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 more for their helpful reviews.

This document is organized into three sections. First, we would like to draw the referees' attention to updated results concerning the DIAL/MLS comparison. The last two sections address the comments and feedback from a specific referee. The responses to Referee 1 begin on page 3 and those to Referee 2 on page 7.

Updated result

An important point worth mentioning is a flaw in our previous comparison between DIAL and MLS data. Shortly after the last re-submission, we realized that the comparison mistakenly involved DIAL data with applied averaging kernels being compared to the original DIAL data itself. This explains why the mean bias profile appeared close to 0 across most of the altitude range. After correcting this error and properly comparing the DIAL data (with averaging kernels applied) to the MLS profiles, we obtained updated results (see Figure 1). As expected, the agreement is now slightly weaker, but the mean bias in the altitude range of interest (20–40 km) remains low at 2.76$\pm$1.40 %, and should not affect any of the MLS-based conclusions presented in the article.

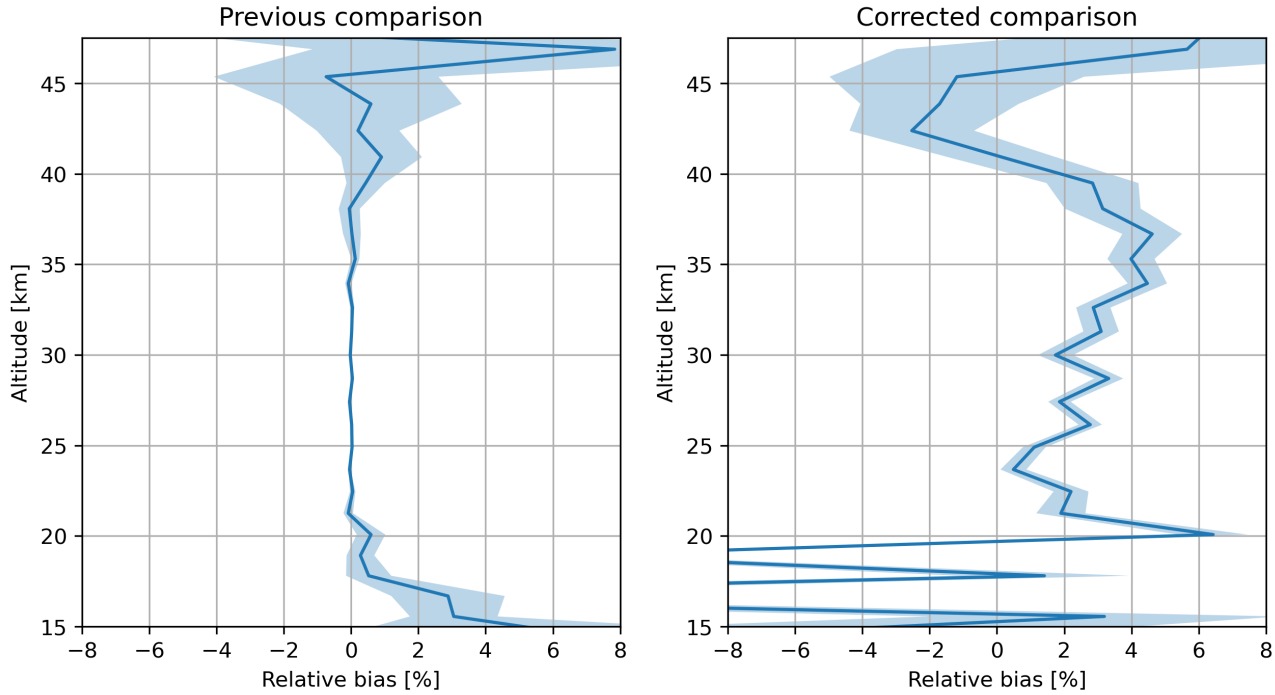

Figure 1: (Left) DIAL/MLS mean bias profile included in the previous version and (Right) updated DIAL/MLS mean bias profile.

