Atmospheric
Chemistry
and Physics
Discussions

Open Access

ACPD

**Interactive
comment**

Interactive comment on "A multi-model approach to monitor emissions of CO$_2$ and CO in an urban-industrial complex" *by* Ingrid Super et al.

Anonymous Referee #1

Received and published: 20 September 2016

The paper by Super et al. seeks to interpret observations of CO2 and CO with a combination of Eulerian and Lagrangian models and attempts to make conclusions about the modeling framework, as well as the observational network.

However, I found the paper to be difficult to follow in what it seeks to accomplish, and I often do not see robust evidence for the claims it makes. Therefore, I cannot recommend publication.

MAJOR POINTS: (1) The concept of a "background" is mentioned throughout the paper, but the concept remains nebulous without adequate clarification. Westmaas is referred to as a "background" site while both Cabauw and Lutjewad are referred to as "regional background" sites. What are the differences between these different categories? Similarly, towards the end of the paper the terminology of "representative back-

ground" versus "regional background" stations are introduced. What do these mean? Can the authors be more quantitative by referring to spatial lengthscales relative to urban lengthscales and backing them up with observations and models? Furthermore, exactly how the background is accounted for within the analyses is also confusing. It appears that sometimes the background comes from global model products (e.g., CarbonTracker) while other times the background is determined from the observations (e.g., average diurnal cycle at Westmaas during westerly winds). What are the pros and cons of using a model-derived boundary condition versus one derived from an observed time series? These approaches are taken without motivation while I believe accounting for the background is a critical part of designing an observational network to isolate and quantify the urban signal. For example, what if the Cabauw observations are used for background determination? How much additional error would this incur? These issues remain unexplored. I also do not see the evidence supporting the claims of "Cabauw is a suitable regional background site" and "Westmaas provides reasonable background constraints to determine the urban plume signal."

(2) For a paper that makes strong claims about the value of the OPS Gaussian plume model I find the paucity of technical details about OPS to be a significant weakness. For instance, what happens to the Gaussian plumes as they are transported far away from the sources? Do they undergo "puff splitting" or "puff merging" as some other models do? Where do the turbulence variables to drive the Gaussian plumes come from? How does OPS make use of observed meteorology? The met variables are observed at point scale; how are they interpolated in space? And exactly where are the meteorological observational sites used in these study? Another OPS detail that need to be brought up earlier is the roughness length, which was only mentioned near the end of the paper.

(3) The comparison of model performance between WRF-Chem versus OPS can be much more sophisticated. First, the weakness of WRF-Chem vis-a-vis OPS is attributed to vertical and horizontal dilution. Can most of this problem (particularly pro-

nounced during stable conditions) can be addressed by simply suppressing the vertical dilution of surface emissions within WRF-Chem? Can the authors test this? And how much of the weaknesses in WRF-Chem was due to erroneous windfields simulated by WRF itself? The authors pick out various schemes (e.g., YSU PBL scheme) as mentioned in Sect. 2.1.3, but how were these selected in the first place? Have the authors compared the simulated windfields against observed windfields?

(4) How the authors determined the "urban plume" mentioned in the first paragraph of Sect. 3.2 was entirely unclear to me. Were the plume strengths based on observations or the model? If the former, then how was the background determined (which relates back to issue (1) above)? On somewhat similar note, I had difficulties later on in Sect. 3.2 regarding the CO:CO2 ratios. If CO:CO2 ratios were observed, did they come from a regression of absolute CO and CO2 concentrations? Or were the backgrounds subtracted out? If so, what was used for the background? And why weren't the observed distributions of CO:CO2 ratios shown in Fig. 5 (only the means were shown)?

(5) The value of Cabauw is difficult to ascertain for me. The model suffers from the largest biases for daily concentrations at Cabauw. Later on in the paper the authors also claim that "WRF-Chem performs best at the Cabauw site" for simulating urban plumes. But wouldn't the biases affect modeling of urban plumes? How these two contrasting points are reconciled is unclear to me. Also see point (6) below regarding the potential problem during the growing season.

(6) The authors chose the months of Oct~Dec to carry out their study. The biospheric photosynthetic signal is much weaker during this time. How would their conclusions regarding the observational network change if months during the growing season are selected? Wouldn't this cause problems at a more removed site like Cabauw?

OTHER SPECIFIC POINTS Line 254~255: I believe that the local contribution also depends on wind direction and not just the regional contribution. Doesn't it matter which part of the urban area a site is sampling? Similarly, I disagree that advection

solely affects the background CO2 mole fraction, as mentioned in Line 130. If one writes out the tracer-transport equation, the advection term shows up prominently.

Table 3: What happens if you subset the time period to afternoon only? Many studies focus only on the afternoon due to difficulties in modeling nighttime mixing.

Fig. 1: The observation sites are difficult to pick out, and the names of the sites should be added to the figure. Lat/lon should also be added to the figure. Another helpful addition would be to overlay the sites onto a map of CO2 or CO emissions from the inventory to help the reader assess the locations of the sites relative to anthropogenic sources.

Fig. 3: How are "ff regional" and "ff local" defined? Should explain in the main text. Also, I suggest using a less prominent color for the background. Perhaps gray instead of the current yellow, which I find very distracting.

Fig. 5: Why are the observed distributions of CO:CO2 ratios not shown?

———————————————

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-807, 2016.