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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the structure of the extra-tropical transition layer from in-situ observations" *by* I. Pisso et al.

Anonymous Referee #2

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This paper evaluates carbon monoxide and ozone tracer observations from a suite of past aircraft campaigns in order to define the structure of the extratropical transition layer. The authors present a new working definition of the extratropical tropopause transition layer (ExTL) based on tracer gradients and infer that this definition is consistent with a mixing layer derived from trajectory calculations. While this work could be of interest to the ACP readership, I feel that the authors fail to proof/discuss the general applicability of the new methodology, do not present the results in a convincing way, and most importantly do not relate their approach and findings to the existing literature on the ExTL well enough. The study does not really come up with new results on the structure of the ExTL, just discusses old results in a (somewhat) new framework. In the current form it is unclear what the authors emphasize, i.e., what the title implies (to yield insight on the ExTL structure) or that they introduce a new methodology (which

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in my eyes is the added value to the existing knowledge). In order to be publishable, I recommend major revisions to this manuscript. The authors should elaborate on what is original in their study, this includes the strengths of their method and its comparability to previously used methods, as well as emphasize the implications of using this method on improving our knowledge of the ExTL structure according to the comments below.

Major comments

There is a lot of valuable and interesting material in your composite of aircraft data, but you need to discuss them in relation to what has already been shown in the literature and guide the reader better what your additional contribution to our knowledge on the structure of the ExTL is.

It seems a major oversight not to include a reference to the study by Tilmes et al. (2010), since this paper evaluates a similar composite of aircraft measurements. Although a different methodology is used, their findings need to be summarized in the introduction.

Tilmes, S., et al. (2010), An aircraft-based upper troposphere lower stratosphere O3, CO, and H2O climatology for the Northern Hemisphere, J. Geophys. Res., 115, D14303, doi:10.1029/2009JD012731.

Concerning the use of tracer gradients as new diagnostic: The study by Hegglin et al. (2009) which you mention for investigating the ExTL in terms of tracer-tracer correlation only, was the first to plot tracer gradients as a function of latitude and altitude in order to investigate the structure of the ExTL. This study was the first to find that the maximum in the absolute CO gradient is co-located with the thermal tropopause (also your P28045 L17). Note, Pan et al. (2004) found in their limited set of aircraft measurements that the region of strong gradients across the tropopause was centered around the tropopause, however, this is not equivalent to your and Hegglin et al.'s finding that actually a maximum in the gradient exists at the thermal tropopause. Acknowledgment

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of this work is needed when you present your findings. Using tracer gradients as an approach to identify the ExTL has further been discussed in Gettelman et al. (2010), to which a reference is also lacking.

Gettelman, A., P. Hoor, L. L. Pan, W. J. Randel, M. I. Hegglin, and T. Birner (2011), The extratropical upper troposphere and lower stratosphere, Rev. Geophys., 49, RG3003, doi:10.1029/2011RG000355.

P28041/section 2.2: The presented study furthermore uses the equivalent latitude-potential temperature framework to 'order' the aircraft data whose spatial and temporal distribution is very uneven (the main reason why you use equivalent latitude). The use of equivalent latitude in the tropopause region has however major weaknesses as discussed in Pan et al. (2012). The validity of or need for your approach needs to be better argued for, just ignoring this paper is not a viable approach.

Pan L. L., A. Kunz, C. R. Homeyer, L. A. Munchak, D. E. Kinnison, and S. Tilmes (2012), Commentary on using equivalent latitude in the upper troposphere and lower stratosphere, Atmos. Chem. Phys.

P28050: I don't think that your sketches and description provide the necessary information and basis to explain the new methodology introduced in this paper and to come up with the α_{PV} and α_{theta}. Maybe it is just not written clearly enough. At least I did not understand. What do your value of strongest CO gradient really mean? Do you mean the upper edge of the region with strong gradients? If it were the maximum gradient in CO then it would coincide with the thermal tropopause as shown by Hegglin et al. (2009) and this would certainly not be the right upper edge of the ExTL. Regardless on what you really mean I guess, can your alphas really be assumed to be seasonally independent? Hoor et al. (2004) found a weak seasonality in the depth of the ExTL (i.e. upper limit), which seems to be ignored here, and also interannual variability in CO emissions or the relative strength of downwelling versus mixing may affect the choice of your definition of the upper limit of the ExTL.

