Review of "Evidence of an Ozone Mini-Hole Structure in the Early Hunga Plume Above the Indian Ocean" by Millet et al.

This paper uses IASI total column ozone (TCO) measurements and MLS vertically resolved ozone profiles to investigate low ozone observed over the Indian Ocean in the week following the Hunga eruption. First, measurements of aerosol from OMPS-LP and two ground-based instruments are used to characterize the passage of the volcanic plume over Reunion. Then IASI and MLS data are compared to ground-based measurements (SAOZ TCO and DIAL profiles, respectively) obtained at Reunion under background conditions to confirm their suitability for the study. Negative anomalies in IASI TCO are linked to a negative anomaly in MLS profiles that peaked around 15 hPa, where the excess water vapor injected by Hunga was maximum. Transport of Hunga-influenced air masses was explored through HYSPLIT back trajectories and inspection of ERA5 PV maps.

In my opinion, both the analysis performed in this study and the presentation thereof are seriously flawed. While it is possible that some of these individual issues could be addressed with more work, others are fundamental in nature. Taken together, I believe that these deficiencies should preclude publication of the manuscript in anything resembling its present form. However, I realize that sometimes manuscripts are published even when a reviewer feels that rejection is warranted. Therefore, in addition to summarizing my major concerns, I have made the effort to describe in detail specific substantive issues that would need to be addressed before the manuscript could be re-considered. I have also listed a number of minor points of clarification as well as grammar/ typo corrections at the end.

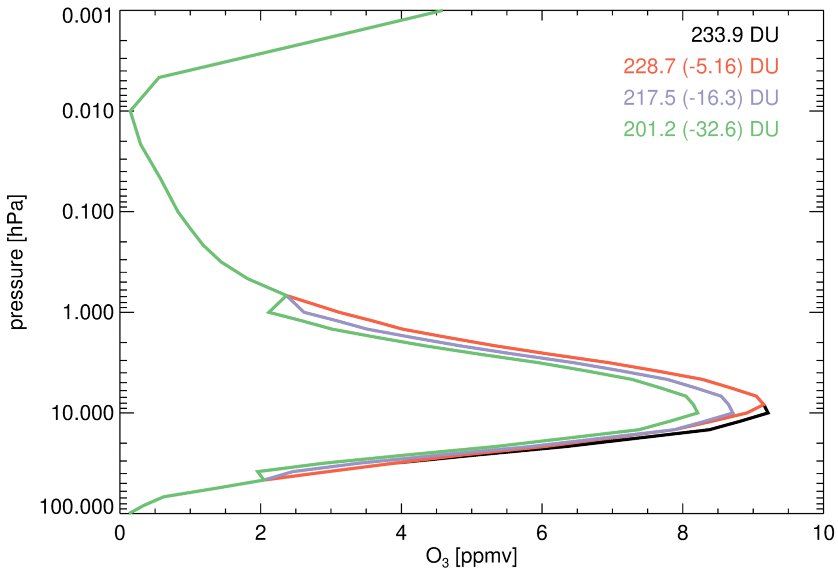
Major comments:

- The term "ozone mini-hole" has a specific meaning – it refers to a transient natural synoptic-scale phenomenon that arises, mainly in midlatitudes, through dynamical and transport processes (a combination of uplift and horizontal advection of ozone-poor air). Total column ozone decreases rapidly during a mini-hole event but returns to its initial levels as the weather systems pass. Ozone mini-holes are unrelated to photochemical processes. Thus, the region of low ozone described and attributed to Hunga in this paper is NOT an "ozone mini-hole". This wording needs to be changed throughout the manuscript, including the title.

