Answers to comments of reviewer 1

We thank the reviewer for the comments and the time to review our paper. The reviewer's comments are written in black. Our answers to the comments are given in blue. Modifications in the manuscript are additionally written in italic letters.

I don't find this paper particularly interesting. It is long and detailed, with multiple small panels in each figure. The results either confirm what we already knew or failed to explain observations. The climate model setup is lacking in many details, and there is no explanation of why ensembles were not used and the statistical significance of the results.

While our work indeed builds on existing knowledge, we emphasize that it advances the modeling of volcanic eruptions like the 2022 Hunga eruption in several important ways that have not been fully addressed in prior studies. In particular, we highlight the following novel aspects of our modeling approach:

1. **Resolving eruption phases:** Our model distinguishes between different eruption phases as a function of both source parameters and evolving atmospheric conditions. This allows for a more physically grounded representation of eruption dynamics, which is rarely done in existing model studies.
2. **Volcanic water vapor's impact on ash aging and masking**: The study highlights the unique role of volcanic water vapor from the Hunga eruption in accelerating ash aging and increasing the coating on ash particles. This was found to potentially mask ash in observations, making it appear as spherical particles. This aspect of the study is novel as it provides insight into how water vapor influences ash properties and alters its detection in satellite observations, which could have significant implications for understanding ash dynamics in volcanic plumes.
3. **Aerosol aging and rapid ash loss**: The study reveals that the rapid loss of ash after the Hunga eruption, as observed by satellite instruments, cannot be fully explained by the typical aerosol aging processes of condensation and coagulation. This suggests that other processes, such as coagulation with sea salt, water vapor accumulation, wet aggregation in the early plume, or aerosol activation and washout may contribute to the enhanced particle growth and removal. This finding is novel as it challenges conventional explanations for ash removal and points to missing physical processes that could be vital for accurately modeling ash behavior in volcanic plumes.
4. **Warming and descent of the water vapor plume driven by aerosol-radiation interaction (ARI)**: The study discusses the complex interaction between water vapor cooling the plume and the warming effect due to aerosol radiative impacts, which influences the descent of the water vapor plume. The comparison with observed descent rates suggests that the warming effect from aerosols might be overestimated in the simulations due to the absence of an ash removal process. This aspect is novel as it introduces the interplay between cooling and warming effects in volcanic plumes and highlights how missing processes, such as ash removal, could skew the interpretation of plume behavior in numerical models.

While some of the broader patterns we observe are consistent with previous findings, our goal was to examine the underlying processes in greater detail. We believe this process-oriented perspective provides value beyond reproducing observations as it helps explain the variability seen in past eruptions and model predictions.

There are various small English errors in the text. I corrected some of them, but the native-English-speaker author should have edited the paper for English grammar and usage, so as to not annoy the reviewers. And multiple acronyms were defined and never used again. Multiple acronyms were used without defining them.

We have thoroughly revised the manuscript with special attention to grammar, sentence structure, and overall language clarity. The revised version has also been reviewed by a native English speaker with scientific writing experience to ensure fluency and readability. We have also carefully reviewed all acronyms and made the required corrections.

I find the initial conditions used for the experiment very confusing. I don't understand "about 500 Tg solid and less than 50 Tg liquid hydrometeors enter the stratosphere." Is solid ice? Are there any observations of this? Where do the numbers come from. And how do the authors know how much water was injected at the surface? There are no observations of this. Are the numbers in Table 1 based on observations? How can the MER of water vapor be calculated from the model without data on how much water there was? How can this model result be validated? I find the presentation of the experiment circular. What were the observed model inputs and what were the model results? How were the model results evaluated with separate observations?

We included further information on the coupling of ICON-ART and FPlume in the manuscript to elucidate our methodology and questions by the reviewers (see answer to your next question). We changed (l. 169-170):

"In our simulations, about 500 Tg of solid hydrometeors (ice and snow) and less than 50 Tg of liquid hydrometeors (cloud and rain water) are released into the stratosphere, which subsequently fall out in the first 1-2 days."

The numbers for the hydrometeor are the result of our coupled simulations, when we emitted the mass eruption rate (MER) of water vapor calculated by the 1-D model into the ICON model. We added (l. 159-160):

"This derived water vapor MER is added to ICON's water vapor mixing ratio. The phase partitioning between vapor, liquid and solid hydrometeors is calculated in ICON's microphysics scheme (Sect. 2.1.1)."

