Response to Referee 2 Comments

We would first like to express our thanks and appreciation to Referee 2 for their review. The comments identified flaws and unclear points in the article, providing an excellent opportunity to improve its overall quality.

Our responses follow the structure of the review document and are divided into three sections: 1) responses to major comments, and 2) responses to minor comments. Referee comments are written in black and authors answers are in blue.

Major comments:

Point 1: The authors present background ozone profiles from different months observed by DIAL and explain the variations in altitude with the highest ozone concentrations in Section 3.2. However, this section seems less relevant to the main topic. Additionally, Figure 3 is not particularly informative, as it merely displays typical tropical ozone concentrations in an altitude-month contour.

Response 1: We acknowledge the referee's perspective and have revised Figure 3 for better coherence with our study. It now presents mean January lidar and an average MLS ozone profile to illustrate background ozone levels at Reunion under unperturbed conditions, along with a representative January MLS ozone profile for the entire Indian Ocean. The close similarity between the lidar and MLS profiles over Reunion demonstrates strong correlation between the datasets. Furthermore, the similarity between the Reunion and Indian Ocean MLS profiles suggests that the Reunion data can be considered representative of the Indian Ocean. The new Figure 3 shows a good comparison between the two instruments and supports the use of MLS data to represent ozone levels across the entire Indian Ocean for this study.

Point 2: The MLS and IASI data are well-validated and widely used in the ozone community, so it may be unnecessary to validate them in this paper. However, it's not a negative addition. Besides, the standard deviation calculation in Figure 4a is inappropriate. The authors calculate the standard deviation for the mean relative bias, which decreases as the number of samples increases. Instead, they should calculate the standard deviation for the relative bias itself (as done in Figure 6), which would represent the variation of each individual relative bias.

Response 2: It is essential to ensure that (1) background conditions are accurately captured by both MLS and IASI, and (2) neither instrument exhibits bias in January 2022 due to the event's exceptional nature. It is necessary to ensure that the instruments used over the Indian Ocean perform consistently with local instruments during a recent period, confirming the absence of retrieval degradation and assessing potential instrumental bias. The comparison between the lidar and MLS profiles serves more as a demonstration of consistency between different observations (giving more robust confidence in the results) rather than a formal data validation. OMI data was excluded due to significant perturbations in its UV radiance measurements caused by stratospheric sulfate aerosols during the Hunga event. Instead, we relied on IASI data which has no known issues in its UV radiance measurements, although it has not yet been validated for this event. Regarding the referee's last point, Figure 6 aims to assess the dispersion of ozone anomaly profiles, where using the "classic" standard deviation is appropriate. However, in Figure 4, we quantify the uncertainty of the mean relative bias, making the use of standard error more appropriate. Statistically, the relative bias results below $\sim$15 km are based

on fewer lidar profiles (< 131), compared to ∼25 km with 470 profiles, necessitating a correction for this difference to ensure a valid comparison.

Point 3: The biggest issue is with Figure 5 and its description: SO2 is not a reliable tracer for volcanic gases and particles. As the authors themselves mention, "the rapid conversion of SO2 molecules to sulfate particles" occurs. Therefore, there are two distinct possibilities for a region with low SO2 concentrations: (1) volcanic gases and particles did not reach this region, or (2) volcanic gases and particles did reach the region, but the SO2 was converted to sulfate. These two possibilities are entirely different and need to be clearly distinguished.

Response 3: SO2 is in fact a reliable tracer for volcanic gases. In the stratosphere, a concentration above 1σ of the SO2 background is very likely to be linked to a volcanic event and thus is a relatively good tracer for an eruption. That being said, SO2 is short lived and quickly converted to sulfates during the Hunga event because of the presence of water vapor. However, in Figure 5, SO2 is not used to argue for the presence or absence of SO2 or sulfates; it is employed solely to localize the plume and associated ozone anomaly, and this information is complemented by Hunga-impacted profiles provided by MLS. Because of the austral summer stratospheric dynamics, all of the regions from Figure 5 located between the HHTH and Reunion must have been impacted by SO2, sulfate or a combination of both.

Point 4: The authors mention a "correlation between ozone, H2O, and SO2." While the correlation between H2O and SO2 is visible, the correlation between ozone and the other two gases is unclear in Figure 5. In addition, the significant ozone anomaly (shown in blue) appears throughout the region, even before the Hunga aerosol transport (e.g., Figure 5 b1, b2), making it difficult to attribute the ozone anomaly specifically to the Hunga eruption.

Response 4: Indeed, in Figure 5, the correlation is best seen between H2O and SO2 (in the right-most column). As an answer to referee n°1, we have revised Figure 5 to only show IASI significant anomalies at the 2σ level, effectively removing most of the anomalies throughout the region. The revised Figure 5 now better shows the anomalies linked to the HTHH event.

Point 5: The largest correlation between ozone and water vapor is -0.68 in Figure 6c, which is not significant.

Response 5: The correlation we used in panel c) was purely indicative. The results in Figure 6 have been updated with a new criterion. However, as the correlation results between water vapor and ozone yielded low values ($|r| < 0.6$) and were not essential to our study, we decided to omit them.

Minor comments:

Point 1: In the abstract, introduction and conclusion, the authors state that "Hunga volcano eruption released significant amounts of aerosols, water vapor (H2O) and a moderate quantity of sulfur dioxide (SO2) into the stratosphere." However, volcanoes rarely release aerosols directly. Instead, sulfate aerosols form from the oxidation of SO2.

Response 1: We acknowledge the referee's point that mentioning the volcano's emission of aerosols could be misleading in the context of ozone loss, as only stating "aerosols" implies ash, which is less relevant compared to sulfates or SO2. We have therefore removed all references to this in the revised manuscript.

Point 2: Line 20: The term "halons (Br)" is vague. Halons are not equal to Br, so further explanation is needed. For example, it could be revised to "halons (including Br)".

Response 2: The text was revised as suggested.

Point 3: Line 28: The capitalization of "Volatile Organic Compounds" is unnecessary.

Response 3: The capitalization of "Volatile Organic Compounds" was dropped.

Point 4: Line 34: See my first minor comment.

Response 4: Amended.

Point 5: Some abbreviations are not explained. For example, "PSC" at Line 50 and "QBO" at Line 64 should be defined upon first use.

Response 5: Amended.

Point 6: Line 315: The phrase "altitude level, date, and time" is vague. Do you mean the time of day?

Response 6: The term "time" referred specifically to the time of day, which we have clarified in the revised manuscript.

Point 7: Figure 4: The statement "p-value = 0.0" is not meaningful in this context. The authors should either remove the p-value or provide a more meaningful value in Figure 4.

Response 7: We removed the p-value from Figure 4.

Point 8: Line 346: The unit of "DU" seems strange for SO2.

Response 8: The native IASI unit for SO2 is mol.m^{-2}. We opted to retain SO2 representation in DU to maintain consistency with ozone measurements.