

General

The study "A multi-model approach to monitor emissions of CO₂ and CO in an urban-industrial complex" by Ingrid Super and co-workers investigates the possibility to estimate anthropogenic CO₂ emissions from an urban complex by a combination of atmospheric observations and transport models. Two different model types and three different simulations of CO₂ and CO are used to demonstrate the ability of the model system to correctly reproduce observed concentrations and their ratios. The study draws some valuable conclusions on the kind of observing system required to monitor city-scale anthropogenic emissions. The work is generally presented well and merits publication after a number of rather minor issues (mostly clarifications and additional discussion and justification) as outlined below have been addressed.

Major comments

Performance of WRF: The manuscript would benefit from a more detailed discussion on the performance of the high-resolution WRF simulations in terms of meteorological variables. For the OPS simulations two sets of simulations (observation-based and WRF-based) are presented to allow for a fair comparison between OPS and WRF concentrations. The results then show that the observation-based OPS outperforms the WRF-based OPS simulations. Part of this is obviously due to the deficiencies of the WRF simulated meteorological conditions. Therefore, it should be shown how WRF performs in terms of wind speed and direction, atmospheric stability and atmospheric boundary layer height. To this purpose a comparison between the meteorological parameters driving OPS would suffice. This subject is briefly touched on in the Discussion section, but it should be given more room in the results as well.

Minor comments

Plume model type: The manuscript introduces the plume model (OPS) as a Lagrangian model (L15), whereas later on it becomes clear that OPS is a Gaussian plume model. Although Gaussian plume models are often categorised as Lagrangian, I would prefer if the term Gaussian would be used throughout the text in order to distinguish the present model from Lagrangian particle dispersion models. See also L64.

L28: Please define 'extreme CO₂ pollution events' here.

L66f: Replace with: 'However, Eulerian models assume that chemical species are instantly mixed within individual grid boxes, which may enhance ...'

L70f: Be more specific. In which situations and on which scales can a plume model improve the simulated concentrations field. Why is this the case? A plume model does not have to be computationally expensive. Actually the main reason for their use is their simplicity and their low computational costs. The question of computational time should rather be discussed from the perspective of a Eulerian model with sufficient grid resolution to resolve the scale that is targeted by the plume model. In that case the plume model most certainly will be the computationally cheaper solution.

L75: It is somewhat confusing to speak of 'three modelling frameworks' and then only two models are listed. Please clarify that one of them is used with two different meteorological data sets.

L77ff: The discussion on other studies that have used high-resolution modelling and emission inventories to simulate greenhouse gas emissions on the urban scale should be extended. As correctly stated Oney et al. (2015) did not focus on urban areas specifically, but there are other studies that have used different model frameworks to tackle urban scale greenhouse gas emissions, some of them are already mentioned later on in the manuscript. However, it would be good to put the present study into context by referencing previous work. For example: McKain et al. (2015); Feng et al. (2016); Boon et al. (2016); Wu et al.

(2016); Bréon et al. (2015); Brioude et al. (2013)

L88: Instead of contrasting 'urban' with 'industrial' it may be better to contrast 'residential and transport' with 'industrial'. 'Urban' is a bit vague and may include 'industrial' sources as well.

CO₂ budget, L112ff: Why is the stratospheric component discussed separate from the background? This seems a bit arbitrary since other CO₂ sources (e.g., biomass burning) are also not explicitly mentioned in equation 1 and are assumed to be part of the background. Would CarbonTracker not cover strat-trop exchange as well? In a way it would be better to first mention how the background is derived and which CO₂ contributions can be expected to be presented by the background and then discuss the regional contributions. What is also missing from the equation is the use of bio-fuels. If these are supposed to be accounted for by the fossil fuel term then the latter should be renamed to 'anthropogenic' instead.

CO budget, L133: The assumption that hydrocarbon oxidation can be neglected as a source of CO should be discussed in more detail, since there are several studies that showed that the 'secondary' CO contribution should not be neglected (Griffin et al., 2007) or is even dominating (Hudman et al., 2008) for summer-time conditions in the eastern US. Please discuss these findings and why your assumption may still hold for the present study. What are the consequences of neglecting the secondary source with respect to the results of the present study? Again, there is no CO source from bio-fuel burning considered in the CO budget (see previous comment). The given CO lifetime is representative for winter-time conditions. Please point out this fact and what it implies for the present study.

L146: What was the vertical grid resolution in the lowest 300 m of the model? This is important for the interpretation of the model-observation comparison. Would the model be able to resolve shallow stable night-time boundary layers? It would also be interesting to know how model concentrations were extracted for the location of observations (nearest grid box, horizontal and vertical interpolation, ...)?

