

Interactive comment on “The Tropical Tropopause Layer 1960–2100” by A. Gettelman et al.

Anonymous Referee #4

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The paper examines the climatology and long-term changes in aspects of the tropical tropopause layer (TTL) as depicted in two reanalyses and in 13 chemistry climate models (CCM). The scope of the paper is very large, including: an attempt to determine whether historical simulations represent observed conditions for 1960-present, introduction of several new parameters to describe the tropical tropopause layer and its horizontal extent, an analysis of projected future changes, and a discussion of model characteristics and potential shortcomings. Unfortunately, the authors have not successfully achieved any of these goals, probably because of the overly ambitious agenda they have set. None of these topics is addressed in much depth, although we are presented with lots of detailed figures. In the end, it is difficult to determine, which results, if any, are significant or valid, for the reasons outlined below.

General Comments

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1. My main complaint is that the authors have not demonstrated conclusively that the 13 models yield credible simulations of the TTL for the late 20th century. The comparison with reanalyses is weak because of the disparity between the two reanalyses (see e.g., Table 1, Fig 4, Fig 6) and the general concern that reanalyses are flawed in this region of the atmosphere and are not well suited for estimating multi-decadal trends. There are other available datasets and relevant published literature based on radiosondes and GPS-RO profiles, some of which the authors mention in passing and some of which are ignored. A full analysis of the quality of the TTL representation in the CCMs would be welcome and would be enough to tackle in one paper.

2. In this light, it is difficult to ascertain whether the 21st century CCM projections deserve our attention. If they do, then it would be helpful to first determine which models are most likely to give realistic projections, based on their simulations of the observed past. Looking at the multi-model ensemble average does not make sense if there are some poor models in the mix. The idea that the ensemble agrees better for some variables (e.g., cold point temperature) than for others (tropical width) is disconcerting and reinforces the need to identify the better models. I don't fully accept the authors' idea (p 1373, lines 15-17) that consistency of trends for a given diagnostic variable among models should give us more confidence in those trends than in trends for other variables.

3. Previous investigators have defined several metrics of the tropical tropopause and TTL. Here two new metrics are introduced: the TTL edge and the zero lapse rate level. Having re-read section 2.1 several times, I still don't quite understand the definition of the TTL edge - "where the LRT pressure is less than the mean tropical LRTP + Δp " - in part because the authors don't say how the mean LRTP is computed (over what space and time domains), in part because the "less than" criterion should logically identify a whole region, not a single location, and in part because the 35 hPa value of Δp seems arbitrary. The reference (page 1377 lines 21-22) to this definition as the "meridional gradient of LRTP" does not seem consistent with the original definition.

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There have been a number of recent studies on the width of the tropical belt that use other measures of tropical width, and no attempt has been made to compare this definition to those, or even to cite much of the other work. Again, this is topic could be the subject of a paper unto itself. Furthermore, the "zero lapse rate level" is another confusing concept. It seems to be simply an inflection point or an isothermal layer in the temperature sounding, so either a cold point or (less likely) a warm point. What is the value of introducing this new variable?

4. Finally, the paper could be a lot more clearly and succinctly written. There are many grammatical problems which made me scratch my head to try to figure out the authors' intent.

Specific Comments

5. Symbols used for variables are confusing to the reader and are sometimes confused by the authors. For example, the sentence "LRTP trends are smaller than ZLRP trends" on line 3 page 1379 is very confusing because Z is zero, not height, T is tropopause not temperature. I strongly suggest using the conventional italicized main symbols for variables (e.g., T for temperature, p for pressure, Z for height) and subscripts for the locations (e.g. CPT for cold point tropopause, LRT for lapse rate tropopause...). Table 1 is well-intentioned but would not be needed if conventional symbols were adopted.

6. Please be clear in selecting verb tenses and use past tense when discussing the historical data and future tense for the projections. Otherwise, the reader can be confused, e.g., in the Abstract (p 1368, lines 10-12) and on page 1380, line 13.

7. Please define "level of main convective outflow" (Intro. p 1368, lines 23-24). Is this a conceptual level, or can it be defined precisely?

8. P 1370, line 15 "changes to sub-grid scale processes". It's unclear whether model tests were made varying the parameterizations of some processes, or whether CMAM and WACCM just use different parameterizations, in which case "changes" is not the

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right word.

9. First and second paragraphs of section 2.1 are redundant.

10. I'm not sure that trend calculation methods for multi-model ensembles described in the last paragraph of Section 2.2 are conventional or correct. Averaging the slopes and their uncertainty estimates doesn't seem the right way to estimate the slope of the ensemble mean and its uncertainty. Also, what is meant by "Multiple regressions, using other climate forcings" (p 1374, line 11)? What forcing has already been included in the linear regression, which is just a fit through the time series?

11. The Son et al. (2008) reference is not included and I can't find footnote 1.

12. It's unclear what "midtropospheric gradients are small" in line 1375, line 14. Vertical gradients in ozone, longitudinal gradients, ...? The paragraph doesn't make sense to me. What is the "linear correlation" in line 21 of the same page? A spatial pattern correlation, time series correlation? And what is meant by "double interpolation" later in the same paragraph?

13. Section 3, paragraph 5, "Difference between 3-D ... water vapor noted by Eyring et al. (2006)." I could not parse this sentence.

14. Table 3: The text (first paragraph of Section 4.2) says that trends are calculated for "1980 to the end of the run (mostly 2050-2100)", but I don't see such a column in the table. I'm also not comfortable with mixing periods that differ by 50 years.

15. Page 1384, line 26. Since when is it acceptable to start a sentence with a numeral?

16. Page 1384, line 20. What is the correlation in question? Ozone and temperature, but are these spatial patterns, temporal changes, model-to-model differences, ...?

17. Page 1385, line 25 "A back of the envelope analysis indicates" Isn't the reader even entitled to know what the analysis was? This allusion to some unexplained quick and easy calculation reveals, probably unintentionally, a little contempt for the reader.

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18. Page 1386, line 7 "In reality ..." Although this introductory phrase seems off-hand, it is really a strong statement and suggests a level of certainty that may or may not be warranted.

19. The first two paragraphs of the Conclusions are inconsistent. Either the models do or they do not well represent historically observed TTL structure, including time changes.

20. Fig. 1. Can you include "error bars" showing the variability about the zonal mean?

21. Most figures could benefit from use of a larger font for the axis and tick mark labels.

Technical Corrections

No technical corrections are suggested, because the more serious problems mentioned above should be addressed first.

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