Author's response

This document provides a point-by-point response to the reviewers including a list of all relevant changes made in the manuscript. All authors sincerely thank each anonymous referee once again for their thorough and detailed reviews, which helped identify flaws and clarify ambiguous aspects of the article.

This document is organized into two sections, each addressing the comments and feedback from a specific referee. The responses to Referee 1 begin on page 2 and those to Referee 2 on page 16.

Response to Referee 1 Comments

We would like to once again express our sincere thanks and appreciation to Referee 1 for their thorough and detailed review, as well as for suggesting numerous grammar and typographical corrections. Their comments highlighted clear flaws, which we hope to have addressed in the revised version, as well as areas of ambiguity in the article.

Our responses follow the structure of the review document and are divided into two sections: 1) a response the general comment, and 2) responses to specific comments and questions. Referee comments are written in black and authors answers are in blue.

General comment:

Point 1: One major comment from my previous review that has not been resolved in the revised draft is the magnitude of the IASI ozone anomaly and its apparent discrepancy with MLS-based estimates. The authors have redone their analysis of MLS measurements and now find average negative anomalies in ozone of 0.7 $\pm$ 0.5 ppmv at 17-12 hPa and 0.6 $\pm$ 0.5 ppmv at 26-32 hPa. Unlike in the original manuscript, these anomalies are now barely significant at 1σ. However, the IASI analysis was also redone, and the maximum (and highly significant) TCO anomaly of 40.1 $\pm$ 4.8 DU is slightly larger than it was before. A stratospheric column ozone (SCO) anomaly is now also calculated; its maximum value (also highly significant) is 49.9 $\pm$ 4.7 DU. TCO / SCO anomalies of this magnitude will be met with skepticism by many readers. As demonstrated in my previous review, even an anomaly as large as 1 ppmv applied uniformly over the entire range from 40 to 1 hPa (the bulk of the stratospheric ozone layer) would not come close to producing an SCO anomaly of 50 DU. In their response, the authors state that "a direct comparison between IASI total column ozone measurements and MLS stratospheric ozone measurements is not appropriate, as the two instruments sample different atmospheric layers and use distinct observation geometries and techniques". This statement misses the point – ozone is ozone, no matter who is measuring it. For convenience, the plot included in my previous review was based on MLS measurements, but it did not depend on them – the same analysis could be done with any ozone profile. I encourage the authors to do such an exercise themselves – take an ozone profile (from anywhere), compute the SCO from it, and then calculate the SCO anomaly based on perturbations to that ozone profile of different amplitudes. This should give a sense of the magnitude and vertical extent of the perturbation necessary to bring about an SCO anomaly of 50 DU. I feel that some discussion about the credibility of the large column ozone anomalies estimated from IASI data – and their inconsistency with the MLS-based estimates – should be added to the text.

Response 1: We appreciate the referee's continued attention to this issue. Following the suggested experiment, we confirm that applying a 1 ppmv anomaly uniformly across the stratospheric column does not produce an SCO anomaly of 50 DU. Such an anomaly is not observed in either MLS or IASI profiles and is roughly twice the maximum anomaly derived from MLS data. This finding prompted a reassessment of our method for computing IASI column anomalies.

To refine our approach, we consulted Anne Boynard, an expert in IASI ozone retrievals and now a co-author of this study. With her guidance, we identified that over-estimated anomalies were partly due to the re-sampling of L2 IASI daily observations onto the regular grid. The issue arose from using nearest-neighbor interpolation instead of averaging, which introduced artifacts. To correct this, we now compute the average of all daily L2 observations over a 1 °$\times$1 ° global grid, aligning with standard

IASI processing procedures.

Additionally, instead of assuming a constant monthly ozone background, we now define background ozone levels as the daily average of IASI/Metop-B data from 2014 to 2021, representing daily means rather than a single January mean. We also apply quality filters, retaining only profiles and columns with more than two degrees of freedom and a retrieval quality filter of 1 to ensure the use of the most reliable observations. Finally, anomalies are now reported only when based on at least three L2 observations.

Figure 1 illustrates the impact of these improvements. The upper panel shows SCO anomalies from the previous approach, while the lower panel presents results from the revised method where at least three observations are available. The figure highlights how the averaging process reduces artifacts.

Furthermore, based on Anne Boynard's input and the referee's earlier comments during the first review, we introduced a regional statistical metric to better capture event-related anomalies while minimizing extreme values. Specifically, we now report the 5th percentile of anomalies, calculated only within the SO2 cloud identified from IASI data. For example, on 21 January, we now report a peak 5th percentile anomaly of -18.6 DU for TCO and at -14.5 DU for SCO.

