Response to the Referee #1's comments on "The impact of El Niño–Southern Oscillation on the total column ozone over the Tibetan Plateau" (egusphere-2023-1452)

We thank the Referee #1 for making detailed comments and very useful suggestions to improve the paper. The manuscript has been revised and improved in response to the referee's comments and suggestions. Below is a point-by-point response (in black) to the referee's comments (in blue) followed by any modifications to the manuscript (*in italics*). We have added a new figure to show the great coherence between the ozone changes and the tropopause changes in terms of their spatial patterns. We have also included a caveat in discussion on the uncertainties of the results.

The line numbers for the changes correspond to the clean/revised manuscript version.

Response to ***Anonymous Referee #1***

General comments

This article aims to explore the impact of ENSO on the variability of the total ozone column over the Tibetan Plateau. The topic of the work is absolutely interesting, as the subject is not much investigated and the mechanisms driving the variability of ozone over the Tibetan Plateau are not fully elucidated. Also, the paper is well written, so below there are only a limited number of technical corrections to improve the readability.

Many thanks for the positive evaluation and the suggestions to improve the paper.

However, the paper suffers from many important drawbacks in the methodology applied, and therefore calls for major improvements before it can be accepted for publication on Atmospheric Chemistry and Physics.

First of all, the analysis is mostly based on the use of correlation coefficients, which alone cannot be used neither to document the performance of model simulations against observations, neither to prove the existence of physical links between two (or more) atmospheric processes. Indeed, the presence of a correlation in two variables is not alone sufficient to claim the existence of a cause-effect relationship. Even more so as in this case the analysed variables present well different spatial patterns (which is never clearly discussed).

Thank you for the comments and concern. For the performance of model simulations against observations, the high correlation (above 0.95, Figure 1) of TP TCO between C3S dataset and TOMCAT simulation from December to May give us confidence that the TOMCAT is able to capture the observed variability in TP TCO during these seasons and that we can thus use it to investigate the impact of ENSO on the TP TCO change.

For the correlation analysis, we agree with the reviewer that the correlation analysis has its limitation. We apologise for the lack of discussion about the spatial pattern. We have now added a caveat in summary and discussion [Lines 406–410], and added a plot about partial column ozone and discussion showing their great coherence in spatial patterns [see Figure 6; Lines 280–283]. The composite analysis from Figure 6 highlights the coherence between the ozone changes and the tropopause changes during ENSO events, which further supports our correlation results.
[Lines 406–410]: "*Our study focuses on the diagnosed ozone changes over the TP during ENSO episodes using both observations and a chemistry transport model TOMCAT as well as several statistical methods, which will have some uncertainties due to large internal variability of ozone and limited ENSO events. Future work is needed for a better understanding of tangible ENSO impacts with more observed ENSO events and a full-chemistry climate model*".
[Lines 280–283]: "*Considering that the area–averaged climate mean of TH over the whole TP is about 150 hPa during December–May, Figures 6c–6d show the composite anomalies of the partial column ozone at 150 hPa. Their spatial patterns (Figures 6c–6d) are in good agreement with the composite TH anomalies (Figures 6a–6b), highlighting the good coherence between the ozone changes and the tropopause changes*".

Secondly, the authors focus the analysis on composite averages analyzing simultaneously multiple El Niño (La Niña) events, and it is not clear if the results can be ascribed to all events or just to some major leading ones. Tropopause height, but also ozone, present relevant day to day changes, which have not been thoroughly analysed and documented here.

The composite results are statistically significant according to the two–tailed Student's *t*–test, ruling out the possibility that the results are dominated by a single event. This is also clear by looking at the scatter plot (Figure 9) with each event represented as a dot showing that the relationship between ENSO index and ozone-related changes is evident with most of the events coherent with composited results.
For the second part of the comment, the reviewer suggests a day-to-day analysis, which will be helpful to take account of the seasonal resolved impacts or subseasonal changes. However, this is out of scope of this study.

Thirdly, the reanalysis, satellite measurements and model analysis utilized have very different (and coarse) resolution, which can lead to significant drawbacks in the analysis. On the basis of my review, I guess that the paper would benefit from a major revision and explanation of the methodology applied and from an improved discussion to motivate, interpret and prove the results obtained. Something also in the direction of the suggestions from the editor, highlighting that other modes of variability can impact on the total ozone time series, can be also beneficial.

