Reviewer #1

Technical comment:

In multiple locations, the text refers to specific panels using only the panel number, without explicitly stating the figure number. For example, check L217, 219, 225, 228, 230, 235, 239, 257, 265, 269, 304, 345, 348.
Corrected.

P12 L246: Consider writing the equation separately and provide an equation number.
Done.

P16 L351: Write as “As discussed in Bednarz et al., (2025),”
Corrected.

P23 L359: Write as “Sellitto et al., (2025) which indicated”
Corrected.

Reviewer #2

MC1) The impact of the specific SD of the Hunga aerosol plume, as well as its evolution, on its radiative impacts is the core of this manuscript and thus this must be addressed in the inherent discussions and cannot be delegated to future further papers. Even if I appreciate the inclusion of SD information, for the different scenarios, this should not be as peripheric as it is now, i.e. please include it somehow in the main paper and please don't keep it exclusively in the Supplements.

As noted in our previous response, we reiterate that the scope of this study is the radiative response to the Hunga eruption, not a detailed microphysical or observational intercomparison. Such analyses, using the same protocol and model simulations, are now available in the recently published Hunga report, which includes an evaluation of aerosol microphysics and comparisons with SAGE III/ISS observations. To avoid duplication, we retain the current structure of the manuscript and refer the reader to the relevant chapter of the report for further details and observational comparison.

“This is in agreement with previous studies indicating that the co-injection of stratospheric water vapor promotes faster particle growth to optically efficient scattering sizes, reaching an effective radius of about 0.4 μm over less than a month (panels a–c for SO2andH2O versus panels d-f for SO2only in Fig. A5). Further details on the microphysical aspects, using the same protocol and model simulations, are provided in Khaykin et al. (2025) (comparison of the particle size with observation in Fig. 3.18).”

MC1bis) Also, as a SD characterisation from observations exist (e.g. Duchamps et al., 2025: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL105076; Kloss et al., 2022: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022GL099394; but also others), I must insist on my previous demand: please at least add a textual discussion where you compare quantitatively the modelling and observational results. As I can see from the new figures in the Supplements, your SDs are quite consistent with available observations, so why not discussing this in the text?

Answered in MC1.

MC2) I don't think that the changes made in the manuscript really clarified this aspect. Even if I appreciate the clarification in terms of the results of Li et al. (2024), I still can't see here a mention to the true specificity of the Hunga's SD: the fact that they have a larger burden than background but also that they are not very large, i.e. unlike Pinatubo's and El Chichon's SDs - Pinatubo's and El Chichon's aerosols were far less effective, per unit sulphate aerosol mass, than Hunga's du to their large sizes (Fig. 7 of Sellitto et al., 2025). In different statements in the text, it sounds like larger average particle sizes mean larger radiative impacts, which is strictly not true (it is rather the opposite), see e.g. your sentence "Our multi-model results confirm previous analyses from Zhu et al. (2022), Stenchikov et al. (2025) and (Sellitto et al., 2025) which indicated a potential net negative forcing from the volcanic cloud, due to the formation of a persistent layer of stratospheric sulfate aerosol that rapidly grew to optically efficient sizes, enhancing shortwave scattering relative to background conditions." This sounds like SDs have to grow to be efficient, which is not true. This must absolutely be clarified throughout the text.

To address the reviewer's concern, we have been even more specific on what “rapidly grew to optically efficient sizes” means in the revised version of the manuscript and referred now to what was found in Asher et al. (2025) and English et al. (2013). We believe that our statement should resolve any confusion.

“Our multi-model results confirm previous analyses from Zhu et al. (2022), Stenchikov et al. (2025), and Sellitto et al. (2025), which indicated a potential net negative forcing from the volcanic cloud, due to the formation of a persistent layer of stratospheric sulfate aerosol365 that **rapidly grew to optically efficient sizes, estimated at around 0.4 µm for sulfate particles,** enhancing shortwave scattering relative to background conditions (English et al., 2013; Asher et al., 2025).”

SC1) Ok but please correct "Hunga Tonga" to "Hunga" and also correct the sentence "…recent decades have seen fewer eruptions with injections exceeding a few Tg of SO2" because actually *none of these exceeded a few Tg of injected SO2 but there has been a series of many smaller stratospheric eruptions *not exceeding a few Tg of injected SO2.
Corrected.
“[...] recent decades have seen no eruptions with injections exceeding a few Tg of SO2, but rather multiple moderate stratospheric eruptions (Carn et al., 2017; Brodowsky et al. , 2021)”

SC9) I still disagree on this point. Jenkins et al. is not only an "early estimation" but is also a "partial estimation" because of the lack of consideration of the aerosol effect. The only early estimation of the warming effect of the initial configuration of the plume (aerosols + water vapour) is in Sellitto et al. (2022) (Tab. 1 and inherent discussion). Please, correct the reference and please, clarify that this "warming effect" was only obtained for the initial dispersion of the plume, before the vertical separation of the water vapour and aerosol components.

We prefer to keep the sentence as written, as it specifically refers to early estimates of the water vapor only forcing (“Early estimates **assumed** the **water vapor forcing** would result in a small net warming”), which clearly constitute a partial estimate. This is clarified by the subsequent sentence, which discusses studies including both aerosol and water vapor effects (Sellitto et al., 2022; Zhu et al., 2022; Schoeberl et al., 2024).