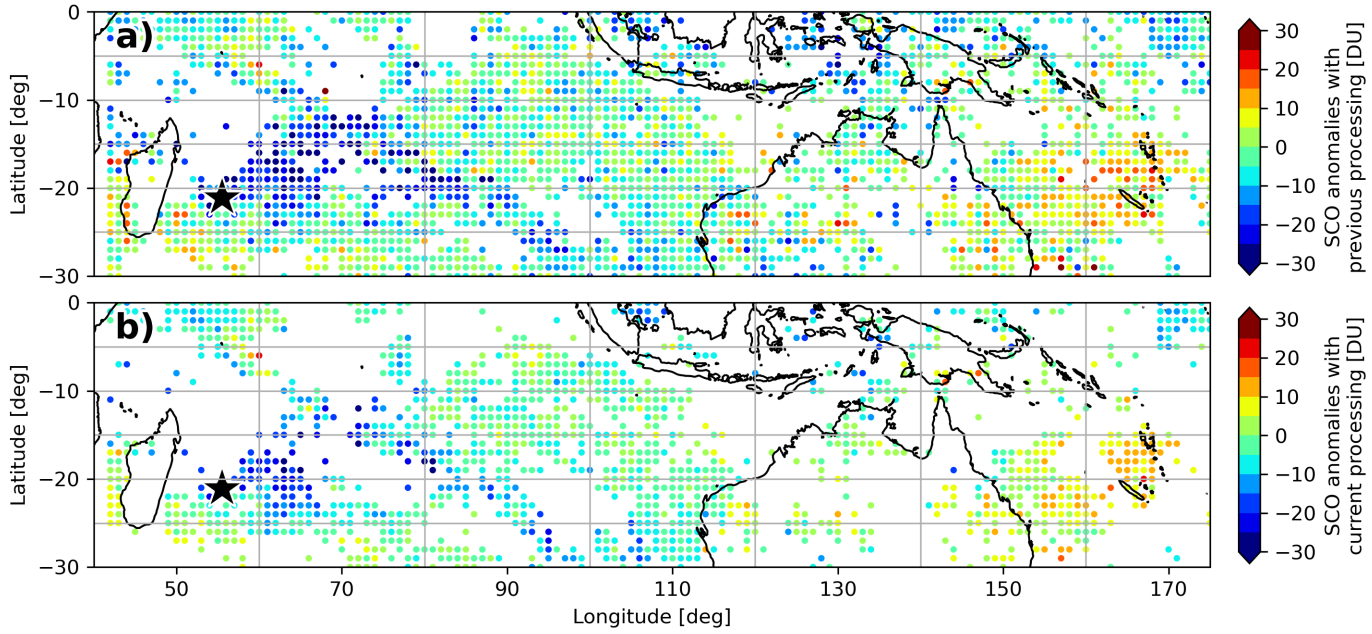

Figure 1: (**a**) IASI SCO anomaly map for 21 January, computed using nearest-neighbor interpolation instead of averaging.(**b**) IASI SCO anomaly map for 21 January, computed using averaging instead of nearest-neighbor interpolation.)

Previously, Figure 5 of the article displayed only anomalies significant at the 2-sigma level to prevent artifacts. With the refined methodology, we now display all anomalies. In contrast, Figure 1b presents only anomalies based on at least three observations to improve clarity in illustrating the new method.

Specific comments and questions:

Point Abstract: The authors need to be mindful that many readers will look to the Abstract to get

a basic sense of the paper (and they may not go beyond that). Therefore the Abstract needs to do a much better job of summarizing the study and clearly highlighting its novel aspects. For example, the fact that this is the first presentation of IASI data in the context of the Hunga eruption should be emphasized here. In addition, the finding that the reduction in ozone appears to have been confined to two distinct layers associated with two separate aerosol clouds is one of the few new aspects of this study and should be more clearly articulated.

Response Abstract: We thank the referee for the suggested revision. The Abstract has been revised to more clearly emphasize the novel findings of the study.

Point L5: Delete "The" in front of "Ozone".

Response L5: This point has been addressed.

Point L9: Given the poor vertical resolution of IASI data, it is not really appropriate to refer to IASI and MLS "profiles" together in the same sentence. I suggest replacing "profiles" with "measurements" in this line.

Response L9: In response to the referee's first point, the abstract has been rewritten: "This study presents the first analysis of Infrared Atmospheric Sounding Interferometer (IASI) ozone data to investigate the impact of the Hunga eruption, while also incorporating Microwave Limb Sounder (MLS) and Ozone Mapping and Profiler Suite Limb Profiler (OMPS-LP) data, as well as ground-based measurements from Reunion."

Point L11-12: The TCO result is not actually covered in the main body of the paper, but I think it should be – see my more detailed comments on this point below.

Response L11-12: The TCO maps for 15-23 January 2022 were added to the Appendix and corresponding results were integrated into the main body of the paper.

Point L18: There is no need to define the acronym "UVR" as it is not used again in the manuscript.

Response L18: This point has been addressed.

Point L34-35: such as that –> such as those; add a comma after "Calbuco (2015)".

Response L34-35: This point has been addressed.s

Point L44: Why is the word "implied" used here? Stratospheric ozone losses and radiative changes have been documented following volcanic eruptions, as noted in this manuscript.

Response L44: We apologize for the confusion. The word "implied" was not the appropriate word; we intended to convey "resulting". The sentence has been revised to: "Because of the resulting ozone losses and radiative forcing anomalies ...".

Point L52: clouds (PSCs) volume –> cloud (PSC) volume

Response L52: This point has been addressed.