- The authors acknowledge that the Hunga plume adversely affected the DIAL and SAOZ ozone retrievals, and therefore the post-eruption data from those ground-based instruments are not used in their analyses. The impact of the extreme stratospheric hydration from Hunga on MLS retrievals is also discussed (although that description needs some clarification, as noted in the specific points below). In contrast, while cloud contamination is noted, the potential effects from Hunga on IASI data are not mentioned, and those measurements are presented with no Hunga-related caveats whatsoever. It is hard to believe that the IR measurements from IASI would be completely unaffected by either the enhanced gas-phase SO_2 following the eruption or the sulfate aerosol that formed from it within the first week. Indeed, as the manuscript shows, the region of low ozone is highly aligned with the region of initially high SO_2. The conversion to sulfate is then inferred from the reduction of SO_2 in the region of low ozone. Whether this is a real atmospheric feature or a measurement artifact is not clear. It is essential that discussion of the IASI data quality in the wake of the eruption be added.

- Further to the preceding point, I find Figure 5 and the associated discussion unconvincing. I have several technical criticisms of the figure/text, detailed in the specific points below. But the big-picture issue is that the depiction of anomalies in the IASI maps is not compelling. Anomalies of apparently comparable magnitude can be seen in many parts of the displayed area, including in the vicinity of Reunion on 15 January before the arrival of the Hunga plume, so the anomalies being spotlighted by the authors hardly stand out. Most of the maximum anomalies quoted in the text are marginal, and some are not significant at even 1σ. Moreover, the focus on maximum anomalies is puzzling. Since in most cases the exact location of these points is not specified, it is not even certain that they occurred in the region near Reunion and not elsewhere in the study area. It is not clear why a regional average anomaly on each day was not computed and related to the passage of the plume.

- The maximum anomaly in January 2022 TCO from IASI (about –39 DU) was linked to the average ozone anomaly in Hunga-influenced profiles measured by MLS, which peaked at 15 hPa. There are several issues with this aspect of the study, starting with relating maximum anomalies in TCO to average anomalies in vertically resolved ozone. In addition, the 15-hPa average ozone anomaly from MLS is not significant (–0.4 ± 0.7 ppmv, 1σ), so these results are even less convincing than those based on column ozone. Most importantly, it is not possible to reconcile the magnitudes of the two sets of anomalies, as illustrated in the figure embedded below. The black line shows a climatological MLS ozone profile (for 2005, a representative year) calculated over the region 10°S–30°S; its associated stratospheric / mesospheric (100–0.001 hPa) burden is 233.9 DU. The red line shows the same climatological profile perturbed with an anomaly like the one indicated in this study (–0.4 ppmv at 15 hPa). This modified profile has an ozone burden of 228.7 DU, only about 5 DU less than the original profile. The purple line shows the climatological profile with 0.5 ppmv subtracted between 40 and 1 hPa, effectively perturbing the bulk of the stratospheric ozone layer. The associated burden for this profile is 217.5 DU, a reduction of about 16 DU. Finally, the green line shows the results for 1 ppmv subtracted from the climatological profile over 40–1 hPa. In this case, the reduction in the burden is 32.6 DU, still about 6 DU less than the maximum anomaly in TCO reported in this paper. The key point is that the entire stratospheric ozone layer would have to be substantially perturbed to achieve an anomaly in total ozone of the magnitude asserted here. If indeed TCO was truly reduced by as much as 39 DU, then the decrease must have occurred in the troposphere rather than the stratosphere, in which case it is very unlikely to have been related to the Hunga eruption.



- I fail to see the point of much of the discussion in Section 3.5 on the transport of Hunga-influenced air masses over the Indian Ocean. Several previous studies tracked the early dispersion of the plume, including Millán et al. (2022), Legras et al. (2022), and Khaykin et al. (2022); moreover, its presence over Reunion within a week has already been established by Baron et al. (2023) and Evan et al. (2023). Even if the authors felt that further confirmation was needed, the HYSPLIT trajectory calculations would have been sufficient. Instead, maps of ERA5 PV are shown and the fact that they reveal no "marked discontinuity" in the PV field during this period is argued to be evidence that east-to-west isentropic transport at 600 K was possible. It is not clear what kind of atmospheric feature it is thought may have impeded such transport. An issue that is overlooked in this discussion is that ERA5 does not assimilate water vapor measurements, and thus it did not accurately capture post-eruption perturbations in stratospheric circulation, as discussed for MERRA-2 by Coy et al. (2022).

