RC1: 'Comment on egusphere-2024-1688'

This paper calculates the radiative forcing of aerosol and water vapor volcanic cloud generated by the Hunga volcano eruption in January 2022. The calculations were performed for the specific location of Reunion Island. The perturbations of stratospheric aerosol were calculated, neglecting the effect of background aerosols, which can cause a 20-25% error; for water vapor, it was assumed that the unperturbed value is 4.5 ppmV for all altitudes, which was not precisely correct. The authors used a Line-by-Line radiative transfer model with the highest resolution of 20 cm-1 for radiative transfer calculations. That might be the course for resolving the effects of stratospheric water vapor, but it worked well. The authors extrapolated the imaginary part of the sulfate aerosol refractive index from near IR to visible and UV. As a result, they overestimated aerosol short wave (SW) absorption. This is especially well seen in stratospheric radiative heating, as the paper reports warming of the stratosphere during, e.g., the first four months after the eruption, while observations show significant cooling. The radiative forcing at the top of the atmosphere is reasonably correct, but SW aerosol radiative forcing at the bottom of the atmosphere (BOA) is exaggerated. These drawbacks have to be rectified before the paper can be published.

Reply: Thank you very much. We greatly appreciate the reviewer feedback and critical comments. In this new manuscript new estimations of the aerosol and WV radiative effects are presented. The greatest modification (wrt the initial estimations) is that the single scattering albedo in visible and UV has now been forced to 1 → no absorption, only scattering. This has had the effect of increasing (in absolute value) significantly the TOA radiative effect while reducing the BOA radiative effect, resulting now in a negative atmospheric (TOA-BOA) SW+LW radiative effect caused by the aerosols and the water vapor on the stratosphere. The discussion in Section 4 has also been changed accordingly.

As volcanologist recently updated the name of the volcano to “Hunga”, the name was updated everywhere in the manuscript, including in the title.

Specific comments:

L38: The mass of water retained in the stratosphere was unprecedented, not the amount of emitted water.

Reply: Corrected.

L42: Do you mean at the location of Reunion Island or globally? I do not think it is right globally.

Reply: Some precisions have been brought in this sentence which now reads:

“Still, the stratospheric aerosol optical depth (sAOD) has been recorded **globally** as the largest since Pinatubo eruption (Taha et al., 2022) and peaked **locally** at values never observed before**, e.g. in the Indian Ocean** (Baron et al., 2023).”

L77: In this context, the reference should be "Jenkins et al. (2023)." Please correct the text in many other similar cases.

Reply: All references have been revised and the format Lastname et al. (yyyy) has been applied everywhere it was needed.

L108: Legrand et al. (2022) reported that the aerosol spatial distribution was patchy due to dynamic instabilities for more than six months.

Reply: The referee probably refers to Legras et al. (2022), as we have not found any article of Legrand et al. from 2022 about the HTHH. Legras et al. (2022) say that "volcanic sulfates and water still persisted after six months". It is true and our work shows that 14 months after the eruption volcanic sulfates and water still persist. Legras et al. (2022) also say that the aerosol spatial distribution was patchy due to dynamic instabilities, but only during the first 2 months (see their Section 6).

L190: Extrapolating the imaginary refractive index could cause spurious absorption in the UV and visible wave bands. It is well known that sulfate aerosols do not absorb in those wave bands.

Reply: In the dataset used, the last value of IRI at 2.36 µm is 4×10^{-6} and it is this value (and not strictly 0) that has been assumed for IRI from 0.2 to 2.36 µm. Although very small, we realize now thanks to this comment that the associated SSA below 2.36 µm is indeed different from 1. We have now forced SSA in all shortwave spectral bands of GAME to the value of 1.00. The results are quite different. Fig. 4, 6 and 7 are new and the discussion has been totally revised.

Figure 1: Please show your aerosol LW SAOD for 10 um.

Reply: Figure 1 shows the spectral refractive index used for the calculation of the aerosol radiative properties. So, we are sorry to say that we don't understand this request at this place.

L236: You should use the word dispersion instead of dilution. SAOD is also defined by the rate of SO2 to SO4 conversion. OMPS-LP misses the initial stage of the SAOD generation, so it is not surprising that you see a discrepancy with OMPS observations at the initial stage.

Reply: Dispersion is used now everywhere in the manuscript instead of dilution.

L257: "zonal scale" - please clarify the sentence.

Reply: The sentence has been reformulated as:

"Such a difference, although not so accentuated, is observed zonally at 15º S during the first six months of year 2022 (Schoeberl et al., 2022)."

L263: Please be more specific.

Reply: The complement “although other mechanisms of volcanic aerosol removal exist” has been removed. Although it is true in a general sense, no other mechanisms in the case of HTHH are mentioned in Schoerberl et al. (2022).