Point L71: An early paper discussing the influence of the excess humidity from Hunga in accelerating conversion of SO2 to sulfate aerosols by Zhu et al. [2022, Comm Earth & Environ, 10.1038/s43247-022-00580-w] should also be cited for this point.

Response L71: We thank the referee for highlighting this reference, which has now been added to the manuscript.

Point L72: The paper by Sicard et al. has now been published, so the citation needs to be updated both here in the text and in the reference list. Moreover, other papers should also be cited for the Hunga-induced stratospheric cooling, such as those by Sellitto et al. (2022), Coy et al. (2022), and Schoeberl et al. (2022, GRL, 10.1029/2022GL100248).

Response L72: The citation has been updated and suggested references were included into the discussion for the Hunga-induced stratospheric cooling.

Point L75-83: I do not think that the listing of stratospheric chemical reactions has added useful information to this paper. I understand that in their comments on the previous draft one of the other referees suggesting discussing in more detail the influence of chlorine and sulfur compounds on stratospheric ozone, including showing some chemical reactions. But in response to that comment the authors have simply listed the set of "key heterogeneous reactions" given in the review paper by Solomon et al. (1999), with absolutely no accompanying text to put these reactions into context or give a sense of which ones are generally more important following volcanic eruptions. Zhu et al. (2023) and Evan et al. (2023, in the supplementary material), both already cited in the manuscript, provide a detailed description of the post-Hunga heterogeneous chemical reactions inside and outside the plume. In addition, Wilmouth et al. (2023, PNAS, 10.1073/pnas.2301994120) and Santee et al. (2023, JGR-A, 10.1029/2023JD039169) discuss the stratospheric chemical processing in the months following the eruption. Thus I feel that the authors would be better off deleting the material in these lines and simply referring readers to the lengthy explanations in those previous papers. If the authors want to retain these equations in the paper, then more in depth discussion of how they are relevant needs to be added to the text.

Response L75-83: As per the referee's suggestion, we removed the list of stratospheric chemical reactions and now refer to the recommended references instead.

Point 88-89: It is not appropriate to say that Evan et al. "documented" a doubling of ozone loss via O3+Cl – they merely reported the results shown by the modeling study of Zhu et al. (2023). Moreover, while the rate of that particular reaction did double, the rates of other reactions changed by even greater amounts, so it is not clear why that one has been singled out. It would be better to make a more general statement that the rates of key reactions increased substantially, leading to the 5% depletion of stratospheric ozone over the Indian Ocean observed by Evan et al.

Response 88-89: We thank the referee for this suggested revision. This point has been addressed.

Point L97: To avoid repeating "eruption", it would be better to say "impacts of Hunga on ozone".

Response L97: This point has been addressed.

Point L97: Although the unique aspects of this study are articulated more clearly in the revised draft than they were initially, I think that it would help to add here something along these lines: "... post-eruption. The goal is not to elucidate the chemical mechanisms giving rise to the observed low ozone, as they were investigated in detail by Evan et al. (2023) and Zhu et al. (2023). Rather, the objectives of the present manuscript can be summarized ...".

Response L97: We thank the referee for this clarification regarding the objectives, which has been incorporated into the manuscript.

Point L100: traversed by –> obtained within.

Response L100: This point has been addressed.

Point L104-106: The sentence "Satellite observations of ozone profiles and columns were exclusively acquired within this region, complementing the ground-based data while offering global coverage and regular monitoring" is problematic. It could be interpreted as saying that the satellites did not make measurements outside of this region, which is not only inaccurate but also potentially confusing since their global nature is mentioned. I suggest instead saying "This study focuses exclusively on satellite measurements acquired in this region."

Response L104-106: This revision has been incorporated into the manuscript for clarity.

Point L139: using MLS data at level 2 and version 4 (v4) –> using version 4 (v4) MLS level 2 data.

Response L139: This point has been addressed.

Point L140-142: The implications of the two different approaches to obtaining instrument pointing information for the MLS data are unclear, and actually this detail is not of much interest for the average reader. It would be better to delete the two sentences devoted to this topic and simply state that the extraordinary enhancement in H2O from Hunga degraded the accuracy of some of the v5 MLS data products in the first few weeks following the eruption.

Response L140-142: This point has been addressed.

Point L144-145: For clarity, it would be better to rewrite the first two sentences of this paragraph as: "Following the recommendations of Millán et al. (2022), the MLS profiles for January 2022 are sourced exclusively from level 2 v4 measurements (Livesey et al., 2020). The MLS profiles are categorized as Hunga-influenced or non-influenced using criteria detailed in the next paragraph."

Response L144-145: This point has been addressed.

Point L148: in any –> on any; to mean –> to the mean.

Response L148: This point has been addressed.

Point L152-153: All v5 ozone and water vapor profiles within a 5-degree radius of each of the January 2022 Hunga-influenced profiles were collected, regardless of the satellite's ascending or descending node –> All v5 ozone and water vapor profiles (on both ascending and descending sides of the orbit) within a 5-degree radius of each of the January 2022 Hunga influenced profiles were collected

Response L152-153: This point has been addressed.

