

Interactive comment on “Variability and budget of CO₂ in Europe: analysis of the CAATER airborne campaigns – Part 1: Observed variability” by I. Xueref-Remy et al.

I. Xueref-Remy et al.

irene.xueref@lsce.ipsl.fr

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The authors thank Referee 1 for his/her constructive comments. We reply here below to each point.

General comments:

* Referee 1: The study reports very interesting results about the CO₂ concentration distribution in Western and Eastern Europe through two surveys carried out on May 2001 and October 2002 from 300 m up to 4000 m. As far as this referee knows, not such wide campaigns studying the distribution of CO₂ have been previously made in Europe. The study characterizes the distribution of measurements and links the ob-

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served horizontal gradient with the air origin through a Backtrajectories Analysis. Furthermore, the second campaign reported in the manuscript sampled carbon monoxide, providing an alternative way to national inventories to characterize the CO₂ signal related to combustion processes through the CO:CO₂ ratio. Moreover, the authors study the variability in the boundary layer and in the free troposphere as a key parameter to test the proper representation of the vertical gradients in atmospheric inversion models. However, even if most of the data needed to exploit the measurements are included, the text fails at drawing conclusions from them. Some key questions raised by the authors are not dealt properly (for example Section 6). Furthermore, some pieces of the manuscript appear to be out of context, such as Section 2.3.4.; which reports the instrument used to analyze the Radon-222 concentration but any of the results are commented latter in the paper; or Figure 9 that only the way how it has been calculated is explained in the text and not the conclusions extracted from it. Other aspect that I am concerned about relates to the back-trajectories analysis dealt in Section 3.2 and 3.3. The back-trajectory analysis is used to explain the differences of the measured CO₂ mixing ratios along the campaigns in different regions/countries. However, any mention to the change of the altitude flight is done when comparing the data (when a change of altitude took place). I would strongly recommend the authors to revise the aforementioned before its final publication. Furthermore, it would need a throughout reading to correct improper English sentences.

Answer: We have made an effort to draw deeper conclusions to our study. Section 2.3.4 has been moved to the companion paper. The role of altitude change has been further discussed.

ABSTRACT

* Referee 1: Page 5666, line 11. Add “(±1standard deviation)” after “the mean CO₂ concentration”

Answer: this has been done.

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* Referee 1: Page 5666, line 13 and further occurrences: air masses (as two words).

Answer: this has been done.

* Referee 1: Page 5666, lines 13-14. The authors talk about pollution when measured high CO₂ concentrations. As CO₂ is not properly an atmospheric pollutant, I would suggest rewrite the sentence as “air masses get the signal of anthropogenic fluxes. . . from Benelux and Western Germany”

Answer: this has been done.

* Referee 1: Page 5666, line 20. I would remove “the so-called dirty thirty from WWF” as this given name is irrelevant to the description of the study in the Abstract section.

Answer: this has been done.

* Referee 1: Page 5666, line 28. Page 5667, lines 3, 8. I would remove the acronyms of the stations. The acronyms are not defined in the abstract and since they are mentioned further in the text, I would just keep the type of sites (ground stations; mountain sites. . .).

Answer: this has been done.

* Referee 1: Page 5666, line 28. Change “stations located near the ground” by “stations sampling within the PBL”

Answer: this has been done.

* Referee 1: Page 5667, line 1. Add “regional” after “local”

Answer: this has not been done, as the term "local regional" seems unclear to us.

* Referee 1: Page 5667, lines 3-4. Rewrite “Stations located several 100 km away...in the free troposphere” for “Stations separated by hundreds of km differ few ppm in their measurements indicating the existence of a gradient in the FT.”

Answer: this has been done.

* Referee 1: Page 5667, line 9. Is the variability expressed in terms of standard deviation? Please, stated how is calculated the reported variability.

Answer: Yes, the variability is expressed in terms of one standard deviation (1-sigma. This has been precised in the text.

* Referee 1: Page 5667, line 19-21. I would remove the last sentence as it is not relevant for the present study (maybe the authors want to move the sentence at the end of the Introduction or Conclusion section where they mentioned the model study done using the measurement reported in Part 1).