SC10) “…allowing particles to reach sizes that are optically efficient scatterers”: this sentence is still ambiguous (see my reply to your reply to MC1 here above) and must be corrected.

We added more details referring to Asher et al. (2025), which should make the sentence more clear.

“Notably, the conditions of significant stratospheric hydration have been suggested as an important factor contributing to the substantial radiative impact of the Hunga cloud, even in the case of a modest SO2injection. This is because enhanced hydration promoted faster aerosol growth and the formation of aerosol that reached optically efficient scattering sizes (see Fig.7 in Murphy et al. (2021)) in a couple of weeks, rather than the four months observed after the Pinatubo eruption (Zhu et al., 2022; Li et al., 2024; Sellitto et al., 2025; Asher et al., 2025).“

SC17) This is ambiguous because the impact of stratospheric water vapour is not small at all if at the tropopause. This is not the vertical region where the Hunga plume is found, especially after the first 2-3 months, but the sentence sounds a general statement. Please rephrase.

We prefer to keep the sentence as written. The sentence regarding the small impact of stratospheric water vapor is specifically supported by our results: Fig. 2 (panels b and e) shows that the H2O-only contribution to the IRF is negligible, being slightly negative at the TOA and only slightly positive at the tropopause and surface, as discussed in the same section. In addition, the vertical distribution of the plume that we state (“Indeed, stratospheric water vapor concentrations are primarily confined to the middle stratosphere (40–10 hPa) by the end of 2022, after which they rise and extend into the lower mesosphere”) is based on observational and modeling results discussed in Chapter 3 of the Hunga report and illustrated in the figure below (taken from Chapter 3).

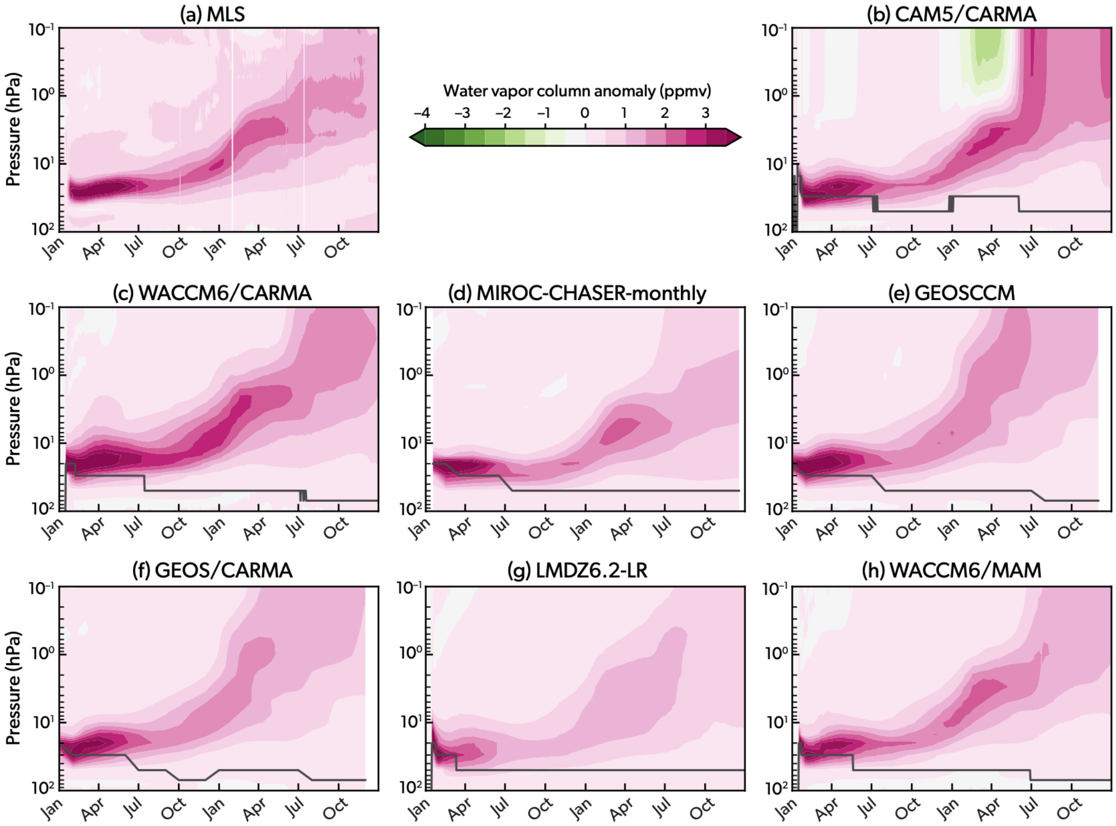

Figure 3.17: *Zonal-mean water-vapour perturbation (colour, in ppmv) as a function of time and altitude, averaged over 30°S–30°N in global models from January 2022 to December 2023. The black line shows the altitude of the maximum aerosol extinction coefficient (or mass-mixing ratio for CAM5/CARMA) above 100 hPa. Corresponding observations are shown in Figure 3.2a. Perturbations are based on daily values except for MIROC-CHASER, GEOSCCM, and GEOS/CARMA, which use monthly means.*