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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropopause referenced ozone climatology and inter-annual variability (1994–2003) from the MOZAIC programme" *by* V. Thouret et al.

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1. General

The MOZAIC data set is perhaps the largest and most valuable ozone data set available. It is good to see that the paper by Thouret and co-authors presents a new climatology derived from this dataset. Apart from the longer duration of the data set available now, two features are noteworthy compared to previous MOZAIC ozone climatologies: First, the climatology is given in a co-ordinate system referenced relative

to the tropopause, which is an improvement over previous climatologies. Second, the paper addresses the interannual variability of ozone and, especially, a large ozone anomaly observed in the years 1998 and 1999. While the authors cannot really explain the origin of this anomaly (apart that it appears to be somehow related to anomalies in the atmospheric circulation, e.g., the North Atlantic Oscillation), it is at least well documented, which is of great value by itself. I find the first part of the paper (i.e., the explanation and presentation of the climatology) very well written. The paper has some weaknesses when it comes to the second part, which tries to explain the 1998/1999 anomaly. I also find it somewhat unfortunate that the paper uses only a fraction of the MOZAIC data set: cruise-level data from the North Atlantic flight corridor (NAFC). I understand that a companion paper presents vertical profiles, also from regions outside the NAFC. However, (without having thoroughly read the companion paper yet) I believe that a combined paper presenting a climatology using the full MOZAIC data set would serve the community much better as a true reference ozone climatology. I would think that such a combined paper would have a much larger impact, in terms of future citations, than the two separate papers combined. Therefore, I encourage the authors to consider combining the two papers to make best use of the data set. However, I recommend publication of this paper even as a stand-alone, given consideration of the following points.

2. Specific points

While the authors discuss the 1998/1999 anomaly in great depth, they hardly discuss the upward ozone trend. What do you believe causes this trend? On page 5453, line 6, it is written that the global increase appears to be due to very high values recorded in 1998 and 1999. However, 1998-1999 is almost exactly in the middle of the analysis period and, thus, the anomaly can hardly cause a trend.

ACPD
5, S1848–S1854, 2005

EGU

Discussion on the 1998-1999 anomaly: Spichtinger et al. (2004) also discuss the anomaly in various trace gases caused by the forest fires in 1998 and show, for instance, that positive CO anomalies throughout the northern hemisphere appeared already early in 1998, due to the burning in southeast Asia, and persisted well into 1999, due to the late-season burning in Canada and Siberia in 1998. Thus, it is not clear that the ozone anomaly you see before July 1998 really indicates that biomass burning cannot be the reason for the ozone anomaly.

Fromm et al. (2005) show that a major pyro-Cb greatly affected also the stratosphere in 1998. They also show large enhancements in stratospheric ozone in the pyro-Cb plume. I've discussed this with Mike and I am not convinced that they actually see such dramatic ozone formation (rather than downmixing of ozone-rich air from higher up in the stratosphere), but the possibility currently cannot be ruled out. If this were true, could the MOZAIC ozone anomaly both in the troposphere and in the stratosphere be explained by the extensive biomass burning in 1997-1998?

Forster et al. (2003) show that flight patterns are adjusted to the weather conditions and this affects the time spent by MOZAIC aircraft in the stratosphere/troposphere. I would think that the positive NAO phase in 1998/99 would have caused also an anomaly in the MOZAIC flight patterns. I don't think this will really explain the observed ozone anomaly (especially, as your climatology is referenced relative to the tropopause), but this should be easy for you to check, as a small bias (positive or negative?) could actually be possible.

I suggest to enlarge most of the figures. Figure 10 is especially bad - I can hardly guess the dates -, but I find most figures too small in the print-out.

Figure 1: Indicate the pressure boundaries between levels 1 and 2, and 4 and 5 (they are also not mentioned in the text, at least not before the following section). I am also not sure what the boxes really show. Are these the average pressure ranges of these boxes?

ACPD

5, S1848–S1854, 2005

Figures 2 and 3: I recommend to overlay one or two isolines contouring the number of data points per grid cell. It seems there is some noise at the boundaries of the plotting region, likely due to relatively poor data coverage. It would ease interpretation if one could see how many data points are available in these regions.