Thank you for this. Yes, the reanalysis, satellite measurements and model analysis have different resolutions in the horizontal and vertical. The current work mainly focuses on the Tibetan Plateau region (27.5–37.5°N, 75.5–105.5°E), so these data have been weight-averaged to the same region which is mentioned in the paper (Figures 1, 2, 4, 5 and 9). For the region results, we use the available reanalysis product and satellite dataset which have been properly cited in their descriptions.

For the TOMCAT model simulation, we use the standard setup (with 2.8° horizontal resolution) for the long-term simulation (1979-2021), which is able to capture the general features of ozone and other chemical species. Previous studies (Feng et al., 2005a, 2011, Chipperfield et al., 2005, 2006, thereafter) showed that different horizontal resolutions would make some changes in the tracer transport and modeled tracers distribution, there is a significant improvement when using the resolution 2.8° compared to 7.5° (e.g., Chipperfield et al., 2005, Feng et al., 2005b), but slight improvement when using 1-degree resolution (T106 Gaussian grid) (Feng et al., 2011). Overall, the 2.8° is still reasonable well able to capture the general features of the simulated ozone and other tracers when compared with different measurements.

Specific comments

1. Lines 14-15: Well, more precisely, the solar ultraviolet radiation and the derived risks to human and ecosystems health are controlled by the stratospheric ozone. It is true that the total ozone column is essentially equal to the stratospheric content as the tropospheric ozone concentration is too low as compared to the stratospheric one, but the sentence should be more precise.

We have modified our sentence as noted: [Line 15] *"……which is mainly controlled by the local ozone in the stratosphere*".

2. Lines 15-18 and 62-64: ENSO has both an oceanic (El Nino) and an atmospheric component (Southern Oscillation), but here you refer only to the oceanic one. Please describe better.

Revised.
[Lines 15-18] "*ENSO……is characterized by the tropical Pacific Ocean sea surface temperature anomalies (SSTA) and sea level pressure change for the warm phase El Niño and cold phase La Niña events."*

3. Lines 28-32: The explanation is quite confused and not straightforward, I would suggest to rephrase to make it clearer.

Revised.
[Lines 25-32] "*…… This reduced temperature associated with El Niño events causes a decrease of the tropopause height, which tends to replace ozone–poor tropospheric air by ozone–rich stratospheric air in the UTLS and hence leads to the increase in TCO*".

4. Lines 32-36: I do not understand the meaning of "descending upper-level geopotential height". Explain better. Also I suggest rephrasing: "Our results suggest that the El Niño events lead to a

descending upper–level geopotential height and hence cause a decrease in air column thickness, which in turn induces reduced tropospheric temperature over the TP."

The "descending upper–level geopotential height" means a "*negative upper–level geopotential height anomaly*", which has been explained in revised manuscript [Line 27].
Thank you for the suggestion, we also have rephrased our sentence [Lines 26-28].

5. Lines 34-36: I do not understand how just a shift in the ozone profile can lead to an increase in TCO.

The shift of ozone profile represents the partial column ozone anomalies associated with TH change. We have updated explanation for clarification, please also see the response to point 3.
[Lines 272-276]: "*Approximately 90% of ozone in the atmospheric column resides in stratosphere; the ozone concentration is much lower in the troposphere with a gradual transition at the tropopause. Therefore, a decrease of tropopause height (TH) will tend to replace ozone–poor tropospheric air by ozone–rich stratospheric air in the UTLS region, and thus increase the partial column ozone, which in turn contributes to the TCO increase, and vice versa for an increase of TH (e.g. Schubert and Munteanu, 1988; Salby and Callaghan, 1993; Steinbrecht et al., 1998; Chipperfield et al., 2003; Varotsos et al., 2004; Tian et al., 2007)*".

6. Lines 38-39: And what about the frequency of the events? If the events of the two kinds change intensity of the same magnitude, but with opposite effects on ozone, wouldn't be the final change almost null? Please explain better.

The frequency of strong ENSO events (El Niño and La Niña) will be increasing under greenhouse–gas–forced warming (e.g. Cai et al., 2015, 2018). However, it is still uncertain the magnitude intensity of events and which phase events (El Niño or La Niña) is increasing, owing to the lack of inter–model consensus (e.g. Cai et al., 2015, 2018; Collins et al., 2010). We have modified our sentence as noted [Line 33]: *"…… climate models project an increase in the frequency of strong El Niño or La Niña events*".

7. Lines 12-41: The abstract is too long and reporting many results of the correlation tests, while it is better to focus on the interpretation of the results and the underlying mechanisms. Please revise.

We have removed some details on the correlation tests and highlighted the impact of ENSO on the TP TCO and its underlying mechanisms [Lines 13-35].

