How is the method sensitive to the initial evolution of the aerosols from initial ash to ice and liquid aerosols? How is the estimated meridional velocity in section 3.2 affected by possible artefacts in the method?

Actually, it seems that as the sulfate aerosol formed quite rapidly, detecting it after 7 days seems quite plausible. I don't think there was much ash in the dispersing cloud, certainly nothing like that expected from such a large eruption. Therefore I don't think it is necessary to discuss these processes in any detail. It is known (from the recent literature) that the aerosol formed quickly and consisted of predominantly sulfate/liquid water aerosols. The main sensitivity of the method is to optical opacity of the aerosol. As explained, initially (less than 1 week) no aerosol is seen in the either limb and this is likely because it was not dense enough to detect and also it had not had enough time to reach the western or eastern limbs. The first detection on 22 January is towards the west; a few hours before looking east there is no aerosol. Thereafter, as the aerosol travels around the Earth, almost periodic peaks in the aerosol are observed (every 16 days or so). Now there may well be other aerosols (smoke from fires, other eruptions, convective outflows into the lower stratosphere) that contribute to changes in the stratospheric optical depth. The ratio was chosen as it is sensitive to liquid water and ice and so is mostly detecting the Hunga Tonga aerosol. The averaging process, time period, coherence and agreement with ERA-5 climatology (and other work, e.g. Legras et al) give confidence that any artefacts due to the methodology are minimal.

I have added two new Figures: one showing the temporal evolution of the aerosol (as measured by the reflectance ratio) and one showing the meridional spread over the 26 months of data analyses. I think these Figures help to demonstrate that the methodology has some value in assessing the transport and spread of the Hunga aerosol.

Minor comments

- The figures' legends are generally incomplete. For instance, legend of Figure 1 does not describe the red, green and dashed black lines. Legend of Figure 5 does not mention the limb (East or West).

Thanks. Descriptions of the red, green and dashed black lines are included in the Figure caption. The direction (

- The initial letter of "Earth" should be capitalized throughout the paper.

Thanks. Fixed.