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Atmospheric Chemistry and Physics Discussions

Interactive comment on "XCO_2 in an emission hot-spot region: the COCCON Paris campaign 2015" *by* Felix R. Vogel et al.

Felix R. Vogel et al.

felix.vogel@canada.ca

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We would like to thank reviewer #3 for their careful review, comments and suggestions The original reviewer comments are labelled as '#3' with our replies below:

#3: This manuscript describes a pilot project of five EM27/SUN spectrometers that were deployed for two weeks in the Paris region to investigate CO2 fluxes from that megacity. They also describe a modeling framework to compare to the column measurements and provide some initial comparisons. While it is commendable that the authors are publishing the results of a pilot phase of a project where important details about the instrumentation are shown, their analysis is incomplete. The "background" on the modeled result was very different from the observations, and no hypothesis was

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given for why that might be the case. Then, the analysis and discussion behind Fig 13 and 14 had several logical gaps and should be completely reconsidered. The conclusions section had several problems and was not well supported by the main text. I do not think it would take that much work to fix these issues, but they are major issues with the analysis and interpretation of the study. Until these are fixed I would not recommend publication.

Reply: We have significantly streamlined the paper and also expanded on our analysis of the data to fill the gaps identified by the reviewer. Specifically, we have added more explanation/interpretation around Figures 13 and 14 and clarified the text to avoid misunderstandings (e.g. Figure 14 is already the mean daily cycle for selected data i.e. when the site is an upwind site). More explanation in individual improvements and corrections is given below.

#3: Line 92: remove the word “by”.

Reply: Corrected – Thanks.

#3: Line 105: “spectrometers” should be singular, ie “spectrometer” Reply: Corrected - Thanks

#3: Line 111: This sentence is grammatically incorrect and should be reworded slightly.

Reply: We have reformulated this sentence.

#3: Line 153: remove the word “for”

Reply: Corrected - Thanks

#3: Line 154: Add “PM” at the end of the line.

Reply: Corrected - Thanks

#3: Line 192: Does instrument 1 have the best agreement with the TCCON instrument? The text here and in the rest of the paragraph indicates that the EM27/SUN

measurements CAN be made traceable to the WMO scale, but it doesn't say IF they were or not. It would be good to explain if they were, and if they were not it is even more important to say that and explain why they were not.

Reply: There is nothing special about spectrometer #1, arbitrarily one of the devices has been used as reference here. Any drift of the calibration in either the selected reference spectrometer or of one of the other spectrometers would induce variations in the table entries (correlated between different spectrometers if the reference is drifting, uncorrelated if one of the other spectrometer is drifting) The measurements can be made traceable to WMO scale to the extent that can be achieved for TCCON. Direct side-by-side comparison with a TCCON spectrometer have been used to estimate the calibration offset of the EM27/SUN wrt TCCON. As the same spectral bands are used for the observations and the instrumental characteristics of both TCCON and EM27/SUN spectrometers are very close to an ideal FTS, it is expected that the calibration between EM27/SUN and TCCON is quite consistent. The paper of Frey et al. provides a good impression of the level of consistency found. (Frey, M., Sha, M. K., Hase, F., Kiel, M., Blumenstock, T., Harig, R., Surawicz, G., Deutscher, N. M., Shiomi, K., Franklin, J., Bösch, H., Chen, J., Grutter, M., Ohyama, H., Sun, Y., Butz, A., Mengistu Tsidu, G., Ene, D., Wunch, D., Cao, Z., Garcia, O., Ramonet, M., Vogel, F., and Orphal, J.: Building the COllaborative Carbon Column Observing Network (COCCON): Long term stability and ensemble performance of the EM27/SUN Fourier transform spectrometer, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2018-146 However, we would not want to claim that this data is traceable to WMO (yet) as more comparison and methodological work should be done in the future.

#3: Line 328-331: I think the authors got this backwards, the text says that the upwind sites had higher XCO2 than their downwind sites indicating that the FFCO2 from Paris is detectable. I think they meant to switch upwind and downwind.

Reply: Yes, indeed we somehow switched this and failed to notice in proof-reading. Thanks for catching this.

#3: Another comment about these lines is that, looking at Fig 4, it looks like RES is lower than MIT for most of the study period, but this switches on May 12/13 and RES is higher than MIT. This makes perfect sense looking at Table 3 where it says that for the first part of the campaign the winds were predominantly from the SW while on the 12/13 the winds were from the NNW and NE. It might be worth explaining this feature in the data here since its interesting.

