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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temperature variability and trend estimates at tropopause and UT-LS over a subtropical site: Reunion (20.8° S, 55.5° E)" *by* N. Bègue et al.

Anonymous Referee #1

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Overall Impressions:

This paper describes a regression analysis on the temperature time series from Reunion Island in the upper troposphere and lower stratosphere and at two definitions of the tropopause, the cold point and lapse rate tropopauses. SHADOZ sondes, once weekly, are used in the analysis, with data starting in the early 1990s. Regression terms include annual and semiannual cycles, QBO, ENSO and solar cycles and a trend term. The paper also looks at variability related to the Indian Ocean Dipole (IOD). The annual cycle is found to be the dominant term, explaining more than a quarter of the variability. The Indian Ocean SST related term, the IOD, is found to explain more than

10% of the variance of the cold point and lower stratospheric temperature. A cooling trend of approximately 1 degree per decade is found in the lower stratosphere.

There are many grammar mistakes in this paper that make it difficult to read. Before it can be published, it needs to be thoroughly edited by a native English speaker. Because the needed corrections are so numerous, other than for the title, I have not made any additional grammatical correction suggestions.

The data set used has not been heavily analyzed, and does show an interesting correlation with Indian Ocean SSTs; hence this paper should eventually be published. However, it needs major revisions. Scientifically, it could use some additional discussion and speculation as to mechanisms for the SST/UTLS temperature relationship.

Specific Comments

An editorial suggestion for the title: Temperature variability and trends in the UT-LS over a subtropical site: Reunion (20.8S, 55.5E) (Given that the tropopause is part of the UTLS, it doesn't need to be specifically called out.)

Pg 10115: Paper states that the "annual variation" (I assume the authors mean in tropical tropopause temperature) "is a fairly direct response to the annual variation in temperature of tropical surface insolation" That is not a currently accepted statement. The Holton et al. 1995 review article (referenced already in this manuscript) gives an explanation. The annual temperature variation is believed to be related to the annual cycle in total wave driving at higher latitudes, with the BD circulation stronger in NH winter than in NH summer, producing colder temperatures at the tropical tropopause in NH winter.

Page 10116, line 19: I think largest should be changed to longest.

Figure 1: This figure isn't really needed, as the text adequately conveys the information.

Page 10117, first half: It would be worthwhile explaining what the Indian Ocean Dipole actually is.

Page 10119, line 16: I'm not sure calling the tropopause a dynamical barrier/filter to vertical motions is really correct. It doesn't act as a filter, and there is transport through the tropopause, so it's not a barrier either.

Page 10119: I'd recommend changing "forcings that govern" to "parameters that explain" in line 25. I also recommend rethinking the use of the term "forcing" on the next page as well, what you're describing are "regression paramters", not forcings in regards to the equations of motion.

Page 10120: Equation 1 does not have a trend term. Is the fit done first with the cyclic parameters, and then the trend fitted to the residual? And why use the 40-hPa winds for the QBO index? Since you're looking at time series closer to 100 hPa, why not use a 70-hPa wind or winds at a level closer to 100 hPa?

Page 10120, line 8: what is the difference between n annual and semi-annual cycles and the annual and semi-annual oscillations? Are you really using some measure of the phase and amplitude of the tropical SAO (seen in ozone and temperature in the upper stratosphere), or are you just regressing against a cycle with a period of 6 months?

Figure 3: Please define in the text and in the figure caption what levels UT and LS actually are (average over a pressure range, altitude range, distance from tropopause, however it was estimated). This is stated in Table 2, but it needs to be in the text and Figure 3 caption as well.

Page 10123, lines 14-28: Please rewrite so it doesn't sound like the annual cycle and semi annual cycles are forcing terms. Instead, they are modes of variability. And refer to them as cycles rather than oscillations. I don't think you're referring to an oscillation with a restoring force that propagates, but simply a periodicity. The SST related parameters (ENSO and IOD) are the terms that can be considered a forcing.

Page 10124, last paragraph: The description of calculating anomalies is confusing.

You can change it to say something on the order of: Anomalies are derived on a monthly mean basis by subtracting the long-term average temperatures for each calendar month of the year (shown in Figure 3) from the actual monthly mean measured values.

Page 10127, last paragraph: Can you speculate as to why only the LS trend is significant whether or not the time period heavily influenced by Pinatubo aerosols was included? My guess is it has something to do with LS temperatures being more strongly controlled by CO2 loading that the UT.

General comment on aerosol impacts: Instead of just deleting the years immediately following Pinatubo from the trend analysis, can you test the sensitivity to including an aerosol index in the regression, perhaps based on data from SAGE?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 10113, 2010.