Point L160-161: Assuming that I have understood correctly, for clarity change 'locations showing high water vapor and a negative ozone anomaly" to "locations showing both high water vapor and a negative ozone anomaly".

Response L160-161: This point was correctly understood. However, following Referee 2's suggestions, the criterion has been adjusted to consider only water vapor anomalies, and the corresponding revision has been made.

Point L162: 23 January –> 23 January 2022.

Response L162: This point has been addressed.

Point L164: Both profile groups –> The two profile groups.

Response L164: This point has been addressed.

Point L169-170: Although the authors' response letter makes it clear that v4 and v5 O3 and v5 H2O data were screened but v4 H2O data were not screened, the manuscript itself is confusing on this point. First it is stated that "all quality flags ... were used on the raw profiles (with the exception of the v4 H2O profiles)". Then it is stated that "Only the v5 and v4 O3 profiles were screened". These two statements are contradictory.

Response L169-170: We apologize for the confusion. The last sentence should have read: "Only the v5 O3, v5 H2O, and v4 O3 profiles were screened...", but was omitted to avoid redundancy.

Point L189-190: The statement "the altitude of the tropopause, as estimated by the instrument" implies that the IASI dataset includes a retrieval of tropopause height. Similar statements are made on L271-272 and in the Fig. 1 caption. Is that really the case, or is tropopause height taken from meteorological analyses? Please clarify and amend these statements as needed.

Response L189-190: The IASI dataset does indeed provide an estimate of tropopause altitude. These sentences have been revised for clarity.

Point L194-199: The new paragraph on IASI retrievals requires clarification on several points:

- **Point 1**: The "significant ozone perturbations" were seen in the ozone retrievals from UV-visible instruments, not in ozone itself.
- **Response 1**: This point has been addressed.
- **Point 2**: spectral ranges of ozone and SO2 do not overlap in the IASI ozone retrieval –> spectral ranges used for ozone and SO2 in the IASI retrieval algorithms do not overlap
- **Response 2**: This point has been addressed.
- **Point 3**: I do not understand what is meant by "ozone vertical variability" – given IASI's very coarse vertical resolution, it might be better to omit the word "vertical" here.
- **Response 3**: We omitted the word "vertical" in the revised version of the manuscript.

Point L209: top of the atmosphere irradiance –> top-of-the-atmosphere irradiance.

Response L209: This point has been addressed.

Point L214 & 215: near-real time –> near-real-time.

Response L214 & 215: This point has been addressed.

Point L237-238: In my original review I noted that the MLS ozone dataset has been very well validated and used extensively in prior studies. In fact, these data have been central to literally hundreds of scientific studies looking at regions all around the globe, including multiple papers by different groups examining the effects of Hunga on stratospheric ozone. Thus, the skepticism about their validity inherent in the statement "Prior to drawing any conclusions based on the MLS ozone profiles, it is

essential to verify their agreement with precise local lidar observations during unperturbed conditions" is completely unwarranted. This language should be moderated. If indeed comparisons between MLS and Maïdo DIAL O3 profiles have not been done previously, as stated in the response letter, then that represents a new contribution whose unique value should be articulated here.

Response L237-238: We apologize for the unintended skepticism regarding MLS measurements. The sentence has been revised to address this and to emphasize the first comparison between these two datasets.

Point L239-240:: Two points: (1) What does "all recovered profiles" mean? Why "recovered"? This word is used again in L258. (2) The phrases "within a 5-degree region around the lidar site" and "setting the inter-comparison radius to a maximum of 5°" are redundant.

Response L239-240:: We recognize "recovered" was an unnecessary and confusing word that we omitted in the revised version. To avoid redundancy, we also deleted the sentence "setting the inter-comparison radius to a maximum of 5°".

Point L240-242: First, these two sentences are also highly redundant and should be merged. Second, "both orbit types" should be "both sides of the orbit". I recommend rewriting these sentences as "We averaged together MLS v5 ozone profiles from both the ascending and the descending sides of the Aura orbit, which have acquisition times near Reunion around 10:15 and 21:45 UTC, respectively. On the other hand, ...".

Response L240-242: We thank the referee for this suggested revision. This point has been addressed.

Point L253: The statement "O3 MLS(z) represents the MLS ozone value from averaging kernel at an altitude z" makes no sense. O3 MLS(z) represents the retrieved MLS ozone value. The MLS averaging kernels were (or should have been) applied to the lidar data for this comparison.

Response L253: The MLS averaging kernels were indeed applied to the lidar data. This statement has been revised for clarity.

Point L257: at different layers –> in different layers.

Response L257: This point has been addressed.

Point L271: The comma after "retrieval" should be a semicolon.

Response L271: This point has been addressed.

Point L293: assumed to be of 0.02 –> assumed to be about 0.02.

Response L293: This point has been addressed.

Point L299: Results show –> Results in Fig. 2b show; both instruments –> the two instruments.

Response L299: This point has been addressed.

Point L305: Add a pointer to Fig. 2a after "respectively".

Response L305: This point has been addressed.

Point L315: also increasing the standard variation –> which also increases the standard deviation.

