Review of "*Multi-model analysis of the radiative impacts of the 2022 Hunga eruption indicates a significant cooling contribution from the volcanic plume*" by Quaglia et al.,

The current manuscript makes a comprehensive analysis of the radiative impacts due to the Hunga volcanic eruption using multiple models and clearly delineates the radiative forcing due to the aerosols and gases, and also discusses its possible uncertainties in the analysis.

I highly appreciate the authors' extensive work employing multiple models to perform decadal simulations of radiative forcing from the Hunga eruption, one of the most significant volcanic events in recent history. The authors also compared the model results with the observational dataset wherever required. The manuscript is technically sound, and the results are presented concisely. However, my main concern is the discussion section, which requires further improvement and refinement. To be specific, several figures are under-discussed, which reduces the overall impact of the work despite the availability of strong results. Providing a more detailed interpretation of the model outputs in each figure would substantially enhance both the clarity and significance of the study. In addition, the novelty of the present study is not clearly argued. I have provided a few major and minor comments to assist the authors in improving the scientific arguments and presentation style of the manuscript. With these improvements, I believe the manuscript will be suitable for publication. I therefore recommend a major revision.

**Major comments**

1. In Sect. 1 (Introduction), the authors summarize previous works that estimated the radiative forcing of the Hunga eruption. However, the distinction between the present study and these earlier studies is not explicitly articulated. Specifically, while the current and previous works (Sellitto et al., 2022; Zhu et al., 2022; Jenkins et al., 2023; Schoeberl et al., 2024; Stenchikov et al., 2025) all examine the radiative impacts of the Hunga eruption, it remains unclear in which aspects this study provides a novel contribution. For example, does the current study differ in methodology, model choice, experimental setup, dataset, study period, atmospheric level of the forcing, or spatial domain? A clearer discussion of the study's novelty would better engage the reader and also highlight the significance of the present work.
2. The study has used GloSSAC for sAOD and SWOOSH for $H_2O$ to compare the model output with observation in Fig. 4. But the details about them are included very briefly in P.11 L.216–218. In addition, information about other observational datasets which are used for the simulations (if any) is missing. I recommend that authors create a dedicated data section which outlines all the observational and meteorological datasets used in the study for the reproducibility of the results and to increase the reliability of the findings.
3. One of the key arguments of this study is that the radiative forcing under the $SO_2$ + $H_2O$ combined condition is higher than under the $SO_2$ condition alone, and the authors have attributed this observation to enhanced sulfate aerosol growth in the presence of $H_2O$. However, this claim is not sufficiently supported with evidence in the current manuscript. This point could be strengthened by including a plot of the aerosol size distribution under the two scenarios ($SO_2$ + $H_2O$ and $SO_2$ alone), which would provide direct support for the stated mechanism.

4. The flow and discussions made in Sect. 3.1 and 3.2 are clear enough, but Sect. 3.3 is confusing and very difficult to follow. The primary reason for the confusion is the authors' abrupt shift to referencing multiple figures. This shift complicates the clarity of their argument. For example, the authors begin discussing Fig. 5 but abruptly shift to Fig. 6 without completing the explanation of Fig. 5. This pattern repeats throughout the section, creating a disorganized flow. Nearly seven figures are cited here, yet none of them are discussed in sufficient detail. I would recommend that authors reorganize this section by discussing each figure with relevant interpretations and scientific arguments, and move on to another figure. I would also suggest that the authors change the title of Sect. 3.3 to make it more descriptive.

5. Another major issue is that most of the figures are underdiscussed, i.e., in any figure, only two to four panels are discussed, leaving out the rest of the panels untouched, which questions their very purpose. Initially, I thought this specific issue was minor and restricted to Fig. 2 alone (please see minor comment no. 7 for more details). But later, I realized it was prevalent among most of the figures, making it a major issue. I highly recommend that the authors increase discussion on each figure (if not all panels, try to explain the most panels), and connect them to the objective of the study. If some figures are not worth discussing or if the authors feel that such discussion may impact the manuscript flow, they can be safely moved to the supplementary section.

**Minor comments**

1. I have a suggestion regarding the title of the manuscript, "*Multi-model analysis of the radiative impacts of the 2022 Hunga eruption indicates a significant cooling contribution from the volcanic plume*." I appreciate that the authors have highlighted the key finding (i.e., the cooling effect of the Hunga eruption) in the title. However, since previous studies have already established that the Hunga plume resulted in a net cooling, the current title does not appear very compelling and intriguing. I believe it would be more impactful to emphasize the amplification of this cooling (net radiative forcing) due to the growth of sulfate aerosols caused by the water-enriched stratosphere. Also, the title looks lengthy at present. So, if the authors are changing the title, it is better to be concise.