Reply: We have added more discussion on this.

#3: Line 350: The word “northeasterly” is used incorrectly here. Change this to “The two (typically downwind) sites PIS and MIT northeast of Paris show a... ”

Reply: Corrected

#3: Line 351: What does the word “background” mean in this context? Background could mean several things in this context, so I would encourage the authors to either define what the background is or use a different word here to avoid confusion.

Reply: We have removed the word background here (and other parts of the manuscript) as it can indeed confusing and means different things in different communities. We have given more specific explanations when necessary.

#3: Line 356: The word “background” is used again with, I think, a different meaning than was used above. It is also undefined here. I understand exactly what the authors mean, but I think it would be good to provide a little bit of explanation here describing exactly what they mean by background conditions (ie the XCO2 of the air mass entering the urban domain that has been affected by emissions upwind or outside of the domain).

Reply: We have removed the word background here as it means different things in different communities – even among the co-authors of this paper. We have added an explanation of the assumption underlying the gradient approach.

#3: Line 360: “are” should be “were”.

Reply: Corrected

#3: Line 361: “measurement period” should be “on this day” since the winds do vary over the whole measurement period, but they were from the SW on this day that is the focus of this figure.

Reply: Corrected

#3: Line 362: This sentence needs to be re-written for clarity. How about this: “The observations from GIF showed only minimal differences with RES while the rest of the sites (PIS, JUS, and MIT) had Δvalues of 1 to 1.5 ppm.

Reply hanks for this suggested - corrected

#3: Line 363: Delete “of most”

Reply: Corrected

#3: Lines 355-376: Be careful of the tense in this paragraph and elsewhere. For most of the paper the data was referred to in the past tense, but I noticed that this paragraph is in the present tense. To fix this, change “is” to “was” (etc) when referring to the data throughout the section.

Reply: Thanks – this error was corrected (all in past tense, where appropriate).

#3: Lines 405: On this line I initially thought that the authors were drawing a comparison between the modeled XCO2 and the measured XCO2, but after reading the sentence several times I now realize they are just talking about modeled XCO2. It would be good to explicitly say “modeled XCO2” on line 405 to prevent any confusion.

Reply: The sections were explicitly separated into measured and modelled to avoid confusion, but we’ve now added more clarifying markers in this paragraph.

#3: Lines 413-415: Might also be worth pointing out that sometimes the NEE flux is slightly positive due to respiration, generally at night.

Reply: We've added an explanation.

#3: Line 424: I don't see any shaded areas on Fig 11. Reply: Removed the mention of the grey shaded area as the wind directions are given on top of graph 11.

#3: Lines 420-436 (and Fig 11): After reading this a couple of times and staring at Fig 11, I finally realized that each of the vertical panels represents a different modeled source. The subscripts on the y-axis are very small and are not explained anywhere. It would be good to explain what each of the panels are showing in the figure caption. The authors should also explain in the text that this figure shows the total XCO2 in the top panel, and below that the three panels show the modeled contributions from FFCO2, biological emissions (NEE), and background conditions (BC) respectively.

Reply: We have increased the size of the labels and added more explanation in the caption to make the information more accessible to the reader.

#3: Line 440: They should reference Fig 12 at the beginning of this paragraph somewhere.

Reply: Figure 12 were references in line 444 now moved to line 440.

#3: Line 441: There is an extra period and spaces on this line.

Reply: Removed

#3: Line 445: I would encourage the authors to not the background offset FIRST in this paragraph as that is the most obvious feature. Then, once the offset is noted they can go on to describe the diel cycle and the difference between the sites. Also, the authors should offer an explanation for why they think their background model is 1-2 ppm off.

Reply: We have restructured this paragraph and also added the information about the reduced impact of BC when using the gradient approach and added the hypothesis where the offset originates (limited ability to model NEE on short time scales).

#3: Line 454: The authors should explain here that this comparison is not sensitive

to the offset in the BC because it is comparing the modeled upwind with the modeled downwind and the measured upwind with the measured downwind. Also, as a general note, the use of the delta symbol is problematic because of its use in radiocarbon nomenclature. It's OK if the authors desire to use it, but I would encourage them to find an alternative way of noting this.

