Reviewer #2 comment on "Variability of temperature and ozone in the upper troposphere and lower stratosphere from multi-satellite observations and reanalysis data" by Shangguan et. al.

Reviewer #2 (Comments to Author):

This paper uses temperature and ozone from satellite measurements and reanalysis products to estimate their variability and trends in the upper troposphere and lower stratosphere (UTLS). Trends are analyzed between 2002 and 2017, and multiple-linear regression model is applied to separate the influences of the Quasi-biennial Oscillation (QBO) and the El Nino Southern Oscillation (ENSO) from trends. In the context of the SPARC Reanalysis Intercomparison Project this paper is an important contribution to the literature. Unfortunately, this paper does not clearly motivate its objective and misses several marks scientifically. In particular, trend analyses over such a short time-period are suspect and (as the paper shows) inconsistent, making interpretation of these results difficult. Furthermore, connections between ozone and temperature are loosely implied in manuscript without detailed analysis, and the modeling results presented herein are not explained in depth. Finally, the paper is poorly written with grammatical and spelling mistakes throughout, making it very difficult to follow at numerous points. If major revisions are made to address these shortcomings, this paper will be a valuable contribution to the SPARC Reanalysis Intercomparison Project.

We thank the reviewer very much for the very constructive and useful comments and suggestions. We have revised the manuscript according to all the comments. Firstly, we have rewritten our introduction to explain our motivation clearly in the context of the SPARC Reanalysis Intercomparison Project. Secondly, we rechecked the significance of the trends by calculating the signal-to-noise ratio. Thirdly, we have made a correlation test between temperature and ozone time series to study the connection between ozone and temperature. We apologize for the grammatical and spelling mistakes and we have checked the whole text carefully and corrected the mistakes. We hope the reviewer could find the manuscript has been improved significantly.

Please see below our point-to-point response to all reviewers' comments and suggestions. Reviewer comments are in black, following by our respective replies in blue.

Kind regards,

Ming Shangguan (on behalf of all co-authors)

Major Comments:

1. This paper is challenging to read because it has significant grammatical errors and spelling mistakes. Often sentences are difficult to parse without several readings, and these problems detract significantly from the scientific content of the paper. For instance, in a part of the paper with an important physically-based discussion (the discussion of model results on pg. 13, line 1), the main sentence of the discussion is so confusing that the message being conveyed is lost. In another example, the primary sentence outlining the paper's goal (pg. 2, line 25) is choppy and unclear, blurring the paper's motivation. I've highlighted some of the more obvious problems in the line-by-line comments below, and at minimum these should be addressed. Preferably, the entire paper would be carefully edited to improve its readability and appropriately convey the authors' scientific findings.

Thank you very much for your comments. We are really sorry for so many grammatical errors and spelling mistakes in the text. We have modified the text according to your suggestions and edited the entire paper carefully. The introduction has been rewritten to explain our motivation clearly. More details can be found in our line-by-line response and the revised manuscript.

2. Because reanalysis products are combinations of observations and models to assimilate the data, it is disingenuous to consider their trends as directly related to observations. Furthermore, interpretation of reanalysis trends is complicated because the assimilation step brings in data which leads to discontinuities which will vary from place-to-place, time-to-time, and reanalysis-to-reanalysis. The authors themselves acknowledge this problem (pg. 2, line 31), but proceed with their analyses without quantifying how discontinuities affect their results. Reanalyses trend results presented here are suspect and must be interpreted with caution. Without significant changes to the trends analyses (some ideas to do this I suggest below), the authors should instead shift the main focus of their paper to the comparisons between the variabilities in the reanalysis and GPS products.

We totally agree with the reviewer that the reanalysis products are influenced by both observations and assimilation systems and should not be compared to observed trends directly. According to your suggestions, we have rewritten our introduction and shift the main focus of the paper to the comparisons between the variabilities in the reanalysis and GNSS products. In addition, we corrected the temperature discontinuities around 2006 in the reanalysis by using a transfer function approach similar to Wargan et al., 2018. The corrected GNSS RO time series was used as a common baseline since it does not have significant discontinuities. Details of the bias correction for reanalysis temperatures can be seen in the supplementary information. The temperature trends from reanalysis data sets were recalculated and their significance was also rechecked using the signal-to-noise ratio.