8. Line 47: Please explain better how the elevation of the site is linked to the low air density and high atmospheric transparency.

As altitude increases, the number of gas molecules in the air decreases, and therefore the air becomes less dense (Ahrens and Samson, 2011). We also changed “atmospheric transparency” to “*clean air (Pokharel et al., 2019)*”. These citations have been added [Line 41].

9. Lines 48-49: How is this connected with the previous sentence?

The connection is: the presence of ozone is crucial for any life because the UV radiation is harmful to the biota. Please see [Lines 41-45].

10. Lines 54-58: Those mechanisms that you listed here are various and of different nature. I would suggest better description.

We have improved these sentences by classifying these proposed mechanisms into two different processes, with one on the stratospheric process and another on tropospheric changes. The revised sentence [Lines 48-52] is also shown below.
“*These studies have argued that summertime TCO low is caused by changes in mass exchange between troposphere and stratosphere due to the stratospheric variability, for example the synchronisation of the quasi–biennial oscillation (QBO) and seasonal cycle (Chang et al., 2022), and tropospheric changes, for example the high topography and thermal forcing of the TP (Ye and Xu, 2003; Kiss et al., 2007; Tian et al., 2008; Guo et al., 2012) and enhanced convective activity in summer (Liu et al., 2003; Bian et al., 2011).”*

11. Lines 58-60: Before you were talking just about summer TOL, now you talk about winter and spring events, perhaps (not clear) connected to different processes. Revise.

We have revised it to make better connection, as noted [Lines 52-55]: “*In comparison to the summertime TCO change, less attention was paid to the TP TCO variability for other seasons. It is worth highlighting that the interannual variability of TP TCO from wintertime to springtime is strongest (Figure S1 of the Supplement)*”.

12. Lines 61-62: As commented above, the link between QBO and TOL is not explained. Also, in this sentence, it is not clear if there is a link between QBO and ENSO. Revise.

We have added the link between QBO and TCO low in the revised manuscript [Lines 55-56]. We also have added a link between QBO and ENSO [Line 186].

13. Lines 64-66: Are you talking about climate or about meteorology or of the Earth system when you talk about the interannual variability of ENSO?

We are talking about climate. This is now specified.

14. Lines 68-69: The sentence and the reason why most studies have focused on the polar regions and the tropical stratosphere is not clear.

We have now made it clear [Lines 65-66]: "*……considering the major ozone production in the tropics and ozone depletion in the polar region (e.g. Staehelin et al., 2001)*".

15. Line 72: I would not label 1979-2002 measurements as "very limited", if not explained better what the limitations are.

As the period 1979–1992 (14 years) used by Zou et al. (2001) is relatively short and the ENSO events (3 El Niño, 2 La Niña) are too few based on Tables 1–2, we feel that their results are based on very limited observations. Added it as noted [Lines 68-69]: "*However, their results are based on very limited ENSO events since the satellite era from 1979 to 1992*".

16. Line 74: You never talked of a limitation in spatial coverage, so the reader has no way to compare the claimed much wider range of your work against previous ones.

Revised.
[Lines 71-72]: "*……we use a longer period of ozone measurements over the period 1979–2021*".

17. Line 75: It is not exactly clear, as stated now, what the chemical transport model adds to the analysis.

We have added it as noted [Lines 72-73]: "*along with the TOMCAT chemical transport model simulations (e.g., Chipperfield et al., 2017, 2018)*".

18. Lines 81-83: This sentence is more appropriate for the conclusion section rather than for the introduction.

Thank you for this. We have deleted this sentence in the revision.

19. Lines 102: The resolution seems to be rather coarse, which can pose limitations to the study.

Please see the detailed response mentioned above (3rd ***General comment***).

20. Lines 86-119: I would suggest explaining better what kind of measurements/observations are derived from each source, as the sources are many and of different kinds: satellite, reanalysis, …

Thank you for this. The source has been labeled in revised manuscript [Lines 80-116].

21. Line 128: Usually ECMWF means that you are not using always ECMWF data? From what does this depends? Also, which ECMWF reanalysis?

All chemical transport models (CTMs) require the forcing files to determine the atmospheric background (winds, temperature, humidity). It also applies to the TOMCAT CTM which is forced by meteorological dataset, usually ECMWF and sometimes UK Met Office (UKMO; Swinbank and O'Neill, 1994). Both show that TOMCAT produces a good simulation compared to the observations (e.g. Feng et al., 2003). In the current study, we have used the latest ECMWF reanalysis product (ERA5) because of its longer coverage period to ensure the consistency (ERA-Interim stopped in August 2019, Li et al., 2020). Feng et al. (2007) noted some abrupt changes in temperature when moving one ECMWF product to another for the long-term simulation (ERA40 to ERA-Interim) and argued the unrealistic variations in the analyses used to force the model. In any case, to make our sentence clearer, we have deleted "*(usually ECMWF)*" in revised manuscript.