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Finally, your abstract states that the use of tropopause relative coordinates eliminates the need for looking at the ExTL in the different seasons. This high-level statement has in my eyes not been proven by the manuscript quantitatively, and in contrast may be even wrong. Evidence for rather strong seasonal differences seen in tracer gradients is provided in the study by Hegglin et al. (2009). A way you could explore the dependence of your results on seasonality in more details is to use vertical profiles in tropopause coordinates (averaged over a certain latitude band) instead of the cross sections.

Minor comments

Large parts of the paper are written in a confusing way and make things appear more complex than they really are. The language of the manuscript should be improved substantially, especially the 'Introduction', section 2.2 'Diagnostics based on ECMWF fields', and section 4 'discussion'. E.g. P28014 L16, I do not understand, are you not simply using the tropopause found either vertically below or above the measurement point?

P28042 L14-18: Especially in the SH, there are also sinks in ozone (polar ozone loss) that may influence your tracer distributions and which needs to be mentioned here.

P28043 L4/5: Do you mean the 'mean vertical displacement of the tropopause caused by the seasonality in the Brewer-Dobson circulation'? Otherwise, using both equivalent latitude and tropopause coordinates you account twice for the wave-induced meandering of the tropopause.

P28044 L3: Do you mean inter-hemispheric differences here during the same time period?

P28044 L27: The major source of ozone is the tropical middle stratosphere.

P28048, Section 3.4, Lagrangian comparison: Could you add information of how your results compare to previous findings more quantitatively? E.g., Hegglin et al. (2009) already state that the ExTL depth derived from their tracer-tracer evaluations is equiva-

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lent to the mixing layer introduced by Berthet et al. (2009) using trajectory calculations. So, what do you add to this discussion? Please refer to these findings.

P28050, L4-5: I do not agree. As shown by Tilmes et al. (2010) or Pan et al. (2004), there is a lot to be learned from using the thermal tropopause as reference point. To be of more value, the study should be repeated in relative coordinates to the thermal tropopause. This would allow for a better comparison with previously established findings of the ExTL structure.

P28047 L18-24: The fact that tracer isolines slope with respect to the PV and Theta-isolines was already noted in Hegglin et al. (2006), which used the potential temperature-equivalent latitude framework for the SPURT ozone measurements.

Hegglin, M. I., D. Brunner, Th. Peter, P. Hoor, H. Fischer, J. Staehelin, M. Krebsbach, C. Schiller, U. Parchatka, and U. Weers (2006), Measurements of NO, NOy, N2O, and O3 during SPURT: Implications for transport and chemistry in the lowermost stratosphere, Atmos. Chem. Phys., 6, 1331-1350.

Conclusion section: When the authors discuss their results, the limitations of the representativeness of their data set is not further discussed. This is despite the fact that when discussing the figures, a great deal of features is being attributed to convective overshooting. Also, you use measurements between 1994 and 2006, where does your confidence come from that the relation between dynamical and chemical measures is not influenced by interannual variability or long-term changes? Adding acknowledgment of other studies into the conclusions, which have seen similar or the same findings using different approaches, would help confirm the results of your study and their representativeness.

Technical comments

P28034 L5: Is equivalent latitude a vertical coordinate?

P28034 L8: Please change to ‘trace gas distributions’

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P28034 L20: ‘and make Lagrangian comparisons’ be more specific. There are lots of different uses of Lagrangian comparisons out there and not necessarily clear to every reader to what you refer to.

P28045 L3: ‘between the troposphere and stratosphere’

P28037 L10-15: Please improve language. In the first 5 lines you overuse the word ‘Section’.

P28037 L15/16: put ‘geographical’ into parentheses. It sounds like longitude is not geographical.

P28047 L2: ‘. . .an undulating curve’

P28048 L19: you cannot ‘inject photochemical production into the LS’. Anyhow, why do you need to invoke here photochemistry at all, don’t you explain later the shape of the ExLT by the Lagrangian trajectory study?

Figures 3-6: Maybe contouring instead of having these somewhat psychedelic dotted plots would possibly better illustrate that the tracer and gradient isolines intersect the isopleths?

Figures 3-6: suggest adding tropopause to the relative to the tropopause plots, i.e., zero line.

Figure 7: I don’t understand what the white lines in the right panel are. Please explain.

Figures 2/8: In your introduction you state that there is a transition layer in both the tropics and the extra-tropics. In your Figures, the ExTL goes to zero in the subtropics?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28033, 2012.

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