Answer: this has been done.

1. INTRODUCTION

* Referee 1: Page 5668, line 13. One of the main reasons why the inverse models are still uncertain is because most of the studies are based on measurements done on remote and marine sites, far from where terrestrial fluxes are taking place.

Answer: this has been incorporated into the text.

* Referee 1: Page 5668, line 26. I would remove “In a recent paper” as it is already 3 years old.

Answer: this has been done.

* Referee 1: Page 5669, line 2. Define what FT stands for and not in line 13.

Answer: this has been done.

2. DESCRIPTION OF THE CAMPAIGNS AND OF INSTRUMENTATION

* Referee 1: Page 5670, line 16. List which meteorological parameters were measured.

Answer: this has been added.

* Referee 1: Page 5670, line 17. Is there any reason why the flight paths are different for

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both surveys? Which were the criteria to choose these flight paths in both campaigns?

Answer: The differences in the flight patterns can be explained by the existence of three constraints: 1) CAATER 1 was coupled with another campaign that required flights above these Atlantic ; 2) during CAATER 2, there was a strong pressure of the community to fly over the observing tower of Hegyhatsal, and over Thüringen where ground measurements and regular flights were undertaken ; and 3) for both campaigns, the plane was based in Germany and there were fundings for a few hours of flight only. The combination of constraints 1)+2) for CAATER 1 and constraints 1)+3) for CAATER 2 lead to differences in the flight patterns of the campaigns.

* Referee 1: Page 5671, line 27-28. I would move them at the end of page 5670. A sentence why the authors are also using other in-situ stations should be mentioned here.

Answer: this has been done.

* Referee 1: Page 5672, line 5. Add the mean altitude for 850 hPa.

Answer: this has been done.

* Referee 1: Page 5673, line 8-13. It is confusing how the calibration gases are measured. I guess that the authors would say that the last minute of acquisition is kept to build the calibration curve. When the outside air is analyzed, it is also the last minute kept to calculate the actual concentration?

Answer: this has been written again. Indeed, only the last minute is kept for calibration gases; but for air, 1 Hz data are produced.

* Referee 1: Page 5672, line 25. LSCE is not defined before in the text (either in the author's affiliation addresses).

Answer: this has been done in the author's affiliation addresses.

* Referee 1: Page 5672, line 25. I am not convinced that a figure of the CO2 instrument

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is needed. It seems, however, that the analyzer has been described previously in Filippi (2002).

Answer: the document from Filippi is only a conference paper, thus we have decided to add a figure of the CO₂ instrument in this article.

* Referee 1: Page 5673, line 10. Add the point in the uncertainty concentration of the High calibration gas.

Answer: this has been done.

* Referee 1: Page 5673, line 14. How often calibrations are done?

Answer: one calibration every 30 minutes. This is explained in the text.

* Referee 1: Page 5673, lines 19-26. These lines appear out off-context. I would suggest moving them at the last paragraph of the Introduction section or at the beginning of the results.

Answer: these lines have been removed, since they were repeating what is already written at the end of the Introduction section.

* Referee 1: Page 5673 Line 23. It is confusing the expression “in the following at two places”. Please, rewrite it and be clearer.

Answer: this has been done.

* Referee 1: Page 5674, line 5. “The CO₂ concentration was measured by gas chromatography in LSCE”.

Answer: this has been done.

* Referee 1: Page 5674, line 20. Remove “due to rotation of the filter wheel”.

Answer: this has been done.

* Referee 1: Page 5674, line 23. Write the name of the IR detector rather to just state

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the chemical abbreviation (PbSe).

Answer: this has been done.

* Referee 1: Page 5675-6. I would remove Section 2.3.4 as the Radon-222 measurements are not reported in the present study.

Answer: this section has been removed and added to the companion paper (Part 2).