Time series plots (Figs. 8-10): It is unfortunate that the major vertical lines are drawn in 1 ¡ years intervals. This makes it unnecessarily hard to see the seasonal cycle or identify a particular month. I recommend drawing these lines every year (or every second year, if this is too much).

Figure 5449, lines 16-17: What are ”upper-level dynamical precursors of the Atlantic extratropical lo”?

Page 5451, paragraph 1: Cooper et al. (2005) discuss some problems with the Wallops Island ozonesonde and show systematic differences to the near-by MOZAIC ozone profiles. They could be related to a combination of instrumental problems and a sampling bias in the Wallops data.

I suggest to remove the paragraph starting on page 5451, line 25, as the mathematical fit is not of any particular interest. Especially Figure 6 adds very little new information and should be removed (if you still want to keep the fit line, you could add it to Fig. 5c).

Page 5453, line 23: The box charts summarize frequency distributions. This should be mentioned.

Page 5459, lines 20-24: This is a bit loosely phrased. Also, the mechanism behind the NAO is not really understood. Thus, a correlation with the NAO per se does not explain anything.

ACPD

5, S1848–S1854, 2005

Interactive Comment

3. Language

Figure caption 12: Ěextend from the 25% TO the 75% percentile.

Page 5443, line 27: I think this should read: on an equivalent latitude scale

Page 5445, line 21: whose instead of which

Page 5446, line 3: interpolation TO

Page 5447, line 6: Ěseparating the stratosphere FROM the troposphere.

Page 5447, line 16: Sentence combines past and present tense. This is true, more generally, for the entire paragraph. Please make consistent.

Page 5450, lines 5-6: remove the minus in front of the 60 and 30 hPa. Pressure is higher below the tropopause!

Page 5450, line 17: stands for altitudeS GREATER THAN 45hPa?

Page 5451, line 19: phase change instead of change phase

Page 5451, line 22: maximize in June AND minimize in Dec.

Page 5452, line 7: use word dataset instead of database, as the database cannot be long and dense.

Page 5452, lines 12-13: erroneous line break.

Page 5452, lines 16-18: I do not understand the sentence starting with: Globally,Ě

Page 5452, line 22: the fitted line shows the linear trend, not the increase rate (the word rate suggests the time derivative of the trend).

Page 5453, line 22: What do you mean with a problem?

Page 5454, line 6: air has no plural (maybe air masses?)

ACPD

5, S1848–S1854, 2005

Page 5454, line 19: maybe better: Ĕsame altitude range as in our study

Page 5454, line 23: Ĕanomalies there is evenĔ.

Page 5454, line 25: Ĕas IS the caseĔ

Page 5455, line 2: maybe better: Extremes of many variables in the years 1998 and 1999 have been documented in the literatureĔ

Page 5456, line 8: Ĕ 2001 while the longer period 1980-2001Ĕ

Page 5458, line 18: remove the word Baldwin in the bracket before web site

Page 5458, line 19: Ĕwhich is correlated with the NAOĔ.

Page 5459, line 3: Ĕ scatter plots of ozone versus NAO Ĕ

Page 5460, lines 14-15: erroneous line break.

4. References

Cooper, O., A. Stohl, S. Eckhardt, D. D. Parrish, S. J. Oltmans, B. J. Johnson, P. Nédélec, F. J. Schmidlin, M. J. Newchurch, Y. Kondo, and K. Kazayuki (2005): A springtime comparison of tropospheric ozone and transport pathways on the east and west coasts of the United States. J. Geophys. Res. 110, D05S90, doi: 10.1029/2004JD005183.

Fromm, M., et al. (2005): Pyro-cumulonimbus injection of smoke to the stratosphere: Observations and impact of a super blowup in northwestern Canada on 3-4 August 1998. J. Geophys. Res. 110, D08205, doi:10.1029/2004JD005350.

Spichtinger, N., R. Damoah, S. Eckhardt, C. Forster, P. James, S. Beirle, T. Marbach, T. Wagner, P. C. Novelli, and A. Stohl (2004): Boreal forest fires in 1997 and 1998: a sea-

ACPD

5, S1848–S1854, 2005

sonal comparison using transport model simulations and measurement data. Atmos. Chem. Phys. 4, 1857-1868.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5441, 2005.