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Interactive Comment

Interactive comment on "Morphology of the tropopause layer and lower stratosphere above a tropical cyclone: A case study on cyclone Davina (1999)" *by* F. Cairo et al.

F. Cairo et al.

Received and published: 19 March 2008

Response to: Interactive comment on 'Morphology of the tropopause layer and lower stratosphere above a tropical cyclone: A case study on cyclone Davina (1999)'; by F. Cairo et al.

All the comments issued by the referees have been considered and the manuscript has been modified accordingly. In particular:

Anonymous referee #1

p.18330 l. 9. The temperature and pressure accuracies of the avionic systems have been checked from comparisons with colocated radiosoundings during the APE-

POLECAT campaign in 1996 (Stefanutti, L., A. R. MacKenzie, S. Balestri, V. Khattatov, G. Fiocco, E. Kyrö, and T. Peter (1999), Airborne Polar Experiment-Polar Ozone, Lee-waves, Chemistry, and Transport (APE-POLECAT): Rationale, road map and summary of measurements, J. Geophys. Res., 104(D19), 23,941;23,959.) and during the APE-Theseo campaign itself (see quotation of MacKenzie et al., 2006, in the text). Moreover, the implementation of Rosemount sensors on board the aircraft for successive campaigns gave the chance of an a posteriori check of the avionic temperature and pressure data system, that was found accurate in almost all the cases (small divergences from the claimed 0.5 K and 0.6 hPa accuracies were found occasionally during severe changes of vertical speed). This has been mentioned in the revised manuscript.

p. 18333 l. 2 fig. 3 has been dropped out of the manuscript, as suggested.

p. 18336 l. 17 the low values of ozone at 360K on the Davina profiles were in line with what measured in the marine PBL during the campaign: there, an average value of 0.015 - 0.025 ppmv was observed.

p. 18337 l. 4 we agree that fig.3 (now dropped out) was not conclusive in supporting the 'point 4'; claim about the presence of clouds up to the cold point. However, fig. 10 (now fig. 9), which is discussed in the following paragraphs, gives evidence of what stated there.

p. 18338 l. 14-21. This remark deserve a throughout discussion:

"First, the mean non-Davina saturation mixing ratio and actual mixing ratio profiles are compared to make the point that the upper TTL was generally unsaturated. (Does this concur with the lidar cloud observations by the way?)." It should have been said in the manuscript that the displayed averages come from data points from cloud-free airmasses. 'cloudy' airsampling have been removed from the computation of those averages. This has now been stated in our text.

"Surely then the same variables should be compared for the Davina case - i.e. the sat-

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uration and actual mixing ratio." True, it would have been good to do so. Unfortunately in the Davina case most of the airmasses were 'cloudy' and we did not rely enough on the accuracy of the water vapour mixing ratio INSIDE clouds to shows those data. "Instead, the non-Davina mixing ratio is compared to the Davina temperature (or saturation mixing ratio) and the conclusion drawn that there is potential for saturation and freeze drying. Would it not make more sense to compare the measured Davina humidity with the corresponding temperature?" Yes, but as we stated earlier, we did not put enough confidence in water vapour mixing ratio inside clouds at those times. The figure aimed at showing the potential for saturation and freeze drying, as correctly said by the referee. "Anyway, the lidar provides a direct measurement of cloud so why not use that?" In fact in fig. 10 (now fig. 9) there is the display of total water mixing ratio, saturation mixing ratio and lidar depolarization, so it met the referee suggestions. There, it can be seen (see the lowermost panel as instance) that condensation is present at extremely low (we would say indistinguishable from zero, from the figure scale) values of super saturation.

p. 18342 conclusions. These have been rephrases, in order to underline the lack of a firm conclusion addressing the question of cloud induced dehydration, and so to meet the referee recommendations, which we truly share.

p.18342 l. 9 the paragraph has been moved to to the discussion section, as suggested by the referee.

All the figures have been reedited in accordance to the referee recommendations.

Anonymous referee #2

Page 18326 l.1 references to the works suggested by the referee have been added and discussed.

Figures 1 and 7 have been changed according to the referee's comments.

We agree that the separation of data between Davina and non-Davina cases has to be done with care in order to avoid long range effects on the non-davina cases prior to our observations. Nevertheless we are confident in justifying our approach given that - in the non-Davina cases - the profiles obtained before the Davina onset (5th of March) did not differ from those obtained when the Davina was existent, but still at thousands of km apart from the measurement sites (basically the sole flight on the 6th of March), thus giving confidence on the fact that the Davina influence on the region was not present - for instance - on the 6th of march, our day of a M55 mission closest in time before the Davina, and was only present on the 9th of march when we actually sampled the cyclone, and most likely afterward its passage.