- Fundamentally, the raison d'être for this manuscript is not clear. Much of the analysis centers on evaluation of MLS and IASI ozone data through comparisons with DIAL and SAOZ measurements made at Reunion under background conditions. The statement is made "Based on the excellent correlation and agreement between satellite (MLS and IASI) and ground-based instruments (stratospheric lidar and SAOZ) over Reunion, it appears relevant to use satellite ozone products to investigate the changes in the distribution of ozone over the study region." But this is hardly a surprising result – both MLS and IASI are very well-characterized data sets that have already been employed extensively in similar kinds of studies, including over the region in question. In fact, arguably the entire intercomparison portion of this study was unnecessary. On the other hand, validation of the satellite measurements – in particular those from IASI – under perturbed post-eruption conditions would have been valuable, but that was not possible using the Reunion data as noted above. Furthermore, this work seems to have provided no additional scientific insights beyond those already presented in the papers by Baron et al. (2023), Evan et al. (2023), and Zhu et al.

(2023). Indeed, as the authors note, Baron et al. (2023) presented the lidar data and talked about the passage of the Hunga plume over Reunion. Evan et al. (2023) presented MLS (and other) data, including ozone, over Reunion during the same timeframe. Evan et al. (2023) and Zhu et al. (2023) elucidated the mechanisms giving rise to the observed low ozone (conclusions that this paper makes no attempt to add to). Nothing in this current study is new, other than the addition of total column ozone measurements, whose reliability in this particular region at this particular time has not been adequately addressed, as noted above.

- Throughout the manuscript, numerical results are reported with what seems to me to be an unjustifiably high degree of precision. As just one example, the maximum anomaly in IASI TCO is stated to be –38.97 ± 25.39 DU. This “false precision” needs to be removed.

<u>Specific substantive issues:</u>

- L1: Most of the aerosols of stratospheric significance were not emitted directly by the volcano, but rather arose through subsequent SO_2 conversion to sulfate.
- L16: It is not clear why the 2018 WMO Ozone Assessment is referenced for this general statement, rather than the most recent Report from 2022, which is cited elsewhere in this manuscript.
- L30-42: I have several comments on this paragraph:
    - It is stated in the first sentence that eruptions can influence tropospheric ozone, but the rest of the paragraph does not elaborate on this point at all, and it is not clear why it is relevant to this paper (unless the observed reduction in ozone is in fact occurring in the troposphere). The connection to tropospheric ozone needs to either be explained better or omitted altogether. Moreover, for clarity, in L36 “contribute to ozone depletion” should be “contribute to stratospheric ozone depletion”.
    - It is stated that eruptions release substantial amounts of aerosols, but the volcanic aerosols of most consequence for the stratosphere are those formed subsequently by the conversion of SO_2 to sulfate, not those (e.g., ash) emitted directly by the volcanoes.
    - Literature citations are inadequate. It is not sufficient to cite only Tie and Brasseur (1995), Hofmann and Solomon (1989), and McCormick et al. (1995) for these points – many more references than these would be relevant in each case. At the very least, an “e.g.,” needs to be added in front of all of these references.
    - It is not clear what is meant by the sentence “*Additionally*, reactive anthropogenic chlorine compounds may be enhanced in volcanically perturbed regions, leading to *further* ozone depletion” [emphasis added]– how is this different from “the activation of chlorine compounds on volcanic particles”, “ozone depletion through heterogeneous chemistry”, and “relationship between SO_2 and chlorine in causing ozone decline post-eruption” that have already been mentioned in the preceding three sentences?
- L59: Wright et al. (2022) is not the most suitable reference for the Hunga aerosol perturbation; in addition to Sellitto et al. (2022), other appropriate work to cite for this point include Khaykin et al. (2022) and Taha et al. (2022) – both already cited elsewhere. Wright et

al. (2022) is pertinent to the statement about the comparative energy release by Hunga, so it should be moved to that part of the sentence.