Response to Referee 1 Comments

Once again, we would like to express our sincere thanks to the referee for their suggested re-wordings and clarifications. Their comments helped identify important ambiguities, which we have aimed to address in the revised version.

Referee comments are written in black and authors answers are in blue.

Specific comments and questions:

Point L5: while also incorporating –> and also incorporates

Response L5: This point has been addressed.

Point L10: "Revealed" has already been used in this abstract; this word should not be overused. In addition, the term "ozone depletion" is easily misinterpreted. For clarity, I suggest rewriting this sentence as "IASI ozone spatial distributions showed marked decreases in total and stratospheric ozone on that date, with the 5th percentile...".

Response L10: This point has been addressed.

Point L12-13: As currently worded, non-specialist readers could misinterpret this sentence as saying that MLS measures aerosol. Rearranging can alleviate this problem: "A key finding, as shown by MLS profiles, is that the ozone reduction was confined to two distinct layers, each associated with a separate aerosol cloud." Since this is indeed a key finding, it is curious that the authors have chosen not to include any details about the magnitudes of the ozone anomalies in these two layers, whereas this information is provided in the Conclusions.

Response L12-13: We have re-worded the message as suggested and added information regarding the magnitude of ozone anomalies.

Point L53: more surface for –> more particle surfaces for

Response L53: This point has been addressed.

Point L74-77: This discussion mixes processes occurring over different timescales and is therefore very likely to confuse readers. Evan et al. (2023) and Zhu et al. (2023) talk about the chemical processing and ozone loss that occurred in the Hunga plume within the first week of the eruption. These companion papers should be discussed together. In contrast, the studies by Santee et al. (2023), Wilmouth et al. (2023), and Zhang et al. (2024) focus on perturbations in stratospheric composition observed months after the eruption. The distinct on between these two sets of studies should be made more clearly. Moreover, although it is good to mention them for completeness, the studies of the chemical processing in subsequent months are less relevant to this manuscript, which concentrates on the immediate aftermath of the eruption. I suggest re-writing these sentences for clarity. Maybe something along these lines would work: "In this context, Evan et al. (2023) provided evidence of HCl activation on sulfate aerosols within the fresh volcanic plume, and Zhu et al. (2023) elucidated the mechanisms giving rise to the changes observed immediately following the event. (For completeness, we note that comprehensive discussions of the stratospheric chemical processes at

work in subsequent months can be found in Wilmouth et al. (2023), Santee et al. (2023), and Zhang et al. (2024).)

Response L74-77: This point has been addressed.

Point L93: Again, Zhang et al. (2024) is not concerned with the immediate aftermath of the eruption (but rather focuses on the following SH winter, JJA) and does not discuss the same processes as the papers by Evan et al. and Zhu et al. Hence the reference to Zhang et al. (2024) here should be deleted.

Response L93: This point has been addressed.

Point L126: "ozone TCO" is redundant, so delete "ozone".

Response L126: This point has been addressed.

Point L133: "data should be used to study observations –> data should be used to study conditions; within the Hunga plume –> within the fresh Hunga plume for the first few weeks after the eruption

Response L133: This point has been addressed.

Point L136: ozone –> MLS ozone

Response L136: This point has been addressed.

Point L154: due to sedimentation –> due to particle sedimentation

Response L154: This point has been addressed.

Point L161-164: This discussion is not quite correct. Neither v4 nor v5 MLS H2O measurements should be quality screened for the first ∼3 weeks a er the eruption. Standard filtering protocols should be applied to the O3 data in both versions, as indicated here, but not to either version of the H2O data.

Response L161-164: We apologize for the confusion. Filtering criteria were indeed not applied to either the v4 or v5 MLS H2O profiles. This has been clarified in the manuscript.

Point L180: in (Boynard et al., 2018) –> by Boynard et al. (2018)

Response L180: This point has been addressed.

Point L182: to the top of the atmosphere (∼60 km): 60 km is not the top of the atmosphere

Response L182: This point has been addressed.