We disagree with the referee comments concerning the Figure 8 (now fig. 7), which in fact has been discussed in the text, as in 18336 l.21 and subsequent, and in 18337 l.13. There, these N2O and CFCs are well mixed tracers in the troposphere, with smooth profiles up to 385 K so from this absence of relevant features in the profiles we do not think there is a way to assess from their tropospheric values a 'typical' cyclonic atmosphere as suggested by the referee. The point there was to show how tropospheric tracers showed no sign of detectable stratospheric intrusion below 385K. The absence of stratospheric intrusions is reinforced by the fact that no enhancement of ozone and no dry layers were observed in the troposphere, so from a purely observational point of view we tend to exclude that occurrence, at the site and time of our observations. This does not imply that STE were excluded during the whole existence of Davina, or that they should be ruled out as possible occurrences. In fact we quote references where such STE effects have been extensively documented.

For what concerns the reduced ozone by a factor 5, it comes from the observation of 0.015-0.02 ppmv at 360 K for the Davina case, compared with the average values observed at that level during the campaign, between 0.06 and 0.01 ppmv. The 0.015 - 0.025 ppmv were the values observed in the maritime PBL during the campaign.

The referee suggests to reshape the discussion on the 7 conclusions presented at page 18837, and this has been done by adding some remarks in the discussion in the new version of the manuscript. P18336 l 24 and subsequent.

The referee raises the question of displaying potential vorticity maps or vertical cross section to further study the STE at the mesoscale level, and points to the fact that large scale trajectory models as HySplit may not be suited to study movements of airmasses near tropical cyclones. Surely this is a point that deserves careful consideration: In fact, in the course of our study we have done MM5 modelling of the Cyclone Davina, and, as a matter of fact, all our statements concerning vigorous uplift of maritime PBL, absence of local STE at the time of our observation and meridional transport of midlatitude stratospheric air could be sided by adequate mesoscale modelling findings. During the manuscript writing, there has been a discussion between the authors whether it was worthwhile to put this modelling material in the paper and the decision was to leave it out, and have it for an eventual subsequent work addressing more specifically the mass flux and budget of the cyclone: its presence here would have substantially expanded the size of our manuscript and unnecessarily shifted its scopes. We decided to not even quote such simulations, and rather to focus on the sole observational evidences whose interpretation was, in our opinion, straightforward enough to sustain our conclusions. In particular, the mesoscale modelling supported the view of a stratospheric meridional transport as shown in the hysplit simulation for the previous 72 Hrs. We agree with the referee that hysplit is not adapted, for algorithm and resolution, to investigate the mesoscale movements in the cyclone, but to the extent it has been used in our paper, i.e. to demonstrate the possibility of a large stratospheric (above 400 K) meridional flux from midlatitudes above the cyclone, we think it is adequate and does a pretty good job.

Anonymous referee #3

In accordance with the referee recommendations, the abstract has been reformulated and refocused to the outlined most interesting results. The paper has been reshaped accordingly and shortened, although not significantly, to fulfil the remarks of the other two referees.

Specifically: Section 2 line 21. Similarly to what was done in Alcala and Dessler 2002 we used 14km as a proxy for the lower boundary of the TTL. Although this can be considered an arbitrary choice it provides a reasonable threshold to evaluate the penetration of deep convection in the upper tropical troposphere. TRMM team doesn't provide an error on the range of the 2A23 product. An estimate of the uncertainty can be deduced considering that the minimum vertical resolution of this dataset is 250 meters at nadir. The process followed for these measurements closely mirror the one adopted by Alcala and Dessler in their work. The dataset used for the calculation was the TRMM tropical cyclone database prepared by JAXA. This is takes into considerations all the tropical cyclones and not only those present in the Indian ocean. The reader can find more information visiting the website http://www.eorc.jaxa.jp/TRMM/typhoon/index_e.htm. One of the reasons we have extended our investigation over an entire year has to do with the global coverage of the dataset. The hurricane season are usually occurring during opposite seasons in the two hemispheres.

In our analysis 3.5% of the overall convection associated with tropical cyclones reaches the TTL. Being this value so close to the one obtained by Alcala and Dessler our investigation suggests that tropical cyclones are not a preferred location for convective overshooting when compared to the overall tropical deep convection. The tropical cyclones can still play a very important role in the water budget of the TTL by providing large areas of relative high clouds, where the thermal effect associated with this cloud deck can be of importance. The climatological relevance of this process has not been studied in this paper and consequently we are not in the position of assessing the importance of cyclones in the global water budget of the TTL, nor this was the aim of the paper.

