**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "CO$_2$ dispersion modelling over Paris region within the CO$_2$-MEGAPARIS project" *by* C. Lac et al.

Anonymous Referee #3

Received and published: 19 December 2012

Overview: The manuscript presents simulated carbon dioxide fields for one week centered over Paris. The work demonstrates and tests the ability of a high-resolution meso-scale model to reproduce observed meteorological and carbon dioxide dynamics, with a focus on urban areas, Paris in particular. This work is appropriately placed in ACP, and contributes to the burgeoning area or studying carbon emissions from urban areas. I have some general and specific concerns delineated below, after satisfactorily addressing these issues I would recommend publication.

General Comments: Overall things look quite nice and interesting, but I have a couple of larger reservations that require more work and must be addressed.

1) CO2 boundary condition. This is only briefly touched upon in section 2, is unclear, and seems inadequate. From what I understand the model is initialized with a flat field

based on observations, and then run from there. Is there any spin-up time? What is done with air flowing into the domain (what value is it assigned)? What impact do varying boundary condition choices make on simulations? We know that in regional studies boundary conditions play a tremendously important role (Lauvaux et al. TELLUS 2012). The authors must better described what they've done for boundary conditions, and make quantitative assessments of impacts of boundary condition choices on simulations.

2) Overall, there are qualitative statements describing how 'small' errors are and how well model represents things, but there is little to not quantitative substantiation. Errors of 100m in nocturnal pbl height are not small by any measure—this is 50% or more of the observed pbl height. Qualitative statements should be toned down. Also, statements attributing all mismatch to surface flux misrepresentation need to be restructured—nowhere has it been shown that remaining transport errors do not explain discrepancies, at least in part. In fact, nowhere has the impact of different transport errors on CO2 fields been shown. This would be a very valuable exercise to do, demonstrating quantitatively the impact of some of the associated transport errors.

3) There needs to be more/better presentation of the model and observed CO2 values (such as in 1:1 plots). As currently presented, it is difficult to assess model performance. Conclusions about 'small' errors attributable to transport need to be toned down. Errors of 10+ ppm at night are not small, and even errors of a couple ppm in daytime could be quite significant in an inverse modeling sense.

4) This study focuses only on one week of modeling and observations. Conclusions thus must be quite limited, as one cannot extrapolate to generalized model performance from such a limited duration comparison, which could be particularly favorable or unfavorable. The limited duration of model/observations must be presented, and its impact on conclusions should be discussed. One element of this is discussing time/computation to simulate one-week, and whether the current model construct could be expected to run for years to compare w/ the observational record being recorded in

Paris & Europe.

Specific Comments: Title: add 'the' as "Modelling over the Paris Region.."

Abstract: rephrase opening line, accurate simulation is very useful, but not necessarily 'essential' (some data driven methods may answer many of the relevant questions without simulations)

Abstract: restate nocturnal BLH only slightly underestimated—errors of 100m may be 50% or more at night, not accurate to present this as 'slightly underestimated'

Abstract: 'mainly linked to the misrepresentation' this should be rephrased, these biases are 'likely' linked to errors in anthropogenic sources, but you have not definitively shown that in the work here.

Abstract: sentence starting 'The CO2 cycle at these sites..." what is the impact of the pbl bias on observations? Should be mentioned here

Intro: sentence starting 'Indeed, with 12 millions of inhabitants" change millions to 'million', and word 'largest' between third and megacity, and rephrase 'Moscow), and is estimated to emit about 14%...'

Intro: modify to 'Moreover, it is an ideal test location...'

Intro, Paragraph 2: Would be appropriate here to acknowledge urban CO2 studies being pursued with different methods on different cities (Indianapolis: Gurney KR et al., Environ. Sci. Technol. 2012; Salt Lake City: Strong C et al., JGR 2011; Los Angeles: Kort EA et al., GRL 2012)

Section 2, Paragraph 1: The concluding sentence is confusing and incorrect 'we will improperly call mixing ratio by concentration'—this should be removed and should be corrected throughout later on

Section 2 Paragraph 2: typo—'miximum' should be 'maximum'

C10790

Section 2 final paragraph: Why replace urban with rock? What is the reason for choosing rock? This should be explained.

Section 3: There needs to be more detailed explanation of observations. I would like to see more detailed explanation of sampling. Are observations being made wet or dry? What is the calibration strategy used? What scale are observations placed on? What are estimated accuracies/precisions/biases?

Section 4.1 final statement: this is qualitative. How does this matter for CO2? 2C seems quite large, not small. Also, in the figure, the model rural-semiurban exhibit greater difference than observations. This isn't discussed at all, and I am curious as to why this is, and how this type of error may impact CO2 fields.

Section 4.3 typo, fix 'bassin' to 'basin'

Section 5.1 'its representation at local scale could be improved with finer emission inventories' This is likely true, but has not been demonstrated, and the role of transport has not been quantified or eliminated. Need to tone down this (and other similar statements) to acknowledge transport may still play a role.

5.2: 'these small errors are not attributed to the vertical transport...' as in comment above, this statement extends beyond findings presented here, as transport has not been eliminated as a significant player.

5.2 paragraph 4: typo, should be 'is not negligible (Fig...'\

5.3 'between 1 and 2 ppm' Where do these numbers come from? This seems exceedingly important and is not at all evident from Figure 11. This discussion and substantiation should be expanded.

Figure 1: Should reduce scale extent on topography. Also, all figures should have units labeled with the scale.

Figure 2b: The model seems far to variable and low compared to observations that are

C10791

more uniform. This, and the impact on CO2 simulations, should be discussed.

Figure 3 (and Fig. 7): There is a large deviation in bias errors in the morning hours with sunrise. This is hardly discussed in the text. Potential reasons for this and its import should be mentioned. Furthermore, this seems to maybe point to errors in boundary layer growth, which in the text is characterized as being very well represented.

Figure 4: These plots need to be all properly lined up and of higher quality.

Figure 6: There are a number of features discrepancies here not addressed in the text. The timing on simulated vs observed BLH does not seem that great (for instance at TRN), and may indeed even be erroneous at JUSS (see the 23rd). REF & RUR simulated pbl heights are extremely similar most of the time, in contrast to the text statement. Further confusing is they appear to produce the same sensible heat flux at SIRTA, and both seem in significant error (far too high sensible heat), but the pbl height looks reasonable. This discrepancy needs to be explored & explained further.

Figure 8 & 10: I would like to see maybe an average daily cycle of CO2 as well. A 1:1 plot of model and observations would be very informative as wellâĂŤtime series are good but prevent more quantitative assessments. Scales of Fig 10 b and d are far too expanded to assess model performance.