L307: "probably correlated" > "caused"

Reply: Corrected.

L309: These results from (Zhu et al., 2022) cannot be used for comparison with your calculations, as without water vapor, volcanic clouds have different evolution and cannot be correctly interpreted.

Reply: The comparison with Zhu et al. (2022) for the aerosols has been removed.

Figure 7 shows Hunga's aerosol heating rate reaching 0.8 k/day in the first four months after the eruption, while after Pinatubo eruption the aerosol stratospheric heating rates were below 0.3 K/day. This cannot be right.

Reply: The new estimations of the radiative effect after the modifications mentioned in our first reply now show an aerosol stratospheric heating rate below 0.25 K/day for the period M2-M4 and below 0.07 K/day for the period M5-M14. Fig. 7 and the discussion have been changed accordingly.

L433-436: This conclusion about stratospheric warming contradicts observations that reported significant stratospheric cooling.

Reply: The new estimations of the radiative effect after the modifications mentioned in our first reply now show a slight stratospheric cooling.

RC2: 'Comment on egusphere-2024-1688'

This paper focusses on the characterization of stratospheric aerosols and water vapor over Reunion Islands in the southern tropical Indian Ocean. The manuscript associated variations in these two atmospheric parameters with the eruption of the Hunga Tonga-Hunga Ha' apai volcano on January 2022. The methodology used are based on backscattering lidar measurements emitting at 355 nm and on the Ozone Mapper and Profiler Suite Limb satellite. The Microwave Limb Sounder is also used for monthly mean water vapor. In general, the use of these instruments can serve sattisfactory for the purpuses of the study. Authors also claim the use MERRA-2 but it is not clear in the manuscript why they use this data. In generall, the authors presents very interesting measurements that are suitable for publications in Atmospheric Chemistry and Physics due to the possible impacts on the climate.

The manuscript also uses the measurements over Reunion Island in the GAME radiative transfer model for computing direct radiative transfer. I agree with the comments made by the previous referee. For aerosols, the input parameters have large uncertainties. For example, with the limited number of lidar measurements it is not possible the retrieval of aerosol microphysical properties that ultimately may affect GAME computations. I understand the proxies made by the authors, but it must be translated in error bars. This is a weak point that must be addressed before the final publication of the manuscript.

Reply: Thank you very much. We greatly appreciate the reviewer feedback and critical comments. An error budget has been calculated using propagation of errors of the lidar ratio and the radius of the size distribution considered on the aerosol radiative effect. A new Section "Error budget" has been added in the revised manuscript. See 7 comments ahead for a more detailed answer.

As volcanologist recently updated the name of the volcano to "Hunga", the name was updated everywhere in the manuscript, including in the title.

The authors in the last line of the conclusions claim that from this volcano eruption there is a clear impact on the regional climate of the Earh-Atmosphere system in the southern tropical Indian Ocean region'. To me this can not be deduced from the measurements and analyses performed in the manuscript. The tittle is confused as it suggests this impact on climate. I think that the title is incorrect and should be modified to reflect the purpursoes of the manuscript.

Reply: We have removed all conclusions on a possible "regional climate impact". The last sentence of the conclusion now reads:

"This study shows that the eruption of HTHH has had, so far, a clear radiative impact on the Earth's radiation budget in the southern tropical Indian Ocean region."

About the title of our paper "Radiative impact of the Hunga Tonga-Hunga Ha'apai stratospheric volcanic plume: role of aerosols and water vapor in the southern tropical Indian Ocean", we think it is quite appropriate with the purpose of the paper. No impact on climate is mentioned. If what is found confusing is the generalization of our results to the whole "southern tropical Indian Ocean region" (which is indeed justified at the end of Section 2.1), we ask the referee to say so, so that we can modify/remove this part of the paper, and then adapt the title of the paper accordingly.

Generally the manuscript is well-written, although there are many naive mistakes that must be improved to make the manuscript better legible:

Introduction Section: In general is very well, but I miss many references. For example:

Lines 33-34: Reference needed after “Several figures are evidences of a record-breaking atmosphere event”. What are you referreing by ‘Figures’

Reply: We mean “features”. It has been corrected. These features are listed next with their corresponding references.

Lines 59-60: Reference needed after “Because ozone is not emitted primarly during volcanic eruptions, its loss or production by post-eruption reactions are more tedious to estimate”

Reply: The reference of Evan et al. (2023) has been added.

Lines 70-71: Reference needed after “… as volcanic sulfates are concerned, these aerosols usually scatter sunlight back to space, cooling the Earth´s atmosphere, and absorb outgoing thermal radiation”

Reply: The reference of Robock (2000) has been added.

Materials and Methods:

A brief overview is needed for this lidar – e.g. number of wavelengths, laser power, type of detection.