The emission occurs along a vertical profile and not at the surface. We have not claimed in the text how much water was injected at the surface. Our assumptions in Table 1 (upper part) are based on previous work, which has used observations (Sellitto et al., 2022, 2024; Gupta et al., 2022) or observations together with 1D-modeling (Mastin et al., 2024), as stated in the table caption. The different MER values are calculated online either directly with FPlume (total MER and MER of water vapor) or through the coupling of FPlume with ICON-ART (MER of very fine ash).

I also don't understand what FPlume is and how it is coupled to the climate model.

The FPlume model is described in the first paragraph of Section 2.1.3. We have now further extended the description on the coupling of FPlume to the numerical weather prediction (NWP) model ICON-ART (l. 145-153). The whole paragraph now reads as follows:

"Volcanic emissions in ICON-ART are calculated online with the 1-D volcanic plume rise model FPlume by Folch et al. (2016). FPlume calculates a total MER based on a given plume height. As input parameters for the plume dynamics, FPlume requires exit temperature, exit velocity, and exit volatile fraction as well as atmospheric profiles above the vent for pressure, temperature, specific humidity and density (Folch et al., 2016). The parametrization by Gouhier et al. (2019) calculates the fraction of very fine ash (particles <30 µm), which is relevant for atmospheric transport, from the given height and the calculated MER. The MER of SO_2 is prescribed based on observations and emitted in the same emission phases as for ash. The vertical distribution of all emitted masses (here ash, H_2O and SO_2) is calculated according to the Suzuki profiles (Suzuki, 1983). Further details on the coupling of ICON-ART and FPlume are given in Bruckert et al. (2022). Figure A1 shows the emission profiles as well as the vertical distribution of ash, SO2, and water vapor after the beginning of the first emission phase."

Furthermore, we included Figure A1 to illustrate the emission profile.

And why was that particular climate model used?

We do not use a climate model in this study. The simulations are based on the ICON model in its numerical weather prediction (NWP) configuration, similar to the setup used operationally by the German Weather Service (DWD) for global weather forecasts. This configuration is continuously evaluated and refined from a meteorological point of view, providing a robust and well-tested framework for coupling with aerosol and chemistry modules.

Our focus is on the early development of the Hunga Tonga plume during the first seven days following the eruption, using a global horizontal resolution of 40 km. On these short time scales and at this spatial resolution, NWP models offer distinct advantages over climate models, particularly in terms of representing dynamical processes and meteorological conditions. These aspects are crucial for accurately capturing the vertical and horizontal evolution of the eruption plume and its interaction with the environment.

Section 2.2 is missing details about the model runs. How long were the model runs? How many ensemble members did each experiment have? On what date and time were the simulations initialized? What did you do about the ocean? How was the climate model initialized as to land state and ocean state?

We did not perform ensemble simulations. However, we added the following information (l. 175-178):

"For each experiment, we simulated seven days initialized on 15 January 2022 at 00:00 UTC with analysis data provided by the German Weather Service (DWD). The analysis data contained variables describing the atmospheric state, variables needed by the land component, and sea surface temperatures. Due to the short time span of the simulation, sea surface temperatures are temporally fixed throughout the simulation."

The term “validation” is used multiple times, when I think it is more correct to use “evaluation.” Validation is only correct if you are sure a priori that the results are correct, and you are trying to prove that.

According to the glossary of meteorology of the American Meteorological Society (https://glossary.ametsoc.org/wiki/Validation), the term ‘validation’ refers to the “Comparison of a measurement from a new instrument or technique with older, established measurements of the same property or parameter”. Given that our study adopts this definition, where we compare outputs from our modeling approach against observations, we believe that the term “validation” is appropriate in this context. Hence, we will retain the current wording..

You need to include this paper in your reference list and address what it has already shown:

Ukhov, A., Stenchikov, G., Osipov, S., Krotkov, N., Gorkavyi, N., Li, C., et al. (2023). Inverse modeling of the initial stage of the 1991 Pinatubo volcanic cloud accounting for radiative feedback of volcanic ash. *Journal of Geophysical Research: Atmospheres*, 128, e2022JD038446. https://doi.org/10.1029/2022JD038446

We listed this paper as one additional example (besides Muser et al., 2019 and Stenchikov et al., 2021) for plume lofting due to aerosol-radiation interaction.

