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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessing the regional surface influence through Backward Lagrangian Dispersion Models for aircraft CO_2 vertical profiles observations in NE Spain" *by* A. Font et al.

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The authors want to thank the anonymous reviewers for their critical but constructive comments about the manuscript. The authors would like to rewrite the manuscript following the reviewer's suggestions in order to better highlight the overall importance of results and findings and to make the manuscript suitable for publication in ACP. A general comment about the reviewer's comments is first presented, followed by a point-by-point answer.

Yours sincerely,

A.Font in behalf of all co-authors

General comment The main objective of the paper is to describe the transport properties within the Ebre watershed (or main Ebre valley) based in the definition of the "footprint" concept (or Potential Surface Influence, PSI) that might afterwards explain the CO2 differences observed in a network of atmospheric sites. The PSI in the manuscript is described only as a property of transport, independent of the surface fluxes.

In the manuscript, the main regional surface influence area (or PSI) of a single atmospheric site is assessed by Principal Component Analysis (PCA). The variables used in the PCA matrix (with n-dimensions; i-pixels) are the different gridcells defining the PSI from the 51 simulations shown. The observations (j, from 1 to 51) are the 51 simulations described by the residence time (Rt) in each gridcell (i), that is (Rt)i,j. The objective of the PCA is to reduce the number of pixels defining the PSI through new independent variables (axis 1, 2, etc. or also called PC1, PC2, etc.). The variability explained by each principal component (in our study, PC1 and PC2) comes from the variability of the residence time in each pixel configuring the regional influence. The unit of the color scale is an arbitrary unit; it shows the value of each observation regarding to the axis defined by the new variables.

When defining a network of atmospheric observations, it should be assured sample the main land uses of the region; and cover the largest area possible. A description of the main land uses around each measurement site belonging to the aircraft network would be given to explain the localization of the measurement sites. The discussion is centered in the concept of watershed as the ecological unit to study nutrients and energy fluxes. The study of regional biogeochemical cycles and fluxes should be assessed in terms of watershed (Likens et al., 1971) which is responding to different climate challenges (i.e. ecophysiological humidity distribution and drying issues). The tridimensional sampling as proposed by the CAS, assures the overlapping and the spatial coverage of the regional scale (in our specific case, the Ebre watershed). However, the size of the tridimensional sampling could differ depending on the size of the watershed, and it should be specifically assessed.

The conclusion shown in the manuscript about the reduction of the PSI overlapping in distance would seem trivial but it has been previously highlighted in the literature. Gerbig et al (2009) stated the dominance of the near field contributions to daytime mixing ratios of CO2 for inversion studies. A small bias in the assumed flux in the near field can cause a large bias in the modeled mixing ratio that can be compensated using multiple monitoring sites in a network. Matross et al. (2006) highlighted that aircraft data and/or towers with overlapping footprints in space and vegetation class will be needed to provide reliable regional CO2 budgets. Lauvaux et al (2008) found an uncertainty reduction of 30% for 4 day flux averages at a spatial resolution of 8 km from an inversion study using 2 tall towers and aircraft transects within the CERES domain. Then, the PSI overlapping reduces the uncertainty in the inversion studies, strengthening and reducing their uncertainty. In the manuscript, a relation of the decrease of the PSI overlap with distance in the Ebre valley is given. The percentage of overlapping chosen in a observational network design should be investigated specifically depending on the inversion system and its uncertainty.

In addition to data of PSI, it would be included in the manuscript the concentration observed in each vertex of a CAS and it would be compared with the PSI overlapping for that day. This comparison would highlight the regional influence versus local one in terms of CO2 concentration.

The authors agree with some lack of reading fluidity in the paper due to the large number of acronyms, permutations of altitudes, sites, seasons, etc. The authors will shorten the paper, just keeping those parameters needed to understand the main objective of the paper, summarizing the other parameters in tables or supplement material (tables or plots in appendix).