Reply: See reply to comment on line 445. Concerning, the general note on nomenclature, as radiocarbon is not mentioned in this manuscript and the remote sensing community does not commonly use ïĄĎ14C we decided to use this nomenclature as it seemed most appropriate to the (co-) authors.

#3: Line 452-465: I really don't understand what the significance of Fig 13 is, and this analysis doesn't make sense to me. I would expect that the observations should only fall on the 1-1 line when the wind direction is directly between the upwind/downwind sites. The fact that most of the observations have a slope close to 1 could alternatively suggest that wind direction doesn't matter! I would also expect that when the wind direction is from a 90-degree angle to the upwind direction (so that the wind is blowing across the city instead of from one site to the other) that there should be much higher variability and potentially no relationship between the XCO2 at the two sites.

Reply: We have added more elements to better explain figure 13. Fundamentally, the point was to investigate the impact of wind direction on the concentration gradient and if our atmospheric transport model predicts concentrations equally well for all wind conditions. Apparently these messages were lost/unclear. As expected, the gradients are strongly positive when MIT and PIS are downwind of Paris and we see negative gradients when they are upwind. Furthermore, the slope of the individual wind-directions does not seem to follow the 1:1 line. Especially, northerly winds (blue colors in Fig13 – now Fig 9) seem to have steeper slopes for PIS. This figure also highlights that when trying to assess the impact of Parisian emissions significant gains in signal amplitude can be gained when using data from upwind-downwind situations only. This is not really a 'surprising' finding, but we can now quantify how much more signal is seen on

average. On the 90 degree question – even in this situation with easterly winds we could expect to see differences. As shown in Figure 1, the RES site is then still downwind of significant emission areas and MIT is technically upwind, which explains the negative gradient in Figure 13. The gradient for easterly winds is however is quite different then for northerly winds as other parts of Paris are then "upwind" of RES As the reviewers will find, we have added much more explanation and interpretation around Figure 13 to clarify our interpretation.

#3: Here are a few suggestions for Fig 13. The authors should only plot the data from when the wind is blowing directly from PIS->RES (or MIT->RES) and when it is blowing back from PIS<-RES (or MIT<-RES). There should only be a narrow range of wind direction angles that this comparison should work, maybe 20-30 degrees or something like that. Also, the authors should indicate on the figure, or in the text somewhere what the exact angle it is between the sites in decimal degrees (not just with letters indicating the cardinal directions). Also, the figure caption says that the vertical bars indicate the standard deviation, but they don't say WHAT it's the standard deviation of! Is it the standard deviation of the measurements from a range of wind directions? Is it over some time window?

Reply: Thanks for the suggestions. We have added exact bearings and distances of all stations relative to RES in table S1 in the supplement. We have kept Figure 13 as it allows seeing the influence of different wind conditions on the concentration gradients. A narrow upwind/downwind window would not allow seeing this and would also remove a lot of the data. We think the color-code in figure 13 allows to see how certain wind conditions are pooled. PIS is optimally upwind of RES at a wind direction of 187 degrees (green) and MIT is optimally upwind for a wind direction of 217 degree (green).

#3: Lines 466-479: This diurnal cycle plot is confusing to me because there are times when the wind is blowing from PIS to RES, and there are times when the wind is blowing in the opposite direction. Shouldn't the XCO2 be negative when it is blowing in the

opposite direction? Wouldn't it also have no relationship between the sites when there is no upwind/downwind relationship? It would be much better to isolate this comparison to ONLY times when the wind is blowing in the appropriate direction, not during the whole campaign.

Reply: This is plot DOES indeed only include data when MIT and PIS are downwind of RES. For the observations (upper panel) only data from days with observations are used. This is also why only so few days (0 to 5) contribute to the mean diurnal cycle (as given by the labels on top panel of Figure 14). In the lower panel all days within the campaign period that fulfill the upwind-downwind requirement are used. We have added text to clarify this.

#3: Line 482: "two-weeks" should be "two-week". Reply: Corrected

#3: Line 485: What do the authors mean by "easily linked"? This is sloppy language that is easily misinterpreted, especially in the conclusions section. This whole sentence needs to be re-written for clarity so that the wrong impression is not given.

Reply: We have reformulated to clarify. "However, we also found that XCO2 cannot be simply interpreted in the context of local emissions as even in such a densely populated area, XCO2 is still significantly influenced by natural CO2 uptake during the growing season".

