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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source-receptor relationships for airborne measurements of CO_2, CO and O_3 above Siberia: a cluster-based approach" *by* J.-D. Paris et al.

J.-D. Paris et al.

jean-daniel.paris@lsce.ipsl.fr

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We thank the handling Editor for having taken the time to review this paper and to make suggestions for improvement, given the circumstances. Our responses are detailed in the text hereunder, denoted by (*).

Introduction It would be nice to at least mention TROICA in this paper; it might be interesting to compare in a future study the surface measurements with the YAK measurements along the west-east direction. There is also the ZOTTO site at 60N, 90E, where all gases are continuously measured (e.g. Kozlova et al., GBC, 2008)

* The suggested references have been added to the paper. Data from YAK and

TROICA are under comparison for 2008 in the frame of the POLARCAT programme, with an emphasis on O3 distribution.

Section 2.1. Please mention the time of day of the flights; this is important especially for CO2 which has a strong diurnal cycle in the PBL in summer.

*Flights were typically carried on between 0900 and 1700LT. This information was added in the revised ms in section 2.1, as well as precise time of flights for the case studies in section 4. More detailed flight information table is found in other papers (e.g. Paris et al., Tellus B, 2008)

Section 2.2. Backtrajectories are computed every 10hPa (or 100m) change in altitude along the aircraft track. Is this justified given that the parent meteorology from ECMWF does not have such a fine vertical resolution in most part of the troposphere?

*The editor is right that this resolution is higher than the resolution of the ECMWF in most parts of the atmosphere. However, to make ideal use of the ECMWF data, the resolution of the transport calculations should be as good as or better than that of the ECMWF data and the aircraft will also have changed its horizontal position while ascending/descending. Furthermore, with a Lagrangian model, it is sometimes possible to obtain realistic higher resolution than that of the underlying meteorological data, as shown for instance in numerous papers on contour advection. In summary, given enough computational resources, there is no reason why trajectory should not be started at relatively high resolution.

Backtrajectories are calculated for 10 days - what is the scientific rationale behind this limit - why not 15 or 20 days or only 5 or 8 days?

*The choice of 10 days is a trade-off between the need for long time of backward transport (related to the long lifetime of CO2 and CO) and the degraded accuracy of Lagrangian modelling with increasing backward travel time. Also, further back in time than 10 days, the retroplumes always cover large areas which can make the clustering

into particular source regions ambiguous. Eventually, it was found that 10 days provide good results for our clustering algorithm.

Section 3.1. The description of the CO2 vertical profiles is written as if no other in situ measurements exist in the are. However, there is an extensive Japanese aircraft measurement program with vertical profile flights over western Siberia (not sure where it has been published, though), and there are PBL measurements from the ZOTTO tower from 2006 and 2007 (Kozlova et al., GBC, 2008). At least the latter could be referred to. Interestingly, the April 2006 data from ZOTTO are around 390ppm confirming the Fl 1 flight data, however, in September 2006 the ZOTTO tower recorded CO2 values of 380-385ppm, which is higher than the values shown in Figure 4 (Fl 5).

*We included a short discussion on the comparison between Zotino tower measurements in section 3.1 as suggested. We found no reference published of the Japanese flights.

Section 4.2, 4.3 The findings reported here with respect to the fire emissions are difficult to understand. What does the empirical negative correlation between CO and CO2 really imply? There exist a lot of studies in the literature reporting on CO/CO2 emission ratios from various types of fires. Clearly, these are not appropriate here, as the approach somehow integrates over large areas. Only the CO signal has some fire signature, while the CO2 signal is confounded by biospheric uptake in the vegetation elsewhere. Thus the regression values between these tracer found here must be very specific to the setup of the calculation and has no universal implications. I'd revise these sections and possibly remove Figure 12.

*The reviewer is correct in stating that CO/CO2 ratio are not emission ratio as they integrate over large areas. However, as was shown for example in the Paris et al. (2008, Tellus B) paper. the CO/CO2 measured even in altitude by the aircraft ratio carries a signal related to the emission and helps differentiating among various anthropogenic and biomass burning sources of CO. We have clarified our analysis in the revised ms.

Section 5. The found correlations are nice, but it would greatly help the non-specialist reader if the implications were a bit more clearly explained. What do the slopes of ppm sËĘ-1 imply? What is the unit "gridËĘ-1" - I presume 1 "grid" is a 1x1 lat-long area?

*As pointed grid-1 refers to 1x1 grid area. We removed this unclear formulation in the revised ms throughout section 5.

A little bit more information of how the correlation numbers have to be interpreted would greatly help the impact of the paper. Are these numbers only specific to the setup of the calculation in this study?

*We thank the reviewer for this suggestion on improving the impact of the paper and agree that more elements of interpretation are required. We improved the discussion accordingly in the revised ms. These number are not set-up specific, but as the rest of our analysis the generality of our finding are limited by the "snapshot" nature of aircraft campaigns.

Fig. 1: The figure mentions the flights 1-8, but not the flights 9-12.

*Figure has been modified to mention all flights discussed in the text

Fig 6 (and 8 and 10): Add in the caption what is meant by the gray areas.

* This was added in the revised ms

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6207, 2009.