

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

Anonymous Referee #1

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1 Overview:

Review of “XCO₂ in an emission hot-spot region: the COCCON Paris campaign 2015” by Vogel *et al.*

Vogel *et al.* present an analysis from a 2-week field campaign in Paris using the Collaborative Carbon Column Observing Network (COCCON). This network uses 5 FTIR spectrometers in and around Paris. The authors compare upwind and downwind concentrations from these spectrometers and use the CHIMERE-CAMS model to simulate XCO₂ at these sites. The campaign was hampered by poor meteorology and most of the results are from 4 days of measurements. Given this, the authors are unable to draw any major scientific conclusions but the work is nevertheless a nice demonstra-

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tion of the viability of this kind of network. My main criticisms are that I feel this work is far too long (14 figures and 3 tables) given that another paper describes the construction of the network (Frey *et al.*, AMTD) and the findings are rather limited given the meteorological limitations for this time period. Overall, I think the work should ultimately be published but could use major revisions to better justify the arguments that are novel. There are also a number of formatting and/or grammatical errors that should be addressed.

2 Comments:

I found myself struggling to characterize what I'm actually learning from the paper because the construction of the network is described in Frey *et al.* AMTD and previous work from this group has already shown the use of a gradient method in Breon *et al.* and Stauffer *et al.* To this reviewer, the major contribution is demonstrating that the gradient method can also work for column measurements and, to a lesser extent, that there is substantial uptake from the biosphere in an urban region like Paris. So I think the message of the paper could be better framed.

Additionally, the manuscript is actually rather weak in the demonstration that the gradient method is working for this region. For example, wouldn't wind shear also adversely impact the gradient method? If there is wind shear, you may have low-level winds that satisfy the upwind/downwind conditions but mid-to-upper tropospheric (or stratospheric?? since it's a column measurement) winds that bring different background conditions. There is little discussion of this (or argument that it is not important in these cases). Are there radiosonde measurements or radar wind profiler data that could be used to demonstrate this?

Overall, I think the manuscript would be far more useful if the authors were to move much of the discussion to a supplement and focus on the main findings. For example,

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many of the figures could be combined or reduced:

- Figs. 1 and 3 could be combined
- It's unclear what Figure 4 is supposed to be telling me, Figure 5 seems to show the same data but in a much clearer form
- Figures 8 and 9 could be combined into a 2-panel figure (would facilitate a visual comparison). However they could probably be moved to a supplement since I'm not sure if they're really necessary. It seems like Figure 10 does a better job of breaking down the contribution from various components (which is actually rather interesting)
- Table 3 could be in a supplement or cut since the locations are shown in Fig. 1.

3 Specific comments:

COCCON is in the title, isn't defined until page 3.

At the beginning of Section 3.1.2 (Page 9), the authors mention that the standard deviation for 1-minute data is 1 ppm. That seems huge given the changes that they're seeing. Does this mean the error bars on all their data points are ± 1 ppm? I suspect there's something I am missing because that would make me rather skeptical of the results. Page 2, Lines 64–67: Just because one single factor doesn't explain the variations between cities doesn't necessarily mean it's uncertain.

Page 2, Lines 70–73: Should give references to these other networks.

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Page 2, Line 74: Urban measurements are representative of a 10000 km² region?? I'm rather skeptical of that.

Page 3, Lines 93–95: The recently funded GeoCARB satellite is a geostationary satellite that will have multiple measurements per day.

Page 3, Lines 108–110: Again, this applies to LEO satellites but there are upcoming GEO satellites as well.

Page 4, Lines 113: Should add the O'Brien et al. AMT (2016) paper because this satellite is actually funded.

Page 4, Line 137: Would be good to flip the order of "airports" and "industrial" because it looks like AIRPARIF just refers to airports (since it starts with AIR).

Page 6, Lines 191–195: Impressive!

Page 7, Line 231: Missing subscript, should be "CO₂". Authors should do a search and replace because there are many instances of incorrect subscripting for CO₂.

Page 8, Lines 276–293: This nomenclature is very confusing. There are subscripts and superscripts on many variables and some of the variables have multiple letters (e.g., "CO₂^s_{model}" is not a great variable name). Would be much better if the authors used standard nomenclature from either Rodgers (2000) or the TCCON group. Either would be preferable to the current.

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Page 8, Line 283: Why is WACCM bolded? Is it supposed to be a matrix (those are the only other bolded terms).

Page 9, Lines 316–317: How are these spectra rated? Unclear.

Page 9, Lines 328–329: Upwind is higher concentration?? Probably a typo, I think you meant downwind. . .

Page 10, Lines 337–338: Are there no other factors?? That seems surprising. Would wind shear or variations in winds, a decreasing anthropogenic source during the day not be able to give decrease? Needs stronger justification w/ data or citations.

Page 10, Lines 341–344: This doesn't seem supported by the analysis. I'd like to see a footprint analysis or some other way for this to be justified. . .

Section 3.13 Page 10: What about wind shear? Were there any radiosondes that indicate the winds are uniform through the column? What about the model? Does that indicate uniform winds throughout the column.

Page 10, Lines 361–365: How representative are the winds at GIF? This could easily be tested in the model, (e.g., look at how variable the winds are over Paris and compare that to the grid cell w/ GIF).

Page 11, Lines 395–397: Couldn't you just coarsen the 1km inventory and then do this comparison?

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Page 12, Lines 413–415: How are you directly linking this to NEE? Seems like this needs more justification.

Page 13, Lines 475–476: How is this being assessed? Does the model agree with this (i.e. is the modeled contribution the same at each site)?

Page 14, Line 492: Would prefer the authors not use "BC" here, was confusing at first read because of NEE abbreviation right before.

Page 14, Lines 501–503: Couldn't the model transport also be wrong?

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