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Interactive comment on "Impact of deep convection on the tropical tropopause layer composition in Equatorial Brazil" *by* V. Marécal et al.

Anonymous Referee #2

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The paper is reporting on CO, ozone and temperature profiles measurements by an in situ balloon instrument flown in Teresina in Equatorial Brazil in June in both 2005 and 2008. The CO concentration higher by 20 ppb on average in 2005 compared to 2008 is attributed to a larger biomass burning in 2005, and the two CO relative maxima (below the cold point tropopause) are interpreted as a signature of vertical convective lifting of CO rich air from biomass burning 2-3 days upwind. Though of lower CO concentration on average in 2008, the presence of a one km thick layer of concentration similar to 2005 between 17-18 km (above the CpT at 16.9 km) is suggested to be due to a layer of well-mixed air due to the breaking of vertically propagating gravity waves associated to a nearby convective system.

Major Comments

The main difficulty I have with this paper is the attribution of ozone and CO variations in the TTL to local events only. CO and moreover O3 are relatively long-lived species thus sensitive to changes in the TTL at the whole equatorial scale and not only to the, relatively weak, convective activity next to the balloon flight. As shown by MLS profiles (Schoeberl et al. 2006), June 2005 was a period of fast drop of the full zonal mean CO concentration in the TTL between $\pm$ 12° latitude after the end of the convective season. In addition, June 2005 and 2008 were right during the transition period of the QBO phases but in the opposite direction: the wind between 50-100 hPa was moving from the West to the East (Atlantic, Africa) in June 2005 and from the East to the West (Amazonia) in June 2008. As an example, the larger CO concentration between 17-18 km in 2008 in an East wind layer between two West wind regions above and below (Fig 9) could be easily attributed to horizontal advection. The same applies to the drop of CO at 19 km coincident with a fast increase of ozone suggesting an advection of stratospheric air from the South, curiously ignored in the paper (and said to be treated in a further paper!). There is almost no information on horizontal transport, except three days backward trajectories at two levels only in 2005, and a single wind profile in 2008. This is totally insufficient. More information on meteorology in the troposphere and the stratosphere, QBO, wind profiles... is required if one's wants the reader to accept the conclusions of the paper.

Impact of biomass burning on CO concentration. The largest CO at almost all levels up to 22 km in 2005 compared to 2008 is attributed a) to a larger number of biomass fires in Central Brazil during the 10 days prior to the measurements and b) to a larger convective activity (mainly the nearby ITCZ over the Atlantic). The ENSO was in an El Nino phase in 2005 and in La Nina in 2008, which seems to be ignored. Usually rainfall stops burning. Here it's not the case because burning is located in a dry area in central Brazil 2000 km south of the convective region. How is CO transported to the North? Nothing is said. How CO could be lofted within 10-20 days in the lower stratosphere

(50 ppb at 18 km)? No explanation.

Stratosphere penetration by overshooting cloud turrets. P16163 and in the abstract it is said that this hypothesis first proposed by Danielsen (1993) can be rejected since no ice particles were detected during the Spirale flight. Danielsen's hypothesis was the opposite: the dehydration of the lower stratosphere following the cooling of air by adiabatic lofting. If the authors are making reference to the several observations of ice crystals up to 19 km above active convective systems, they should be aware that these crystals are evaporating within less than half an hour. The rejection of a possible signature of overshooting turrets would require water vapour measurements. In their absence, it is not possible to conclude like this.

Top TTL characterisation. The top TTL is said to be located around 17.8-18 km based on methane profiles not shown (where, when and from which instrument?). I think they should be shown. Indeed, looking at Fig 4 and 9, this conclusion is in contradiction with CO profiles where the MR is still 50 ppb at around 18 km. If the TTL is defined as the layer under influence of both the troposphere and the stratosphere, CO is a good indicator of tropospheric polluted air. Following this definition, the top TTL should be around 19 km in 2008 and even higher in 2005. Note that Schoeberl et al (2006) are seeing CO enhanced layers rising up to 19-20 km at a vertical speed of few weeks maximum, incompatible with the hypothesis of slow ascent by radiative heating which will take 9 months to rise from 14 to 20 km.

Other comments P 16151. The reason for choosing June for the balloon flight in Teresina is very strange, and form what I heard, not this one at all. P 16154. Location of Teresina. Where are the crosses in Fig 1 and 3. There is no indication on its longitude in the paper. Fig1. Left panels. I suggest adding an IR image of brightness temperature to see what kind of convection was present on the day of flight. Figure 1 (right panels) and Fig 3 on same lat/long scale if possible, showing where is biomass burning compared to convection. Figure 4. Wind profiles (direction and speed) in both years would be helpful. Figure 5. Very strange to see the fast CO concentration de-

creasing above the top TTL. Compatible with CO lifetime? Figs 6 and 7. Trajectories at two levels only in 2005 only. Trajectories at several levels in the troposphere and the stratosphere on a single plot fro each year would be more informative. Why the model is amplifying rainfall compared to TRMM. Again brightness temperature would be more, helpful. Rainfall does no necessarily require deep convection. Fig 8. Map of pressure or wind at 200 hPa could help understanding from where is coming CO at that level. Fig 9. Left and right panels at different vertical scale. Should be homogenised.

In conclusion, as it stands today the paper is not acceptable. If the authors want to demonstrate that local convection and biomass burning are responsible for the change in CO concentration at all altitudes up the lower stratosphere between 2005 and 2008 and for the various CO enhanced or depleted CO and the transition between wet, and dry season is the right time to study this, far more information is required on meteorology, long range transport etc. meaning very deep revision of the paper.

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