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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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This paper shows airborne measurements of trace gases obtained during the APE-THESEO mission taking place in the Indian Ocean in March 1999, near the Davina cyclone.

The structure and the writing of the paper are good for me. The topic is interesting. Most part of the paper is qualitative and descriptive, but the interpretations seem to be quite straightforward, and I recommend its publication, following consideration of the following comments and responses to questions. . The introduction (sections 1 and 2) is clear and well referenced.

Page 18326, line 1, the authors cite Baray et al 1999, that suggested mesoscale trans-

port of air-masses by strong ageostrophic movements around the cyclone. This suggestion has been confirmed by a mesoscale modelisation (Leclair De Bellevue et al., Atmospheric Environment, 41, 6510-6526, 2007.), and another study showed that this king of event is not exceptional in this region (Leclair De Bellevue, et al., J. Geophys. Res., 111, D24107, doi:10.1029/2005JD006947, 2006.), event it seems not to be the case in the Davina study (It is not possible to conclude on this point with data provided in this paper, comment for the section 5 about this later).

The section 3 is clear and seems not to be redundant with the description of the instrumentation and method of the other paper about the APE-THESEO mission, as MacKenzie et al 2006.

Some figures must be improved :

The figure 1 shows the satellite image of the cyclone (perhaps a combination of several images), with the trajectories of the plane and the track of the cyclone superimposed. All this is a little bit confused and difficult to read. The satellite image is too little, the date is not given, it could be interesting to have several days, before and after the airborne measurements, to know with precision where is each measurement in relation to location the cyclone and its fine scale structures. Can the satellite imagery give the information if the cyclone reaches the tropopause or not?

The figure 7 shows profiles of ozone mixing ratios, but the logarithmic scale is not adapted for the troposphere, it is very difficult to see on the figure if some ozone enhancement or low values occur near the cyclone. I think it is necessary to separate the stratosphere and the troposphere, and for the troposphere, use a linear scale like the figure 8.

In addition, I think that the separation of the data-base between Davina and non-Davina profiles is to be used very carefully, because the cyclone are strong mechanisms able to influence the troposphere far from the cyclone (several hundred of kilometres far from the cloudy area of the cyclone).

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The figure 8 (profiles of N2O and CFC12) is not exploited in the text. Are the values obtained typical of a cyclonic atmosphere ? What conclusion can we do from the figure 8 ? I think it is necessary to discuss about this figure in the text, and if not, retire the figure.

Page 18837, the authors present 7 conclusions, which seem to be quite straightforward, except perhaps the conclusion 3.

From the conclusions 1 and 2, what is it possible to conclude in relation to the permeability of the TTL in the Davina case, in terms of STE and TSE ?

For conclusion 3, I don't understand where the factor 5 come from ? What exactly are the tropospheric ozone values observed (figure 7 with a linear scale). What are the values used by the authors for the comparison giving a factor 5 (climatology? Ozone profiles when the cyclone is far?). In my opinion, this point seems need to be reinforced and precised to be pertinent.

Section 5 discussion, page 18337 line 22 to page 18338 line 4, the authors write that no stratospheric intrusion into the upper troposphere were observed during the Davina event, but the ozone profile are difficult to analyse (previous comment), and the author have shown no Potential vorticity map or vertical cross sections, no lagrangian or mesoscale modelisation. With the elements given by the authors, it is not possible to conclude on stratospheric intrusion or not during the Davina event.

The discussion on dehydration mechanism is interesting, but I am not a specialist of this kind of mechanisms to detects some eventual problems in this discussion. The opinion of another reviewer is important for this part of the paper.

Page 18341 line 2. I think that Hysplit is not a trajectory model adapted (resolution and algorithm) to the study of mesoscale movements of air masses near a tropical cyclone, where strong wind shears occur in the atmosphere.

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Interactive Comment