22. Lines 127-129: The sentence is not clear, as presented now, as it repeats that the model is forced with ECMWF winds and temperatures to specify atmospheric transport and temperatures. Revise.

We have revised repeated sentences [Lines 124-127].

23. Lines 130-131: Yet another different, and coarse, resolution...

Please see the detailed response mentioned above (3rd ***General comment***).

24. Lines 135-136: This also depends on the purpose of the investigation...

We have deleted the judgement on different definitions [Lines 130-131].

25. Lines 157-168: The correlation coefficients alone cannot prove the goodness of the simulations against observations, as they indicate only that the model reproduces the temporal variability observed, but the presence of biases cannot be detected by correlations. In any case, the fact that in some seasons (months? Please see next comment) you have low correlations

points out that there could be differences also in the simulated temporal patterns, at least in some seasons, and this needs to be better discussed. Revise.

Yes, we have revised.
[Lines 177-184]: "*Although the TOMCAT overestimates the SD (**Figure 2a**) because of its biases (Li et al., 2022), it can be seen from **Figure 2a** that TOMCAT matches well the SD variability and correlation coefficients with ENSO in the C3S dataset. These biases of TOMCAT simulation are likely due to (1) the incomplete presentation of complex atmospheric process in the TOMCAT, or (2) the uncertainties in the TOMCAT's meteorology (ERA5) reanalysis scheme (Mitchell et al., 2020; Dhomse et al., 2021). Nevertheless, the high correlation (above 0.95, **Figure 1**) of TP TCO between C3S dataset and TOMCAT simulation from December to May give us confidence that the TOMCAT is able to capture the observed variability in TP TCO during these seasons and that we can thus use it to investigate the impact of ENSO on the TP TCO change*".

26. Figure 1: If the correlations are 3-months, and we have 4 seasons, why do you have 12 columns in the plot?

Sorry for the typo. It is the correlation coefficients between the time series of 3-month running mean TCO anomalies over the TP. We have corrected the caption of Figure 1.

27. Line174: The seasons are already utilized previously, so must be explained previously.

We have added explanation previously [Lines 159-160].

28. Line 177 and also 136-137: The use and the explanation of the lead-lag correlation coefficient is not clear.

We have added its use and explanation into Section 2.4 as noted [Lines 131-133]: "*In order to find out during which months there is a significant response of TP TCO to ENSO, we use lead–lag correlation coefficient, which is calculated according to cross correlation function (Chatfield, 1982).*"

29. Line 183-186: Not clear, revise.

Revised.
[Lines 202-205]: "*Therefore, the following composite TCO anomalies are averaged during December–May to maximize the signal, while we should note the general relationship between ENSO and TCO is not sensitive to the chosen period as one could expect from the positive correlation during December–May*".

30. Line 191: Is it standard deviation or variance?

It is standard deviation. We have revised Figure 2 and associated description [Lines 210-211].

31. Table 1 and 2: What is the meaning of the mean Niño 3.4 index?

It is the mean Niño 3.4 index for total events of El Niño (Table 1) and La Niña (Table 2). We have added it into revised manuscript [Line 220].

32. Lines 211-221: The spatial pattern of the variability is different in the two phases, and should be discussed. I would also recommend discussing the relevance of the changes in percentages (are 8 DU anomalies relevant?) The discussion is in any case not clear and should be revised.

Thank you for this. Now we have added the discussion:
[Lines 223-226]: "The TCO spatial patterns between El Niño and La Niña events are generally opposite despite some differences (Figure 3), which may be related to the asymmetric features in ENSO itself and its climate impacts (Hoerling et al., 1997; An and Jin 2004; Gao et al., 2019)".
We have also added the definition of the relative percentage change.
[Lines 228-229]: "On average, the ENSO events correspond to about ±1.2% relative percentage change (i.e., the anomaly divided by its climate mean) of TP TCO in terms of C3S dataset".

33. Figure 4 and 5: The plot documents that there are significant disagreements between the model and the observations, especially in the La Nina phase where the simulated vertical profile is remarkably different than the observed one. I am not sure if this depends from one particular event which is not simulated correctly or it is a general problem, since the authors have always used the composite seasonal means considering together DJF and MAM rather than analysing single events..