Reply: A full description of the lidar systems and their aerosol products at OPAR (Observatoire de Physique de l’Atmosphère à La Réunion) has just been accepted for publication in ESSD journal (Gantois et al., 2024). The last sentence of the first paragraph of Section 2.1 has been replaced by:

“A full description of the system is available in the data paper of Gantois et al. (2024).”

Gantois, D., Payen, G., Sicard, M., Duflot, V., Bègue, N., Portafaix, T., Marquestaut, N., Godin-Beekmann, S., Hernandez, P., and Golubic, E.: Multiwavelength, aerosol lidars at Maïdo supersite, Reunion Island, France: instruments description, data processing chain and quality assessment, Earth Syst. Sci. Data Discuss. [preprint], https://doi.org/10.5194/essd-2024-93, Accepted, 2024.

Authors mention 87 nights of measurements. How frequently are acquired the measurements.

Reply: The measurements are made twice a week on Monday and Tuesday nights. This information has been added in the first paragraph of Section 2.1.

Authors use 30 sr as lidar ratio. This is a potential source of errors because i) lidar ratio affect for the computation of the entire profile and ii) it might not be the real values. How do you accounts this possible source of uncertainties in GAME computations ?

Ozone Mapper and Profiler Suite Limb: Authors just use public data (that must be correctly referenced). But they are introducing additional errors in sAOD by forcing lidar to 745 nm for comparisons. Why not using 510 nm that is the closest wavelength to lidar measurements. If I am right, authors use $AE_{355/745}$ of -0.14 that might not be the real value for each specific measurement. That could add errors in direct radiative forcing computations.

The GAME radiative transfer model

I see that size distribution, single scattering albedo and assimetry parameters must be inputs and assumptions are made. This ok. But what is the final error in the computations? This could be computed assuming other aerosol optical and microphysical properties in the literature. Have you made these computations ?

Reply: We are answering in one place to the 3 comments above. We performed a sensitivity study on the lidar ratio and its associated error, and on the geometric median radius for which we assume a possible small decrease as observed by Duchamp et al. (2023). The results are compared to the nominal estimation and uncertainties in relative terms for TOA and BOA and in absolute terms for ATM are given in a new Table 1. This information fills in a new Section 2.3.2. Error budget. The sensitivity study on the geometric median radius results obviously in a change of all optical and radiative properties, including $AE_{355/745}$. This new Section 2.3.2. reveals an uncertainty of $DRE(ATM)$ in the same order of magnitude than some of the $DRE(ATM)$ retrieved in the Section 4, and the results are discussed more cautiously in this respect. We copy paste here the new Section 2.3.2.

2.3.2 Error budget

An error budget is performed to quantify the uncertainties made on the radiative effect estimations using GAME and caused by the model itself, our parametrization and the hypothesis made. GAME model participated to an intercomparison exercise (Halthore et al., 2005) which concluded that it is accurate to a few units of watt (< 5 W) for a flux reaching 1000 W m^{-2}. The impact of this uncertainty on our estimations should be even less since only daily averaged fluxes are considered. It is thus reasonable to consider an uncertainty in relative terms of 0.5 %.

Two other sources of error are considered: one associated to the lidar ratio selected and another associated to the size distribution selected. The constant lidar ratio used in the elastic, 2-component inversion algorithm is 30 sr. Baron et al. (2023) estimated an uncertainty of ± 10 sr for the HTHH plume over Reunion Island in January 2022 (see Section 2.1). New profiles of the extinction inverted using (30 + 10) sr and (30 - 10) sr were used in GAME to quantify the deviation from the nominal (LR = 30 sr) radiative effect estimations. As far as the size distribution is concerned, Duchamp et al. (2023) detected “a small decreasing trend in the size” without quantifying it. We have assumed a decrease of the geometric median radius of -0.01 µm. Thus, a new Mie calculation was performed with a geometric median radius of 0.34 µm and the resulting radiative properties were used in GAME to quantify the deviation from the nominal (geometric median radius of 0.35 µm) radiative effect estimations. The results from these uncertainties are given in Table 1 in relative terms at BOA and TOA and in absolute terms in the atmosphere. Logically, the lidar ratio error which impacts almost proportionally the sAOD error is by far the strongest. We can reasonably consider that the aerosol daily radiative effects are estimated with an uncertainty better than 48 % at TOA and better than 42 % at BOA. The resulting atmospheric radiative effect (TOA – BOA, see Eq. 2) is given with an uncertainty of +0.09 / -0.06 W m^{-2}.

Source of error	TOA	BOA	ATM
GAME model	< + 0.5 %	< + 0.5 %	< + 0.5 %
LR (+10 / -10 sr)	+47 / -40 %	+42 / -38 %	+0.09 / -0.06 W m^{-2}
Geometric median radius (-0.01 µm)	+4 %	~0 %	< 0.01 W m^{-2}
Total	+48 / -40 %	+42 / -38 %	+0.09 / -0.06 W m^{-2}

Table 1. Error budget of the aerosol daily radiative effect.

