

## ***Interactive comment on “Assessing the regional surface influence through Backward Lagrangian Dispersion Models for aircraft CO<sub>2</sub> vertical profiles observations in NE Spain” by A. Font et al.***

**Anonymous Referee #2**

Received and published: 4 June 2010

Review for Font et al.: “Assessing the regional surface influence through Backward Lagrangian Dispersion Models for aircraft CO<sub>2</sub> vertical profiles observations in NE Spain”; MS No.: acp-2010-5

In this paper the Crown Aircraft Sampling strategy (CAS) is evaluated for retrieval of the regional surface CO<sub>2</sub> 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

C3562

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<sub>2</sub> 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.

### 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.

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.

C3563

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?

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

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

8110-19 to 8111-19 Almost impossible to follow

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.

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.

---

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

C3564