L150: What kind of advection scheme was used in WRF? Was it locally mass conserving? This is important in terms of the analysis of CO/CO₂ ratios, which are not necessarily conserved by every advection scheme. What is the impact of using the urban module in WRF? Does it improve the model's performance in the study area?

equation 4 and 5: The units and/or magnitudes of SW and the factor on R, as explained in the text, must be wrong. SW and the factor on R should be unitless and yield unity if integrated over a whole month.

equation 4: The sub-monthly scaling for photosynthetic uptake only depends on available solar radiation and neglects any short term effects of water stress, temperature and CO₂ concentration on photosynthesis. Can this be justified in the context of the study?

section 2.2: Spend more time on the limitations of the OPS model approach. Basically the plume model gives a steady state solution under given meteorological conditions whether or not these really persisted for an hour. The model probably also assumes horizontal homogeneity. Is this justified? Therefore, it is important to know up to which transport distances (and times) the model was considered. What kind of horizontal wind speed was used in the model (10 m from observations)? Does the model consider a vertical gradient in the horizontal wind speed? Gaussian plume models are known to be invalid under low wind speed conditions. What was done for horizontal wind speeds < 1 m/s?

L188: Please clarify what is included in 'local' emissions. Are these the same as 'the Rijnmond area' emissions, which were treated separately in WRF and from the area indicated by the blue line in Fig 2?

L203: Use 'photosynthetic uptake' instead of 'biogenic' and refer to equation 2 and 4 for clarification.

section 2.3: It would be interesting to get some numbers on total population and emissions in the area, which would allow putting this study into perspective.

L211 and elsewhere: Are the inlet heights given in m above ground or sea level?

L255: Is 'the Rijnmond area' the same as indicated by the blue line in Fig 2? Please clarify.

L267 and Fig 4: Please indicate the period of easterly winds as a gray shaded area behind the CO₂ and CO time series.

L271f: Obviously the bias in CO₂ will be small compared to its atmospheric abundance. Discuss some of the comparison statistics as given in Table 3. It would also be good to contrast these daily statistics with hourly statistics in order to see how well the diurnal evolution can be captured.

L276f: What could be the reason of the reduced performance during easterly advection? Is this more likely related to transport errors or to underestimated emissions? CO emissions used in TNO-MACC are known to be too low for most of central and eastern Europe. Could this be the main reason for the underestimation?

L287: Add another reference to Table 3.

L289f: Did the bias for CO₂ at Cabauw exist at all elevations?

L296: Which simulation is referred to in this sentence?

L298: Noise as in observational noise or variability from other sources?

L314f and Fig 5: It is not clear how this pdf was derived. Is it the variability over time or somehow accessed from transport uncertainties? Why would it follow a Gaussian distribution? Why was the same kind of pdf not given for the observations as well? Please add the empirical pdf for Zweth-port and Zweth-Rotterdam and not just the mean values (maybe as bars). Give the emission ratios for the port and Rotterdam area (as described in the text) also as vertical lines.

L328f: I don't understand this conclusion. You can distinguish those sources in the WRF simulations anyway, since you are using tagged tracers for different regions. Wouldn't it make more sense to conclude that you can distinguish between urban and industrial in the measurements?

L343f: However, this conclusion relies on the applied model system. Using a local scale dispersion model that considers individual stack emissions and plume rise, one should be able to use even the Zweth data in an atmospheric inversion.

L465f: This should also be related to the limitations of the plume model as to be discussed in more detail in the methods section.

L467: Be more concrete about the distances here. Due to its assumptions of horizontal homogeneity a Gaussian plume model should not be applied at distances larger 50 km anyway.

L491: This should be discussed in more detail in the results section. See major comment above.

L501f: The possibility to combine the high-resolution WRF simulations also with Lagrangian particle dispersion simulations in the receptor mode (e.g. FLEXPART or STILIT) should be discussed here as well.

L505: The distinction between the two background sites has to be clarified. One represents a larger scale background mostly unaffected by emissions from the region of interest, whereas the second represents the emissions from the region of interest at a distance that allows for sufficient mixing similar to that in the applied transport model.

Fig 1: Please include longitudes and latitude labels on the axis.

Fig 2: Please include a scale indicator and/or longitudes and latitudes.

Fig 3: What is the time base of the shown data (daily/hourly)? Please add information to caption.

Fig 6: Somehow it looks as if the integral over the different pdfs does not yield 1 as it should. For example the red curve is almost always larger than the blue curve. How is that possible? Please check. Are negative values for ΔCO_2 considered?

Technical comments

L22: Add 'model' in front of 'framework'.

L69: Replace with: 'The magnitude of the corresponding error depends on ...'

L75: Replace 'Therefore' by 'Here'.

L306: There is an unintended '=' in the text.

References

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