3. The problem of interpreting trends from reanalysis is exacerbated by the very short time period considered in this study. A 15-year period (2002-2017) to calculate trends is quite short, and I suspect this contributes to one of the main results of this paper (Table 1), that trends vary in sign and significance depending on the region (except in the tropical middle stratosphere, 10hPa, where trends are more robust, but which is not the focus of this UTLS paper). By eye, the trends appear to be in agreement with one another (Figure 11) in the stratosphere, but there are clear distinctions which makes overall interpretation challenging. This is an inherent difficulty for the study, because GPS data does not extend earlier than 2002. The authors themselves note (citing Santer et al., 2017) that the trend assessment from such short periods can be strongly influenced by start/end years (see also Bandoro et al. 2017, Santer et al. 2011). Given how short the period of record is, without a detailed signal to noise study, is too early to make decisive or defensible claims about UTLS temperature trends in the 21st century. If this study was improved to include a signal-to-noise study which showed the trends are robust, the study results would be more compelling.

Thank you very much for the constructive comments. Yes, the 16-year period is relatively short to calculate trends and there is clear distinction between different data sets especially in regions with insignificant trends. According to your suggestion we have made a signal to noise study based on three 145-years CESM simulations. The CESM runs were integrated in a fully coupled mode with an interactive ocean for the time period 1955 to 2099. All anthropogenic forcing, e.g. GHGs and ODSs were fixed to values at the year 1960. The three simulations are slightly different with the natural forcing. The first run used observed solar irradiance, time varying volcanic aerosols and a nudged QBO, while the second run fixed the solar irradiance as a constant and the third run did not include a QBO. More details of the simulations can be seen in the supplementary information. The influences of solar cycle, volcanic aerosols and QBO were excluded by a multiple linear regression before the calculations of the background noise.

To assess the effect of seasonal and interannual variability on 16-year temperature trends, we fit linear trends to overlapping 192-month segments of the 1740-month in each of CESM runs. For maximally overlapping 192-month intervals (i.e., for overlap by all but one month), one simulation yields 1549 samples of 192-month trends. Following the method described by Bandoro et al. 2017 and Santer et al. 2011, we exclude the largest cooling or warming trends from our analysis and calculate the standard deviations of the 16-year trends (right panel in Fig.1). Note that the method used here is slightly different with that in Bandoro et al. 2017. We estimated the standard deviation of by different overlapping 16-year trends from the same model while they used a large ensemble of simulations with different models. The advantage of their approach is that the results are not model dependent. However, our results based on the CESM model should be helpful since it is one of the best models and has been widely used in UTLS studies.

The signal to noise ratios of 16-year GNSS RO temperature trends are shown in Fig.1 (left panel). Here we use the 90% and 95% significance level, which corresponds to a signal to noise ratio close to 1.65 and 1.96. Seen from Fig. 1, the areas with significant trends are smaller than that shown in Fig. 11 in the main text. However, there are still significant signals in the mid-latitudes of the upper troposphere, around the tropopause and in the southern hemisphere in the middle stratosphere. All the significant regions in Fig. 1 are actually the most important areas with strongest and significant trends in Fig. 11. This suggests that the significant trends shown in Fig. 11 are robust except that in the tropics whereas the standard deviation of the trends are the strongest.

To my understanding, the signal-to-noise ratio suggested by the reviewer and the significance test used in this manuscript are actually two methods to test the significance/robustness of the calculated trends. The main difference between the two methods is the way to estimate the standard deviation/noise. Since the standard deviation of the residuals of the linear fit has been widely used in trend analysis (e.g., Wigley et al., 2006), we would like to keep the significance test as it was in the manuscript. At the same time, we have put Fig. 1 in the supplementary and added some discussions correspondingly in the revised manuscript.