These disagreements are most likely due to the biases of TOMCAT, which may be related to (1) the incomplete presentation of complex atmospheric process in the TOMCAT, or (2) the uncertainties in the TOMCAT's meteorology (ERA5) reanalysis scheme (Mitchell et al., 2020; Dhomse et al., 2021). Please see [Lines 177-184 or Specific comments 25]
To Figures 4-5, we also added the associated discussion as noted [Lines 254-257]: "……*there are disagreements between the TOMCAT and SWOOSH dataset due to the model's biases (Dhomse et al., 2021)*……".

34. Figure 6: Are the spatial changes in TH similar to those in TOC? The spatial pattern seems different, so it is not clear how the two changes can be actually connected. Also, the implied

changes in ozone documented in Figure 6c and d are remarkbly lower than those presented previously...

The difference in their spatial pattern is because the TCO represents total column ozone, but there is a great coherence between the TH and partial column ozone. We have added a plot and discussion [see Figure 6; Lines 280–283]. Please see the detailed response mentioned above (***1st General comment***).

For the second part of the comment, Figures 6 show composite partial column ozone anomalies rather than the TCO, therefore their values are lower than those of Figure 3.

35. Lines 282-283: Please explain better how TH and the SSTA are linked to atmospheric circulation, and if they are linked. Up to now you were talking of just TH...

We have revised it as noted: [Lines 301-304]: "*According to equation (3), the anomalous tropospheric upper–level geopotential height can induce the tropospheric temperature change via modifying the air thickness (e.g. Wallace et al., 1996; Sun et al., 2017; Li et al., 2022). As the TH is closely related to tropospheric temperature change (e.g. Seidel and Randel, 2006), it is suggested that the anomalous upper–level geopotential height could influence the TH change.*".

36. Lines 288-308: the discussion is confused and not straightforward.

Revised.

[Lines 323-326]: "…… *The possible mechanism for the ENSO teleconnection over the TP has been discussed by previous studies, including excited Rossby wave from tropical Pacific and Indian Ocean to extratropical regions (e.g. Jin and Hoskins, 1995; Trenberth et al., 1998; Zhang et al., 2015b), and enhanced land-sea temperature contrast between tropical Indian Ocean and TP (e.g. Chen and You, 2017; Zhao et al., 2018)……*".

37. Lines 317-319 and 336-349: The analysis of correlation coefficients alone cannot justify the physical mechanism that you are implying. Please better discuss.

Yes, we agree that the correlation analysis has its limitation, which we have added a caveat in summary and discussion [Lines 403–407]. In addition, the analysis is shown not only through correlation coefficients but also through physical linkage, including the equation (3) and WMO's TH definition.

[Lines 406–410]: "*Our study focuses on the diagnosed ozone changes over the TP during ENSO episodes using both observations and a chemistry transport model TOMCAT as well as several statistical methods, which will have some uncertainties due to large internal variability of ozone and limited ENSO events. Future work is needed for a better understanding of tangible ENSO impacts with more observed ENSO events and a full-chemistry climate model*".

38. Figure 8: Yet another spatial pattern, different than those presented previously...

We have plotted the composite anomalies of the partial column ozone at 150 hPa (Figures 6c–6d), which has a good coherence with the TH (Figure 6a–6b) and tropospheric mean temperature (Figure 8). They all show that the composited anomalies decrease from south to north of the TP.

39. Figure 9 and line 338: It is not clear the meaning of "temperature associated with air thickness..

The "temperature associated with air thickness" is calculated from equation (3), indicating the air temperature caused by air thickness. We have added it into caption of Figure 9 [Line 368]

Technical comments

1. Line 19: Perhaps remove "of ENSO"?

Deleted.

2. Line 25 and 363: "lead"?

Yes, it is "lead".

3. Lines 69-70: I would suggest rephrasing: "The effects of ENSO on ozone changes at mid-latitude and in particular over the TP are less studied and discussed."

Rephrased.

4. Lines 70-71: Rephrase "... suggest the amplitude of ENSO signal in TCO over the TP to be of the order of 20 DU (their figure 3)"

Rephrased.

5. Line 78: Add "following" after "three"

Added.

6. Line 86: what do you mean by “merged”?

The “merged” means that this data is created by combining ozone data from 15 satellite sensors. The modification has been made in Section 2.1 of revised manuscript [Lines 82-87].

7. Line 220: Change “understanding” to “understand”

Done.

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