2. P.2 L.36: The authors have used the phrase "net warming" to refer to positive forcing. And at L.38 and L.40, the phrase "negative forcing" is used to refer to net cooling. These lines (L.35 to L.40) highlight the contrasting findings among the previous studies, as any good introduction should. But inconsistent usage of the phrase referring to the radiative forcing decreases the readability of these sentences. Hence, I would like to suggest that the authors use either "net warming" and "net cooling" or "positive forcing" and "negative forcing" to increase readability.

3. I appreciate the authors for providing details about the types of experiments and models employed in this study in Sect. 2.1 in P.3 and about the injection parameters through Table 1. However, for a common reader like me, it takes two to three reads to get clarity and

understand the method itself. It also disrupts the flow of the manuscript. To rectify it, I highly encourage the authors to add a graphical representation of the methods or a table which clearly describes all the experiments and models concisely. In addition, even though the current manuscript is part of the HTHH-MOC project and details of the employed methods are already furnished in the main paper (Zhu et al., 2025), I believe it is important to furnish minimal information (if not extensive) about the experiment design in the current manuscript for enhanced clarity.

4. In P.5 L.109–124, a few radiative forcing (RF) formulas are mentioned. Firstly, please consider adding equation numbers for all of them. Secondly, as the authors have clearly mentioned that "*Unless otherwise specified, all values are assumed to be calculated under Clear-Sky conditions*" at L.107, it is not essential to include 'clear' as a superscript in each term. In addition, the definition of '$F_{volc}^{clear}$' and '$F_{control}^{clear}$' varies in each formula, and their description is given at the end. However, it looks a bit odd to write them in this fashion. I suggest the authors consider rewriting the equation with more detailed subscripts and superscripts, something like what is given below.

$$RF^{Coupled} = F_{volc}^{coupled,SO_2,H_2O} - F_{control}^{coupled,SO_2,H_2O} \quad (1)$$

$$ERF^{Fixed-SST} = F_{volc}^{Fixed-SST,SO_2,H_2O} - F_{control}^{Fixed-SST,SO_2,H_2O} \quad \text{(2) and so on.}$$

5. In Fig. 1 (in all panels, except H2O_only cases), negative RF (~-0.4 $Wm^{-2}$) is observed at the high-latitude (60°–90°S) from November 2022 to February 2023. But thereafter, the RF becomes close to 0 $Wm^{-2}$ during April–September 2023 (Austral winter). This feature is consistent across all models and experiment setups and also appears in the Austral winter of 2022. Since the authors have clearly stated in P.6 at L.148 that IRF also considers the polar stratospheric clouds (PSCs), these RF values correspond to PSC as well as other aerosols too (as it is a clear-sky condition and not clean-sky). In this scenario, is RF ~0 $Wm^{-2}$ caused by the formation of PSC during the Austral winter? Is it because net cooling by stratospheric aerosols is countered by net warming by the PSC? Please describe.

6. Please explain the cause of the data gap in Fig. 1e and m.

7. The authors have estimated the RF at three key atmospheric levels: TOA, TROP, and SURF, and done an excellent job of explaining physical implications and reasons for choosing these three levels in P5 at L.126–132. These results are shown in Fig. 2 as well, with interpretation included on P.6 at L.156–162. While the discussions of RF at TOA and TROP are briefly touched on, the SURF RF has not been addressed. If the SURF RF is not required for the discussion, then its presence becomes questionable. Moving less important plots (panels) to the appendix or supplementary section may help the readers to be focused on the main argument which authors are trying to make. In addition, the discussion could

be strengthened by elaborating on the causes of the observed variation in RF across latitude bands and their temporal evolution.

(i) To be specific, for the case of SO2andH2O, the global mean (Fig. 2a, black solid line), and 60°S–Eq mean RF at TOA (Fig. 2d) show that the strongest negative RF occurred almost at the end of 2022. In contrast, at the TROP and SURF, the strongest negative RF occurred much earlier, around February 2022. What could be the possible physical mechanism of this result?

(ii) In the high-latitude band (60–90°S), RF is close to 0 $Wm^{-2}$ during the first six months of 2022 at all three levels (TOA, TROP, and SURF; third row in Fig. 2). However, a sudden strong negative RF exceeding -0.5 W $m^{-2}$ appears in January 2023, which rapidly decays and returns to ~0 W $m^{-2}$ by the peak Austral winter of 2023. Following this, another negative RF peak occurs at the end of 2023. What physical processes drive this temporal variation, particularly the ~0 W $m^{-2}$ RF during the Austral winter? This point is directly connected to my earlier comment on Fig. 1 (see major comment no. 5). A solid discussion on these aspects could potentially strengthen the discussion and make it comprehensive.