3. ORIGIN OF SAMPLED AIR MASSES.

* Referee 1: In this section is lacking a description where/ at which altitude backtrajectories are computed. Which is the longitude/latitude range where the backtrajectories presented in Figures 4, 5 and 6 are representative for? Are the release points defined as a single point? Or it is defined as a range of longitude/latitude? Which type of backtrajectories are used? Please, describe better how backtrajectories were computed.

Answer: We propose to add the following text p.5677 line 6: "In order to investigate the origin of the sampled air masses, we computed for each flight, 5-days backtrajectories at different measurement points of the flight path. The HYSPLIT-4 (Hybrid Single-Particle Lagrangian Integrated Trajectory) was used to compute these backtrajectories (Draxler and Hess, 1998). Four-dimensional (x,y,z,t) gridded meteorological fields from NOAA/NCEP (National Centers for Environmental Predictions [<http://www.ncep.noaa.gov/>]) global reanalysis data were used to drive HYSPLIT-4 (every 6h, 1°x1° horizontal resolution, 14 vertical levels). Each trajectory was calculated as the time integrated advection of a single particle. The integration time-step can vary during the simulation. It is computed from the requirement that the advection distance per time-step should be less than 75% of the meteorological grid spacing. This linear integration method is common (e.g. Kreyszig 1968) and has been used for trajectory analysis (Petterssen, 1940) for quite some time. Advection is computed from the average of the 3D-velocity vector for the position at time t and the position at time t-1. The accuracy of the model has been quantified by testing the model trajectories against

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balloon data : the difference was about 10 to 20% (Draxler, 1996).

Kreyszig, E.: Advanced engineering mathematics, 2nd Ed. J. Wiley and Sons, New York, 898p., 1968. Petterssen, S.: Weather analysis and forecasting, McGraw-Hill Book Company, New York, 221-3, 1940.

* Referee 1: It seems that the altitude of the flight changed along the campaigns. Changes in the altitude are important as there is a change of the measured concentration in altitude linked to the vertical mixing and then, the vertical spread of surface fluxes. Linked to this point, it would be useful to have an estimation of the boundary layer height along transects where the results are reported (mainly Section 3.2 and 3.3) as it would help the reader to know whether the measurements were taken within the BL or in the FT.

Answer: The authors agree that it would have been very useful to have the boundary layer height along the flight paths. However, it was not possible to compute this parameter from the data along the transects, and no radiosounding data are available at the exact time and location of the flights. The authors have tested boundary layer heights inferred from ECMWF numerical weather prediction system against boundary layer heights inferred from the CAATER observed profiles. The differences were comprised between 25% and 90%. These results are further supported by work of Gerbig et al. (2008) who compared boundary layer heights inferred from radiosounding data and diagnosed in the ECMWF model. The differences were about 40% during daytime, and 100% during nighttime. Thus, we decided not to use modeled heights from ECMWF, given their large uncertainty. We are conducting an extensive comparison on a large dataset of boundary layer heights obtained from vertical profiles recorded over the Orleans forest in 2006-2008 and modelled by ECMWF (one profile every 5 days on average) in order to better understand the causes of the discrepancies. In the revised manuscript, when it was possible to infer without ambiguity and approximately the PBL height during a transect from the CO₂ variability along that transect and some vertical profiles (recorded close to the observing point), we indicated in the text whenever the

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airplane was sampling in the PBL or in the FT (ex. lines 11 and on p.5677, see here below).

* Referee 1: Linked to that point, for the flight done on the 23th May (Figure 4a), there is a clear decrease of the CO₂ concentration in the region from -2_ to 2_E (_360ppmv) compared to the region from -4 to -2_E (_373 ppmv), that is coincidental with a change of the altitude of the flight (the altitude of the flight was between 500 and 1000 masl in the -2° to 2°E region whereas it was between 0 and 500 m in the -4° to -2°E one). Was the vertical structure of the atmosphere influencing on the CO₂ horizontal distribution?