Section 4.2 Fig. 2 has been reformatted to meet the referee's remarks, fig. 3 has been removed.

Section 4.3 (1) We agree with the referee that, although there is a clear indication of

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a colder cold point, there is a questionable clear indication for a higher cold point with respect to the mean (it happens on one Davina profile out of three, while the remaining two stays at the same altitude than the mean). We have dropped the 'higher then the mean' claim.

Section 4.3 (2): The referee correctly states that ozone perturbations tops at 375 K, and tropospheric tracers shows tropospheric values up to 385 K, which, incidentally, is also the highest cold point potential temperature value. To our opinion, below 385K we are still in the 'tropospheric part' of the TTL. so we do not see the need to suggest that 'longer lived tropospheric tracer reach the lower stratosphere up to 385 K while boundary layer with low ozone remains in the TTLwith low ozone remains in the TTL'. In fact the Davina N2O and CFC-12 profiles stays aligned to the campaign mean up to 385 K (see fig 8, now fig. 7) which we tend to consider still the upper part of the troposphere. At the same time, given the lack of differences between the Davina tropospheric tracer profiles and the campaign mean, up to 385K, we do not see the need to invoke 'meridional advection of tropospheric air near 380 K'.

Section 4.3 (3): Most of the APE theseo dataset comes from observations in the inner tropics. So when comparing the Davina profiles with the campaign averages, we purposely draw in the figures the subset of the non-Davina profiles which were taken southward of 13°S. These are only two vertical profiles sampled three days before the Davina measurements, between 17°S and 19°S, and one profile sampled two days after between 13.5°S and 15°S. This separate display of the southernmost subset was done to rule out possible differences between averages and Davina cases that could have been erroneously attributed to the cyclone influence, while mirroring solely the effect of the different latitude of the measurements. In fact, these two observations are often at the extrema of the campaign dataset variability.

In figs. 4 to 8 (now 3 to 7), and fig. 10 we have now drawn in gray the 6th march profile, in black the 11 march one, to differentiate the before and after the event. The referee's observation of a large differences before and after the Davina passage are correct: before the Davina, both ozone and temperature are higher in the 'tropospheric part' of the TTL. These differences in temperature and ozone (unfortunately we miss part of the water data for the 11th of march) tend to taper out as we go higher, and in the lower stratosphere the before and after profiles are essentially coincident for what concerns temperature and ozone. Different is the behaviour of the tropospheric tracers N2O and CFC12 in fig 8 (now fig. 7). In the 11 th march profile, the profiles closely resembles the Davina cases taken two days earlier, and it suggest of a permanence of the cyclone influence in the area its passing. This is further substantiated by the analysis of the scatterplot N2O vs O3 in fig. 11 (now fig. 10) where the southernmost, earlier non Davina profiles are well aligned with the campaign average profile, while the southernmost non Davina profile after the cyclone passing has still the midlatitudes characteristics the cyclone brought in. As suggested by the referee, there issues have been discussed an length in the text, when introducing the figures.

Section 5. It is unclear to us whether the referee is suggesting that our observations do not support our conclusions, or whether our conclusion does not correspond with what reported extensively in the literature. We can agree with this last conclusion. In the literature, even in some paper we have referred to in our work, there are documented observations of STE in cyclones and we totally agree that this is a process happening, probably extensively, in the southern periphery of the Indian Ocean as the referee is pointing out. Our statement is that in our observations this process does not show up and we think we have good foundations to affirm that (smoothness of tropospheric tracer profiles up to 385 K, absence of dry and ozone rich layering in the upper troposphere and so on).

5.1 (1) As the referee noted, there was an extensive deep convective activity going on at 7-8 °S but the aircraft flight profile was not adapted to sample such convective effect. Moreover, during the APE theseo campaign there were no large convection going on, but a case study was reported in the quoted article by Santacesaria et al. (2003).

5.1 (2) Unfortunately we miss part of the water data for the 11th of march, nevertheless a moist layer (in absence of clouds) it is apparent, topping at 360 K, probably due to refreshing of air in the lower part of the TTLwhile higher up the post-Davina profiles resembles the Davina ones, thus suggesting again a persistence of the effects of the cyclone in the area, two days after our observations. This has been discussed in the text.

5.2 potential vorticity maps have been displayed (fig. 11) and discussed in the text.

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