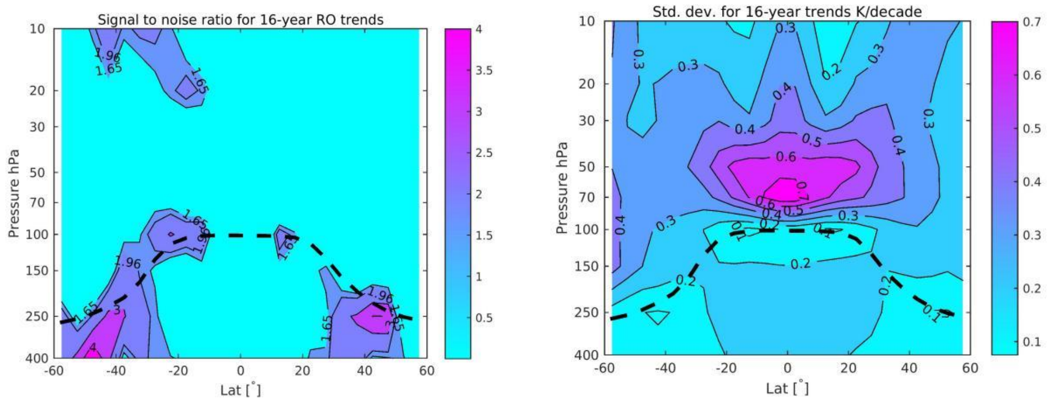

Figure 1: Signal to noise ratios (left) are estimated RO trends divided by the standard deviations of model trends (right), calculated using overlapping time series segment.

4. One of the main reasons short trend calculations here are challenging is because of biases early in the time period (2001-2006), as noted in the text and shown in Figures 1. These biases early in the period will drive trends in the underlying data which will factor into the trends calculated with the MLR method. For instance, I can quickly estimate the following trends in the biases: @400hPa: +0.2 K/decade, @100hPa: +0.35 K/decade, @70hPa: +0.25 K/decade. Each of these is on the order of the trends found in Table 1 for those regions, making it very difficult to determine whether trends found to be “significant” are actually just trending because of early period biases. Table1 should be updated to include the trends in the biases (like the estimates above) for each product and region (or some similar analysis), and to directly with the calculated trends (e.g., this method is used to examine radiosonde

trends in Wang et al. 2012). Where the bias trend is on the order of the product temperature trends, the robustness of those trends should be reconsidered.

Thank you for your suggestion. We tried to add the bias trends in table 1. However, there are too many numbers and hard to clearly show the important information. Therefore, we put the uncorrected and corrected trends in a Figure similar to Wang et al. 2012. We use the following figure instead of Table 1 in the revised manuscript. The impacts of biases on calculated trends are also discussed in the text.

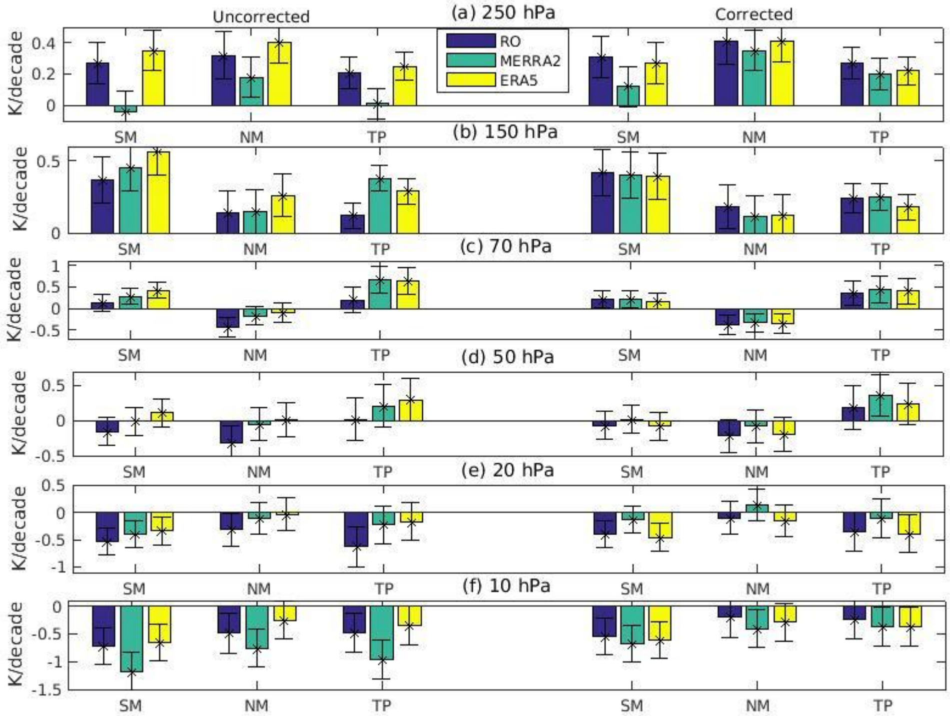

Figure 2: Estimated temperature trends in K/decade in different regions (SM: 25 °S-45 °S; NM: 25 °N-45 °N; TP: 10 °S-10 °N) from 2002 to 2017. (a-f) Trends in corrected and uncorrected data sets at 250, 150, 70, 50, 20 and 10 hPa. Error bars represent 95% confidence intervals.