Results

Generally, I would like to point out a naive mistake: Many Figures are not introduced in the text and they just show up in the discussions. For a mature paper, every Figure must be appropiately introduced. The same happens for Tables. For example, in 282 says ' 4 runs of GAME are performed and summarized in Table', and when going to the Table I only find the configurations used in GAME.

Reply: We have been to all first calls of the figures and tables of the paper.

Fig. 1 (line 197) is properly introduced.

Fig. 2 (line 241), now introduced in the text.

Fig. 3 (line 259), now introduced in the text.

Fig. 4, 5 and 6 (line 314) are properly introduced.

Fig. 7 (line 325) is also properly introduced.

Table 1 (NEW, line 235) is properly introduced.

Table 2 (line 308) is properly introduced. We have changed the word "run" by "parametrization".

Table 3 (line 325) is already properly introduced.

Line 217: Background sAOD of 0.00259. How this value is computed ? I guess that the error associated with the measurements is larger than your standard deviations and might not have sense to give three significative values.

Reply: The background sAOD of 0.00259 is the mean of the monthly sAOD of the unperturbed years 20212 and 2013 (see Line 214 of the original manuscript). It is true that the error associated with the measurements is larger than the standard deviations found for this background sAOD. We have removed 1 digit and the background sAOD is now given as $(2.6 \pm 0.1) \times 10^{-3}$.

Line 237: The volcano also injected particles in the troposphere.

Reply: True, but tropospheric effects are out of the scope of this work.

Line 437-438: The study does not show the impact of HTHH on the regional climate in the southern tropical Indian Ocean region. To me, it deals with the aerosol and water vapor characterization plus radiative forcing computations. It might the impact claimed by the authors, but it can not be deduced from the results and discussions presented.

Reply: We have removed all conclusions on a possible "regional climate impact". The last sentence of the conclusion now reads:

"This study shows that the eruption of HTHH has had, so far, a clear radiative impact on the Earth's radiation budget in the southern tropical Indian Ocean region."

And I would like to add that I agree with the comments made by the other referee

Reply: All comments of RC1 have been taken into account in the revised manuscript. Please see the answers to that referee's comments.

RC3: 'Comment on egusphere-2024-1688'

The paper presents a good radiative characterization of the Hunga Tonga Hunga Ha'apai eruption. The authors present measurements and observations obtained at Reunion Island with Lidar and satellite measurements.

in the work an analysis of the results is presented in an analytical but very clear way making the paper clear and sequential in reading. Regarding the methodological part I think that some more details without having to resort to the references indicated would have been useful to make the reader easily informed on the observational capabilities. Specifically a more exhaustive description of the lidar system would give the reader the possibility to understand the characteristics and observational potential of the Reunion observatory. Even a few brief additions on why certain assumptions were chosen in the data analysis would have provided the reader who is not an expert in Lidar with a more comprehensive explanation of the work (e.g. Line 91 LR=30).

Reply: Thank you very much. We greatly appreciate the reviewer feedback and critical comments.

A more exhaustive description of the lidar system has also been requested by Referee #2. A full description of the lidar systems and their aerosol products at OPAR (Observatoire de Physique de l'Atmosphère à La Réunion) has just been accepted for publication in ESSD journal (Gantois et al., 2024). The last sentence of the first paragraph of Section 2.1 has been replaced by:

"A full description of the system is available in the data paper of Gantois et al. (2024)."

Gantois, D., Payen, G., Sicard, M., Duflot, V., Bègue, N., Portafaix, T., Marquestaut, N., Godin-Beekmann, S., Hernandez, P., and Golubic, E.: Multiwavelength, aerosol lidars at Maïdo supersite, Reunion Island, France: instruments description, data processing chain and quality assessment, Earth Syst. Sci. Data Discuss. [preprint], https://doi.org/10.5194/essd-2024-93, Accepted, 2024.

In addition, a sentence summarizing the finding of Baron et al. (2023) for justifying the choice of the lidar ratio at 355 nm has been added. It reads:

"Indeed the latter found values of LR at 355 nm in the range 29 – 35 sr with small standard deviations (< 7 sr) by applying the transmittance method during several nights in January 2022."

Finally, as volcanologist recently updated the name of the volcano to "Hunga", the name was updated everywhere in the manuscript, including in the title.

From my point of view therefore the work is important to be published also given the low frequency of these events which as illustrated by the authors see in literature still relevant presentations of the eruption of Pinatubo and El Chichon underlining to the scientific community the importance of these ground and satellite observation systems for the study and characterization of these events.

Reply: We do also hope that our work will be published in order for our results to serve as constraint reference points for future works estimating HTHH forcing impact at larger scales.