- L61: Sellitto et al. (2022) is not really the best reference for the magnitude of the Hunga water vapor injection; it should be replaced here by Khaykin et al. (2022) and Vömel et al. (2022, https://doi.org/10.1126/science.abq2299).
- L63-65: The sentence "As a result of the main austral summer stratospheric circulation and the prevalent phase of the QBO, the first signs of the Hunga aerosol plume's passage over Reunion were noticed only 4 days after the main eruption" is problematic for several reasons. First, it's not clear what "main" means in this context (and the word "main" is used in three other places in the paragraph in reference to the eruption, so it is confusing). Second, the QBO is mentioned, but its influence is not made clear – was the QBO in an easterly or westerly phase at the time of the eruption, thus did it delay or accelerate the plume's arrival over Reunion? I believe that the authors mean that the prevailing westward flow brought the plume to the region of Reunion very quickly, such that it could be observed by instruments there within a short period of 4 days, but the wording is ambiguous and could be misinterpreted. Third, this is the first mention of Reunion in the main text. Since its importance to this work has not yet been established, it comes out of the blue and is a bit jarring. A lead-in sentence introducing Reunion and giving the reader a hint about its role in this work would be good. Otherwise, the relevance of the following information is unclear.
- L67-79: This discussion of the results of Evan et al. (2023) and Zhu et al. (2023) could be better organized – it jumps back and forth between heterogeneous and gas-phase reactions, making it difficult to follow. More importantly, some of the results of those studies are misstated. First, the Hunga-induced stratospheric cooling enhanced heterogeneous reaction rates but was not a factor in the rapid conversion of SO_2 to sulfate aerosols, as is implied by the current wording (L69-70). (Also, a reference to the earlier paper by Zhu et al. (2022, https://doi.org/10.1038/s43247-022-00580-w) should be added for the impact of abundant OH from the Hunga hydration on the rapid sulfate formation.) Second, I was puzzled by the emphasis on photolysis of Cl_2 (L76), as this is not part of the conclusions about gas-phase chemistry reported by Zhu et al. (2023) as is suggested, but then I found a similar sentence in the paper by Evan et al. (2023). However, Evan et al. are talking about the negative HCl anomaly arising from *heterogeneous* chlorine activation on sulfate. Their statement about Cl_2 photolysis is made in connection with the colocated positive anomaly seen in daytime ClO. It is not correct that this is a "key gas-phase mechanism contributing to ozone loss".
- L80-93: Although the longer-term evolution of the Hunga water vapor and aerosol plumes is certainly interesting, it is not clear what relevance any of this has to the ozone distribution in the first week following the eruption, which is the focus of this study. If such discussion is retained, then it needs to be much more comprehensive in its summation of the existing literature on Hunga's radiative impact. Moreover, if the radiative effects from Hunga in subsequent months are covered here, then why are its chemical effects ignored?
- L95 (and also L136): Livesey et al. (2008), which is a conference proceeding, is not a suitable reference for Aura MLS. The paper by Waters et al. (2006) is sufficient.
- L98-99 and L102: The phrase "dynamics of its advection" seems strange to me, since "dynamics" and "advection" are essentially synonyms. I suppose that the authors mean that

they will show details of the plume's transport, but this should be clarified. Moreover, it is not clear what "its" in this sentence is referring to – grammatically it does not make sense.

