RC1: 'Comment on egusphere-2024-1688'

The paper calculates the radiative forcing at Reunion Island in the southern tropical Indian Ocean. The radiative transfer calculations use lidar observations combined with reanalysis data. The methodology sounds. The results could be helpful for a deeper understanding of physical processes and validating the model output. In the revised version, the results are reasonable except for the aerosol LW DRE at TOA (Fig. 4a), Which cannot be negative. This should be corrected. However, the paper is poorly written and suffers from multiple misinterpretations. The radiative forcing over Reunion Island is correlated with the hemispheric radiative forcing but is not representative of the entire Southern Indian Ocean, as the authors suggest. The volcanic cloud homogenized quickly in the zonal direction, so it would be correct to talk about the forcing in the Southern Hemisphere. The poleward dispersion in the stratosphere is slow, and significant latitudinal gradients of stratospheric aerosols and water vapor remain longer. Consequently, the radiative forcing calculated in this study is 2-3 times bigger than the hemispheric mean forcing calculated in other studies.

Reply: The referee spotted a hidden shortcoming of our analysis. Thank you very much for the detailed review. Yes, indeed LW DRE(TOA) cannot be negative. Several tries were made in which we wanted to test the implication of subtracting a non-zero aerosol background in the stratosphere. Those tests gave mostly positive LW DRE(TOA) with occasional negative values. And one of those tests was used in the original manuscript. We are deeply sorry for the inconvenience. When forcing sAOD to zero in the unperturbed simulations, as it has been stated from the very beginning, we obtain strictly positive LW DRE(TOA). This has also the effect of reducing the SW+LW DRE(TOA) which now agrees better with hemispheric studies (see Gupta et al. (2023) and a comment of the referee ahead). All calculations for the Aerosols and for the Aerosols and water vapor cases have been redone. Figures 4, 6 and 7 as well as Table 2 have been updated accordingly.

We approve the referee's statement to say that our site is not representative of the entire Southern Indian Ocean. The last paragraph of Section 2.1 has been removed. We now state that the in the revised manuscript that our results as representative of Reunion Island or at the most of the zonal hemisphere at 21º S.

With the overall review of the manuscript, the new calculations in the LW for the aerosols and the elimination of the discussion about DRE(ATM) which indeed made no sense without including the troposphere (see a couple of comments of the referee on that topic), we now hope that most of the misinterpretations have disappeared and that the manuscript has reached sufficient quality to be published in ACP.

Specific comments:

L23: Water vapor practically does not have any radiative effect at the surface.
Reply: The sentence "Water vapor has hardly any radiative effect at the surface." has been added in the abstract.

L24-26: Short-term and long-term periods are not defined.
Reply: These periods are defined in terms of months after the eruption. We have also added the periods now.

L33: Volcanic clouds cannot be dispersed from pole to pole in three months.
Reply: We have re-read Taha et al., 2022 and have smoothen this sentence which now reads: "The event showed a fast spatio-temporal, global dispersion of the stratospheric volcanic matter that circulated the Earth in only one week (Khaykin et al., 2022) **with small parts of the main aerosol layer** dispersed pole-to-pole in three months (Taha et al., 2022), first in the form of concentrated patches (Legras et al., 2022).".

L40: 37 Tg was probably retained in the stratosphere as a result of cross-tropopause transport. Evidence shows that Pinatubo injected more than 100 Tg of water vapor, which was quickly sedimented.
Reply: The sentence has been rewritten as: "The mass of water retained in the atmosphere was unprecedented: (Millán et al., 2022) estimated to 146 Tg the mass of water injected in the stratosphere (.e.g. 37 Tg of water was retained in the stratosphere as a result of cross-tropopause transport after the 1991 Pinatubo eruption (Pitari and Mancini, 2002)).".

L44-45: It is misleading to make a comparison in one point. Of course, Pinatubo and El Chichon produced higher concentrations of SO2 and SO4 locally.
Reply: The last part of this sentence has been deleted.

