

Interactive comment on “A multi-model approach to monitor emissions of CO₂ and CO from an urban-industrial complex” by Ingrid Super et al.

Anonymous Referee #2

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Summary/General comments:

Super et al. combine observations of CO₂ and CO from urban and ex-urban sites in the Netherlands with an Eulerian modeling scheme (WRF-Chem) that explicitly accounts for plumes for large point sources to evaluate the utility of different urban/exurban observations and determine the utility of an Eulerian model in quantifying urban fluxes of CO₂. This is a thorough, well written paper that contributes significantly to the field of urban GHG research and is well placed in ACP. I enthusiastically recommend publication once these minor comments have been addressed.

Major comments: The largest critique is the breadth of the conclusions implied in the abstract. Most pointedly, line 25, should instead state a plume model can be added to the model framework to account for point sources – the authors have shown that in

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an Eulerian model of typical regional resolution plumes an incorrectly represented and a plume model can fix this. However, a Lagrangian model, LES model, or very high resolution Eulerian model may not require this and the authors have not demonstrated as such. Similarly, line 33-34 are overstated. Integration of a plume model is not inevitable, as the authors have not shown alternatives are inadequate. The authors have shown that integration of a plume model is a possible solution for using a regional lagrangian model and surface point observations for CO₂.

The authors have shown in compelling fashion the need for accounting for stack CO₂ emissions w/ a plume framework. It is interesting that this is not the case for CO, and it would be nice for that to be highlighted. Further, I wonder then if a plume model representation would be important for methane? Also, the authors are considering surface, point observations. If total column observations are considered, is a plume model essential or is the vertical dilution now irrelevant? This is perhaps a question beyond the current analysis, but it would be an interesting point to comment on.

Detailed comments:

Line 57: This is dependent on urban typology and emission characteristics. The authors should acknowledge this limitation here.

Lines 86-90: Other cities have also been studies – most notably Boston and Indianapolis, there are a sequence of INFLUX papers that it would be appropriate to cite here.

Line 175-183: I worry about this sweeping the VOC CO production under the rug. How much does this really matter? I suspect the authors' analysis is robust to this as the VOC CO production is embedded within the determination of the boundary condition, and thus ignoring it is ok as the amount produced in the near field (within 24 hours) is modest. I'd like a little more discussion of this, and estimates of how much this may matter if the same approach is taken in the summer?

Title: I'd suggest a change as the manuscript is really not monitoring CO emissions,

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but leveraging CO to better interpret CO₂ emissions, and the current title is a little misleading.

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