- L111-116: It is not sufficient to simply state that the temporal and vertical resolution of the lidar data is "high". This information should be specified, especially the vertical resolution. Moreover, it is not appropriate to characterize a data set consisting of a total of 470 profiles obtained over a 9-year period as having "high" temporal resolution.
- L132-135: Aspects of the MLS description need to be improved. The term "consistent measurement frequency" is ambiguous – initially I thought it was referring to spectral frequency. Thus, "spatial sampling" would be better. Also, it is not clear what "consistent" means in this context (and the MLS orbit ground tracks do differ slightly from one day to the next) – I would delete this word. It is not quite correct to refer to MLS as "a radiometer" (the instrument actually consists of seven radiometers); this is an unnecessary detail that it would be better to omit.
- L136-140: The recommendations of Millán et al. (2022) are slightly mischaracterized. That paper stated that the reliability of MLS measurements *inside* the Hunga plume (not "close to" it) was degraded in the first few weeks immediately following the eruption, because of the enormous enhancement in H_2O concentrations. The statement that "MLS v4 relies only on profile retrievals from O_2 signals whereas v5 also uses the H_2O line" is unclear – this statement refers specifically to how information about *instrument pointing* (required for the retrieval of atmospheric composition profiles) is obtained in the two versions. This should be clarified. In addition, Millán et al. (2022) indicated that the standard MLS data quality screening protocols should NOT be implemented for the v4 H_2O data during that initial post-eruption period. On the other hand, such filtering should still be performed for the O_3 measurements, whose quality, as noted in L139, was unaffected by Hunga. The description of the MLS v4 data handling is unclear on this issue – since both the v4 and the v5 MLS Data Quality Documents are referenced in L157, the implication is that the v4 data (both H_2O and O_3) were screened, but the data filtering recommendations should be followed and the approach taken should be stated explicitly.
- L144-146: Some rearrangement of this discussion is needed. The numbers of MLS profiles being examined here – 113 influenced by Hunga and 2190 in unperturbed conditions between 15 and 23 January – only make sense in the context of a limited region (since MLS measures ~3500 profiles per day). However, the information that the comparison is restricted to a 5-degree radius around Reunion is not provided until much later in the paragraph.
- L147: Is the standard deviation calculated separately at each pressure level or over the whole 10–100 hPa range? In other words, is the maximum value of 0.05 ppmv quoted here never exceeded at any single level? If the standard deviation is being calculated over the entire profile, then a larger value (at, say, the level of peak ozone anomaly) could be getting "diluted" in the overall profile standard deviation.
- L152: I am interpreting the statement "repeated for both ascending and descending nodes" to mean that for the comparison between perturbed and unperturbed conditions, background values were calculated separately for the measurements obtained on the two sides of the orbit. Since ozone at these altitudes does not display large diurnal variations, I am wondering why it was considered necessary to derive both daytime and nighttime background profiles.

- L206-207: While the three references cited in this sentence are pertinent to the statement that persistent Hunga effects were confined to the stratosphere, they are completely unsuitable for the point that they immediately follow, which is that the stratospheric circulation is stable and stratified. In fact, no such statement is needed to justify the use of trajectory calculations (a very common technique). I recommend deleting everything in this sentence up to "we used HYSPLIT".
- L218-219: Similarly, PV is so widely used now for characterizing isentropic transport in the stratosphere that not only is such a list of citations unnecessary, but also the one provided is so seemingly arbitrary and self-referential that it does more harm than good. This sentence should be deleted.
- Section 2.4: I have several concerns about the DIAL/MLS intercomparison and its description:
  - MLS retrievals are output on a pressure grid, whereas the comparison with the lidar measurements uses altitude as a vertical coordinate. How the MLS measurements are placed onto an altitude grid needs to be explained.
  - As noted above, the vertical resolution of the lidar measurements is not given, but I presume that it is much higher than that of the MLS ozone profiles (which is ~2.5–3 km in the lower stratosphere). Simply sampling the finer profile at the MLS retrieval surfaces is not the best approach. To make a truly fair comparison between high-vertical-resolution profiles and coarser-resolution MLS data, it is necessary to follow the guidance in the MLS Data Quality Document to apply the MLS averaging kernels to and perform a least-squares "smoothing" of the high-resolution data set (Sections 1.8 and 1.9 of the MLS Quality Document, respectively). Although performing such a procedure may not make a substantial difference to the bottom-line results, this issue is nevertheless worth some investigation. At the very least, an experiment in which the lidar profiles are smoothed over ±1.5 km (i.e., boxcar smoothing) should be conducted to explore the impact on the comparisons with MLS.
  - The notation in the numerator of Equation (1) is confusing: $O_{3\ MLS} - O_{3\ DIAL}(z)(z)$. Why is the first term not written $O_{3\ MLS}(z)$ (as in L240), rather than putting both "(z)"s at the end?
- L267 and Figure 1: It is difficult to judge where 5°S and 25°S are located on these panels, as the x-axis latitude grid is odd. Instead of placing the vertical lines at the x-axis major tick marks, it would be more helpful to draw vertical lines at 5°S and 25°S. I also request that minor tick marks be added to both x- and y-axes.
- L281-282 and Figure 2: I do not understand what "This multi-year average represents an average of AOD data which is grouped into months, irrespective of the years" means – does the blue line in Figure 2 show the overall average over the 2003–2021 period, or is it just the January mean over all the years in that period? The uncertainty error bars on the red and black lines in Figure 2b are nearly impossible to see, even when zooming in on the panel. Greater contrast between the lighter and darker shades is needed. I also request that minor tick marks be added to both x- and y-axes.
- L320-332 and Figure 4: This discussion requires clarification in several respects:
  - Saying "the bias decreases to –3.73" makes it sound as though the bias has gotten smaller, whereas it has changed sign but actually is larger in magnitude.
  - Given the large oscillations in the differences below 20 km, the average bias carries little meaning, so there is no real benefit in stating it.

