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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A numerical study of tropical cross-tropopause transport by convective overshoots during the TROCCINOX golden day" *by* J.-P. Chaboureau et al.

Anonymous Referee #3

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This paper uses a quadruply nested three-dimensional cloud resolving model to simulate tropical convections over Brazil (near 22S) on 4 February 2005 when high-altitude aircraft measurements were conducted around the tropopause. Comparisons of the simulation are made not only with the aircraft water-vapor and cloud-particle measurements but also with the geostationary satellite brightness temperature maps. The simulation is found to agree with these observations quite reasonable, and clearly shows the impact of cumulonimbus overshoots to the water budget around the tropical tropopause.

This is a very important work which shows the ability of the latest modeling technique to simulate deep convections which affect the tropical tropopause. I think the paper is acceptable for publication in ACP only with minor revisions.

Before making some specific comments, let me add some historical review on the role of deep convection in the water vapor budget and transport around the tropical tropopause. I believe this will demonstrate the impact of this paper to the issue much clearer.

The role of deep convection in the troposphere-stratosphere exchange in the tropics was first discussed in the late 1970s (e.g., Johnston and Solomon, JGR, 1979). Water vapor and temperature soundings in the 1960s and 1970s, being made mostly in the Central and South America, found that (1) the tropical tropopause is not enough cold and that (2) there is a "hygropause" a few km above the local cold-point tropopause (e.g., Kley et al., GRL, 1982). At that time, midlatitude cloud overshoots were known to moisten/hydrate the lowerst stratosphere by cloud-particle transport. But, people thought that the above two facts would be explained only by overshooting clouds which can make substantially cold temperatures near the tropopause. Then, Danielsen (GRL, 1982) proposed a dehydration mechanism by the anvil part of deep convection which has a life time much longer than the overshooting part (see also Robinson and Atticks Schoen, QJRMS, 1987).

Newell and Gould-Stewart (JAS, 1981) showed a different view and pointed out that there are other regions with much colder tropopauses. Actually, water vapor measurements in the tropical western Pacific (over northern Australia) showed profiles whose tropopause and hygropause are located at the same altitude and are colder and drier, respectively, than over tropical America (Kelly et al., JGR, 1993). Later, Mote et al. (JGR, 1996) gave a clear answer to the "hygropause" puzzle, and Holton and Gettelman (GRL, 2001), Fueglistaler et al. (JGR, 2005), and others discussed a quasi-horizontal-transport theory in which deep convection is not necessary at least explicitly.

However, many overshooting cumulonimbus clouds do exist in the tropics, and there-

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fore, they should have some or important or essential role. The problem is that both observations and simulations of overshooting clouds are technically difficult, and thus there is only very limited quantitative information on it. The paper by Potter and Holton (JAS, 1995) is probably the first one, as far as I know, that investigated such clouds quantitatively and directly using a meso-scale model. They used boundary conditions over south-China sea to Borneo. Their clouds however never reached the tropical tropopause. Instead, they found that small-scale gravity waves were generated by clouds and affected the lower stratospheric water vapor.

Then, this paper (as well as recent works cited in this paper) clearly shows the ability of the latest modeling technique to simulate the observations quite reasonably. This would be proposing a new direction for the quantitative study of the role of deep convection around the tropical tropopause.

Minor comments.

The title:

What is the "golden" day? This is probably not an appropriate word for a title because it is meaningful only for the project members.

Section 1. Introduction:

"the water vapour transport is controlled by the cold point" –> the concentration, not the transport, is controlled by the temperature distribution. The final concentrations would be controlled by the cold point temperatures.

Add the pioneering work by Potter and Holton (JAS, 1995).

Section 2. Model and experimental design:

Not only the information on the four horizontal grid spacings, but also that on the four domains (in km x km) should be given. The "model-3" and "model-4" domains (which appear later in section 3 without any explanation) should be defined here.

The vertical spacing around the tropopause should also be described.

How was the initial condition of the stratospheric water vapour given? Was the water vapor concentration in most parts of the stratosphere stable during the simulation?

What do you mean by "the choice of the model configuration is a trade-off between momentum accuracy and computational expense"? Here, which model configuration is relevant? What is the momentum accuracy?