5. The residuals and the anomalies of the multivariate regression (Figures 6 and 7) have same exact temporal structure and nearly the same magnitude. Do you know why? Can you directly compare and contrast your results with those of Randel and Wu (2014) who completed a detailed analysis using this method? It is concerning that the residuals have a magnitude that is roughly the same as the signal, suggesting the majority of the signal is unexplained (e.g. QBO and ENSO both have amplitudes of less than 0.05K at this height)

According to your suggestion, we have made a detailed analysis using the method in Randel and Wu (2014). Fig.2 shows the vertical profile of GNSS RO temperature variance in the deep tropics. The magnitude of annual cycle, QBO and ENSO related temperature anomalies shown in Fig. 2 is comparable to Randel and Wu (2014, Fig. 7). The residual at 150 hPa is much larger than the ENSO and QBO term at the same level. This explains the residuals and the anomalies of the multivariate regression have same temporal structure and nearly the same magnitude. At 70 hPa the QBO50 term is much larger than ENSO and QBO30 terms but still less than the residuals.

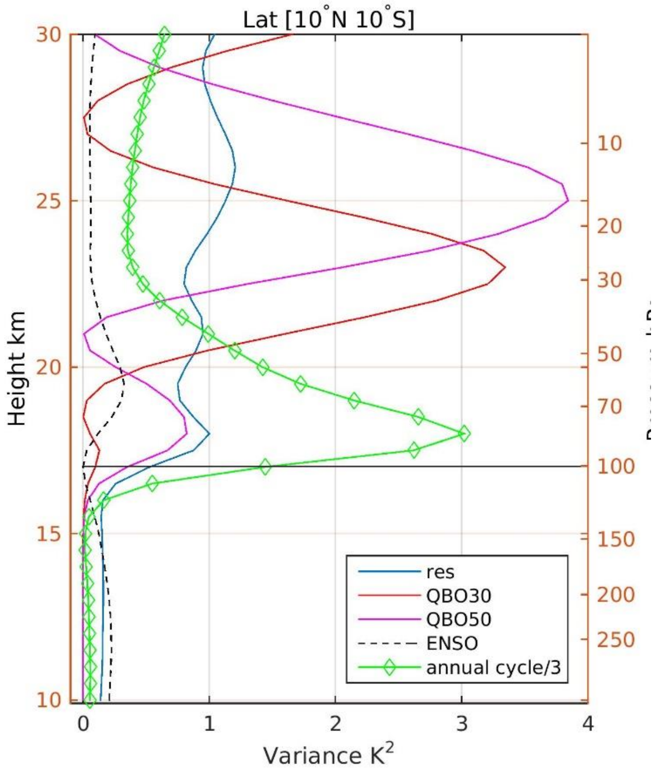

Figure 3: Vertical profile of GNSS RO temperature variance in the deep tropics (10°S-10°N) associated with annual cycle, QBO, ENSO, and residual variability. The variance for the annual cycle has been divided by three to fit within this scale. The horizontal line denotes the altitude of the time average lapse rate tropopause.

6. Another concern I have with this study is that the connections between ozone and temperature are very loosely made, and there are no analyses to support them. Calculations (such as changes in temperature structure through changes in ozone through either a climate model or radiative transfer model) have not been made, and not even a simple correlation analysis was performed. Many previous studies (e.g. Abalos et al. 2012, Maycock 2016, Gilford et al. 2016, to name just a few) have done detailed modeling, radiative calculations or statistical analyses, quantifying the relationship between temperature and ozone. Instead, this paper simply notes “In the stratosphere, ozone distribution is highly correlated with the temperature change” (pg. 14, line 3) without actually showing any such correlations, and discusses some loose connections between temperature and ozone in section 3.4. Furthermore, it claims we need to “await further investigation” (pg. 3, line 27), but extensive research on this topic has been done! There is very little acknowledgement of the vast literature which has discussed this topic in detail, and the results herein are not framed within that context. Its important to perform some analysis to show how this work is valuable and contributing to our knowledge of ozone/temperature

links (especially in the context of how this relationship changes between reanalyses and GPS).

