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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Passenger aircraft project CARIBIC 1997-2002, Part I: the extratropical chemical tropopause" *by* A. Zahn et al.

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This paper presents results from the CARIBIC project on the chemical composition of the tropopause region in terms of a stratospheric and a tropospheric tracer (O3 and CO). The main idea of the paper is to define a chemical tropopause based upon correlations of O3 and CO measurements. This is certainly an interesting and novel approach that should be published. However, there are some important aspects of the paper that are unclear/confusing (see comments below). After they are clarified I recommend acceptance of this first part for publication in ACP.

Major comments:

A) The proposed chemical tropopause definition is based upon O3 and CO measurements during one entire flight (for instance from Germany to the Maldives as shown in Fig. 4). For such a flight, that may cross the dynamical tropopause (2-pvu surface) several times, the authors derive a chemical tropopause with an accuracy of 150-200 m in the vertical and 100-200 km in the horizontal (last sentence in section 4). It is not explained, and to me totally unclear, how one gets from Fig. 4 to the exact location of the chemical tropopause (lon, lat, pressure). I could see how it works if the data was from a vertical sounding (where all data is obtained from more or less the same horizontal position) - but in case of a rather horizontal sampling of O3 and CO with an aircraft, and thinking of a complex structure of the dynamical tropopause with multiple tropopause crossings (mentioned as a possibility in section 1, para "The dilemma ..."), I do not see how the combination of all chemical data from the entire flight can yield precise information about the tropopause location.

B) In section 5 (para "(iii) The sharpness ...") the authors discuss why over the Atlantic their definition can not be applied in about 50% of the cases. The argumentation is very strange to me: "The reason is that over the Atlantic also upper tropospheric air often showed a negative O3-CO correlation". But, according to the proposed definition, exactly this negative correlation is used to characterise stratospheric air! How can you say that (in some cases) the air with a negative correlation is "tropospheric" if you used it to DEFINE "stratospheric"?

C) The behaviour shown in Fig. 4 is essential for the proposed definition. However, it is shown only for a single flight. In order to clarify the robustness of the approach, I suggest to include an additional figure showing for instance all correlations from the Indian Ocean flights for one season.

Futher comments:

1) section 1, para "Although mostly ...": Why are the physical tropopauses not suitable when studying the transport of trace-constituents? (In your second part, the authors

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also use the thermal TP above Hohenpeissenberg (and other information) to estimate cross-TP ozone fluxes.) Please explain or delete this remark.

2) In the same paragraph you emphasise the need for a spatially highly resolved TP. I agree, but is this really a question of the definition and not rather of the data set? Also, cf. comment A above, does the new chemical definition provide more than one single point per flight? Is this high resolution?

3) section 2, para "From November ...": I don't understand "Crucial for the present analysis is that stratospheric air was ever intersected ...". Do you mean "was sampled at least once"?

4) section 3, para "Stratospheric O3 is ...": please change the formulation "lower lower-most stratosphere". Below, the second sentence of the next paragraph does not make sense. Please change wording.

5) section 5.1, para "We assume that ...": Some references are not correct: Wernli and Bourqui, and Sprenger and Wernli did not identify the UT over the Middle East as a favoured WCB outflow region. A comprehensive WCB climatology appeared recently in J. Climate (Eckhardt et al. 2004, pages 218-237) and it shows some WCB outflow in the Iran/Irak area, but only during winter and not summer. Also, they might not contain polluted but clean marine ABL air. The discussion in this paragraph should be revised and it should be clearly explained in what way the presence of WCB outflow air in the upper troposphere influences the chemical TP definition.

6) final part of section 5.2: I don't understand: If a constant O3 TP definition was used, then there would be not a damped seasonal variation but, per definition, no seasonal variation of near-tropopause ozone(?).

7) section 5.3, para "Fig. 6 demonstrates ...": I don't understand/agree: "The ozone variation at the chemical TP are derived from data that were almost exclusively collected near the jet stream, i.e. in air masses that freshly descended from the lower

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stratosphere": near what jet-stream? Is the TP crossing of the aircraft mainly near the subtropical jet? (I suggest to include a Figure showing all the identified chemical TP locations.) And what means "freshly descended from the lower stratosphere"? Is this in agreement with the second part, where the authors emphasize the mixing of strat and trop air masses in a 2-km thick layer above the TP?

8) section 6: I suggest to omit things that are not shown in this first part from the conclusions section (i.e. point (iii) in the para "Main strength ..." and the sentence "As shown in part II ...".

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1091, 2004.