Answer to comments of anonymous Referee #1 This paper constitutes an in-depth assessment of transport and surface influence properties using a Lagrangian particle dispersion model (LPDM) in the frame of a specific aircraft sampling strategy. The sampling strategy and first results were introduced in a precedent paper by the same au-

thors (Font et al., 2008). So-called “Crown aircraft sampling” (CAS) aims to investigate the distribution of CO2 at the mesoscale in order to allow characterization of surface exchange. The sampling strategy consists in weekly flights during which the lower troposphere is sampled along the three vertices of a vertical prism. The study presented here is mostly based on statistics derived from simulations obtained with FLEXPART, a well-established LPDM. No observational data collected during the aircraft flights are used here. The purpose of the study is to quantify regional surface influence at the aircraft receptor. The paper interestingly describes transport and surface influence for the CAS novel atmospheric sampling strategy. As hinted (but not demonstrated) in this paper, the scale covered by this sampling strategy may contribute to filling a methodological gap between large scale inverse models and bottom up approaches, along with other concurrent efforts such as Lauvaux et al. (2009) or Sun et al. (2010) However, Font et al.'s study arrives at the notion that spatial separation between sites reduces the overlap between their “footprint”, which seems trivial. Other conclusions reached by this study are rather specific, and, besides, do not succeed in my opinion at demonstrating the validity of the sampling strategy in view of its objectives. The relation between PSI overlap and the correlation length notion (used in inverse modelling) could be discussed, with a central role in the argument of this paper. The study could additionally deserve an improved written style. The text is difficult to follow, introduces a lot of very specific acronyms and details, and provides the reader with an excessive amount of specific numerical values, often disrupting the reading flow. In summary, I suggest that this manuscript undergo an extensive simplification and careful rewriting, is augmented with an improved discussion section, and is eventually submitted to another journal, more relevant to observational strategies (maybe Atmos. Meas. Tech.). I would also suggest introducing observational data to validate the modelling approach in order to further improve the quality of the paper. Further explanations and suggestions of possible improvements are given hereafter.

Thank you for the comments. The discussion of the PSI overlap and the correlation length scale is addressed highlighting the raised issues of choosing scales of observations inside a watershed. A reduction of the manuscript and the diagnostics used would be done.

General comments - The authors must shorten and simplify their paper, and give it a clearer structure. Notably, I would suggest o 1) limiting the study to a single site of CAS, and, for inter-crown comparison, a combination of two legs, and discussing eventually possible differences between the other crowns without full details. This should also help reducing the number of new acronyms. o 2) significantly reducing the number of altitude, xxx-s-PSI, seasons and levels considered to focus on the major relevant aspects o 3) sharply reducing the amount of numerical values provided in the text, and provide the necessary ones in Tables

The authors fully agree in shorten the manuscript. Only one CAS would be fully discussed (LIN-MEQ-BIN), and just two sites of the other two CAS presented would be shown (LMU and ULL) in the inter-Crown comparison. Only the 600 and 1200 masl altitude would be deeply discussed and for the other two altitudes (2500 and 4000 m), only the main diagnostics would be kept (basically, the lack of surface influence at these altitudes). The number of numeric values reported in the manuscript would be reduced, and the other would be kept summarized in Tables or supplemental material. Furthermore, only the 102 km length scale would be focused.

- why to choose residence time thresholds (Rttc) in function of the area that the authors want to find in the end (poor justification and possible circular reasoning) instead of selecting this threshold's value according to the residence time needed to get a significant signal, e.g. change the signal by 1 ppm under certain average surface flux value (e.g. from Carbon Tracker)? The authors' approach here appears to be disconnected from the phenomena under consideration (fluxes retrieval) but more connected to their model's properties. This leads to the following point.

