Atmos. Chem. Phys. Discuss., 8, S5268–S5272, 2008
www.atmos-chem-phys-discuss.net/8/S5268/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Airborne in-situ measurements of vertical, seasonal and latitudinaldistributions of carbon dioxide over Europe" *by* C. Gurk et al.

Anonymous Referee #4

Received and published: 25 July 2008

General comment

This paper presents CO_2 data from the 7 aircraft campaigns during the SPURT project to describe the seasonal and latitudinal variations of vertical profiles from the boundary layer to the lowermost stratosphere over Europe. It is an important dataset, because it has been noted recently that more CO_2 vertical profiles from aircraft observations will result in improved estimates of global carbon cycling (Stephens et al., 2007, Science, 316, 1732-1735). Since the CO_2 data around the tropopause region during the SPURT project was already reported by the previous paper (e.g. Hoor et al., 2004), the vertical distributions of tropospheric CO_2 and their latitudinal and seasonal changes should be

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

more focused on in the present study.

Specific comments

P7316, line 8: Not only CO_2 uptake but also ecological respirations drive the CO_2 seasonal cycle in the atmosphere?

P7316, lines 10-11: It is not clear what is meant by 'resulting in ..... during the summer', because it is not logistically connected to the former phrase in this sentence.

P7316, lines 20-21: Not only fossil-fuel combustion but also deforestation is responsible for the CO_2 increasing trend?

P7317, line 4: I think it would be useful here to make a short comment on how the vertical CO_2 distributions from aircraft observations are significant, referring to a recent publication (Stephens et al., 2007, Science, 316, 1732-1735).

P7317, line 27 - P7318, line 10: I am not sure whether these descriptions are necessary, because the principles of the CO_2 analyzer (LI-6262) are already known for the readers in this field.

P7318, line 17: 5% uncertainty for the H_2O measurements is negligible for the correction of CO_2 concentration? Did you compare the CO_2 measurements before and after drying to validate your method without drying procedure.

P7319, lines 1-2: When the pressure is regulated by the PC (Pressure Controller), indicate it in this sentence. In addition, precision of the pressure regulation should be also described, because it would be helpful to know its impact on the CO_2 measurement error.

ACPD

8, S5268–S5272, 2008

P7319, lines 9-10: It would be helpful to know how many and what kinds of standards use to get the multi-point polynominal calibration function. In addition, it is not clear whether the calibration function changes with time.

P7319, line 14: It would be helpful to describe the cited paper for the NOAA reference standards used here.

P7319, lines 16-17: It would be important to know whether the temperature of air sample in the sample cell is similar to that of the standard gas in the reference cell when the analyzer is mounted in the sealed box. In addition, it would be important to know whether the temperature of air sample is kept constant.

P7319, line 19: The number of the digits for 0.055ppm is beyond the precision limit of the NDIR.

P7321, line 4: It would be interesting to know whether the vertical profiles during the landing and take-off are similar each other, although they largely depend on the time lag between the landing and take-off.

P7321, line 12: It is not clear what is meant by 'from take-offs ..... the final descents'.

P7321, line 11: I think it would be useful here to make a short comment on relatively larger 1σ-standard deviations around 6 km in high latitudes during Aug/02 and Oct/02, as shown in Figure 3.

P7321, line 17: I think the phrase of 'a gradient of about -0.2ppm/degree latitude' is not appropriate, because only two latitudinal bands are compared. Suggest using 'latitudinal difference' rather than 'gradient'.

P7322, lines 5-9: Since the discussion in this paragraph is too general, this should be rewritten to make this easier to follow based on the results in this study.

P7322, line 20: It would be interesting to know whether the local enhancements of CO_2 are similar to those of other trace gases such as CO observed simultaneously during

ACPD

8, S5268–S5272, 2008

the same SPURT project (Fischer et al., 2006).

P7323, lines 1-3: I am not sure whether this conclusion is necessary, because it is general statement.

P7323, lines 15-19: I found this paragraph a little confusing. Since the CO_2 temporal trend during this period of time is around 2ppm/yr, larger annual increase rates of 4 or 3.7ppm in this study is thought to be affected by the larger variability due to the local emissions as well as the snapshots of the observational data. Some comments can be rewritten to make this easier to follow.

P7323, lines 20-26: The vertical decay of the seasonal cycle should be compared with the results from other studies (e.g. Nakazawa et al., 1993, Tellus, 45B, 258-274) and more discussed. It is noted that the phase shift of the seasonal cycle with altitude is not well captured in this study because of the limited number of the flights.

P7324, line 12: It is not clear what is meant by 'likely a different transport pathway'.

P7324, lines 13-15: Some sentences can be added to make it easier to follow the discussions reported by the previous SPURT studies. In particular, it would be helpful to know the seasonal change of the cross-tropopause transport.

P7325, lines 3-4: I think it would be useful here to make a comparison with the observed CO data during the same SPURT project (Fischer et al., 2006) on how the local anthropogenic emissions affected the increased CO_2 in the CBL rather than MBL.

P7326, lines 5-11: This section and Figure 7 are interesting but also subject to a number of questions, especially the conclusion of 'most likely due to STT'. The altitude gradient in at least troposphere is significantly influenced by the large seasonal increases and decreases of CO_2 in the surface air driven by photosynthesis and ecosystem respirations on the land, because its propagation with the decay of the seasonal amplitude from the surface to the upper troposphere is clearly observed in Figure 5 in this paper as well as other studies (e.g. Nakazawa et al., 1993, Tellus, 45B, 258-274). It

ACPD

8, S5268–S5272, 2008

Interactive Comment

indicates that not STT but strong sources and sinks of CO_2 in the land biosphere are responsible mainly for the seasonal change in the altitude gradients of CO_2. The relationship between the stratospheric O_3 fraction and delta CO_2 as seen in Figure 7 may be apparent, because the both parameters are roughly related to the distance from the tropopause. When the authors quantitatively examine the STT impact described in this study, it may be worth testing the sensitivity of altitude gradient to the CO_2 seasonal pulse observed in the stratosphere. At the moment I feel that the present conclusion in this section without the sensitivity test is overstated.

P7327, lines 4-6: This is also overstated as pointed out above.

Figure 1: The full names of MFC and PC should be indicated in the figure or the figure caption. In addition, it is not clear what is meant by the dashed line.

Figure 3: It would be helpful to know how the tropopause altitude is determined. This should be described in the figure caption or text.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 7315, 2008.