Section 3. Overview of the convective event:

How about the optics contamination and the evaporation contamination for the FLASH Lyman-alpha water-vapor sensor in the presence of cloud particles in the measured airmass? Or, is it a total water sensor? The measurements of supersaturation should be carefully interpretted.

The temperature sensor should also be described because it is essential for the calculation of relative humidity. If the temperature sensor would have some errors (e.g., Murphy, ACPD, 2463-, 2005), the relative humidity values would have errors accordingly.

The tropical tropopause layer (TTL) is a concept for the average and/or environmental structure around the tropopause. In the vicinity of deep, overshooting convections, where the rapid changes produce smaller-scale features of the atmospheric structure, the definition of "TTL" should be made with "environmental" profiles, not with the "local" profiles of the regions directly affected by clouds. Furthermore, the latter has rich information about the detailed processes and thus should not be overlooked. The "strong change" that the authors mention about might be due to gravity waves generated by the clouds. See also the hypothetical profile changes by Danielsen (GRL, 1982). I think there were some radiosonde soundings at nearby stations. These profiles may be used for defining the TTL. Or, aircraft ascent/descent data between the surface and the level-flight altitude may be usable.

In Fig. 2, the authors discuss the comparisons of observations with the model simulation. The vertical spacing of the model around this height region should be mentioned again so that the readers would be convinced that the model has enough resolution for the discussion here.

As for the "overshoot" definition using the brightness temperature data, why can the information from 6.2-micron channel distinguish the cloud tops higher or lower than the "environmental" cold point? Add some more physical explanation for this. (Water vapor has the 6.3 micron band, and the 10.8 micron is within the so-called "atmospheric window" wavelength region where there is almost no atmospheric gas that can interact with the radiation. But, I could not understand why the above two wavelength information can define the "overshoot.")

Section 4. Overshooting plumes:

Use the same unit for temperature throughout the paper. Perhaps, degree C would be better because Kelvin is used for potential temperature.

As for the vertical wind velocity, the cited numbers are all from numerical models, not from observations. Atmospheric radars can measure vertical winds quite precisely, although we should note that each radar system has its own maximum wind limitation to reduce the data size. For example, Shibagaki et al. (JMSJ, 2003) investigated the three dimensional wind feild near the center of a typhoon, one of the severest meteorological disturbances. They reported the maximum upward velocity of the order of 1 m/s, probably as a 2-minute average (or shorter). I am afraid vertical winds more than 20 m/s are too large and perhaps unrealistic. At least, the authors should note the temporal resolution (or averaging period) for each numerical experiment result for future comparisons with observation results. This is because the maximum wind velocity is highly dependent on the length of the averaging period.

It would be very interesting to see if the simulated vertical profiles of temperature and their time evolution are consistent with the hypothesis by Danielsen (GRL, 1982;

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see also Robinson and Atticks Schoen, QJRMS, 1987) or not. This is because the Danielsen model has long been considered (often implicitly) as the working hypothesis for many researchers. As I wrote previously, it is not surprising that the overshooting turrets "hydrate" the lower stratosphere by cloud-particle transport and subsequent evaporation in the warmer stratosphere.

Section 5. Mass and water vapour transport:

In Fig. 8, are the shown fluxes daily averages? If so, mention about this.

One problem of satellite measurements is that there is often no simultaneous measurements of temperature. Overshooting clouds (and thin tropopause cirrus may also) greatly change the temperature structure around the tropopause locally and temporarily, as shown in this paper. Therefore, the "overshooting" frequency estimated by satellite data should be carefully interpreted. I think this is one of the important messages of this paper.

Some comments for Figures:

I think the time should be in LT, not UT throughout the paper because tropical convections are known to have clear local-time dependences.

The time axis of Fig. 2 should be ** hr ** min in LT, not in seconds. This figure is very difficult to be compared with other figures because of this. Also, add the explanation of the two vertical dashed lines in the figure caption.

In Fig. 5, why is only the decaying phase shown? Perhaps the figure should start from 12 UTC (9 LT?).

In Fig. 6 (and also Fig. 3), the numbers for the color-contour bar are hard to see.

Are the mass fluxes in Fig. 8 daily averages? Specify this in the figure caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 13001, 2006.

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