Following the reviewer's suggestion, the mean residence time needed to detect a change in 1 ppmv in the receptor site is $\sim$450s, using CarbonTracker fluxes (max-

imum and minimum diurnal fluxes; and maximum and minimum values from the 3-hourly-fluxes). The value could be approached to 500 s. This new reasoning would be described in the revised manuscript.

- What is it exactly that the authors want to investigate? Is it the sampling strategy validation? Model transport properties/spatial statistics? General discussion about short-range transport of surface fluxes? At times the objective is unclear. In my opinion, any validation of such a sampling strategy must convince the reader by showing either how it will lead to reduction in errors on fluxes retrieval at the relevant scale, or how it is a novel (self-consistent or complementary) technique for flux estimation. Neither one of these options are demonstrated here as the authors focus their study on the notion of PSI overlap.

The main objective of the paper is to study the model transport and the spatial distribution of the footprint in a network of atmospheric sites in a specific watershed (Ebre) to point out some challenges regarding atmospheric network design.

- I do not understand the purpose and added-value of introducing PCA (in section 3.1). It introduces new elements that are not participating significantly to the conclusions of the authors and to the interest of the paper, while requesting a new effort from the reader. Moreover, when authors state that PCA 1&2 explain 75% of the variance, it is not clear to me the variance of which quantity we are considering. If it is the variance of a passive tracer concentration (in which case please explain how you calculate it), maybe it should be interesting to discuss that CO2 is driven not only by transport but also by local and remote surface fluxes having diel variability.

The PCA has been introduced in order to account for the mean PSI regional area influencing each site. The variables are all the PSI pixels, and the observations are the residence time in each of them in the 51 simulations. The variance explained by each PC is coming from the variance of the correlations of the residence time among all pixels.

Specific comments - P. 8105, l. 2 and 5: about the alleged spatial gap between local and global: There is indeed a gap between "small" and "large" scales. But since the papers by Lafont et al. (2002) and Gurney et al. (2002), inverse modelling technique has significantly progressed towards the retrieval of fluxes at the regional scale and even, more recently, the mesoscale (e.g. Lauvaux et al., ACP, 2008). This should be acknowledged by the authors. Moreover, local fluxes estimates can be upscaled using satellite remote sensing and process-based biospheric modelling. However, although not so wide anymore, a gap remains that could be addressed by the CAS strategy.

Thank you for the comment and for the suggested references. Changes would be implemented in this section.

- P. 8105, l. 25-27: the authors should take into account the substantial differences between the cited aircraft studies. For example, Schmitgen et al., (2004) did not used the data from vertical profiles per se but rather performed Lagrangian boundary layer budget. A same type of approach was used by Sarrat et al. (2009). The difference between Lagrangian approach and the strategy presented by the authors (and, e.g. that of Lloyd et al., (2002)) should be acknowledged. Maybe the study by Stephens et al. (2007), based on vertical mixing in models used for inversions, would be more relevant in this Introduction.

Thank you for the comment. The suggestions would be implemented in the revised manuscript.

- The expression "watershed scale" is misleading as watershed is more referring to hydrology than to relevant carbon cycle phenomena.

The watershed is the ecological unit to study the biogeochemical cycles (Likens et al., 1971). In a watershed, the evapotranspiration fluxes and the production/respiration processes are organized. Besides the paper is not discussing about the intensity of carbon fluxes and is focused on transport processes, the term is conserved as an integrative concept in ecological studies.

- P 8108 l. 4 why to mention the absence of "removal" processes representation in the model runs when authors' discourse is on CO2? The authors could simply remind the reader that the notion of Potential surface influence is only related to transport and therefore implies no consideration for actual surface fluxes of CO2.

Thank you for the comment. The sentence about the "removal" processes would be removed in the revised manuscript. The fact that in this study the PSI is only related to the transport processes would be reminded here.

- P 8108, l. 7: Term "climatology" relatively poorly chosen for weekly runs in the course of a single year. (do they cover all daytime hours, e.g. 00-06-12-1800 LT?)