- Livesey et al. (2022) discuss the presence of known systematic vertical oscillations in the MLS ozone retrievals in the UTLS; these likely play a role in the differences below 20 km seen here. However, they do not explain the rather large relative bias at 20 km.
- Given the stated caveats on the lidar data, it is not justifiable to say "the MLS mean bias profile seems to under-estimate ozone concentrations by 20.73±1.89%". All that can be said with confidence is that there is a relative bias of ~21% between the two data sets.
- Similarly, the linear regression shows that "MLS profiles tend to slightly over-estimate ozone concentrations" *relative to DIAL*.
- I request that minor tick marks be added to both x- and y-axes.

- L345-371 and Figure 5: I have several issues with Figure 5 and the accompanying discussion:
  - The east-to-west movement of the plume during this time period was shown also by Millán et al. (2022).
  - Schoeberl et al. (2022) is not an appropriate reference for the rapid conversion of SO_2 to sulfate aerosol – Zhu et al. (2022) and Asher et al. (2023) are better citations for that.
  - It is not clear what "important" means in the context of the negative ozone anomaly.
  - I am confused about what the error bars on the daily minimum TCO and maximum TCO anomaly values represent – please clarify.
  - Why is the IASI TCO anomaly in January 2022 (which is derived relative to the 2014–2021 IASI climatology) being compared to the SAOZ climatological January TCO rather than to the IASI January climatology?
  - The color palettes used for the TCO maps need improvement, especially the anomaly one. For one thing, the color bar saturates below –10 DU, making it impossible for the reader to judge the ~20–40 DU maximum negative anomalies noted in the text. Although a bright blue color is used for those largest negative anomalies, the contrast between it and the color used for the negative anomalies with magnitude smaller than 10 DU is too weak to be readily discernible without extreme magnification. In addition, positive anomalies should be easily distinguishable from negative ones – as it is now, the zero line falls in the middle of a continuum of bluish-greenish colors.
  - The overlaid contours depicting SO_2 are red, not blue.
  - It makes little sense to highlight the MLS profiles with high H_2O values in green, when the orbit tracks are overlaid on SO_2 maps plotted using a yellow-green color palette.
  - The black and white star denoting the location of Reunion is very difficult to spot.
- L379: While the largest anti-correlation may coincide with the maximum ozone anomaly, the r value a couple of levels below is nearly as large.
- Figure 7: Why are the trajectories plotted using such thick lines? The individual trajectories are completely indistinguishable. Perhaps that is the point, but it could still be easily made using differently colored lines of more moderate thickness.
- L392-402 and Figure 8: As stated in the general comments, I do not see the added value of this discussion. In addition, I have some specific comments:
  - Of what possible relevance is the location of the *global* average latitude of the subtropical barrier on these dates? In the context of this study, only its location in the region of the Indian Ocean is important.
  - The anomaly does not stay completely north of the subtropical barrier – some solid green dots are clearly present poleward of the barrier on 18 and 19 January (panels (d) and (e)).