Answer: Indeed, no, but we found a mistake in our interpretation of the May 23, 2001 flight (no anthropogenic-influenced emissions there). In order to correct for this mistake, and to better explain the observed "jump" in CO₂ concentration notified by Ref.1, we suggest to replace lines 11-19 page 5677 by the following text: " On May 23, 2001 (Figure 5a), between 2°W and 4°E, the back-trajectories indicate a continental origin from the north-east, with air masses being advected in the boundary layer (< 2000 meters) and carrying low CO₂ values of ≈ 360 ppm. As the aircraft moved west of 2°W, the sampled air mass reached values in the range of 372-375 ppm. Indeed during that day, from ECMWF reanalysis, we know that there was no special feature in the vertical structure of the atmosphere that can explain this difference in concentration. Backtrajectories show that between 2°W and 2°E, airmasses were advected from the east-south-east, and from the north-east between 2°E and 4°E before landing in Brest. It is very likely that the depletion of CO₂ observed between 2°W and 2°E on May 23, 2001 was due to advection of air formerly exposed to terrestrial CO₂ uptake. Indeed two days later (see Fig.5c) we encountered a similar CO₂ depletion between 2°W and 2°E, also below 2000m, around midday and with similar pressure and wind conditions than on May 23. For this flight, simultaneous in-situ CO₂ and 222Rn measurements allowed us to identify the role of terrestrial CO₂ uptake over France at that period (Xueref-Remy et al, 2010, part 2). Between 2°E and 4°E, the HYSPLIT backtrajectories show that an oceanic airmass was sampled, whose CO₂ concentration was

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close to the MBL value (374.50 ppm)."

And to replace as well lines 25-29 p.5677 and line 1 p.5678 by the following text: " On May 25 (Figure 5c), the air mass was first oceanic (374 ppm). Moving eastwards between 3°W and 1°E, sampled continental air masses gave CO₂ concentrations of approximately 360 ppm (biospheric-influenced values, that are similar to those sampled during May 23 on roughly the same route westwards : see above), then oceanic air, followed again by anthropogenic emission plumes over the Ruhr area with CO₂ ~ 380 ppm. The signal of this high emission region of Europe when compared to the 'biospheric' minimum further east is of the order of 20 ppm in the whole boundary layer."

* Referee 1: Another example of the change of the CO₂ concentration in the altitude was presented on the 26th May 2001 (Figure 4d). For that day, in the first paragraph of page 5678 is stated that "air masses coming from the west and north-west, which were more exposed to more urban areas are associated with CO₂ higher by 25 ppm above this minimum". I am not really convinced that this difference is only related to the advection of polluted air masses as the backtrajectories show that air masses were always above 2000 m, then, uncoupled with the surface fluxes. In these cases it is difficult to make a clear statement that the CO₂ variability is only related to changes of the air masses origin rather than CO₂ concentration sampled at higher altitudes have more concentration compared to air masses sampled below, close to vegetation which is uptaking CO₂ by photosynthesis during the growing season/ daytime when the campaign was undertaken.

Answer: Indeed on the timeseries of Fig.5d one can see that the peak of CO₂ occurred when the aircraft was flying at low altitude, below 400m, in the PBL. The HYSPLIT backtrajectories of Fig.5d further indicate that during this flight (corresponding to the easternmost observation point of the campaign) airmasses came from the West below 500m, with an origin that can be approximately traced to Northern Germany and Denmark. It was surely not clear enough, that we were talking about the point

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located around 14°E (not 12°E). To better explain the CO₂ variability encountered during that flight, we suggest to replace lines 2 to 8 page 5678 by the following text : " On May 26 (Figure 5d), we measured the lowest CO₂ concentrations (355 ppm) of the whole campaign at 12°E in an air mass that clearly came from the east. Note that the easterly flow associated with this absolute minimum CO₂ values corresponds to air masses advected into the boundary layer, and thus directly exposed to continuous biospheric uptake. In contrast around 14°E, an air mass coming from the west and north-west was sampled. Backtrajectories indicated that this air mass is exposed to anthropogenic emissions, and contains CO₂ values 25 ppm above this minimum level. Note that all these measurements were recorded in the PBL at altitudes lower than 400m a.s.l. ; backtrajectories show that the high-CO₂ air mass sampled at 14°E was advected from the north-west at altitudes below 500m, with an origin that can be approximately traced to anthropogenic emissions over Northern Germany and Denmark. On the other hand, airmasses coming from altitudes higher than 1700 m, with tropospheric air decoupled from surface fluxes, are associated to CO₂ concentrations close to the MBL value (374.50 ppm)."