You should also consider this paper, although to my knowledge it has not been accepted for publication yet:

Georgiy Stenchikov, Alexander Ukhov, Sergey Osipov. Modeling the Direct Radiative Forcing and Climate Impacts of the 2022 Hunga Volcano Explosion. *ESS Open Archive.* July 11, 2024. DOI:10.22541/essoar.172070583.36131358/v1

We included this very important paper in the introduction and used it for the interpretation of the results in several sections.

And please address the 51 comments in the attached annotated manuscript.

In the following, we list the comments from the attached manuscript that needs further discussion and have not been answered already above. Comments that are not listed, have been corrected and do not need further discussion.

- l.28: VEI is not the correct index to use for the size of climate-impacting volcanic eruptions. The size depends on the sulfur injection to the stratosphere, not the explosiveness.
  The authors of the paper which we have cited in this context have used the VEI to classify moderate-size eruptions and we will therefore keep it here, too. Furthermore, it is not relevant for our study to discuss different indices for the size of volcanic eruptions.
- l. 58: Why not define this acronym, too?
  Himawari is not an acronym. However, the spelling in capital letters was wrong and has been corrected.
- l. 85: Stenchikov et al. (2024) modeled the water vapor.

In this section, we explained our modeling system and our work on previous eruptions with ICON-ART coupled to FPlume, where we only considered SO_2 and ash emission. Therefore, we here only rephrased the sentence to clarify this and added the suggested paper to the introduction part and section 3.2.

- l. 147-148: What data do you have to support this assumption?
  In typical large subaerial eruptions, the contribution of magmatic water vapor (~3-5% of the total MER, e.g., Mastin, 2007) is larger than the entrained water vapor (Woods, 1993; Glaze et al., 1997). In case of the Hunga eruption, large amounts of water vapor are originating from vaporized ocean water instead of magmatic water vapor (Mastin et al., 2024), a water vapor source that is missing in subaerial eruptions. Therefore, we can assume that the amount of entrained water is small compared to the injected water vapor from the ocean.
- l. 150-152: I don't understand. Why is this a problem with the model and how does this affect the results?
  As our water vapor is emitted into the ICON water vapor mixing ratio, which is also used as input for the FPlume model and affects its plume dynamic calculations, we used the water vapor mixing ratio from an external file, which does not include the volcanic emission.
  We provided more details on the coupling of FPlume and ICON-ART in our manuscript in general.
- l. 177: But where are observations of water vapor?
  Our paper focuses on the role of aerosol dynamical processes in the Hunga plume and a detailed investigation of the fate of water vapor is beyond the scope of this study. Nevertheless, we found a model configuration with which we are able to reproduce reported water vapor masses, which is relevant for the realistic representation of the aerosol dynamical processes.
- l. 177: has provided
  This is a general description of the instrument. Therefore, we stay with present tense.
- Figure 1: Where is the plume? Does it move horizontally and change in size?
  This figure shows the total burden of different components of the volcanic plume. As the plume is advected with the winds, it moves horizontally and also changes in size. But these effects are not relevant for the discussion of the burden.
- l . 187: Why? Can't your model simulate these processes?
  Indeed, ICON-ART does simulate the processes related to ARI. However, with respect to SO_2 oxidation there are two opposing effects, which could not be separated easily in the model: (1) ARI leads to a warming of the surrounding air and a lofting of the plume to layers with larger ozone concentrations, which will increase the production of OH and oxidation of SO_2; (2) the reduction of incoming solar radiation due to scattering by aerosols at the plume top reduces the production of OH, which decreases the oxidation of SO_2.
- All comments on Figure 3 and 4: meaning of the contour lines
  We revised the description of the contour lines and included labels.

Additional references used in the answers:

Glaze, L. S., S. M. Baloga, and L. Wilson (1997), Transport of atmospheric water vapor by volcanic eruption columns, *J. Geophys. Res.*, 102(D5), 6099–6108, doi:10.1029/96JD03125

Mastin, L. G. (2007), A user-friendly one-dimensional model for wet volcanic plumes, *Geochem. Geophys. Geosyst.*, 8, Q03014, doi:10.1029/2006GC001455

Woods, A. W. (1993), Moist convection and the injection of volcanic ash into the atmosphere, *J. Geophys. Res.*, 98(B10), 17627–17636, doi:10.1029/93JB00718