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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Isentropic advection and convective lifting of water vapor in the UT– LS as observed over Brazil (22° S) in February 2004 by in situ high-resolution measurements of H_2O, CH_4, O_3 and temperature" *by* G. Durry et al.

Anonymous Referee #3

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General Comments

This manuscript presents in situ measurements of H2O, CH4, O3, and temperature from two balloon flights over Brazil at 22S in February 2004, which provide a high resolution view of the composition of the UT and LS. As the authors discuss, these measurements show some interesting fine-scale structure in both regions, that can be interpreted in terms of different transport processes. The analysis of these measurements will be of interest to many readers of ACP. However, I think some revision of the manuscript is required before it is acceptable for publication. In particular points 1 to 3

below need to be addressed.

Specific Comments

1. The manuscript includes a lot of discussion of the TTL, including a whole subsection (3.2). However, these measurements were made in the subtropics (22S) and not in the tropics, and I don't think the region around the tropopause analyzed here can be said to be the TTL and compared with previous studies of the transition layer in the tropics. There is a big difference in the height of the tropopause and role of different processes between 22S and the equator. The region examined here will have a lot more influence of mid-latitude processes and much less influence of deep convection.

2. Similarly, in several places comparisons are made with previous tropical measurements of tropopause temperature (section 3.2) and water vapor (section 3.3 and 5), and differences are said to be related to continental versus oceanic conditions. However the previous "oceanic" measurements were made close to the equator (within 10 degrees) and the differences in T and water are I suspect more likely to the different latitudes. The impact of latitude must be discussed before drawing any conclusions about land versus ocean sites.

3. The discussion in section 4.2 is not very conclusive. What is the claimed mechanism for the high water with no signal in the ozone? It is unclear to me what the final proposed mechanism is. The conclusions indicate that it was due to convective transport, but no real evidence for this is presented. Was there convection in the area and how long before the measurements (data in fig 2 should provide this information), and where do trajectories indicate the sampled area came from. You have done the MIMOSA calculations so can look at trajectories in this model, without needing to use a mesoscale model. If MIMOSA can reproduce a fine filament then can be used for this part of the study.

4. Figures 6 and 7 need to be revised. There is no color bar so it is not possible to know what values of PV the contours correspond to. Also, there are too many plots

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and they are too small. It was really hard for me to see the features that are referred to in the text. Only a couple of levels are actually referred to in the text, so there is no need to to show all levels.

Minor Comments

1. Another paper that is relevant for the Introduction is Waugh (JGR, 2005).

2. pg 12484, line 9 "exmaination"

3. The discussion in second paragraph of the Conclusions is a bit confusing. It jumps from one flight to another (talks first about SF4 then SF2 and back to SF4), and refers to "the same altitude range" on line 23 when two altitude ranges have been discussed above.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12469, 2006.