* Referee 1: Page 5677, line 9. Remove "(37%)" as the percentage is stated before.

Answer: this has been done.

4. RELATIONSHIP BETWEEN CO AND CO₂ IN POLLUTED AIR MASSES

* Referee 1: Again, as the authors are mainly reported the CO₂ measurements with the aim to understand the underlying flux, I think that it is inappropriate to talk about polluted air masses. I would suggest titling the Section "RELATIONSHIP BETWEEN CO AND CO₂ IN ANTHROPOGENIC INFLUENCED AIR MASSES". Other aspect that I am concern is that the authors are comparing the CO/CO₂ ratios obtained from measurements with the annual national inventories. In one hand, anthropogenic fluxes show a strong diurnal cycle that is smoothed with the annual inventories. In the other hand, authors are comparing the national inventory even only a small region of one

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particular country is sampled. If the authors have no access to inventories with more temporal resolution, they can make a weighted mean of the countries that a sampled air mass is influenced for and maybe they can match better the observations with the inventories.

Answer: The title of section 4 has been modified according to Ref.1's suggestion. The authors agree that the annual inventories mask the emission ratios variability, and that the CO/CO₂ emission ratio has variability. High resolution inventories are recently available at least in France and Germany, and we are conducting new studies using these tools. Furthermore, as backtrajectories only give a qualitative but not quantitative information on the role of each region/country in the footprint of the sampled air mass, it was very difficult to properly define the emitting region and compute a reliable weighted CO/CO₂ emission ratio from inventories.

* Referee 1: To all R2, an estimation of its significance is lacking (add p-values to all R2).

Answer: the p-values have been added in Table 5, and we have added the following sentence p.5681 line 23: "To all R2, we computed the associated p-value (Table 5): in all cases the correlation significance is higher than 99%".

* Referee 1: Page 5681, lines 5-10. Remove sentences from "In pollution loaded. . . (Palmer et al., 2006), after (e.g. Levin and Karstens, 2007) in page 5680, line 26.

Answer: this has been done.

* Referee 1: In lines 26 (page 5680) and 11(5681) the adjective complicated appears twice. Remove this adjective and explain better the constraints of the application of this method.

Answer: this has been done.

* Referee 1: Page 5682, lines 5-11. Why referring to the NDVI maps and not the terrestrial fluxes shown in Figure 1 when talking about the photosynthetic activity?

Answer: this has been done.

* Referee 1: Page 5682, line 28. What do you mean “which can be >0 in the North and <0 in the south”)?

Answer: this has been clarified, the text is now: “which can be >0 in the northern part of Europe and <0 in its southern part ; Figure 1)”.

5. COMPARISON OF AIRCRAFT WITH SURFACE STATIONS MEASUREMENTS

* Referee 1: Page 5683, lines 11-13. How do you compute the mean concentration for the surface stations? Is the mean concentration calculated for the entire time of the surveys, that is, for CAATER1, from the period 23-26 May taking only the midday values? Or just taking into account the time when the aircraft was flown close to the station? Maybe a better approach would be calculating the mean CO₂ concentration for the surface sites only during the period when the aircraft observations could match in time and space. Following this approach, the early morning and night values should not be discharged whenever the aircraft observations were carried out simultaneous. Like this, a better picture of the 3D CO₂ distribution in Western Europe would be attained and the aircraft and ground stations more comparable.

Answer: As it is written p.5683 lines 11-12, we computed means on afternoon (12h-18h UTC) values only. We suggest to add the following information in the manuscript: "In the morning, most of the time, the PBL is growing and encroaches air from aloft while losing CO₂ accumulated by respiration the former night (Gibert et al., 2007), making CO₂ concentrations more variable. The PBL gets well-mixed and reaches to a relatively stable height only around the middle of the day. The footprint of the stations is thus better defined with afternoon values, and more adapted to do a comparison with aircraft data."