- It is stated that the anomaly exits the region on 22 January, but a couple of green dots still appear on the map on both 22 and 23 January, and on 23 January (panel (i)) they fall south of the subtropical barrier.
- The red line marking the subtropical barrier is dashed, not solid as implied in the caption.
- Using red for the subtropical barrier is a poor choice since it is overlaid on contours of similar color (ranging from purple to orange).
- As in other plots, it is very difficult to spot the black and white star indicating Reunion.

- L407: As noted earlier, most of the aerosols of stratospheric significance were not emitted directly by the volcano, but rather arose through SO_2 conversion to sulfate.
- L417: It is not appropriate to say "the ozone reduction occurred at the level of the ozone layer". The ozone layer is a broad feature, extending over roughly 15–35 km altitude. MLS showed substantial (but not significant) anomalies only in a narrow layer around 15 hPa.
- L431-441: The statistical quantities described here are common and widely used, so I am not convinced that this Appendix is really needed.

<u>Minor points of clarification, wording suggestions, and grammar / typo corrections:</u>

- The word "highlight" is used about a dozen times throughout the manuscript, but in many cases its usage is inappropriate. I suggest alternatives in the specific comments below.
- L4: Why are the observations used here (obtained in January 2022) referred to as "current"?
- L8: Spectrometer --> Sounder
- L9: add ", respectively" after "maps", to avoid giving the impression that vertically resolved profiles and total column ozone are provided by both instruments
- L24: "end of the century" – clarify by changing "the" to "this" or adding "21st" in front of "century"
- L25: On the contrary --> In contrast
- L29: enhanced by anthropogenic activities such as agriculture, industry and transport, that release NO_x and aerosols --> enhanced by anthropogenic activities that release NO_x and aerosols, such as agriculture, industry and transport
- L32: add a comma after "(1982)"
- L34: add a comma after "(SO_2)"
- L36: add a comma after "(H_2SO_4)"
- L39: delete "Justifiably" (it is not clear what this word is intended to convey here, but it is not appropriate)
- L50: "PSCs" – acronym not defined
- L53: volcanic sulfate aerosols penetration --> penetration of volcanic sulfate aerosols
- L60: add a comma after "scrutiny"
- L64: QBO" – acronym not defined
- L74: again, "justifiably" is inappropriate and should be deleted
- L94, 97: Why are the observations used here (obtained in January 2022) referred to as "currently available" and "current"?
- L95: add a comma after "2004)"

- L99: analyses --> reanalyses
- L102: add a comma after "Ocean"
- L105: how could Hunga have had any impacts on ozone before the eruption?
- L111: resolutions --> resolution
- L114: add a comma after "low"
- L131: satellite observations of ozone profiles and TCO were used in complement to ground-based data --> satellite observations of ozone profiles and TCO complement the ground-based data; a global --> global
- L133: solar time --> local solar time
- L134: calculate --> observe
- L144: 113 ozone and water vapor profiles --> 113 Hunga-influenced ozone and water vapor profiles
- L150: each January 2022 impacted profiles --> each of the January 2022 impacted profiles
- L154: accuracy and precision that are both lower than 10 % --> accuracy and precision that are both better than 10 % ("lower" can be interpreted to mean "worse")
- L167: as a representative --> as being representative
- L172: correlation between … anomaly with --> correlation between … anomaly and
- L188: located in --> located at the
- L205: to compute and simulate trajectories --> to compute trajectories
- L206 and L208: "air masses" should not be hyphenated
- L208: simulation to highlight the trajectories --> simulation of the trajectories
- L210-211: the NOAA citation is out of place – it should immediately follow "GDAS" in L209
- L209: 240 hours --> 240-hour
- L210: equitably --> equally
- L214: a citation is needed for ERA5 (e.g., Hersbach et al., 2020)
- L222: localization --> identification
- L226: of the dynamical barriers --> of dynamical barriers
- L233: time --> times
- L260: ozone impacts --> impacts on ozone (as written, it sounds like the effects are from ozone rather than on it)
- L263: compared to the troposphere --> compared to that in the troposphere
- Figure 1 caption: background conditions prior to the passage of the volcanic plume on 17 January --> background conditions on 17 January prior to the passage of the volcanic plume (as written, it sounds like the plume passed over the area on 17 January)
- L270: enabled to monitor the --> enabled monitoring of the
- L272: colocalized --> colocated
- L273: highlight --> emphasize
- L274: AOD --> total AOD
- L278: AOD (in red) at 532 nm --> AOD at 532 nm (in red); also, add a comma after "2022"
- Figure 2 caption: both --> the two
- L284: both --> the two
- L297: total … were --> total … was
- L298: delete the comma after "km"; highlights --> illustrates

