

## ***Interactive comment on “The CO<sub>2</sub> tracer clock for the Tropical Tropopause Layer” by S. Park et al.***

### **Anonymous Referee #2**

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Variations in CO<sub>2</sub> are difficult to interpret. It has a positive long term trend, a seasonal cycle, an interhemispheric gradient, a strong diurnal variability in the continental boundary layer, a source from biomass burning, etc. Though limited at times by the number of flights, I think this paper is the first to establish the usefulness of CO<sub>2</sub> as a tracer for interpreting air mass origins in the upper tropical troposphere, and for helping determine mixing and dynamical timescales in the TTL. I therefore recommend publication. I do have some comments, listed below. I would categorize most of these as being in the minor revisions category, but would encourage the authors to take them into consideration.

1. page 2, Introduction. "The lower boundary is generally defined as the level of minimum potential temperature lapse rate, indicating the level where vertical velocity driven by clear sky radiation is larger than upward convective mass transport ( ~ 10-12 km altitude, ..)". In most of the tropics, clear sky radiative subsidence balances the net

upward convective mass flux (updrafts + downdrafts). This is not true, however, above the Level of Zero radiative Heating (LZH) near 15 km, where both fluxes are likely upward and their sum is presumably approximately equal to the B-D mass flux. In the 10-12 km range, the clear sky mass flux is certainly downward, and I don't see how it could exceed the net convective mass flux - where would this sinking mass be coming from if not from convection? I am guessing that the physical origin of this statement is that the clear sky mass flux equals the heating rate divided by the static stability. Near the lapse rate minimum, where the stability is small, the rate of clear sky subsidence would be expected to increase, but not enough to exceed the convective mass flux. I think this confusion is reflective of a broader fuzziness in the literature of how to define the base of the TTL. I see the LZH definition as being preferable, but even if one adopts the lapse rate (LR) definition, it should be motivated more clearly. I am uncomfortable with the LR definition, since I don't think it denotes the onset of anything peculiarly stratospheric. Actually, I also see much of tracer and convective influence evidence in this paper as supporting the primacy of the 360 K ( $\sim$  LZH) level.

2. A General comment. Some of the conclusions in the paper are established by recourse to individual profiles, rather than calculation of mean profiles, or their variance. This applies, I think, to the comments near the bottom of page 5 on the latitudinal variation of the strength of STE, the argument that the TTL has two layers, the comments on page 9 on the emergence of a global CO<sub>2</sub> signal above 360 K (which appears to be compromised by the lack of TWP measurements above 360 K). Some of these observationally motivated conclusions are strengthened by the models, and I realize that there simply may not be enough CO<sub>2</sub> measurements at this point to do more than has been done.

3. page 12. It is my understanding that the calculated upwelling velocities refer to NH winter only. This should be stated more clearly.

4. According to my tropical mean PT-Height climatologies: 12 - 14 km corresponds to 346 - 353 K rather than 350 - 360 K. 14 - 18 km corresponds to 353 - 408 K rather than

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360 - 390 K.

5. page 6, bottom. Is the air with high condensed water really "outside" the convective cloud? Or were these falling ice crystals in a subsaturated environment?

6. Bill Randel has submitted a paper to JAS, now accepted, entitled Randel, W.J., M. Park, F. Wu and N. Livesey, 2007: A large annual cycle in ozone above the tropical tropopause linked to the Brewer-Dobson circulation. Although the conclusions of this paper on the Upper TTL are broadly consistent with those given here, Figure 6 from Randel's paper does also suggest a velocity of 0.5 mm/s at 17.5 km during NH winter. It may be useful to reference this paper as well in this and other contexts.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6655, 2007.

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