Simulations are started at 12 UT, one day each year. The term climatology would be reworded as "study composed by 51 LPDM simulations in backward mode for 2006".

- P 8111 l. 17. The authors mention the assumption that CO2 is well mixed zonally above 1200m. It might be more appropriate to consider the boundary layer height rather than a specific altitude, as the 1200m value may not be valid in many situations. Moreover, tropospheric gradients of CO2 are also sensitive to continental to regional signals under synoptic meteorological variations, which occur at a temporal scale (few days) not covered by the weekly sampling of the CAS.

Thank you for the comment. It would be stated the "boundary layer height" rather than a specific altitude. A comment about the possible CO2 gradient at high altitudes due to continental/regional fluxes would be stated.

- P 8111 l. 28 please better explain "missed" CO2. I understand that the missed CO2 idea is relative to a CO2 "enhancement" (either positive or negative) due to regional/local fluxes affecting the airmass relative to a "baseline" concentration. If this is correct, this should be made more explicit. Maybe this is were the study could benefit from actual observational data.

The term "missed CO2" refers to the fact that not considering the pixels with less than

500 s (or a given threshold), some information would be missed (therefore when retrieving CO_2 fluxes, some CO_2 could be missed).

- p. 8117 l. 5: "short-term variations": please indicate which timescale is considered short here (hourly? Diurnal?).

It refers to diurnal variability.

- p. 8119, l. 26-27. "CO_2 mixing ratios : : : still retrieve fluxes: : :". Two remarks: 1) mixing ratios do not retrieve as such, but "provide constraints on"; 2) it is not proven in the paper that they help to retrieve actual fluxes. The paper has only shown that it had the potential to inform about unspecified surface fluxes. But to retrieve fluxes, an accumulation of data over some period of time is probably needed. Furthermore, at what accuracy could fluxes be retrieved? This should be at least suggested in the paper.

Thank you for the comment. The suggestion would be implemented in the revised manuscript.

Technical corrections - p. 8117 l. 5: please change "sort-term" to "short-term"

Thank you for the comment.

- p. 8117 l. 10: "10ËĘ2 km regional surface: : :": for a surface, unit should be kmËĘ2 and not km. This remark applies to many other occurrences before and after in the manuscript.

Thank you for the comment. Changes would be done here and in all occurrences.

Answer to comments of anonymous Referee #2 In this paper the Crown Aircraft Sampling strategy (CAS) is evaluated for retrieval of the regional surface CO_2 budget using the lagrangian particle dispersion model Flexpart. The study focuses on three 'prisms' in and around the east of Spain at a latitude of 42°N. The paper seems to serve as a justification of this specific sampling strategy, in which it partly falls short, especially

since no real observations are used in this study. The paper is also not very clear on its main research question. The paper is not easy to read and lacks choices of good diagnostics to evaluate the model outcomes. By discussing all permutations of prisms, sampling heights, sampling season and possible footprint areas with different diagnostics the text is made heavy of numerical comparisons that are hard to follow. I would suggest to focus in the text only on those results that provide the most promising diagnostics for the research question (and possibly leave the rest to tables in an appendix). The main message of the paper seems to be that in order to capture the CO_2 budget of a study area with aircraft sampling, the CAS has the potential to capture the flux of the study area as long as most of the flight legs are within the PBL, and that the background can be captured by a small number of vertical profiles at the prism's corner points. Other than just the footprint analyses in this paper it would be very useful to demonstrate this in a synthetic model experiment, using flux fields that vary in space and time as much as possible as the expected real fluxes, which should then also be evaluated by comparing with other strategies (e.g. Sarrat et al, 2009; Stephens et al., 2007). I would like to stimulate the authors to strongly shorten the current analysis in the paper and to extend the paper in the directions indicated in order to make the paper more useful and practical for its implications. As this comes down to a major revision, no specific comments will be given on the current text as most of this will have to be rewritten or can be assumed to disappear from the final text.

