Response to Referee 3 Comments

We would first like to express our thanks and appreciation to Referee 3 for their review. The comments identified flaws and unclear points in the article, providing an excellent opportunity to improve its overall quality. In the following, Referee comments are written in black and authors answers are in blue.

Comments:

Point 1: Several papers were cited by the authors which analyzed the Hunga volcanic eruption and its influences. The authors should describe better their contributions on the ozone study after the passage of Hunga plumes. What is the difference for this work with the others? The objective of the paper is not very clear.

Response 1: The paper's objective has been clarified both in the manuscript and here. Our aim is to provide a fresh perspective on the first 10 days following the Hunga eruption, using satellite ozone data from MLS and IASI to track the daily movement of the ozone anomaly across the Indian Ocean and identify the formation of a distinct ozone-depleted area by January 21. We have made it clear that our work is the first to use and validate IASI data for this event. Furthermore, by refining our selection criterion, we could characterize the effects of both Hunga aerosol plumes, as discussed by Legras et al. (2022), on ozone levels. Figure 6 was revised to illustrate ozone anomalies associated with each individual aerosol cloud. This new result suggests that ozone depletion was localized and followed the movement of both clouds, strengthening the link between ozone loss and sulfate aerosols.

Point 2: In the Introduction, the chemical influence of chlorine and sulfur compounds in ozone should be discussed further with showing some chemical reactions. After all, the objective of the paper is to show the influence of the volcanic plume on atmospheric ozone.

Response 2: To further discuss the chemical influence of chlorine and sulfur compounds on ozone, we added appropriate chemical reactions, as suggested.

Point 3: In the methodology, it should be explained what location (latitude, longitude, grid used for satellite data, . . .) the paper analyzed. This should be put in the beginning. The methodology should be expanded to explain better the instruments and the methodology used.

Response 3: Suggested information was added into the manuscript.

Point 4: In item 2.1 - Ozone Measurements, for better understanding the work, it is needed to create separate subtitles to describe each instrument, their data and its problems with the Hunga plume.

Response 4: This point has been addressed by dividing Section 2.1 into subsections, with one dedicated to each instrument.

Point 5: In the results part, it is important to change Figure 5. How it showed the ozone total column and ozone anomaly. It was not possible to see the reduction discussed in the paper based only in this figure.

Response 5: Figure 5 was revised to better show the transient ozone depletion area. The revised version of Figure 5 now only shows significant IASI anomalies at the 2σ level, allowing to better constrain the anomaly. Clarity of the information within the text was also improved.

Point 6: L.16: Why cited WMO (2018) and not WMO (2022)?

Response 6: The WMO reference has been updated to WMO (2022).

Point 7: L. 17: A reference for the ozone absorbing range must be cited

Response 7: An appropriate reference was given for the ozone absorbing range (Oprhal et al., 2016).

Point 8: L. 20 – 21: The authors wrote "In the past decades. . . ", however the only reference cited is from 1996.

Response 8: Molina and Rowland (1974) and Solomon (1988) were included to reinforce this point.

Point 9: L. 15 – 29: The text should be separated in two paragraphs for a better understanding in stratospheric ozone and tropospheric ozone.

Response 9: As suggested, the text was dived to clearly separate tropospheric ozone from stratospheric ozone.

Point 10: L. 30 – 41: In this part, the authors described the influence of some volcanic eruptions on ozone. However, only two are cited. They were important eruptions, but other eruptions and new studies should be analyzed also. Calbuco was only cited in line 52.

Response 10: Eruptions of Fuego (1974), Cerro Hudson (1991), Calbuco (2015), and their relevant references have been added to the discussion on L. 30 – 41 to provide a broader context on volcanic contributions to stratospheric ozone impacts.

Point 11: L. 63 – 64: "As a result of the main austral summer stratospheric circulation and the prevalent phase of the QBO. . . " – Explain better this main circulation and what phase of QBO were acting in the period analyzed.

