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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.

Anonymous Referee #1

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Review for Font et al., "Assessing the regional surface influence. . .", ACPD

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 authors (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.

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

- 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.

- 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.

- 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.

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

- 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.

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

- 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.

- 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?)

- 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.

- 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.

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

- p. 8119, l. 26-27. "CO2 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.

Technical corrections

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

- p. 8117 l. 10: "10ˆ2 km regional surface...": for a surface, unit should be kmˆ2 and not km. This remark applies to many other occurrences before and after in the manuscript.

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