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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'd like to thank reviewer #1 for their careful review and useful suggestions. Original reviewer comments are labelled as '#1:' with our replies below.

#1: 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

demonstration 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.

Reply: We have addressed the specific and general comments as well as addressed formatting issues. The text has been clarified and slightly re-focused and 4 figures and 2 tables were moved to the supplement.

#1: 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.

Reply: We did indeed highlight these aspects in the abstract, but we agree that it felt 'watered-down' in the main text of the initial manuscript. The manuscript has been streamlined and we have moved 4 figures and 2 tables into the supplement. Some more specific discussions and clarifications were added instead.

#1: 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?

Reply: Our model is based on ECWMF Integrated Forecast System (IFS) weather data, which has been demonstrated to compare fairly well with radiosonde data. We have added hodographs to show the wind-shear in the domain in the supplement (example attached). As expected there are wind speed and direction changes in higher layers of the atmosphere. However, the wind conditions in higher layers (mid tropo-sphere and stratosphere) are not of key interest for this study as most of the XCO2 variability is driven by the CO2 mixing ratios in the boundary layer (typically well below 650hPa). The distance between upwind and downwind sites in our experiment is below 45km and there are no indications that significant concentration gradients exists in the stratosphere or even the mid/upper troposphere at this length-scale that would be comparable to the horizontal gradients within the PBL. Even in remote regions without strong urban CO2 emissions vertical profiles are dominated by lower levels (below 650hPa) and compare well with ECMWF-CAMS, which was used in this study as boundary condition (e.g. Membrive et al. 2017; https://doi.org/10.5194/amt-10-2163-2017) We have added a discussion of wind-shear and why we have made the assumption that it is not a major influence (compared to other factors) here.

#1: 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, many of the figures could be combined or reduced.

Reply: Thanks for this suggestion we have streamlined the manuscript.

#1: âĂć Figs. 1 and 3 could be combined

Reply: Thank you for the suggestion, we actually did try a combined graph before, but it was not easily readable. We have opted to move figure 3 into the supplement now.

#1: âĂć 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

Reply: Figure 4 and 5 are indeed the same data, however, Figure 4 allows comparing how similar/different the XCO2 variability is across different sites (spatial variability), while Figure 5 allows to assess the temporal XCO2 variability at an individual sites.

#1:âĂć 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)

Reply: Agreed – we have moved Figure 8 into the supplement.

#1:âĂć Table 3 could be in a supplement or cut since the locations are shown in Fig. 1. Specific comments:

Reply:Agreed – we have moved Table 3 into the supplement.

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

Reply: We have added this information in the abstract now.

#1: 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 $\pm$1 ppm? I suspect there's something I am missing because that would make me rather skeptical of the results.

Reply: This was indeed quite misleading. The 1-ppm standard deviation mentioned here is NOT the instrumental precision, but is driven by the variability of atmospheric conditions (in time and space). I.e. all minute data were used to calculate the mean concentration of the campaign period and their variability is 1ppm. We have added text to clarify the issue.

#1: Page 2, Lines 64–67: Just because one single factor doesn't explain the variations between cities doesn't necessarily mean it's uncertain.

Reply: This criticism is not completely clear. We did not claim (and don't want to) that the uncertainty of emissions is caused by the fact that urban emissions cannot be explainable by a simple factor, but we rather wanted to highlight that cities cannot be easily generalized and need to be investigated individually. We have added wording to make this point more clear and avoid confusion.

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

Reply: These networks were referenced in line 74. We have moved the references to clarify and we also added other relevant references.

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

Reply: The Ile-de-France region (surrounding Grand Paris) is 12'012km2 and the atmospheric GHG and air pollutant composition in this region is often dominated by Parisian emissions, which are constraint by regional observations (Breon et al. 2015 and Staufer et al. 2016). Furthermore, our measurements are conducted in the outskirts of the urban area to not be influenced by local sources only, but to be sensitive to the larger area. We have added text to clarify.

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

Reply: Thanks, we have clarified this here.

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

Reply: We specifically mentioned LEOs in line 107 and GEOs in line 112. Where we highlighted that GEOs are not restricted to the LEO time window.

#1: Page 4, Lines 113: Should add the O'Brien et al. AMT (2016) paper because this

satellite is actually funded.

Reply: We have added the reference to GEOCARB here. (O'Brien was also cited in line 89 of the original manuscript).

#1: 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).

Reply: We have added an explanation here. AIRPARIF is the air quality association that monitors/manages the airshed of Paris. It is not related to the airports agency of Paris (PARIS AEROPORT).

#1: Page 6, Lines 191–195: Impressive!

Reply: Removed "impressive" and we will leave it to the readers to judge the performance – sometimes good results get the (co-)authors excited.

#1: 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.