L50: Larger in comparison with what?
Reply: Larger compared to low-concentration volcanic sulfate that do not coagulate. The sentence has been modified as "Higher concentrations of volcanic sulfate led to more rapid coagulation and thus to particles quickly growing in size.".

L51: Faster SO2-to-SO4 conversion causes faster growth (in time) of sAOD but cannot change the total sAOD if the same mass of SO2 is converted.
Reply: The change in sAOD is caused by the coagulation mechanism which is enhanced in dense plumes, as evidenced by Zhu et al., 2022.

L63-74: Please be more specific. Water vapor emits and absorbs LW radiation. The injection height is an important factor, but not a driver. Sulfate aerosols absorb LW and SW near IR. Schoeberl et al. (2024) extended the analysis to 2 years.
Reply: This paragraph has been replaced by "In particular, the climate forcing will depend on the radiative effect produced by the water vapor longwave emission and absorption, and the sulfate aerosol longwave and shortwave near-infrared absorption (Robock, 2000). These interaction mechanisms

(emission and absorption) with the longwave and shortwave near-infrared radiation are height-dependent and determine the sign of the differential of energy gained (positive) or lost (negative) in all layers of the atmosphere.". The reference of Schoeberl et al., 2024 is very interesting. Thank you. It has been added also.

L75: More uncertain than what? Greater sensitivity to variations of what?
Reply: This sentence has been removed.

L92: remove "coefficient."
Reply: Corrected.

L101: What uncommon properties? It was sulfate aerosols most of the time.
Reply: The uncommon adjective refers to the lidar ratio values found in the range 29-35 sr, which are not common values for sulfate aerosols. This sentence was reformulated as "These uncommon lidar ratio values for sulfate aerosols were proved to be stable over time by Duchamp et al. (2023) using SAGE-III (Stratospheric Aerosol and Gas Experiment) observations.".

L113: It is not a scientific argument. Please remove.
L115-122: Why is this important if lidar observes the stratosphere? Does the retrieval use the AERONET observations?
Reply: We agree with the referee, in view also of the recent publication of Schoeberl et al. (2024), that a clear latitudinal gradient exist. We do not generalize anymore our results to the southern tropical Indian Ocean region. The paragraph mentioned has been deleted. The discussions about the regionalization of our results in the text and in the conclusions have been deleted. Consequently, the title has been modified as follows: "Radiative impact of the Hunga stratospheric volcanic plume: role of aerosols and water vapor over Reunion Island (21º S, 55º E)".

The second comment about L115-122 becomes therefore irrelevant.

L137: It should be "parameterize." It is written correctly in one place and incorrectly in many others.
Reply: Corrected.

L177: DISORT requires a phase function, not an asymmetry parameter. Do you use a parameterization for the phase function?
Reply: The DISORT module in GAME is parameterized with the Henyey-Greenstein analytic formula that approximates the shape of the actual phase function as a function of the asymmetry factor.

L198: What is the value of the imaginary refractive index in the 0.2-2.36 um range?
Reply: Its value has been forced to 1, so as to answer to Referee #1 and #2 of the initial review.

L220-221: "budget" > "estimate"
Reply: Corrected.

L226-235: Repetition
Reply: The second sentence of this paragraph has been deleted. The rest has not been said before.

L236: "strongest" > "largest"
Reply: Corrected.

L250-253: It is misleading to make this comparison in one point for volcanoes located in different places.
Reply: The comparison is made for estimations of global sAOD. It has been emphasized in the sentence which now reads: “Further back historically, the 40+ year satellite record of monthly sAOD at the scale of the globe (i.e. for the 60º S – 60º N latitude band) in Khaykin et al. (2022) shows that only the eruptions of Pinatubo (1991) and El Chichón (1982) exceeded the Hunga one in terms of absolute stratospheric AOD (by a factor of 6 and 3, respectively).”.