Thank you for the comments and valuable suggestions. The manuscript would be shortened and only the relevant diagnostics would be kept in the manuscript.

General comments - 8105-2: Significant research has focused and progress has been made on the regional scale between local and global in the last few years. The text here seems to suggest currently nothing exists between the local and global scale.

Thank you for the comment. Results from recent literature about regional studies (Sarrat et al., 2009; Lauvaux et al., 2009; Sun et al., 2010, etc.) would be included.

- 8106-15: That the model is sensitive to surface fluxes according to the PSI does not necessarily imply that a correct and representative flux can be retrieved from observations due to variabilities and associated uncertainties. These need to be addressed here as well.

Thank you for the comment. This point would be highlighted using observational data and two specific days would be deeply discussed. Those days are the 7th February 2006 and 29th November 2006 for the LIN-MEQ-BIN CAS. The first survey, a mean difference of 0.7ppmv between sites is found with a shared regional surface influence of 7%; whereas the second survey, a difference of 10 ppmv is found with a large surface regional influence share of $\sim$80%.

- 8107-21: Why has been choosen for the GFS meteo fields, it can be expected they don't work that well for this complex study area in Europe?

The GFS meteo fields have been chosen instead of the ECMWF ones for its easy availability and for its temporal resolution. Analysis at 00, 06, 12 and 18 UT are available for both models. To assure trajectory accuracy, forecasts at 03, 09, 15 and 21 UT are added. Forecast at 3 hours at every analysis times are available for GFS whereas for ECMWF, only forecast from 0 and 12 UT are available.

- 8108-24: The PBL in winter time is always higher than 300m?

The PBL height can be lower than 300 m above the ground level (magl) especially at night. The PBL height from the GFS meteofields at 00 UT for 2006 show that the median value is 320 magl with 202 occurrences with lower altitudes. Scarce cases occurred at 12 UT. Therefore, the 300 m altitude is a commitment threshold assuring than most of the times, air masses are within the boundary layer, and a large number of particles are sampled.

- 8110-4/9:Please rewrite this essential description of the PSI, the current text is very hard to follow

Thank you. The PSI is the layer adjacent to the ground (300 magl) where air masses remain before arriving at the receptor point (measurement site).

- 8110-19 to 8111-19 Almost impossible to follow

In order to enhance its readability, most of the values would be dropped from the manuscript. The main idea of these paragraphs is that the area occupied by the PSI is reduced to the regional scale when applying the 500 s threshold. Moreover, the surface regional influence is scarcely recovered at high altitude (2500 and 4000 m). The seasonality of the PSI area wouldn't be discussed neither the reduction of the PSI area when other residence time thresholds are applied.

- 8113-20 to 8115-5 The PCA is introduced to reduce the number of variables, here to identify the main area of influence and the transport direction connected to the influence of the region. Figure 5 is a mystery, what is unit of the colour scale? It is not clear what this analysis adds to the message of the paper.

As stated in the general answer, the mean PSI of a single atmospheric site is assessed by the Principal Component Analysis (PCA). In order to reinforce its value, the PCA analysis would be given in Section 3.2, where the PSI relationships between sites are given. Only the LIN-MEQ-BIN analysis (for the intra-Crown comparison) and the LMU and ULL ones would be given in a revised Figure 5. Each mean PSI for each site assures the spatial coverage of the Ebre watershed, gathering the influence of the main land uses in the region.

- 8115-8 The (yet another) diagnostic intra-crown overlapping percentage does not bring much information than the obvious that when stations are closer they see more of the same.

However, a mathematical description of the reduction of the shared surface influence is given for the Ebre watershed. This equation could be related to the correlation length used in inversion models. Depending on the observed CO2 mixing ratios and the variability within a CAS, the PSI overlapping would give robustness to the measurements and would enhance the determination of the continental/regional/local influence.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 8103, 2010.