- L300: add a comma after “Reunion”
- L305: add “at that time” at the end of the sentence
- L307: relative to --> for
- L312: standard deviation --> the standard deviation
- L317: agreements are --> agreement is
- Figure 4 caption: MLS ozone --> daily MLS v5 ozone; side each --> side of each
- L319: high number --> large number
- L321: 20 km of altitude, with --> 20 km altitude of
- L327: lower number --> smaller number
- L328: 453 start below 20 km, 409 before 17.5 km and 131 before 15 km --> only 453 extend below 20 km, 409 below 17.5 km, and 131 below 15 km
- L333: high number --> large number; indicates --> leads to
- L334: “elevated correlation (r=0.87)” – elevated over what? The comparison here is unclear, especially since r=0.99 for the DIAL/MLS comparison
- L336: seem to be --> are
- L337: being --> that are
- L347: circles) comprising the MLS profiles with high H_2O values which met the criterion selection (green circles) --> circles), with the MLS profiles with high H_2O values that met the criterion selection marked by green circles
- L348: highlight --> indicate
- L352: visible on --> visible in
- L353: The first appearing important negative ozone anomaly linked to the Hunga appears --> the first important negative ozone anomaly linked to Hunga appears (although see above for a comment on this sentence, in particular the use of the word “important”)
- L354: with --> with a value of
- L370: transition --> shift; Similarly to Evan et al. (2023), they indicate the co-localization of --> Similar to the findings of Evan et al. (2023), our results indicate the colocation of
- L372 and L395: criterion selection --> selection criterion
- Figure 6 caption: ozone profiles determined using MLS profiles which met the criterion selection --> ozone from v4 MLS profiles that met the selection criterion; also, the thin blue lines presumably represent the 113 individual profiles going into the average, but that should be explicitly stated in the caption
- L376: the correlation is between H_2O and ozone itself, not ozone loss (some of the anomalies are positive); also, “holds” is not quite the right word here – “leads to” would be better
- L377: highlighted with --> marked by
- L377-378: largest ozone anomaly reads --> largest average ozone anomaly is; largest water vapor --> largest average water vapor
- L379: As Fig. 5 --> While Fig. 5
- L382: this sentence basically says nothing – by definition, a negative anomaly in stratospheric ozone is linked to a reduction of the ozone layer
- L383, L384, and L386 (twice): “air masses” should not be hyphenated
- L385, L388, L398, and Figure 7 caption (twice): “back trajectories” should not be hyphenated
- L386: delete “, and”

- L392: highlight --> investigate
- L395: large water vapor level detection --> detection of enhanced water vapor
- L397: enters the Indian Ocean --> enters the region of the Indian Ocean
- L410: from 5°S and 25°S --> from 5°S to 25°S
- L415: emphasized --> observed
- L416: add a comma after “January”
- L417: reads --> is
- L420: highlighted --> reflected; summer --> austral summer
- L425-426: evolution of the localization of the early ozone and water vapor anomaly in the Hunga volcanic aerosol plume in the Indian Ocean --> evolution of the colocated ozone and water vapor anomalies in the early Hunga volcanic plume over the Indian Ocean
- L430: the Hunga --> Hunga
- L434: both --> the two