Response to anonymous referee 1's comments

First of all, the authors acknowledge the editor and referee for their constructive comments and suggestions. The revised manuscript has been read by a native English speaker, in order to correct grammatical errors and awkward sentences.

Comment 1

Referee 1:

An editorial suggestion for the title: Temperature variability and trends in the UT-LS over a subtropical site: Reunion (20.8S, 55.5E).

Authors:

We agree with the editor. Now the title has been rewritten.

Comment 2

Referee 1:

Paper states that the "annual variation is a fairly direct response to the annual variation in the temperature of tropical surface insolation". That is not a currently accepted statement. The Holton et al. (1995) review article 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.

Authors:

We thank for the reviewer concern. We have removed the sentence in order to avoid the misunderstanding.

Comment 3

Referee 1:

I think largest should be changed to longest.

Authors:

It is being corrected as suggested.

Comment 4

Referee 1:

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

Authors:

Now, we have removed the figure as suggested by the referee. However, the number of observation is mentioned in the text wherever it requires.

Comment 5

Referee 1:

It would be worthwhile explaining what the Indian Ocean Dipole actually is.

Authors:

We have included few more references, to improve the explanation of the Indian Ocean Dipole (see page 4 of the revised manuscript). Indeed we have added the results from recent studies about IOD, particularly the studies reported by Izumo et al. (2010)[1] and Morioka et al. (2010)[2]. These two recent studies show that the IOD is now accepted as major climate mode in the southern Indian Ocean.

Comment 6

Referee 1:

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

Authors:

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[1] Izumo et al: Influence of the state of the Indian Ocean Dipole on the following year's El Niño, *Nature Geoscience*, 10.1038/NGEO760, 2010

[2] Morioka et al: Climate variability in the southern Indian Ocean as revealed by self-organizing maps, Climate Dynamics, doi: 10.1007/s00382-010-0843-x, 2010.

We agree with the reviewer. To avoid the readers misunderstanding, we have removed the sentence in the revised manuscript.

Comment 7

Referee 1:

I'd recommend changing "forcing that govern" to "parameter 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 parameters", not forcings in regards to the equations of motion.

Authors:

We have modified the term appropriately as suggested by the reviewer (including the comment that follows on ENSO and IOD).

Comment 8

Referee 1:

Equation 1 does not have a trend term. Is the fit done first with the cyclic parameters, and then 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?

Authors:

We agree with the reviewer. The equation does not have any trend terms. The regression is being performed to understand/explain all kinds of variations which influence the temperature.

We have followed the statement by Naujokat et al.[3] (1986) and Politowicz et al. (1997)[4] , saying that the maximum QBO forcing occurs generally at 20hPa. Furthermore, Naujokat et al. (1986) showed from wind data that the 50 hPa is the best level for correlation between QBO and tropopause. Since our objective is to illustrate for both upper troposphere and lower stratosphere, we have chosen the 40 hPa level, in order to obtain a significant QBO signal

[3] Naujokat, B : An update of the observed quasi-biennale oscillation of the stratospheric wind over the tropic, *J. Atmosp. Sci*, 43,1873-1877, 1986.

[4] Politowitcz, P.A and Hitchman, M.H : Exploring the effect of forcing quasi-biennal oscillations in a two model, *J.Geophys. Res*., 102, 16, 481-16, 497, 1997.

and, at the same time, to be close enough to the tropopause. This was added in the revised manuscript. (See page 7 line 10 in the revised manuscript)

Comment 9

Referee 1:

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), ore you just regressing against a cycle with a period of 6 months?

Authors:

For the regression analysis we have used regressed the cycle with a period of 6 months.

Comment 10

Referee 1:

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 in the figure 3 caption as well.

Authors:

It is now defined in the text and in the figure as suggested by the reviewer. The UT stands for the height region from 14 km to 15 km and LS stands for 18 km to 19 km. The figure depicts the monthly climatological temperature and height values and the corresponding standard deviation. (See page 25 of the revised manuscript)

Comment 11

Referee 1:

Please rewrite so it doesn't sound like the annual cycle and semi-annual cycle 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 terms that can be considered a forcing.

Authors:

We have rewritten the sentences in order to depict the annual and semi-annual as cycles rather than oscillations. As a consequence, we have rewritten the term throughout the manuscript. Further, we have changed SAO and AO (respectively Semi-Annual Oscillation and Annual Oscillation) into SAC and AC (respectively Semi-Annual Cycle and Annual Cycle).

Comment 12

Referee 1:

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 temperature for each calendar month of the year (shown in Figure 3) from the actual monthly mean measured values.

Authors:

We thank for the reviewer suggestion. It is now rewritten as suggested. (See page 11 line 13 in the revised manuscript)

Comment 13

Referee 1:

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.

Authors:

We have focused particularly on the effects due to ozone and CO2 changes in the stratosphere. Moreover, Langematz et al. (2003)[5] have shown that the additional CO2 increase has a minor effect in the lower stratosphere below 20 km. Furthermore, they suggested that other effects than ozone and CO2 changes have to be considered, in order to explain the observed changes in the lower stratosphere. Due to the above reasons, we have not deliberated. (See page 13 line 21 of the revised manuscript).

[5] Langematz et al : Thermal and dynamical changes of the stratosphere since 1979 and their link to ozone and CO2 changes, J. Geosphys. Res., 108 (D21), 4027, doi:10.1029/2002JD002069, 2003

Comment 14

Referee 1:

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?

Authors:

We thank for the reviewer's valuable suggestion. At the present stage, we focus on the UT/LS temperature variations in the Indian Ocean region. We have followed the analysis by neglecting the years immediately following Pinatubo, as mentioned by Bencherif et al. (2006)[6], rather to include an aerosol index in the regression model. However, including an aerosol index in the regression model is a good suggestion in order to improve the evaluation of the impact of aerosol on the temperature in the UTLS. Indeed there are different methods to parameterize the aerosol in the model: by including an aerosol index based on data from SAGE, as suggested by the reviewer, or by making use of an aerosol monthly means backscatter ratio from continuous lidar observations (Pastel et al., 2007)[7], or by use of an analytic function (Bodeker et al., 1998)[8]. Such work requires, in itself, a specific study. In a future work, we shall address and compare several aerosol indexes in order to estimate temperature trend without deleting post Pinatubo data from the trend analysis.

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[6] Bencherif H., Diab, R.,. Portafaix, T., Morel, B., Keckhut, P., and Moorgawa,A.: Temperature climatology and trend estimates in the UTLS region as observed over a southern subtropical site, Durban, South Africa, Atmos. Chem. Phys., 6, 5121-5128, 2006.

[7] Pastel, M., Goutail, F., Pazmiño, A., Pommereau, J.P and Held, G., Proceedings of Reunion Island International symposium, 143-146, 2007.

[8] Bodeker et al : Trends and variability in vertical ozone and temperature profiles measured by ozondes at Lauder, New Zealand : 1986-1996, J. Geosphys. Res., 103, D22, 28, 661-28, 681, 1998.