Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-431-RC1, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A multi-model approach to monitor emissions of CO_2 and CO from an urban-industrial complex" *by* Ingrid Super et al.

J. Turnbull (Referee)

j.turnbull@gns.cri.nz

Received and published: 21 July 2017

This paper describes a combined Eulerian/plume model approach to evaluate CO2 (and CO) emissions, using Rotterdam, The Netherlands as an example. The authors show clearly that embedding a plume model within the Eulerian model improves the overall model fidelity in areas close to point sources. The results demonstrate that this is due to the Eulerian model resolution being insufficient to capture the details of nearby plumes. Presumably with infinite computing power, the Eulerian model could overcome this limitation, but embedding the plume model is a more computationally efficient solution. Further away from the point sources, the plume model doesn't add much, since the Eulerian resolution becomes sufficient at these spatial scales. They also evaluate the effect of wind direction biases in the Eulerian model (WRF-Chem is

used in this case), and show that using observed meteorology makes a big improvement in the model fidelity close to point sources – this is not a surprise, but nonetheless is a nice result.

This is a well-written, clear and easy to read paper. It is entirely appropriate for publication in ACP and I recommend it be accepted with minor modifications, detailed below.

General comments: 1. Is there a reason why there are no runs where WRF-Chem is nudged with the local meteorology? Given the results that show using local meteorology in the plume model really helps, this would be an obvious test to do. 2. I would like to see a bit of discussion about the relative difficulty of running the combined Eulerian/plume model simulations. How much more computation time is needed vs running WRF-Chem alone? How much additional effort (not computation time, but people time) is required – is it set up to run easily, or does someone have to sit there and do each plume run individually? 3. The method presented here requires that the point sources are known from a bottom-up inventory and that the bottom-up information has the correct locations. That's going to be difficult in many cities where the information simply isn't available. Some comment on this (in the conclusions or discussion) is needed.

Specific comments: Line 33 (abstract). "Inevitable" seems a strong word to use – clearly the plume model helps a lot, but one could imagine other ways to address the same problem. Tone down the words used. Same for line 558 in conclusions.

Line 65. Consider revising wording from "...that mask the urban signal" to "that mask the overall urban signal".

Lines 175-189 – CO fluxes. I agree that ignoring oxidation of hydrocarbons is probably reasonable for the winter months considered here, but I suspect that biofuel combustion might be important. Biofuel combustion (such as wood fires for home heating) tends to be quite inefficient with high CO:CO2 ratios, so that even a small contribution to the CO2 source might mean a significant CO source. The CObiofuel source could be estimated by combining the CO2biofuel flux estimate with an estimate of the emission ratio. Andreae and Merlet 2001 is a good (even if old) resource to make some guesses about the emission ratio. Andreae, M.O., Merlet, P., 2001. Emission of trace gases and aerosols from biomass burning. Global Biogeochemical Cycles 15, 955-966.

Lines 196-200. I take it the plume model is run forward (not backward as is common when plume models are run as a stand-alone)? Consider stating this explicitly to clarify.

Lines 280-291. The choice of background definition from the baseline values is clearly more workable in this particular environment than using an upwind background. Nonetheless, it could be a problem on occasions when the incoming air is unusually polluted – the baseline background will not account for that. Please add a comment on this.

Lines 333-347. I agree with the interpretation as described here, but I think you also need to discuss other possible explanations for the difference between observed and modeled CO:CO2 ratios, and why these possibilities are less likely. Is it possible that the point source CO:CO2 ratio is in fact higher than reported? Perhaps the inventories are wrong, and/or the industries are not scrubbing CO as effectively as they claim to? CO from biofuel is not included in the model (see also earlier comments) – how would including this alter the modelled ratios?

Line 357. I don't think you ever spell out what RMSE is. Please do so the first time you use it.

Lines 365-367. This effect has been seen before. Please add appropriate references.

Line 368. I am not sure what you mean by "there is no co-sampling for this comparison". Please revise for clarity.

Lines 369-370. You remove low wind speed data. Some additional discussion about the overall performance of the model when all data is included is needed. Do you conclude that it is generally difficult to model low wind speed time periods and they should always be discarded? In many environments, low wind speeds are when it is

particularly cold and more CO2 is generated for heating, so removing this data might bias the overall analysis to lower emissions.

Lines 374-376. Sentence beginning "However, if we co-sample…". I don't understand how this is different than the previous analysis you discuss. Please clarify.

Lines 393 – 395. "At Cabauw…" This sentence seems out of place.

Section 3.3. I would like to see some plots of the comparison in addition to the summary in the table. Perhaps as supplementary material?

Lines 415-417. Clarify that you don't show the data for this particular test.

Line 470. "specifically constrain", not "constrain specifically".

Jocelyn Turnbull, July 21 2017

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-431, 2017.