We apologize for didn't clearly introduce results about the connection between ozone and temperature in previous studies. A correlation analysis was performed between temperature anomalies and ozone anomalies from 2005 to 2017 and the potential contribution of ozone changes to temperature trends was also estimated. Fig.3 show the correlation coefficient between ozone and temperature and the ozone contributions to temperature trends. In general, all strong positive correlation (>0.6) between ozone and temperature can be found from 100 to 20 hPa. The correlation coefficients of ozone/T are highest in tropics (~0.9). The correlation coefficient between SWOOSH ozone and GNSS RO temperature is highest in average. MERRA2 shows a similar correlation between ozone and temperature while the correlation in ERA5 is slightly weaker. While ozone and temperature are positively correlated, a decrease of ozone contributes to a cooling in the NH and in the tropical upper troposphere and mid stratosphere. Increases of ozone lead to a warming effect in the SH and the lower stratosphere in the tropics.

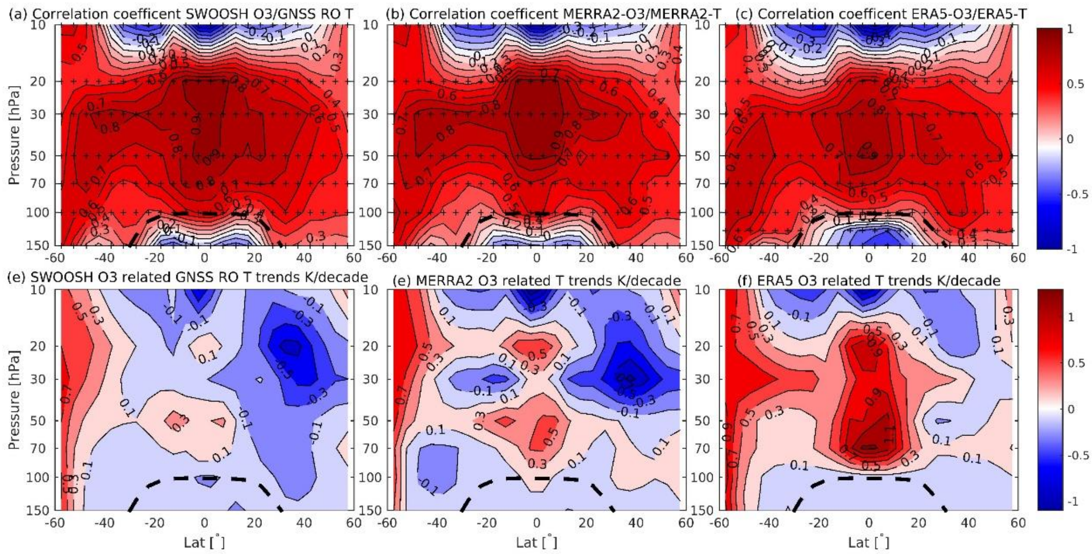

Figure 4: The correlation coefficients between SWOOSH ozone and GNSS RO temperature (a), MERRA2 ozone/T (b) and ERA5 ozone/T (c), which are calculated from monthly deseasonalized anomaly time series from 2005 to 2017. The '+' marked the significant values using a p-value 0.05 for testing the hypothesis of no correlation. (d) SWOOSH ozone regressed GNSS RO temperature trends in K/decade; (e) MERRA2 ozone regressed temperature trends in K/decade; (f) ERA5 ozone regressed temperature trends in K/decade.

7. My primary concern with this paper is that it does not successfully and clearly distinguishing itself as novel. The trend calculations (for instance for ozone, pg. 3, line 21) have been updated through 2016 in previous studies, so this paper represents a2-year improvement (and as noted above, the depth ozone research herein is not at a level commensurate with previous studies). Studies of UTLS

temperature variability from GPS measurements have been very robustly presented in previous works (e.g. Abalos et al. 2012, Randel and Wu 2014). The use of the model to explore these processes is not well explained in the text, or compared with recently published studies which have done this (e.g. Randel et al. 2017).

To address this, I recommend the authors realign their motivation, highlighting that they are primarily concerned with comparing reanalyses and GPS in the UTLS with ERA5, in accordance with the S-RIP. Improvements in the ozone analyses and trend bias estimations in the context of comparing reanalyses will further improve on this narrative. Furthermore, the model should be brought introduced earlier in the paper as part of the motivation. This study can and will be valuable, but you need the tell and show the readers in clear language!