Response 11: Our original intent was to highlight that the combination of the austral summer stratospheric eastward circulation and the QBO easterly anomaly allowed Hunga's plume to reach Reunion quickly. However, we chose to omit this detail because: 1) it is the only mention of the QBO in the article, and 2) including it would require additional, unnecessary context. Specifically, we would need to explain (see Stocker et al., 2024) that the QBO phase during the eruption likely induced an easterly anomaly above ~30 km, which accelerated the plume detected at ~34 km over Reunion four days later (Baron et al., 2023). Below 30 km, the QBO produced a westerly anomaly. We recognize that the word "main" was overused and have replaced it with more precise terms. Additionally, we have added a preceding sentence to better explain the significance Reunion in the context of our study, making it clearer for the reader.

Point 12: L. 83 – 84: Explain "the remaining aerosol plume consisted of two concentrated patches"

Response 12: This information was revised and clarified within the new version of the manuscript. The study by Legras et al. (2022) showed that the Hunga eruption injected two distinct volcanic clouds into the stratosphere, initially positioned at ~30 km and ~28 km altitude on 15 January. These clouds remained separated throughout our study period, gradually descending to ~27 km and ~25 km by 28 January.

Point 13: L. 94 – 96: What kind of observations were described in the paper? What species were analyzed from satellite and ground-based observations?

Response 13: This information is included in the following lines.

Point 14: L. 100 – 103: It is not necessary.

Response 14: As suggested by the referee, these lines were suppressed in the revised version of the manuscript.

Point 15: L. 147: "The maximum 1σ standard deviation found in the stratosphere was close to 0.05 ppmv". What is the mean ozone mixing ratio for this region? How much percentage it represents?

Response 15: In the stratospheric region mentioned at L147 (10—100 hPa), the mean ozone mixing ratio across all co-located v4 and v5 profiles not meeting the selection criterion is 4.4 ppmv. The 2σ standard deviation of 0.1 ppmv reflects a negligible 0.02% variation relative to this mean, and this information has been added to the manuscript. Throughout the manuscript, we updated 1σ standard deviations and uncertainties to 2σ, where possible, to reflect a 95 % confidence level. Thus, for this sentence, the standard deviation of 0.05 ppmv (1σ) was updated to 0.1 ppmv (2σ).

Point 16: L. 153: The "established criterion" is not clear.

Response 16: The formulation was changed to improve clarity.

Point 17: L. 211: What is the lat, lon for the location of Saint-Denis?

Response 17: This information is now added into the manuscript. The location of Saint-Denis is approximately 20.90° S; 55.48° E.

Point 18: L. 231: What is the location of the lidar site? Lat, lon.

Response 18: This information is now added into the manuscript. The coordinates of the lidar site are: 21.08° S; 55.38° E.

Point 19: L. 255: Describe first what panel (a) in Figure 1 represents.

Response 19: The information was changed as suggested.

Point 20: L. 309-316: In Fig. 4 were described two kinds of data. In this paragraph, the analysis was confused. Please, rewrite it.

Response 20: We apologize for the confusion. This paragraph was revised as suggested.

Point 21: L. 322: Define SNR.

Response 21: SNR is defined in section 2.1 as "signal-to-noise ratio".

Point 22: L. 333: Standardize RMSD or RMSE.

Response 22: We thank the referee for pointing this typo within Figure 4. We have corrected "RMSE" to "RMSD" in the figure.

Point 23: L. 345: "All maps are overlaid with blue contours of the SO2 plume...". It should be red contours, not blue.

Response 23: We thank the referee for pointing this typo. This was corrected in the revised manuscript.

Point 24: L. 354: How could be seen "222.87" DU if the figure scale begins in 240 DU? Figure 5 needs to change the data range from legend and colors pattern, because it was impossible to see the reduction in ozone total column and the anomalies showed by the red areas.

Response 24: The colorbar was changed to clearly show the reduction in column ozone, improving the visualization of the observed anomalies.

Point 25: L. 352 – 371: Where is the data from this paragraph? It was not possible to see the number cited based on Fig 5.

Response 25: This paragraph references IASI ozone observations and anomalies presented in Figure 5. In the updated Figure 5, only significant anomalies (at the 2σ level) are displayed, enhancing visibility of the specific anomalies discussed in the text. Following Referee 1's suggestion, all mentioned anomalies are now indicated along with their corresponding lat-lon coordinates for easier localization on the figure.

Point 26: L. 701: For Zhu et al. (2023), it should be cited as the doi for final version of the paper, not the preprint doi. The correct doi is: https://doi.org/10.5194/acp-23-13355-2023.

Response 26: We thank the referee for providing the updated reference.