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Interactive comment on "Diurnal, synoptic and seasonal variability of atmospheric CO_2 in the Paris megacity area" *by* Irène Xueref-Remy et al.

Anonymous Referee #2

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Journal: ACP Title: Diurnal, synoptic and seasonal variability of atmospheric CO2 in the Paris megacity area Author(s): I. Xueref-Remy et al. MS No.: acp-2016-218 MS Type: Research article

General Comments

This paper analyzes nearly 1 year of CO2 data from the Paris megacity greenhouse gas measurement network. The analysis focuses on deciphering the CO2 observations on diurnal and seasonal time scales, and includes a careful examination of the influence of the atmospheric boundary layer height (ABLH), wind speed and direction, and local anthropogenic emissions on these signals. The measurement network contains six total sites across Ile de France spanning a range of conditions from rural to the Eiffel Tower in the heart of Paris. The report presents measurements that pro-

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vide an important baseline for emissions from Paris and for comparison to other global megacities.

Specific Comments The authors present a detailed analysis of the CO2 observations based on time, location, and wind speed/direction to infer the seasonal influence of local and background contributions at each site. This analysis is largely qualitative, but could be made far more quantitative and definitive if based around back trajectory analyses, such as those shown in Figure S1. We strongly suggest that the discussion of Section 3.1 be expanded and used to validate the conclusions of Section 3.5 which appear to be based on site wind measurements.

The study concludes that the level of CO2 enhancement varies with urbanization level local to the site; however, the paper does not directly discuss estimates of enhancement relative to background (or the concept of background) until much later in the paper. While diurnal and seasonal variability and the gradients between sites are the primary focus of this paper, background estimation is an important topic and which merits more introduction. Overall, there are two key points that should be incorporated: (1) the concept of background should be defined relative to the domain of interest and (2) a single site may not represent background CO2 mole fractions under all meteorological conditions. Additionally, the paper should use CO2 enhancement values relative to some chosen background rather than absolute CO2 values (eg 410 ppm) since the global background will surpass even these “elevated” values in the near future.

The challenges of analyzing these measurements raises several priority questions regarding the Paris network. We note that the INFLUX network in Indianapolis, IN USA contains 13 towers for a smaller, less populated urban area and approximately 1/10th the emissions of Paris/IdF [Turnbull, Jocelyn C., et al. "Toward quantification and source sector identification of fossil fuel CO2 emissions from an urban area: Results from the INFLUX experiment." Journal of Geophysical Research: Atmospheres 120.1 (2015): 292-312]. We would have expected some discussion of the density of the Paris network, the potential benefit of additional sites, and where they would ide-

ally be located for maximum impact. This is particularly relevant for the “background” discussion since it is clear that Mace Head alone is insufficient for this analysis and that a full understanding of Paris CO2 monitoring may well require observations from as far away as the Ruhr or the Benelux region. Given the topographical similarities of Paris and Indianapolis, we were also surprised that more discussion was not presented comparing the CO2 concentration “plume” patterns from these urban areas.

Newman et al. [Newman, S., et al. "Diurnal tracking of anthropogenic CO 2 emissions in the Los Angeles basin megacity during spring 2010." Atmospheric Chemistry and Physics 13.8 (2013): 4359-4372] showed diurnal patterns for CO2 from the Los Angeles megacity, but there was no comparison made with these data. This is particularly relevant since Los Angeles CO2 emissions are well known to be dominated by vehicle/transportation and impart significant rush hour maxima (0700-1000 and 1500-1900) that are absent from all but the EIF signals in Paris. The arguments for winter vehicle emissions in Paris are not obvious from the figures as presented.

The Eiffel Tower (EIF) site offers unique observations that might be more fully exploited in future studies. Complete diurnal and day of week sampling at this site would enable greater understanding of variability across the network. Adding vertical profile measurements at eg 50, 100, and 200 m to complement the 300 m inlet height would add tremendously to understanding the ABLH/CO2 linkages as well as providing different spatial sensitivity footprints within the Paris/IdF region. Increasing the sampling of meteorological fields at different heights would also prove valuable.

It would be useful to present the more details about the AIRPARIF inventory in the text, e.g., how it was constructed, its spatial resolution, etc.

Comments on treatment of MHD and “background”: P.7, line 6: MHD is described as a remote location. State here that this site was specifically evaluated as a potential background site. See also comments below. P.16, line 3-4: The conclusion that MHD is not a relevant site for background on the seasonal scale does not seem to be fully

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supported by results. In some instances, a site that is classified as rural or peri-urban (or possibly urban) could represent background mole fractions under certain meteorological conditions. Selection of background can performed with using many methods, including meteorological filtering, analyzing tracer/tracer correlations, or evaluating the stability of observations. There is a significant body of literature detailing methods for selecting observations that represent background mole fractions (as an example, see Ruckstuhl et al., 2012, http://www.atmos-meas-tech.net/5/2613/2012/).

P.18, lines 5-7: The conclusion here that MHD is not a relevant background site for Paris or other Western European cities also does not seem to be fully supported by the evidence. The definition of background depends on the domain of interest and also the timescale. For example, a single site may not be relevant for selecting background observations at all times and under all conditions. It is not clear whether there are ever any meteorological conditions that support MHD as a relevant local and/or regional background site. In general, the conclusions regarding MHD could be further supported by the evidence from the back trajectory and fine wind sector analysis (Sections 3.1 and 3.5.2) and/or the Supplemental materials (Figures S1 and S2).

Technical Corrections The manuscript could further benefit from more labeling figures to classify sites as “Urban” and “Periurban/Rural”. Regarding analytical methods, the paper would also benefit from stating early on that all 7 sites (new and previously published) are on the same CO2 calibration scale (WMO X2007), use similar analytical procedures, and have relatively small uncertainties. This could be stated perhaps in the introduction or at the beginning of the methods section. Introduction: Suggest presenting the site code QUA to associate this site with the ABLH measurements from the time they are first introduced. Figure 6: May help to include inlet heights. Also, maybe label plots as Urban, peri-urban, rural/remote, etc.

P.4, line14: The reference Schmidt et al., (2014) first appears here, however it was not included in the list of references at the end of the paper.

P.7, line 23: The authors mention the cell temperature of the analyzer at the EIF site was modified to undergo cell temperature set point at 60°C, however do not discuss what impact (if any) this may have on the results. Details of such analytical differences could be useful for others in the community conducting studies using similar analyzers.

Please be clear when meteorological data is measured vs. modeledâĂŤe.g. add the word modeled to Figure 3 caption.

Figure 5: might be useful to add inlet heights to the site key

Figure 7: What is the difference between the violet and red traces? Please describe in the text.

Figure 12, the wind roses highlighting CO2 concentrations and indicating the origin of the air masses being measured, was particularly interesting. Unfortunately, the discussion of this figure includes a lot of discussion of background, but it isn't clear exactly how the authors determined the background. I would also like to see explicit explanation of how the seasonal adjustments to the CO2 concentrations were made.

Table 4: The use of "N" is confusing since this is a percentage, not an integer. Consider renaming "coverage"?

Page 10 Line 20: shouldn't this section be titled, "Results and Discussion?"

Page 12 Lines 28-32: What about the effect of inlet height? MON is much lower than TRN50.

Page 13 Line 6: Max interseasonal difference is higher than the mean annual afternoon dispersion: what does this imply?

Page 13 Line 10: "strong impact of regional CO2 emissions variability:" why? Please elaborate a bit more.

Page 14 Lines 5-34: Please put the seasons in the same order in the text and in the plot.

Page 18 Lines 21-22: Define local in terms of spatial scale.

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