Thank you very much for the constructive comments. We agree to the reviewer that the motivation and the novel findings of this manuscript was not clearly addressed. We have rewritten the Introduction to highlight that our primary concern is to compare reanalysis data (in particular the ERA5 data) with the GPS-RO as the reviewer suggested. Other potential improvements of this manuscript than previous studies, i.e. an update of the temperature trend in the UTLS, the relationship between ozone and temperature changes and the attribution by model simulations, are also reorganized and addressed clearly in the revised manuscript.

Figure Comments:

All Figures: Please include units in all of your figure captions and titles/axes (where relevant).
Thank you for your remarks. We have added units in all figures.
Figure 1: One of the ranges in the caption should be “SM” instead of “NM”. Also, it is not explained anywhere what is meant by SM and NM. Please add an explanation in the text of the manuscript.
Sorry for missing the information. The SM and NM indicate Southern hemisphere Mid-latitude and Northern hemisphere Mid-latitude, respectively. We have corrected the caption and added explanations in the revised manuscript.
Figures 4-5, 8-12, 14-15: Zonal mean figures would be improved if a line was added to indicate the climatological zonal mean tropopause height (using either the lapse rate tropopause or the cold-point tropopause, see Munchak and Pan 2014). These will likely vary from product to product and in the model, but it will help the read understand how your results vary with respect to the tropopause height.
Thank you for the suggestion. We have added the lapse rate tropopause in all figures.
Figures 1-3, 13: The x-axes on these timeseries plots are very hard to read because the years are all squished together.
Yes, we have renewed figures.
Figures 11-12, 14-15 (and timeseries plots): Readers who are green-red will find it

very difficult to parse the green “+” markers or green lines in these figures. Please use some other way or color contrast this data which is color-blind friendly.
Sorry, we have changed the green “+” markers to black.
Table 1: This is a key result in the entire paper, yet its unclear. What are the +/- values in this table, are they the confidence intervals from your t-test? If so, please indicate so. It’s also important that trends in the biases from GPS RO be included as a column at each level, for comparison.
The +/- values in this table are 95% confidence intervals for the coefficient estimates. We have added the explanation in the text. The trends of the biases data are added in the table2.

Line-By-Line Comments:

Pg. 1, line 1: This first sentence is confusing as written.
We have rewritten this sentence.
Pg. 1, line 2 and elsewhere: Replace “were” with “are”, and use present tense language throughout.
Thanks, we have checked carefully and updated the whole text.
Pg. 1, line 3+15: The first few sentences need to motivate the reader as to why your study is a valuable contribution and novel. I recommend mentioning the model here in addition to later, and be specific about what model you are using and in what mode.
Thank you for the kind suggestion. We have rewritten the sentences as suggested.
Pg. 1, line 13: replace “the change of” with “discontinuities in”
Corrected.
Pg. 1, line 16: The use of “could be” shows how the shallow the ozone and physically based analyses in this study are. Further analyses should allow you to be more definitive here.
Yes, we have changed it.
Pg. 2, line 1: It is not “the” key region, it is “a” key region. Coupling is also important at high latitudes (e.g. sudden stratospheric warmings).
Corrected.
Pg. 2, line 3: Do you mean that temperatures in the UTLS respond to climate change? That they affect other things (like water vapor) so they indirectly affect climate change? Please rewrite for clarity.
Yes, we have rewritten the sentence.
Pg. 2, lines 7-9: This sentence is confusing and should be rewritten.
Corrected.
Pg. 2, line 9: “through” should be “between”
Corrected.
Pg. 2, line 11: The term “underlying mechanisms” is used 4 times in this text without any clear explanation of what it means. Its use is vague and unspecific, please rewrite to clarify exactly what is meant when you say “underlying mechanisms”.

“Underlying mechanisms” mean any possible mechanism/process that may influence the UTLS temperature, such as dynamical processes associated with SST, radiative effects by GHGs and ozone. We have updated the description in the manuscript.

Pg. 2, line 11: You are talking about trends in this paragraph, but now you mention variability (which could be construed as interannual variability). important to keep them distinct throughout the paper, because they could be changing in different ways.

Thank you for your suggestion and we have deleted the word.