#3: Line 488: The authors don't actually know what is impacting remote CO2. This should instead say something like "... greatly reduced the impact of background CO2 fluxes."

Reply: Thanks – we have reformulated to clarify. Using the gradient does indeed reduce both the influence of boundary condition CO2 and biogenic fluxes within the CHIMERE domain

#3: Line 491: the word "significant" has statistical meaning and shouldn't be used in this instance. Also, "enhanced background" seems incorrect since they never offered

a hypothesis about why the background was higher. Actually, just the word “enhanced” should be changed to “higher” or something that is more objective.

Reply: We have reformulated and change the wording where appropriate.

#3: Line 492: Here is the word “significantly” again. The authors should use a different word here, like “... also predicts that NEE and BC only has a large impact on XCO2 during a few situations ...” Reply: We have reformulated and change the wording (removing significantly) where appropriate.

Lines 491-494: Actually, this whole sentence is problematic and needs revision. The first half of the sentence seems to refer to the discussion surrounding Fig 10 (which is great) but the second half of the sentence referring to upwind and downwind (as it relates to NEE and BC) seems unrelated. If this stays in the text, it needs more detail to explain what the authors were thinking about.

Reply: We have reformulated to clarify.

#3: Line 494-496: This section refers to Fig 13, and this methodology is flawed since an alternative explanation is that wind direction doesn’t even matter in this data set.

Reply: Unfortunately, we can’t really follow why the wind directions do not matter in this data set. It seems apparent that southerly winds produce strong positive concentration offsets, while northerly winds cause negative concentration gradients. Looking at the colors they seem to group very consistently implying a strong correlation of wind conditions on DXCO2, both modelled and observed. The slope is not (or less) wind dependent, but it is not clear why it should be, as we assume that the model performance should be similar in all wind conditions.

#3: Line 496-498: This is wrong. I assume they are referring to Breon et al 2015 Fig 6 where the highest R = 0.90 (not 0.91). Also, this was a straight measured/modeled mole fraction comparison, whereas the analysis in Fig 13 is supposedly the upwind/downwind measured/modeled gradient. Even if the analysis in Fig 13 were done

correctly, this would be a different metric for model evaluation and should not be compared with Breon et al 2015. Its comparing apples (measured/modeled in-situ mole fractions) and oranges (measured/modeled GRADIENTS BETWEEN SITES across a city during a 2-week period with a lot of wind direction changes).

Reply: Concerning Breon et al. 2015: that study also compared measured and modelled in-situ CO2 gradients between sites across the city for a period of 4 weeks see figures 8, S5 and section 4.4. (https://www.atmos-chem-phys.net/15/1707/2015/acp-15-1707-2015.pdf). R2 for GIF and GON gradients is 0.91 as previously mentioned in the manuscript. The sites chosen in our COCCON campaign are also similar to the locations used in Breon et al. 2015. Some of the (co-)author of this study are indeed (co-)authors of Breon et al. 2015 and although it might not be exactly an 'apples to apples' comparison when comparing in-situ CO2 gradients to XCO2 gradients we wanted to cite this study to show that we are attempting to applying an analogous approach.

#3: Line 498-505: This whole section needs to be redone after the analysis in Fig 13 is fixed. Also, the speculation about model dispersion is not based on anything and therefore it has no place in the paper unless the authors care to actually try to do some analysis to quantify it.

Reply: We have re-worked this section. The hypothesis that the dispersion is an issue is indeed only based on experience using the CHIMERE model and the fact that the model resolution is limited to 2x2km2 which leads to numerical diffusion that is likely larger then real dispersion. In a recent study, model simulations of CO2 in the boundary layer of Paris at 5m x 5m resolution where performed using MICRO-SWIFT-SPRAY. A lot of heterogeneity is visible (the Figure attached is a 2x2 km2 pixel of downtown Paris: Jardin du Luxembourg to Jussieu with the Seine River in the top of domain). Such localized sources and plumes visible at 5m x 5m resolution are immediately dispersed within a 2x2km2 CHIMERE cell.

#3: Line 509-511: I actually agree with this statement, but it's exactly the opposite

of what the analysis in this paper shows. Fig 11 shows that the biospheric flux in a gradient sense is small (less than 1ppm almost all the time).