Point L214: near-real time –> near-real-time

Response L214: This point has been addressed.

Point L321: I'm not sure that ACP style will allow the ampersands ("&") in these lines, and in any case I do not think that their meaning is clear. I suggest just using a forward slash instead (e.g., "MLS/DIAL"). Alternatively, "vs" might also work.

Response L321: We have replaced the ampersands with the forward slash, both in the text and within

the corresponding figure.

Point L330-345: I find this discussion a bit confusing. First it is stated that MLS has a relative bias and error with respect to DIAL measurements of 0.11 ± 0.20% in the 20–40 km altitude range. In this case a statement such as "MLS slightly overestimates DIAL in this region" would be appropriate. But then it is stated that over the whole altitude range, the linear regression y = 1.00 x shows that "MLS profiles tend to slightly over-estimate ozone concentrations relative to DIAL ... irrespective of the altitude". I do not see how the statement "slightly overestimate" is justified given the value of "1.00" in the linear relationship.

Response L330-345: As mentioned in our comment on the second page of this document, the comparison between DIAL and MLS data was updated, and the corresponding results were revised accordingly. The updated results should now eliminate any confusion, as both the mean relative bias profile and the linear regression (y = 1.02x) clearly indicate a slight over-estimation by MLS relative to the DIAL data.

Point L347: "an elevated correlation" –> "a fairly strong correlation" (the word "elevated" raises the question "compared to what?")

Response L347: This point has been addressed.

Point L350: altitudes of the Hunga volcanic plume ... that are –> altitudes of the Hunga-affected layers ... that are

Response L350: This point has been addressed.

Point L351: low deviation –> low relative deviation

Response L351: This point has been addressed.

Point L375-376: This wording is unclear. To avoid misinterpretation, it would be better to rewrite this sentence as "IASI recorded the highest number of negative ozone anomalies linked to Hunga on 20 and 21 January (panels (a6)-(a7) and (b6)-(b7) of Figures 5 and A1).

Response L375-376: This point has been addressed.

Point L182: It's possible that I have misunderstood the point here, but to me it seems that the sentence "These values significantly exceed climatological variability" is redundant with "meaning this anomaly is more than three times larger than the typical variation". The first sentence should be either deleted or rewritten to clarify what information it provides that is not covered in the second sentence.

Response L182: We have deleted the first sentence to avoid repetition, as suggested.

Point L400: with respect the –> with respect to the

Response L400: This point has been addressed.

Point L400-405: This discussion is opaque and hard to follow. For one thing, "resp.", used repeatedly in these lines, is not a common abbreviation, and I am not sure what it means here. I believe that the authors intend to provide percent anomalies for the upper and lower aerosol clouds relative to both the MLS averaged Indian Ocean profile and the mean lidar profile from DIAL, but if so this is a very awkward way to go about doing so. It is also confusing to call an anomaly expressed in terms of

percent a "volume mixing ratio anomaly". Finally, panel (e) of Figure 6 is no longer referenced in the text. I think that it would be much clearer to not only rewrite these sentences, but also to rearrange this entire paragraph such that the percent anomalies for each layer are given immediately following their associated absolute anomalies. Assuming that I have understood correctly, I suggest something like: "The ozone mean anomaly associated with the higher-altitude aerosol cloud is (1σ) significant at the 12 hPa level and barely (1σ) significant at the 14 hPa pressure level, with an average anomaly relative to the average background MLS profile of -0.7 $\pm$ 0.6 ppmv (-1.0 $\pm$ 1.0 DU/km) across these two pressure levels. In percentage terms, this corresponds to –5.5 $\pm$ 4.7% and -6.3 $\pm$ 4.8% with respect to the average MLS profile over the Indian Ocean (Figure 6e) and the mean lidar profile (Figure 3), respectively. For the lower-altitude aerosol cloud, ($1\pm$) significant ozone anomalies occur across the 21-32 hPa pressure range, with a mean anomaly of -0.6 $\pm$ 0.5 ppmv (-1.7 $\pm$ 1.4 DU/km), corresponding to (-7.5 $\pm$ 7.0% and -8.5 $\pm$ 8.1% with respect to the mean MLS Indian Ocean and the mean lidar profiles, respectively."