Response L315: This point has been addressed.

Point L317-318: Again, the language used here – "MLS appears to be a suitable substitute for lidar data in studying ozone levels" and "... supports the use of MLS data across the region" – gives the impression that the reliability of MLS O3 data for this purpose was in doubt. This wording should be toned down. I suggest at least adding "as expected" to the first phrase and simply deleting the second one. In fact, the sentence about the representativeness of Reunion data for the Indian Ocean region works better logically without that statement. Also: strong agreement –> good agreement.

Response L317-318: The suggested changes have been applied to the manuscript. This point has been addressed.

Point L320-326: Why is this discussion of the ozone annual cycle of relevance for this paper? If this information is needed to help interpret any results shown here, then that needs to be made clear; otherwise, this text seems to be a pointless digression.

Response L320-326: This paragraph has been omitted in the revised version.

Point L330: The authors state that they compared two datasets, but actually they made two sets of comparisons involving four different datasets altogether: MLS vs DIAL and IASI vs SAOZ.

Response L330: This point has been clarified.

Point L333: The panel titles in Fig. 4 ("MLS & Lidar comparison" and "IASI & SAOZ comparison") give no hint of which way the subtraction goes, nor does the figure caption make it clear. In the text, the results are characterized as "MLS–DIAL" and "SAOZ-IASI". Please clarify whether these differences are "spaceborne" minus "ground-based" data or vice versa; also, if they are not already, make the two sets of differences consistent in terms of direction (i.e., to be parallel with MLS–DIAL, the TCO differences should be taken as IASI–SAOZ, not SAOZ–IASI).

Response L333: The MLS & Lidar comparison is performed as spaceborne minus ground-based, as specified in Equation (1). This information has been added to both the caption and the text for clarity. However, the comparison between IASI and SAOZ data does not involve any subtraction. Instead, the figure shows IASI TCO as a function of SAOZ TCO, from which statistical quantities are derived. To avoid confusion, we have revised the text in L333 from "... the MLS-DIAL and SAOZ-IASI comparisons ..." to "... the MLS & DIAL and IASI & SAOZ comparisons ...".

Point L340: with higher and –> with larger biases and.

Response L340: This point has been addressed.

Point L344: I find this discussion of the relative bias between MLS and DIAL O3 confusing. It is stated that "the bias decreases to 0.24 $\pm$ 2.12%" from 40 to 45 km. However, to me it looks like the bias goes from roughly +0.5% at 40 km to nearly -1% at 45 km; that is, the relative bias grows in magnitude but changes sign over this altitude range. I do not see where the quoted value of 0.24% comes from.

Response L344: We apologize for the confusion. The value 0.24 $\pm$ 2.12% referred to the average relative bias between 40 and 45 km. This point has been revised for clarity.

Point L345: difference and error –> relative bias and standard error.

Response L345: This point has been addressed.

Point L347: bias of ozone –> bias in the ozone.

Response L347: This point has been addressed.

Point L349-350: Note also that the increased difference and error at altitudes lower than 20 km may be due to the reduced satellite accuracy and precision –> Note also that the increased relative bias and standard error at altitudes below 20 km may be due to the reduced satellite accuracy and precision at those levels.

Response L349-350: This point has been addressed.

Point L353-354: Here again I am confused by the wording of the text. It is stated that "MLS profiles tend to slightly under-estimate ozone concentrations relative to DIAL". But the relative bias shown in Fig. 4a is positive through most of the vertical domain; since the differences were characterized as "MLS–DIAL" on L333, those results indicate that MLS over- (not under-) estimates DIAL concentrations. This needs to be clarified.

Response L353-354: Prompted by the referee's comment, we have re-calculated the linear regression between MLS and lidar data. The updated regression equation ($y = 1.00x$, previously $y = 0.99x$) is very slightly greater than 1.00 but rounds down, indicating a minor over-estimation of MLS, consistent with the observed mean bias profile (as opposed to a minor under-estimation with the previous value). This clarification has been incorporated into the text.

Point L356-357: The relative dispersion RMSD=3.26% for the IASI/SAOZ comparison is characterized as "very low". But for the MLS/DIAL comparison, RMSD=1.27%. Why was that value described a "low" (L354) while the larger value for IASI/SAOZ is "very low"?

Response L356-357: We apologize for the wording issue. The text has been revised to describe the relative dispersion for IASI/SAOZ as "low" and for MLS/DIAL as "very low.

Point L360: plume (25-30 km) being –> plumes (25-30 km) that are.

Response L360: This point has been addressed.

Point L364: "it appears relevant to use" is very odd wording. I suggest simply saying "we now use".

Response L364: This point has been addressed.

Point L367 Fig. 5: The figure has been greatly improved. However, it is virtually impossible to see the red contours on the SCO panels (a1-a9), where they could be mistaken for very high DU values. It would be better to simply omit the total SO2 contours from those panels and amend the text in this line accordingly.

Response L367 Fig. 5: Total SO2 contours were omitted from panels a1-a9.

Point L369: the selection criterion –> the Hunga-influenced selection criterion.

Response L369: This point has been addressed.