8. The authors have written, "*The fact that the forcing becomes similar later on and shows a slight reduction in the last few months of 2023, may suggest a balance between larger particles and shorter lifetime for the two cases*" in P.7 at L.170–172. Could it also be due to the polar transport of volcanic emissions from mid-to-high latitude, as evident from Fig. 1?

9. P.7 L.185–187: This is one of the interesting and important findings made in the present study and also included in the abstract. However, without a plot or supporting data, it appears speculative. To strengthen this claim, consider adding a bar plot (or any other suitable plot authors wish) comparing aerosol sizes under SO2andH2O and SO2 experiments, for both clear-sky and all-sky scenarios. This would provide clearer evidence to support the finding.

10. P.7 L.194–195: Any possible explanation for the discrepancies between the models at the tropopause?

11. P.10 L205: What does 'cloud' refer to in this context? Are the authors referring to the aerosol plume from the Hunga eruption? If so, please use the term 'aerosol plume' for consistency, since this phrase is already used at l.204. If not, kindly clarify what is meant by 'cloud' here.

11. P.13 L.249: The Fig. A8 is cited before Figs. A1–A7 are not even mentioned. Except Fig. A7, which is cited at L.275 in P.14, Figs. A1–A6 are never cited at all. If these figures are not even needed to be cited in the main text and do not contribute to the discussion, their inclusion even in the appendix is questionable.

**Technical comments:**

1. Throughout the manuscript, the unit for the radiative forcing is written as ‘$W/m^2$.’ I would like to suggest changing them to ‘$Wm^{-2}$’ both in the main text and plots as per the ACP template.
2. P.4 L91–92: The instantaneous radiative forcing is already abbreviated as IRF at L.82. Hence, it is not necessary to abbreviate it again. Please correct.
3. P.5 P.120: Write ‘instantaneous radiative forcing’ as IRF.
4. P.6 L.134: Please write ‘Section 3.1’ as ‘Sect. 3.1’
5. P.6 L.138: The first paragraph of Sect. 3 provides the outline of the result and discussion section. While doing so, it would be better if the corresponding subsections are cited at the appropriate place, as shown below.
6. “*Following that, we provide analyses of the two models which provided the 10-year long free-running simulations with prescribed climatological SSTs and sea ice* ***(Sect. 3.2)****: these analyses allow us to understand the long-term behavior of the forcing as well as include the combined chemical and dynamical impacts and temperature adjustments*.”
7. “*Finally, we complete those with an analysis of fully-coupled simulations in WACCM6-MAM* ***(Sect. 3.3)***.”
8. P.6 L147: Replace ‘timeseries’ with ‘time series’
9. P.7 L.165: Replace ‘Section 2.2’ with ‘Sect. 2.2’
10. P.7 L189: Replace ‘Fig.4’ with ‘Figure 4’
11. P.7 L189: Does the ‘t’ in the formula refer to ‘time’? Please clarify.
12. In the caption of Fig. 2, it is mentioned that the first row plots correspond to 60°S–90°N. But in the title of the panel a–c, it is written as 90°N–S. Please correct.
13. P.7 L200: The stratospheric aerosol optical depth is abbreviated as ‘sAOD’ at L.199 and referred again as ‘stratospheric AOD’ at L.200. Once an abbreviation is introduced, it should be used consistently throughout the main text (except in figure captions and the conclusion). Consistency in terminology will improve both readability and the overall flow of the manuscript.
14. In Fig. 3d and e, please make a common y-axis limit. At present, the panel d has a slightly wider ylimit than panel e.
15. In the caption of Fig. 3, replace ‘Section 2.2’ with ‘Sect. 2.2’
16. P.11 L.222: Replace “(Zhuo et al., 2025)” with “Zhuo et al., (2025)”

17. P. 12: In Fig. 4’s caption, clearly indicate what the green and magenta shading region (panels a to c) represent.
18. P.11 L.232: Replace ‘Fig. 5’ with ‘Figure 5’
19. P.11 L.234–235: Replace ‘stratospheric aerosol optical depth’ with ‘sAOD’ as the acronym is already introduced.
20. In the caption of Fig. 4, replace ‘timeseries’ with ‘time series’ and in the first line write ‘effective radiative forcing’ as ‘effective radiative forcing (ERF)’.
21. In Fig. 4 (e, f, h, i, and k), ‘Altitude (hPa)’ is given as the ylabel. Usually, altitudes are given in units of m or km. Is it supposed to be ‘Pressure (hPa)’? Please check and correct.
22. P.13 L.262: What does LW mean here? Please expand it.
23. P.13 L270: Expand ENSO.
24. Figures 6, 7, and 8, and Table 3 appear in the Conclusion Sect. 4. I suggest the authors move them to Sect. 3.3, where relevant discussion using these figures and tables is made.

***References***

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