Response L400-405: We thank the referee for this suggested clarification. This point has been addressed.

Point L417: This construction ("the latter shows") appears to point only to Figure 5. For clarity, this should be rewritten as "... in Figs. 5 and A1; these two figures also show a westward ...".

Response L417: This point has been addressed.

Point L421: amounts water –> amounts of water

Response L421: This point has been addressed.

Point L423-425: The way these sentences are written makes it sound like IASI "observations are derived from IASI, MLS, and OMPS satellite data", which makes no sense. This problem can be solved by re-wording / rearranging: "Here we use satellite observations from IASI, MLS, and OMPS, complemented by ground-based measurements from Reunion, to provide a detailed view of the evolution of ... Indian Ocean. This study presents the first analysis of IASI data in the context of Hunga."

Response L423-425: This point has been addressed.

Point L436: exceed –> exceeding

Response L436: This point has been addressed.

Point L438-440: Anomalies expressed in terms of percent will be more meaningful to many readers than the values given here. It would be good to add the corresponding relative anomalies in a manner similar to that suggested above.

Response L438-440: This point has been addressed.

Point Figure 6 caption: Panels (a-b) presents –> Panels (a-b) present; panels (c-d) shows –> panels (c-d) show; influenced by one of the aerosol clouds –> influenced by the aerosol clouds

Response Figure 6 caption: This point has been addressed.

Response to Referee 2 Comments

We wish to express our sincere thanks to the referee for their suggested clarifications.

Referee comments are written in black and authors answers are in blue.

Specific comments and questions:

Point 1: Throughout the paper–including the abstract, introduction, and conclusion–the authors emphasize that "the ozone loss happened in two layers of aerosol clouds" (e.g., line 12), implying that aerosols were the primary driver of the observed ozone loss. However, Zhu et al. (2023) attributed the ozone anomaly to a combination of processes, with aerosols being a contributing but not dominant factor. Therefore, the use of "associated with aerosol clouds" may overstate the role of aerosols. I suggest replacing "aerosol clouds" with a broader term such as "Hunga plume" or "aerosol clouds with excess water vapor" to avoid potential misinterpretation.

Response 1: We acknowledge the referee's comment that the discussed impact on ozone results from the combined effects of water vapor and sulfate aerosols, rather than aerosols alone. To avoid overstating the role of aerosols, we have replaced most occurrences of "aerosol clouds" with the suggested terms.

Point L27: The paragraph discussing tropospheric ozone seems less relevant to the main focus of the paper. This is more of a comment than a request to remove it, as I respect the authors' writing style.

Response L27: This point has been addressed.

Point L33: For improved clarity, I recommend revising "global chemistry" to "global atmospheric chemistry".

Response L33: This point has been addressed.

Point L40: Consider simplifying "heterogeneous chemical reactions" to the more commonly used "heterogeneous reactions".

Response L40: This point has been addressed.

Point L333: The statement "Between 40 and 45 km, the average relative bias increases to 0.24 $\pm$ 2.12 %" seems inconsistent with Figure 4a, where the relative bias values between 40 and 45 km appear to be all above 0.24%. Please double-check this value for accuracy.

Response L333: As mentioned in our comment on the second page of this document, the comparison between DIAL and MLS data was updated, and the corresponding results were revised accordingly. The updated results should now eliminate any confusion.

Point L360: It would be helpful to briefly remind readers of the detail of the "Hunga-influenced selection criterion" at this point in the text or in the caption of Figure 5 for easier reference and clarity.

Response L360: As suggested, we have included a reminder detailing the selection criterion. Ad-

ditionally, we added a clarification at its first mention (Lines 156–157 of the updated manuscript) to improve its initial presentation.