* Referee 1: Page 5683, line 25 and latter occurrences. Why now talking about ABL? It would be better to keep consistency through all the manuscript and kept the defined

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PBL.

Answer: this has been corrected through all the manuscript.

* Referee 1: Page 5683, line 24. As the Figures are showing the CO₂ measurements, I would suggest changing “show a strong variability” by “shows a large range”. I would also add the range of CO₂ measured mixing ratios in the PBL and in the FT for both campaigns in the text.

Answer: this has been done.

* Referee 1: Page 5684, line 15. Change “profil” by “profile”.

Answer: this has been done.

* Referee 1: Page 5684, line 17. Change “has encountering airmasses...West” by “was sampling air masses coming from the South, East and West as well.”.

Answer: this has been done.

* Referee 1: Page 5684, line 22. Add “s” to “observation”

Answer: this has been done.

* Referee 1: Page 5684, lines 25-26. Remove the first sentence and start the paragraph “Both PUY and SCH stations are located on the top of mid-elevation mountains...”

Answer: this has been done.

* Referee 1: Page 5685, line 3. Change “polluted” by “influenced by the valley processes”

Answer: this has been done.

6. ANALYSIS OF THE VERTICAL VARIABILITY

* Referee 1: Page 5685, line 21. Change “comparing” by “compared”

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Answer: this has been done.

* Referee 1: Page 5685, line 24. Reported the mean and standard deviation values for CAATER 2 with the same significant numbers as CAATER 1 (two decimals are given in the first campaign, only one in the second). Same in page 5686, line 2, give the CO2 jump with two decimals.

Answer: this has been done.

* Referee 1: Page 5686, lines 6-11. Why the air masses are here separated depending on its origin from the LMDZ model and not from the HYSPLIT analysis presented in the current study?

Answer: The HYSPLIT-4 analysis in section 3 is based on back-trajectories, while in section 6 we were interested in fetch areas that LMDZt was capable of producing.

* Referee 1: Page 5686, line 11. Add "(Figure 12)" after CAATER 1.

Answer: this has been done.

* Referee 1: Page 5686, line 11. I would suggest the "CO2 range" rather than "the CO2 variability" as the authors are afterwards stating the range of the measured values and not any estimation of the variability.

Answer: this has been done.

* Referee 1: Page 5686, line 11. Change "high" by "large".

Answer: this has been done.

* Referee 1: Page 5686, line 23. Add "(Figure 13)" after CAATER 2.

Answer: this has been done.

* Referee 1: Page 5686, line 23. I would suggest the "CO2 range" rather than "the CO2 variability" as the authors are afterwards stating the range of the measured values and not any estimation of the variability

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Answer: this has been done.

* Referee 1: Page 5686, line 29. When talking about an advanced fall in the west part in Europe compared to the East, why not contrast this hypothesis with the surface fluxes show in Figure 1? From the NEE map on Figure 1e, it is observed that South/West Europe fluxes are $-0.0\text{gCm}^{-2}\text{d}^{-1}$, whereas positive fluxes are shown in Eastern Europe.

Answer: We agree with Referee 1. We suggest to add the following text: “However, this hypothesis does not match with the flux maps given in Fig.1, that indicates on averaged higher fluxes in the East than in the West of Europe. Also, it does not fit with the NDVI maps as well, given in Annexe 1”.

* Referee 1: Page 5687, lines 2-5. The same about when talking about CO₂ emissions. Why only comment the national inventories from UNFCC? Figures 1 c and 1f show the anthropogenic fluxes (with the NEE). Why not discuss this fact using the provided information?

Answer: We agree with Referee 1. We suggest to add: “Furthermore, the fluxes shown on Fig.1 do not match with this hypothesis”.

