

Interactive comment on “Variability of temperature and ozone in the upper troposphere and lower stratosphere from multi-satellite observations and reanalysis data” by Ming Shangguan et al.

Anonymous Referee #2

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Summary and Scientific Contribution:

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 Niño 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

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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.

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.

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

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shift the main focus of their paper to the comparisons between the variabilities in the reanalysis and GPS products.

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.

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, 2, and 3. 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. Table 1 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).

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Where the bias trend is on the order of the product temperature trends, the robustness of those trends should be reconsidered.

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).

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).

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

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21) have been updated through 2016 in previous studies, so this paper represents a 2-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 to tell and show the readers in clear language!

Figure Comments:

All Figures: Please include units in all of your figure captions and titles/axes (where relevant).

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.

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 reader understand how your results vary with respect to the tropopause height.

Figures 1-3, 13: The x-axes on these timeseries plots are very hard to read because

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the years are all squished together.

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.

Table 1: This is a key result in the entire paper, yet it's 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.

Line-By-Line Comments:

Pg. 1, line 1: This first sentence is confusing as written.

Pg. 1, line 2 and elsewhere: Replace "were" with "are", and use present tense language throughout.

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.

Pg. 1, line 13: replace "the change of" with "discontinuities in"

Pg. 1, line 16: The use of "could be" shows how shallow the ozone and physically-based analyses in this study are. Further analyses should allow you to be more definitive here.

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).

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.

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Pg. 2, lines 7-9: This sentence is confusing and should be rewritten.

Pg. 2, line 9: "through" should be "between"

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".

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.

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

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..."

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.

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

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.

Pg. 3, line 10: "various of" should be "various"

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

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

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

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.

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

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

Pg. 4, line 17: Seven years is not one decade. This is also very confusing as written.

Pg. 4, line 22: Are these measurement errors? Or differences from some other instrument?

Pg. 4, line 34: Can you provide a magnitude estimate for this "low effect"?

Pg. 5, line 14: Was this linear interpolation done on a pressure grid or a height grid?

Pg. 5, line 17: What is meant by comparable here?

Pg. 5, line 25: add "to" before "which"

Pg. line 27: There's no transition between these paragraphs. Are you introducing a new dataset you will also use?

Pg. 6, line 2: On what basis can you call this "a time period suitable for trend evaluation"?

Pg. 6, line 7: introduce this as version 3 in the very first sentence of this paragraph instead.

Pg. 6, line 16: As written, this sentence is unreadable. I don't understand what it is trying to say.

Pg. 6, line 20: The link doesn't work as written, and should be more carefully cited in

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the bibliography.

Pg. 7, line 10: Please rewrite this confusing sentence.

Pg. 7, line 11: I recommend renaming this section "Trend Calculations"

Pg. 7, line 15: "Phenomenons" should be "phenomena"

Pg. 7, line 20: You have "a4" twice, but no solar component in equation 1.

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.

Pg. 7, line 29: The 400hPa level is well below the tropopause, especially in the tropics.

Pg. 8 line 11: What do you mean by "more disturbed" here?

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.

Pg. 9, line 27: "getting less" should be "smaller"

Pg. 9, line 29: The sentence is very confusing as written.

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.

Pg. 10, line 17: This title isn't worded correctly. I suggest "Temperature Trends"

Pg. 10, line 28: I don't know what you mean by this sentence, you might be missing a word?

Pg. 10, line 31: "MEERA2" should be "MERRA2".

Pg. 11, line 5: Which tropopause? The cold point? The tropopause is a transition layer in the tropics (Fueglistaler et al. 2009).

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Pg. 11, line 17: what dynamic process do you mean? Do you mean the influences of SSTs on circulation? If so, please say so.

Pg. 11, line 28: "so many" should be "as many"

Pg. 12, line 35: This is a nice physical discussion which is mired by very unclear writing.

Pg. 13, line 1: Can you cite this? Many papers have shown this result.

Pg. 13, line 3: "That is not the truth" is not professional; please rewrite.

Pg. 13, line 5: There is no observational evidence for ozone recovery yet, outside the spring SH stratosphere (Randel et al. 2017).

Pg. 13, line 16: You haven't done any attribution work, so this claim should be removed.

Pg. 13, line 22-24: These lines are very confusing; I don't understand what you mean.

Pg. 13, line 29: 15 years is not "nearly 2 decades".

Pg. 14, line 1: This is a run-on sentence, and its very hard to parse what your point is here. Please rewrite.

Pg. 14, lines 3: You have not shown this result.

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.

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).

Pg. 14, line 17: Poorly written.

References:

Abalos et al. (2012): Variability in upwelling across the tropical tropopause and corre-

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lations with tracers in the lower stratosphere. *Atmos. Chem. Phys.*, 12, 11505–11517, www.atmos-chem-phys.net/12/11505/2012/ doi:10.5194/acp-12-11505-2012

Bandoro et al. (2018): Detectability of the impacts of ozone-depleting substances and greenhouse gases upon stratospheric ozone accounting for nonlinearities in historical forcings. *Atmos. Chem. Phys.*, 18, 143–166, <https://doi.org/10.5194/acp-18-143-2018>.

Fueglistaler, S., A. E. Dessler, T. J. Dunkerton, I. Folkins, Q. Fu, and P. W. Mote, 2009: Tropical tropopause layer. *Rev. Geophys.*, 47, RG1004, doi:10.1029/2008RG000267.

Gilford, D. M., S. Solomon, and R. W. Portmann, 2016: Radiative impacts of the 2011 abrupt drops in water vapor and ozone in the tropical tropopause layer. *J. Climate*, 29, 595–612, doi:10.1175/JCLI-D-15-0167.1.

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Maycock, A. C. (2016), The contribution of ozone to future stratospheric temperature trends, *Geophys. Res. Lett.*, 43, doi:10.1002/2016GL068511.

Munchak, L. A., and L. L. Pan (2014), Separation of the lapse rate and the cold point tropopauses in the tropics and the resulting impact on cloud top-tropopause relationships, *J. Geophys. Res. Atmos.*, 119, 7963–7978, doi:10.1002/2013JD021189.

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Randel, W. J., Polvani, L., Wu, F., Kinnison, D. E., Zou, C.-Z., & Mears, C. (2017).

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Troposphere-stratosphere temperature trends derived from satellite data compared with ensemble simulations from WACCM. *Journal of Geophysical Research: Atmospheres*, 122. <https://doi.org/10.1002/2017JD027158>

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