L265-266: This proves homogenization in the zonal direction. We know it is fast.
Reply: “global dispersion” has been replaced by “dispersion in the zonal direction” in this part of the manuscript which now reads: “This time difference is an indication of the dispersion time of the volcanic matter injected by Hunga volcano in the stratosphere in the zonal direction. Other studies confirm that some parts of the volcanic plume dispersed pole-to-pole in three months (Khaykin et al., 2022; Taha et al., 2022). Another indicator of this dispersion in the zonal direction is the standard deviation (calculated as a 15-day rolling standard deviation) associated to OMPS monthly sAOD: once passed the first month, it steadily decreases all along year 2022.”.

L268: No
Reply: The sentence has been deleted. See the answer to the comments about Line 113 and 115-122.

L273: Remove "coefficient."
Reply: Corrected.

L336:This is misleading. DRE(ATM) reflects absorption in the entire atmospheric column. But sulfate aerosols warm the stratosphere and cool the troposphere. You cannot use it to characterize the impact on the stratosphere. Look at your heating rates (Fig. 7).
L339: I'm afraid that's not right. The stratospheric sulfate aerosols warm the stratosphere (see Fig. 7).
Reply: We thank the referee for this important comment. Without the analysis in the troposphere, we have decided to remove all statements about DRE(ATM) which cannot be inferred from our current analysis. Indeed stratospheric sulfate aerosols warm the stratosphere and this is now shown only from the analysis of the H/C rate profiles (Figure 4 and Figure 7).

L349: And absorption
Reply: Corrected.

L368-369: These high heating rates are inconsistent with your calculations
Reply: This comparison with Sellitto's strong estimations in the plume just 2 weeks after the eruption have been deleted in the revised manuscript.

L379: Your forcing is 2-3 times bigger than in Gupta et al.
L385: You cannot bluntly extend your results to the entire region. I agree that radiative forcing at Reunion Island correlates with the hemispheric mean forcing. Still, qualitatively, it is 2-3 times larger, manifesting that the aerosol distribution is not latitudinally uniform.
Reply: By presenting in the revised manuscript our results as representative of Reunion Island or at the most of the zonal hemisphere at 21º S, the explanation of the differences observed with Gupta et al. (2023) is more evident. Also, the revision of our LW calculations lead now to a smaller difference between our results and Gupta's. This sentence has been modified as follows: "Our results, representative of Reunion Island and likely of the zonal hemisphere at 21º S, are a little less than twice larger than those of Gupta et al. (2023) manifesting that the aerosol distribution is not latitudinally uniform.".

L388: You cannot make conclusions about the "stratospheric volcanic layer" using DRE(ATM) because it characterizes the entire atmospheric column.
Reply: We thank the referee for this important comment. Without the analysis in the troposphere, we have decided to remove all statements about DRE(ATM) which cannot be inferred from our current analysis.

L395-400: What is this about? Please clarify.
Reply: With the new Figure 7, this part has been completely re-written. It now reads:

"It is clear from Figure 6d that the negative longwave H/C rate caused by water vapor and the positive one caused by the aerosols coexist at different altitude levels. During M2 – M4, the small height

difference between the sulfate and the moist layers and the higher rate of cooling of the latter result in an aerosol and water vapor H/C rate negative in most of the altitude range considered (red, solid line in Figure 7). During M5 – M14, H/C rate profiles show a clear vertical difference locally in the stratosphere between the aerosol warming impact (18 to 26 km) and the water vapor cooling (22 to 30 km). The resulting aerosol and water vapor H/C rate profile follows a S-shaped curve with peaks slightly larger for the moist layer (-0.09 ºK day^{-1} at 26 km) than for the sulfate layer (+0.06 ºK day^{-1} at 22 km."

L407-414: This is misleading. The LW effect of stratospheric sulfate aerosols is well understood and calculated properly. Do not miss it with cases in the past when SW forcing was observed, but LW was not, and there is no information to reconstruct it. Then, people have to make assumptions.

Reply: We have changed this part of the paragraph to fit to the referee's suggestion which is actually reflected in the work of Schmidt et al. (2018) already cited in the manuscript. Now this part reads: "For the longwave component, persuasive evidence of the volcanic longwave effect has been missing for a long time in the past. However, the longwave effect of stratospheric sulfate aerosols is now well understood and calculated properly (Schmidt et al., 2018).".