Pg. 2, line 24: This is very poorly written sentence, please rewrite for clarity.

Corrected.

Pg. 2, line 27: “Plenty” is a slang term and not professional. Please look throughout your manuscript and replace these slang terms with more specific ones (e.g. “On one hand”, pg. 3, line 4; “Same as”, pg. 6, line 24; etc.). Here I suggest: “assimilate ground-based, satellite-based, and other data sources to provide the current...”

Thank you for your suggestion and we have corrected them in the text.

Pg. 2, line 31: The use of “perform” here is not correct. “may exhibit” would work. Other times in this paper “perform” is also not used correctly (e.g. pg. 13, line 24); please rewrite each of these.

Corrected.

Pg. 3, lines 1-2: This sentence is poorly written and distorts the communication of your goal.

We have rephrased this sentence.

Pg. 3, line 9: While ozone changes could be a helpful indicator as you claim, you’ve barely touched on how complicated this is. Schoeberl et al. (2008) did a rather complete study of this, but others (e.g. Polvani and Solomon 2012) have shown that it has rich nuances. You skip over that richness in your literature review here. I think its worth noting the efforts those papers made, and how your work is different.

Thank you for your suggestion and we have added literatures in the manuscript and the sentences to explain our work.

Pg. 3, line 10: “various of” should be “various”

Corrected.

Pg. 3, line 17: Very confusing as written.

Corrected.

Pg. 3, line 19: 15 hPa is well above the UTLS region!

We have deleted the sentence.

Pg. 3, line 29: The sentence is confusing as written.

Corrected.

Pg. 3, line 34: This a very abrupt transition introducing the model. This needs to be done more smoothly and with better motivation as to why we are using the model.

Yes, we have added one sentence before introducing the model.

Pg. 4, lines 3-10: Much of this paragraph is repetive with previous ones and can be removed.

Done.

Pg. 4, line 10: What is meant by “dynamical processing with SST”?

It means atmospheric circulation changes associated with SST. We have updated this sentence in the revised manuscript.

We thank the reviewer for all the comments and suggestions on the Introduction. The Introduction has been rewritten completely with all of comments considered.
Pg. 4, line 17: Seven years is not one decade. This is also very confusing as written.
Yes, we have changed the sentences.
Pg. 4, line 22: Are these measurement errors? Or differences from some other instrument?
They are estimated uncertainty for climate monitoring using GNSS radio occultation data.
Pg. 4, line 34: Can you provide a magnitude estimate for this "low effect"?
References show that less than 0.2K and I have added it in the manuscript.
Pg. 5, line 14: Was this linear interpolation done on a pressure grid or a height grid?
The linear interpolation has been done with logarithm pressure.
Pg. 5, line 17: What is meant by comparable here?
It means "similar".
Pg. 5, line 25: add "to" before "which"
Corrected.
Pg.5 line 27: There's no transition between these paragraphs. Are you introducing a new dataset you will also use?
Yes, we have added a sentence for transition as follows:
"For better study the ozone variability, an independent data sets namely C3S SAGE-II/CCI/OMPS ozone products version 3 with 10 ° latitude bands are used."
Pg. 6, line 2: On what basis can you call this "a time period suitable for trend evaluation"?
Sorry for the vague description. What we want to say here is that the C3S covers the year 2002 and 2017, which can be directly compared with SWOOSH data. We have corrected this sentence.
Pg. 6, line 7: introduce this as version 3 in the very first sentence of this paragraph instead.
We have introduced the version of data in the first sentence.
Pg. 6, line 16: As written, this sentence is unreadable. I don't understand what it is trying to say.
We have rewritten this sentence as follows:
"The newest ERA5 reanalysis, which is released by ECMWF in 2018, is also used."
Pg. 6, line 20: The link doesn't work as written, and should be more carefully cited in the bibliography.
Corrected.
Pg. 7, line 10: Please rewrite this confusing sentence.
We have rewritten this sentence as follows:
"The differences between these two simulations help to estimate the contribution of SST changes to temperature and ozone trends."
Pg. 7, line 11: I recommend renaming this section "Trend Calculations"

