

Interactive comment on “XCO₂ in an emission hot-spot region: the COCCON Paris campaign 2015” by Felix R. Vogel et al.

Anonymous Referee #3

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General Comments

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 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

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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.

Specific Comments

Line 92: remove the word “by”.

Line 105: “spectrometers” should be singular, ie “spectrometer”

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

Line 153: remove the word “for”

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

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.

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.

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.

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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 . . .”

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.

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).

Line 360: “are” should be “were”.

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.

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 Δ values of 1 to 1.5 ppm.

Line 363: Delete “of most”

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.

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.

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

Line 424: I don't see any shaded areas on Fig 11.

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.

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

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

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.

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.

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

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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.

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?

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.

Line 482: "two-weeks" should be "two-week".

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.

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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."

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.

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..."

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.

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.

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).

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

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therefore it has no place in the paper unless the authors care to actually try to do some analysis to quantify it.

Line 509-511: I actually agree with this statement, but its 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).

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

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.

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

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

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

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.

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