Point L370-371: I have two comments about "reveal an east-to-west displacement of both plumes ... supports previous studies": (1) it's not clear what "both" means here. This word immediately follows

"H2O and ozone anomalies", so the reader naturally associates it with those two quantities, but the deficit in ozone does not constitute a "plume". I assume that SO2 and H2O are meant. In any case this needs to be clarified, perhaps by saying something along the lines of "Hunga-affected air masses" instead. (2) The east-to-west displacement of the Hunga plume is not a new result "revealed" by Fig. 5. It was reported in several previous studies, including Millán et al. (2022), Khaykin et al. (2022), and others. As the authors noted in their response, the prior studies did not specifically talk about ozone. Nevertheless, they did identify the movement of the Hunga plume, so they should be credited here; the vague allusion to "previous studies" is not sufficient.

Response L370-371: The suggested wording has been implemented, and appropriate references have been added.

Point L372: Three points: (1) It would be better not to repeat "rapid" in this line; (2) influence of H2O –> influence of excess H2O; (3) Zhu et al. (2022) could also be cited for the rapid conversion of SO2 to sulfate.

Response L372: This point has been addressed.

Point L374: illustrating –> suggesting.

Response L374: This point has been addressed.

Point L375: the Hunga –> Hunga.

Response L375: This point has been addressed.

Point L381-382: The last sentence in this paragraph essentially repeats what was said in L372-373. The repetition is confusing since the reader is expecting new information to be conveyed.

Response L381-382: The repetitive sentence has been removed.

Point L384: record anomalies of -49.9 $\pm$ 4.7 DU were recorded 76.5°E –> a record anomaly of -49.9 $\pm$ 4.7 DU was measured at 76.5° E.

Response L384: This point has been addressed.

Point L385-386: this IASI SCO anomaly is more than 14 times below the average variability –> the magnitude of this IASI SCO anomaly is more than 14 times larger than the climatological variability.

Response L385-386: This point has been addressed.

Point L386: anomaly map ... suggests –> anomaly maps ... suggest.

Response L386: This point has been addressed.

Point L390: emphasize –> indicate.

Response L390: This point has been addressed.

Point L395: Two points: (1) selected by the criterion –> identified by the selection criterion; (2) it would be good here to remind readers what the two groups of Hunga-influenced profiles are.

Response L395: This point has been addressed.

Point L400: by one of –> by each of.

Response L400: This point has been addressed.

Point L403: highest –> higher-altitude.

Response L403: This point has been addressed.

Point L405: lowest –> lower-altitude.

Response L405: This point has been addressed.

Point L406-407: ozone reduction in –> low ozone at.

Response L406-407: This point has been addressed.

Point L407-409: Presumably the ozone anomalies for the two clouds stated in absolute units (ppmv, DU/km) in L404 and L406 are computed from the MLS climatology. It is then a bit jarring to have another set of ozone anomaly values for the two clouds relative to the average lidar profile given in percent terms. This approach precludes easy comparison of the magnitude of the ozone anomalies based on MLS climatology with those based on lidar data. The MLS-climatology-based anomalies should also be quoted in terms of percent. Moreover, it is not clear why the anomalies calculated by differencing the Hunga-influenced MLS profiles and the average lidar profile are emphasized over those based purely on MLS data.

Response L407-409: The ozone anomalies for the two clouds are indeed computed from the MLS climatology, and we have clarified this in the text. To facilitate comparison, we now also express the MLS-climatology-based anomalies in percent terms. This ensures that lidar-based and MLS-based anomalies can be directly compared, rather than relying solely on the lidar-derived values.

Point L408: highest –> higher; this change should be reflected in the Fig. 6 legends as well.

Response L408: This point has been addressed.

Point L409: lowest –> lower; this change should be reflected in the Fig. 6 legends as well.

Response L409: This point has been addressed.

Point L409: coherent with Evan et al. (2023) who –> consistent with those of Evan et al. (2023), who.

Response L409: This point has been addressed.

Point L413: Two points: (1) "confirms previous research" – both Evan et al. (2023) and Zhu et al. (2023) should be explicitly cited here; (2) "the ozone anomaly is linked to a reduction of the ozone layer": by definition, a negative anomaly is a reduction – what is at issue here is the cause. It would be better to say "the ozone anomaly arose from chemical loss".

Response L413: We thank the referee for the suggested revision. This point has been addressed.

Point L415-422: Some acknowledgment that the results of this trajectory investigation are further confirmation of the passage of the Hunga plume over Reunion as established by Baron et al. (2023) and Evan et al. (2023) is needed in this paragraph; i.e., those papers should be cited.

Response L415-422: This point has been addressed.

Point L420: trajectories simulation –> trajectory simulation.

Response L420: This point has been addressed.

Point L426-427: As with the Abstract, the authors should bear in mind that many readers will skip most of the detailed discussion in the text and jump straight to the Conclusions. Therefore the Conclusions section needs to do a better job of summarizing the study and identifying its novel aspects. For example, the last sentence of the first paragraph could be amended to better capture the diversity of measurements used: "... using IASI, MLS, and OMPS satellite observations, in conjunction with ground-based measurements from Reunion". The fact that this is the first presentation of IASI data in the context of Hunga should also be emphasized.