Updated.
Pg. 7, line 15: “Phenomenons” should be “phenomena”
Corrected.
Pg. 7, line 20: You have “a4” twice, but no solar component in equation 1.
Corrected.
Pg. 7, line 25: Is this a one-sided or two-sided t-test? Also, is this significance level the p-value? Please clarify your method.
It is a two-sided t-test and the significance level is 95%. We have clarified it in the text.
Pg. 7, line 29: The 400hPa level is well below the tropopause, especially in the tropics.
Thank you for your remarks. We use the Figure of 250hPa instead of the 400hPa level in the revised manuscript.
Pg. 8 line 11: What do you mean by “more disturbed” here?
The annual cycle at 100 hPa has substantial variability, which is not as regular as the annual cycle in the troposphere.
Pg. 9, line 22: why does the shortness of the period change this result? The shorter period means that interannual variability should have more influence on the trend calculations.
Yes, we have added the sentences in the text.
Pg. 9, line 27: “getting less” should be “smaller”
Corrected.
Pg. 9, line 29: The sentence is very confusing as written.
This sentence has been rewritten as follows:
“By such a multiple linear regression, the influences of ENSO and QBO as well as the linear trend can be separated.”
Pg. 10, lines 4 and 12: What phase of ENSO or QBO? Please clarify throughout your paper what phase you mean each time you discuss results for QBO and ENSO.
Positive phase ENSO and westerly QBO. We have clarified the phase in the paper.
Pg. 10, line 17: This title isn’t worded correctly. I suggest “Temperature Trends”
Corrected.
Pg. 10, line 28: I don’t know what you mean by this sentence, you might be missing a word?
Corrected.
Pg. 10, line 31: “MEERA2” should be “MERRA2”.
Corrected.
Pg. 11, line 5: Which tropopause? The cold point? The tropopause is a transition layer in the tropics (Fueglistaler et al. 2009)
The lapse rate tropopause.
Pg. 11, line 17: what dynamic process do you mean? Do you mean the influences of SSTs on circulation? If so, please say so.
Yes, we have changed it.
Pg. 11, line 28: “so many” should be “as many”
Corrected.

Pg. 12, line 35: This is a nice physical discussion which is mired by very unclear writing.
We have rewritten the discussion.
Pg. 13, line 1: Can you cite this? Many papers have shown this result.
Yes, we have cited previous studies.
Pg. 13, line 3: "That is not the truth" is not professional; please rewrite.
Yes, we have rewritten it.
Pg. 13, line 5: There is no observational evidence for ozone recovery yet, outside the spring SH stratosphere (Randel et al. 2017).
We have rewritten the sentence.
Pg. 13, line 16: You haven't done any attribution work, so this claim should be removed.
Corrected.
Pg. 13, line 22-24: These lines are very confusing; I don't understand what you mean.
We have updated the sentence as follows:
"ERA5 shows obvious improvements of temperature data compared with ERA-I and also a slight better agreement with GNSS RO measurements than MERRA2."
Pg. 13, line 29: 15 years is not "nearly 2 decades".
Corrected.
Pg. 14, line 1: This is a run-on sentence, and its very hard to parse what your point is here. Please rewrite.
This sentence has be updated as follows:
"Again, ERA5 shows improved quality compared with ERA-I and has the best agreement with the GNSS RO data in the three reanalyses."
Pg. 14, lines 3: You have not shown this result.
Yes, we have added the content.
Pg. 14, line 5: This result isn't true for all datasets in your study, and you haven't clarified what period these trends are considered over in this discussion.
We have clarified the period in the discussion.
Pg. 14, line 14: Your results do not show this link, please don't make false claims without evidence. In fact, it has been shown previously to not be the case (Randel et al. 2017).
We have deleted it.
Pg. 14, line 17: Poorly written.
Corrected.

References

Wargan, K., Orbe, C., Pawson, S., Ziemke, J. R., Oman, L. D., Olsen, M. A., Coy, L., Knowland, K. E: Recent decline in extratropical lower stratospheric ozone attributed to circulation changes. Geophysical Research Letters, 45, 5166–5176, https://doi.org/10.1029/2018GL077406, 2018

Wigley, T.: Appendix A: Statistical issues regarding trends, in: Temperature Trends in the Lower Atmosphere: Steps for Understanding and Reconciling Differences, edited by: Karl, T. R., Hassol, S. J., Miller, C. D., and Murray, W. L., A Report by Climate Change Science Program and the Subcommittee on Global Change Research, Washington, DC, USA, UNT Digital Library, 129–139, 2006.