Reply: We have clarified that the influence of the biosphere on XCO2 is a major factor but not as important when considering ïĄĎXCO2 here. The point we want to make is that biospheric fluxes are not always important, however there are periods when they cannot be ignored, even when using a gradient approach.

#3: Line 514: They forgot the word “not”. It should be “. . . and underlying fluxes could NOT be investigated here.”

Reply: Corrected

#3: Line 522: I would disagree that they have demonstrated that the modeling framework is “suitable”. They have provided some initial modeling results from a pilot test field campaign and the modeling framework will need a lot of work before it can be usefully applied to interpret fluxes.

Reply: We have reformulated to highlight that we also see this as a step forward towards our inversion system.

#3: Figure 3: The x and y axes should be labeled longitude and latitude.

Reply: We’ve moved the figure into the supplement and made it bigger (also the lat-long labels)

#3: Figure 7 (top): the y-axis scale could be 0-10 instead of 0-16. Reply: Thanks

#3: Figure 8: The acronyms MACC is not defined anywhere in the manuscript.

Reply: Thanks – we’ve added this information in the caption

#3: Figure 10: In the caption the authors should add “(BC)” so that the reader knows that the legend entry “CHIMERE BC only” means background conditions.

Reply: Corrected

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-595, 2018.

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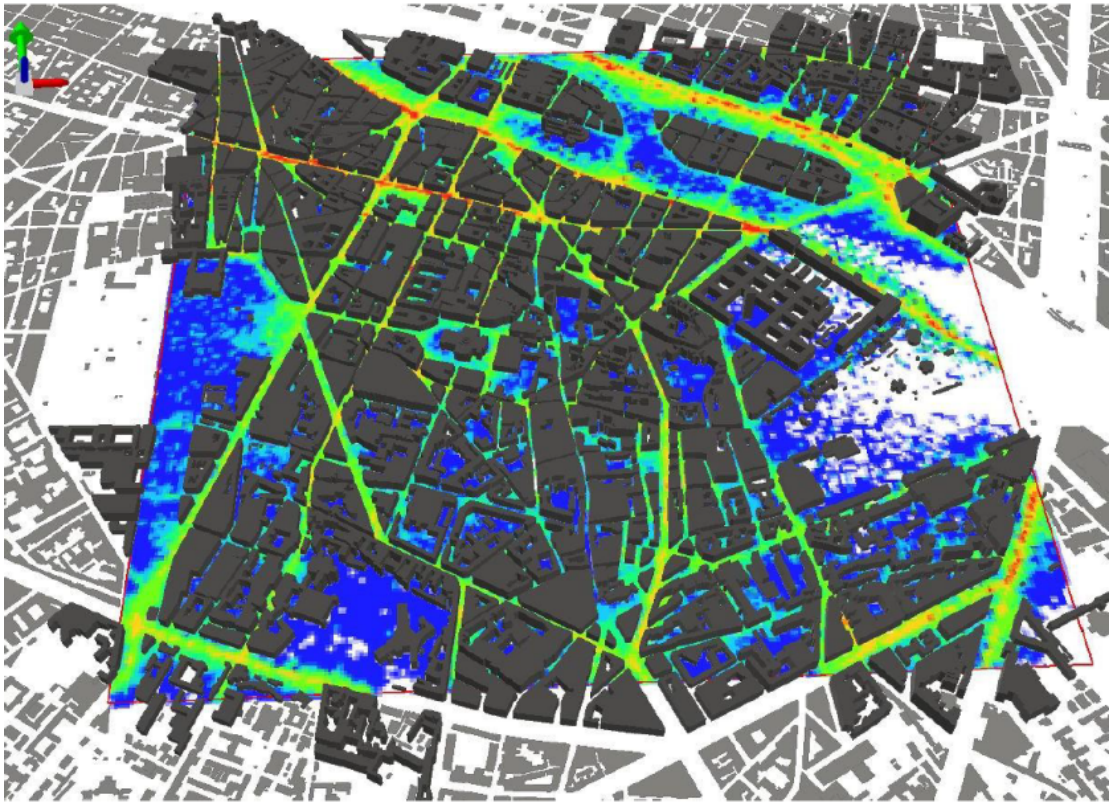

Fig. 1. High-Res simulation (5m x 5m) of surface layer CO2 in the centre of Paris

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