Response L426-427: We thank the referee for the suggested revision. The Conclusions have been revised to better summarize the main results of the study.

Point L431: was passing –> passed.

Response L431: This point has been addressed.

Point L434: levels –> abundances.

Response L434: This point has been addressed.

Point L436: "TCO" and "SCO" should be redefined in the Conclusions or just written out. In addition, I find it strange that the TCO result is considered sufficiently important that it is highlighted in the Conclusions (and the Abstract, as noted above), yet was relegated to an Appendix. I come back to this point below.

Response L436: "TCO" and "SCO" have been redefined in the Conclusions, and the TCO results have been integrated into the main body of the article.

Point L437: indicated –> indicated that.

Response L437: This point has been addressed.

Point L437-440: The final sentences in the manuscript are not well composed. In my opinion they could be rewritten to better convey the message: "Hunga-influenced MLS profiles show a significant reduction in ozone over the 30-12 hPa pressure range. Ozone depletion occurred in two distinct layers, associated with two separate sulfate aerosol clouds. Within the higher altitude (17.78-12.12 hPa) aerosol cloud, ozone decreased by an average of 0.7 $\pm$ 0.5 ppmv (1.1 $\pm$ 0.7 DU/km). Within the lower-altitude (31.62-26.10 hPa) aerosol cloud, ozone decreased by an average of 0.6 $\pm$ 0.5 ppmv (1.7 $\pm$ 1.4 DU/km)."

Response L437-440: We thank the referee for the suggested revision which has been incorporated into the Conclusions.

Point L440: The paper ends rather abruptly. In addition to rewriting the last few sentences as suggested above, the authors should consider adding some sort of final sentence to put their results into context. For example, they could say something about how their finding that the observed ozone reduction appeared to be confined within two distinct aerosol layers adds new perspective to the studies that had previously reported chemical ozone loss in the week following the eruption.

Response L440: This point has been addressed.

Point Appendix A: Although I do not disagree that Fig. A1, while helpful, is the sort of ancillary material that belongs in supplementary information rather than the main body of the paper, I am less convinced that that is true of the accompanying text. To me, if a result is sufficiently noteworthy to report in both the Abstract and the Conclusions, then it should be discussed in the paper itself, not just in an Appendix. I was struck by this when I got to the Conclusions and found a number (for the max TCO anomaly) that I had not seen in reading the paper. I feel that the TCO information in this short paragraph should be integrated into the discussion in Section 3.4 (which can still refer to Fig. A1, as it already does now).

Response Appendix A: Information regarding total ozone was integrated into the discussion in Section 3.4.

Point L444: anomalies, both in –> anomalies in both.

Response L444: This point has been addressed.

Point L445-448: Clarification is needed in several places in these lines:

- **Point 1**: this IASI TCO anomaly is more than 3 times below the climatological variability –> the magnitude of this IASI TCO anomaly is more than 3 times larger than the climatological variability.
- **Response 1**: This point has been addressed.
- **Point 2**: this anomaly is about 5 times below the variability –> the magnitude of this anomaly is about 5 times larger than the variability
- **Response 2**: This point has been addressed.
- **Point 3**: When I first read this paragraph, I thought that it contradicted what was stated earlier. If this paragraph is kept in the Appendix (i.e., if the authors choose not to integrate it into the main text as suggested), then to reduce the possibility of confusion, I suggest adding this sentence at the end: "As discussed in Section 3.4 in the main text, the magnitude of the anomaly in SCO exceeds the climatological variability to an even greater degree."
- **Response 3**: This point has been addressed.

Point L459-472: The authors may wish to review the Acknowledgments carefully – there are some typos and missing words.

Response L459-472: This point has been addressed.

Point L489-490: The second entry for Baron et al. (2023) appears to point to the preprint of a paper that has now been published and thus should be deleted.

Response L489-490: This entry does not cite a publication but instead refers to the dataset from Baron (2023), which corresponds to aerosol lidar observations at Maïdo.

Point L650-652: As noted earlier, the paper by Sicard et al. has now been published, so the citation needs to be updated.

Response L650-652: This citation has been updated.

Point L673: The Earth observing system microwave limb sounder (EOS MLS) on the aura Satellite –> The Earth Observing System Microwave Limb Sounder (EOS MLS) on the Aura satellite.

Response L673: This point has been addressed.

Point L680: 2018 –> 2022.

Response L680: This point has been addressed.

Point Figure 2 caption: between 2003 to 2021 –> between 2003 and 2021.

Response Figure 2 caption: This point has been addressed.

Point Figure 3 caption: average ... profile from 2013-2021 observations at –> average ... profile calculated from observations taken over 2013-2021 at.

Response Figure 3 caption: This point has been addressed.

Point Figure 4 caption: The solid red and dashed blue lines in panel (b) should be explained.

Response Figure 4 caption: The solid red and dashed blue lines represent the linear regression line and the 1:1 line, respectively. This information was added in the caption of Figure 4.