* Referee 1: Page 5687, lines 10-15. I don't see the point that to understand a measured gradient in October 2002 more regular flights are needed and then, the CARBOEUROPE network is essential to understand this gradient. Do the authors think that this gradient is persistent through all seasons? I think that in the current study the authors present a huge amount of information (meteorological parameters, NEE, anthropogenic and oceanic fluxes; backtrajectories analysis, etc.) and they should be able to point the observed gradient. Which are the mean fluxes in that area? How is the vertical stability?

Answer: The authors agree with this point, and have removed the sentence about CARBOEUROPE flights. We have made a deeper analysis on the meteorological conditions during which the different profiles have been recorded. We suggest to add

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the following sentences at the end of that section: “Indeed, from ECMWF reanalysis data, we inferred that some convection activity occurred in France on Oct 2 along the flight path to Orleans: this can explain that the dark-blue profiles are straight, since the air was well-mixed likely containing a mixture of CO₂ biospheric emissions from the ground and CO₂ emissions advected from the Rhone valley (blue footprint on Fig.13), thus charged in anthropogenic CO₂ (CO in the range of 130-160 ppm). Also, the green profile, recorded above Thüringen is straight. There was some convection developing in this region, meaning that likely the air was well-mixed and influence by anthropogenic CO₂ emissions (CO again in the range of 130-160 ppm) as well as a priori less-concentrated CO₂ signals from the biosphere activity advected from Germany (green footprint on Fig.13). The shape of the turquoise profiles and even more of the red profiles is very different, with a marked depletion in the mid-PBL. No convection activity could be identified at these locations and times. The CO signal reaches values as low as 100 ppb, thus, excluding anthropogenic contribution. The airmass footprints are continental (east Germany for the turquoise profiles, Central Europe for the red profiles), excluding as well oceanic signal contribution. It is thus likely, that there was some biospheric sinks still acting in east Germany and even in a stronger manner in Central Europe at that time. Once again, model fluxes do not relate this point, but biospheric fluxes are not always properly modelled as we discuss in the companion paper (Xueref-Remy et al., part 2, 2010). It would be interesting in the future to conduct new airborne campaigns to assess if the observed gradient is always present at the fall season, or if it was only punctual. Using tracers such as CO, Radon 222 and carbon isotopes would also help to discriminate the role of anthropogenic, biospheric and oceanic sources and sinks.”

CONCLUSIONS

* Referee 1: Page 5687, line 25. I don't know if averages of CO₂ measured in both campaigns are comparable since different flight paths were followed. Maybe a sentence of the mean surface fluxes of the underlying paths should be mentioned.

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Answer: We propose to add the following sentence p.5687 line 18: “Although the aircraft paths were slightly different during CAATER 1 and CAATER 2, the campaigns give a good picture of CO₂ concentration variability over Europe during one Spring and one Fall”.

* Referee 1: Page 5688, lines 9. A “c” is lacking in bac-trajectories.

Answer: this has been done.

TABLES

* Referee 1: Table 4. It lacks “_C” in the Temperature row for the CO analyzer

Answer: this has been corrected.

* Referee 1: Table 5. It would be nice to have an additional column with the “mean” inventories slopes for each flight.

Answer: It seems to us that it would no be accurate to calculate a mean inventory slope for each flight, because the CO₂-CO correlation does not last during the whole flight duration, but only during special events lasting some minutes.

FIGURES

* Referee 1: Figure 1. “(for the days of the campaigns)”

Answer: this has been done.

* Referee 1: Figure 2. Last sentence in figure caption “Longitude (horizontal scale) is given in _E and latitude (vertical scale) in _N.”

Answer: this has been done.

* Referee 1: Figure 11. CO₂ in subscript.

Answer: this has been done.

* Referee 1: Figure 12. A way to express the y-axis units for the left panel would be

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“z/zi”. Right panel is missing axis labels.

Answer: we have modified the y-axis label for “Altitude / PBL height” as we used that label in Fig.11. Axis labels have been added on the right panel.

* Referee 1: Figure 13. CO₂ in subscript in the left panel x-label. Right panel is missing axis labels.

Answer : this has been corrected.

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