Reply: We have corrected all CO2 and XCO2 and FFCO2 to have correct subscripts.

#1: 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., "COs2_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.

Reply: Although we agree that multiple having sub-scripts and super-scripts in equation are suboptimal when considering readability, this is also the case in TCCON publications, e.g. Kuai et al. 2013 (doi:10.5194/amt-6-63-2013). The nomenclature, here, was chosen to be consistent with previous COCCON publications, but we would definitely encourage that a common nomenclature is established across different remote sensing efforts/networks.

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

Reply: Thanks - corrected

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

Reply: The quality of the data for each day was rated according to the overall data availability and consistent with previously published work by Hase et al. 2015 www.atmos-meas-tech.net/8/3059/2015/. We have added the reference.

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

Reply: Yes, thanks for catching this -> corrected

#1: 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.

Reply: We have corrected this sentence as other factors could contribute. However, our simulation attribute the decrease of CO_2 to NEE (uptake) within the domain, but the underlying biospheric fluxes could indeed be wrong.

#1: 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. . .

Reply: our modelling framework does not provide footprints and running a lagrangian model in addition to CHIMERE is beyond the scope of this work. We have added citations of other studies where XCO_2 column footprints were investigated.

#1: 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.

Reply: See general comment on wind shear and hodographs above

#1: 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).

Reply: Looking at the hodographs for the lower model levels they seem very consistent across Paris

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

Reply: Sorry, this point was not very clear. We cannot compare different anthropogenic emission products in this study as no other 'inventories' are available at this resolution. Coarsening IER to the resolution of e.g. EDGAR V4.2 would still not yield a fair comparison to assess the performance/influence of the spatial disaggregation at the 2x2km2 scale. We have added text to clarify this point.

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

Reply: We have added clarification. In our model $FFCO_2$, NEE and BC are transported separately, so we can directly identify which sources/sinks contributed CO_2 to our simulated XCO_2 in our domain.

#1: 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)?

Reply: Yes, the model predicts similar biogenic contributions and we ASSUME that the influence of rural biogenic fluxes in our domain affect our sites in a similar way. We have added text to clarify.

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

Reply: Changed to . . . and boundary conditions (i.e. the influence of CO2 being transported into our domain) only. . ..

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

Reply: This is a definite yes – the transport model could be wrong (and likely is wrong to a certain degree most of the time). Our reasoning not to assume that this is the dominant factor here is that a.) the wind observations are fairly well reproduced by CHIMERE, see figure 7 and b.) the PIS-RES gradient falls onto the 1:1 line - compared to the 1.7 of MIT-RES. It seems unlikely that the model properly models transport from RES to PIS but fails for RES to MIT as there a no major topographic disturbance in this part of the Paris Basin. We have added a note of caution that the model could be wrong in the discussion, as well as why we think this is not the biggest contributor to the disagreement here.

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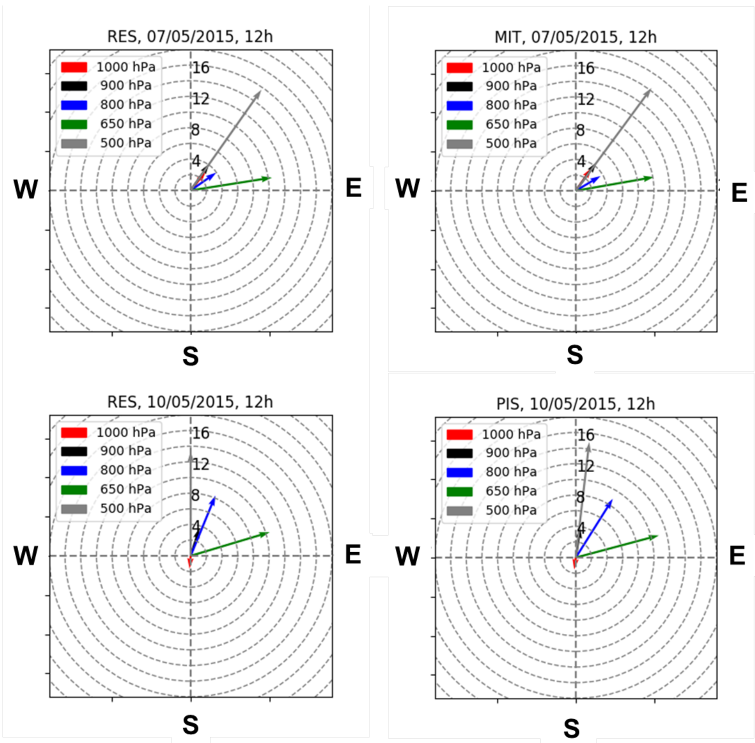

Fig. 1. Hodographs (wind speed direction in different heights)