Point Figure 5 & caption: Two points: (1) the selection criterion –> the Hunga-influenced selection criterion; (2) As noted above, the red SO2 contours should be omitted from panels (a1)-(a9).

Response Figure 5 & caption: This point has been addressed.

Point Figure 6 caption: highest and lowest –> higher-altitude and lower-altitude.

Response Figure 6 caption: This point has been addressed.

Point Figure 7 caption: for the 23.5 km –> for the trajectory ending at 23.5 km.

Response Figure 7 caption: This point has been addressed.

Response to Referee 2 Comments

We would like to once again 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 two sections: 1) response to the main comment, and 2) responses to minor comments. Referee comments are written in black and authors answers are in blue.

Main comment:

Point 1: The criterion for diagnosing Hunga-influenced profiles includes the presence of positive H2O anomalies and negative ozone anomalies at 25 or 28 km. However, the authors then use these same profiles—selected based on the presence of negative ozone anomalies at the certain levels—to argue that negative ozone anomalies exist at these levels, which are caused by aerosol clouds. This constitutes a circular argument because the negative ozone anomalies are both part of the diagnostic criterion and used as evidence to support the claim. This reasoning is not appropriate, as it relies on the defined criterion to prove the relationship. To strengthen this argument, the authors need to provide additional evidence linking the negative ozone anomalies to aerosol clouds. For example, the authors could demonstrate that these negative ozone anomalies spatially overlap with the regions of aerosol clouds at 25 or 28 km. That said, I agree that the negative ozone anomalies are related to the Hunga event. In Figure 6, the co-occurrence of positive H2O anomalies and negative ozone anomalies at the same levels supports this connection. However, the specific relationship with aerosol clouds remains unsubstantiated and requires further confirmation. At the very least, the authors should weaken their claims about this linkage to better align with the evidence presented.

Response 1: We appreciate the referee's insight regarding the circular reasoning in our initial criterion. Based on the studies of Legras et al. (2022) and Schoeberl et al. (2022), we have refined our approach to avoid this issue. These studies show that the Hunga aerosol and water vapor plumes initially coincided before diverging due to aerosol sedimentation. Legras et al. (2022) further indicate that during the first phase of sedimentation (until about 20 February), water vapor followed the descending aerosol. Given that our study period (15-23 January 2022) falls within this phase, we assume that the water vapor and aerosol clouds overlap. To eliminate circular reasoning, we now base our criterion solely on MLS water vapor profiles. Originally, we identified Hunga-influenced profiles by selecting those with negative ozone anomalies and water vapor mixing ratios exceeding 100 ppmv in the 10-100 hPa range, yielding 72 profiles. We have revised this by selecting only profiles where water vapor exceeds 100 ppmv specifically at the 26.10 hPa and 14.67 hPa pressure levels (corresponding to $\sim$25 km and $\sim$28 km, the estimated altitudes of the plumes). This refinement reduces the number of selected profiles to 47 (26 at 14.67 hPa and 21 at 26.10 hPa), particularly decreasing the number of profiles linked to the higher-altitude aerosol cloud. Although the results remain significant, this adjustment reduces the significance of the ozone anomaly associated with the higher aerosol cloud.

Minor comments:

Point 1: Line 54: The phrase "offering more surface for halogen-ozone reactions" is inaccurate be-

cause reactions involving ozone do not require surface area. Consider rephrasing it to "heterogeneous reactions".

Response 1: This point has been addressed.

Point 2: Paragraph starting from Line 63 (Introduction): In the discussions on heterogeneous reactions, Zhang et al. (2024; https://doi.org/10.1029/2024GL108649) should be cited, which provides a detailed analysis of heterogeneous reactions triggered by the Hunga event. Similarly, in the section discussing gas-phase processes, Wilmouth et al. (2023; https://doi.org/10.1073/pnas.230199412) should be included, which highlights the importance of water vapor on chemistry.

Response 2: We appreciate the referee's suggestion and have incorporated these references into the manuscript as requested.

Point 3: In Figure 5b: It is surprising to see many data points exceeding 2-sigma uncertainty, with some even showing significantly positive anomalies.

Response 3: Initially, the significance of the anomaly was determined based on the uncertainty of the daily observations, which sometimes led to anomalies exceeding the 2-sigma uncertainty due to day-to-day variability. However, with the assistance of Anne Boynard, a new co-author of this study, we have refined the processing of IASI maps to align with standard IASI procedures. This includes applying data quality filters that retain only profiles and columns with more than two degrees of freedom and a retrieval quality filter of 1, ensuring that only the most reliable observations are used in our analysis. More importantly, anomalies are now computed by averaging nearby points rather than using nearest-neighbor interpolation. Consequently, we now display all anomalies for both TCO and SCO, rather than only significant ones, and high positive anomalies occur less frequently.

Point 4: Line 414, "This observation confirms previous research and indicates that the ozone anomaly is linked to a reduction of the ozone layer." The logic of the statement is unclear.

Response 4: This statement has been changed to: "This observation confirms previous research (Evan et al., 2023; Zhu et al., 2023) and indicates that the ozone anomaly arose from chemical loss."