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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric ozone climatology over Beijing: analysis of aircraft data from the MOZAIC program" *by* A. J. Ding et al.

A. J. Ding et al.

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General comment This is a very well written paper which discusses the climatology of ozone near Beijing as measured through MOZAIC in a clear and well-organized way. The figures are of high quality, the discussion is supported by an extensive list of relevant references and the paper is of general interest. I can without doubt recommend that the paper is published in ACP, provided that a number of comments are considered. My main critic is that the authors for some questions give more firm conclusions than the data material really supports.

Response: We thank the Reviewer #2 for his/her encouragement, and have addressed the comments/concerns.

Response to Specific comments by Review #2

1. The authors should explain how the clustering of trajectories discussed in 3.3.2 was done. Did they use some sort of clustering algorithm or was it only based on the origin (start position) of the trajectories?

Response: We classified the trajectories based on their origins and transport pathways. We will add this in the manuscript.

2. Further to the use of trajectories: Apparently trajectories for 3 arrival times per day (7-8, 11-12 and 15-16) were used in the analyses. I would guess that the trajectories for the same day would normally be fairly similar to each other (for the same arrival heights). Thus, the number of trajectories given in Fig. 10 is not really indicative of the number of independent episodes. Rather these numbers divided by 3 would tell how many individual days this analysis is based on. Thus, one should be careful drawing firm conclusions when these numbers are low, as e.g. the the number of S trajectories in the low and middle troposphere (and W in the low trop). These results are actually based only on approx 4 individual days, clearly too little for a climatological study. In my view the results for the PBL is significant as the number of trajectories/days are larger, whereas for the lower and middle trop, the minor differences seen among the three clusters may well be just a matter of coincidence due to very few episodes. Another complicating factor is the seasonal cycle. When the number of samples is as low as this, the time of year these episodes occured may be the whole reason for the differences seen among the clusters (although a screening using only May-July was indeed used). It would be fine if the authors could comment on this and include it in the discussion, and I think the statements and conclusions drawn form this analysis should be moderated.

Response: We have double checked our data, most of the trajectories in the cluster analysis in fact spread in different days, only a few days had 2-3 trajectories falling in the same day. We agree that the sample number for trajectory analysis is relatively small. In the revised version, we have included all profiles (142 profiles in total) instead of those only obtained on sunny days in selected years (also in response to the Reviewer

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#1's comment). With a larger number of samples, the numerical result has changed a little, but the general pattern is same. We will use the updated result in the revised manuscript. For categories with relatively small sample number (e.g., S trajectories in mid troposphere), we will mention the uncertainty and moderate the conclusion in the revision.

3. Following the previous point, the authors write that: "... reveals that the broad midtropospheric summer maximum over Beijing ... was due to transport of biomass burning plumes from Central Asia and Russia or due to upper-trop/strat sources" Although this is a likely explanation, the amount of data and analyses provided to support this statement is very sparse. Furthermore, one could ask to what extent low-level single trajectories for the summer at this latitude is suited for tracking air mass origins. Presumably convection would be very important, and that is difficult to take into account in this kind of study. It would be good if the authors could include some words about the influence of convection for their results and conclusions.

Response: We will soften the wording on the contribution from biomass burning or STE, and state uncertainty of the trajectories in resolving mixing/convection.

4. Based on the MOZAIC data from two periods, 1995-99 and 2000-05, the authors calculate a time trend of ozone in the PBL of the order of 1 ppbv/year. Estimates of the ozone trends in China is of course of large interest. However, the scattered data through this time period makes the reader wonder how representative and certain this trend estimate really is. The experience from other areas is that the inter-annual variations in tropospheric ozone caused by differences in prevailing meteorological conditions from one year (or summer) to another is so large that it may easily mask the changes induced by changes in anthropogenic emissions and photochemical formation. Without long timeseries (10-20 years of continuous data) it is normally very hard to draw conclusions about the ozone trends based on measurements alone. Figure 1 shows that the 1995-2005 period is dominated by the years 1997, 1998 and 2005 as also mentioned in 3.3.2. The question is then how "normal" in a meteorological sense

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these years were? If 2005 happened to be an extreme year that alone could explain the whole "trend" estimated in the paper. Thus, I think the authors need to be less conclusive regarding the quantification of the ozone trend over Beijing and clearly state that this is based on a few years of data only. It would help if some sort of analyses of the meteorological situations in each of these years' summers is included in the paper. How differed the transport, temperatures, sunshine, general synoptic situation etc during this period? Detailed photochemical modelling would of course be interesting to include to further assess the trend, but that may be beyond the scope of the paper.

Response: This is a good comment. The year-to-year difference in meteorological condition could have important impact on ozone trend, particularly for our MOZAIC data mostly dominated by the year of 1997, 1998 and 2005. We have examined the El Nino index data which indeed show a strong El Nino signal during 1997.6-1998.4. We further compared the annual sunshine hour, temperature and wind flow on the different altitudes, but did not find significant difference for 1997,1998 and 2005 over North China when compared to and the 10-year average. Nevertheless we will point out that more continuous data would be needed to verify the ozone trend derived from our analysis.

Technical comments 1.

The given web link to MOZAIC did not work when I tried it. It may be a temporary error by the server. However, it seemed as the link http://mozaic.aero.obs-mip.fr/web/ worked better.

Response: We will change that.

2. It would be nice to have the site Lin'an (mentioned in 3.3.1) marked on the map. Response: Lin'an Site will be added in Figure 7c.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9795, 2007.