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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimating CO_2 emissions from point sources: a case study of an isolated power station" *by* S. R. Utembe et al.

Anonymous Referee #1

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General comments:

1. The authors present observations from in-situ measurements of CO2 and CO, and ground-based column measurements of XCO2, for sites near an isolated power plant in Port Augusta, South Australia. Observations are compared with WRF-Chem modeling results, and scaling factors for prior emission estimates are derived from a comparison of observed and simulated values. While the paper is well written overall (methods and data are described clearly, relevant literature is cited appropriately, and the material is organized clearly), the material presented is too preliminary to be published in its present form.

2. My main concern with this paper is that the data sample is too small to reach

meaningful conclusions. Only 9 days of data went into the analysis, and there were obvious problems with the modeled near-surface wind fields on 4 of those.

2.1. The results for the in-situ data appear to be dominated by just a few isolated simulated peak values that were not observed. While the data presented suggest that modeling short-comings make it impossible to use in-situ data for emission factor estimation, this conclusion may not hold in other synoptic regimes. A larger data set may permit assessment of other options to avoid/mitigate the problems in transport modeling (restrict by times of day, days without a seaabreeze or smaller errors in the seabreeze simulation, ...).

2.2. Because of the small sample size, uncertainties of the scaling factors derived from column data are large (much larger than the 6% accuracy cited in the conclusions), and the differences between any of the sensitivity tests are all within the uncertainty interval.

3. Methodological comments:

3.1. Treatment of the background and individual tracers

I found the approach taken (adding the background to each individual tracer) confusing and not well motivated. I can follow the argument that the in-situ enhancements over the advected background are dominated by a single source, and hence estimating the emission factor as described in p. 31568, lines 11-20 is appropriate. A puzzling result is that adding the background seems to worsen the fit to the observations, even though the optimization procedure was for the enhancements above the background.

However, for the column data, other influences and their uncertainties are non-negligible, such as the biosphere (p. 31573-31574) and variations in the advected background. The procedure assumes these are perfectly known. It is also not clear to me how to interpret the error statistics of (bg+biosphere) and (bg+NPS) separately. If both are important, wouldn't a multiple regression procedure be more appropriate?

3.2. Injection height sensitivity tests

I was surprised to see that these tests were performed for the column-integrated, not in-situ data. Except for cases of strong wind shear in the layer included in the emission heights, the effect on XCO2 should be small, but it could make a big difference for the in-situ values.

4. Discussion of Results

4.1. In sections 4.1 and 4.2 there is some discussion on which sources are responsible for the observed variations, but unless I misunderstood this, this discussion is based on a qualitative analysis and conjecture, not on modeling results. Since the modeling was performed for the individual tracers, the results should be used to back up the assertions made in this section.

Detailed comments:

1. p. 31555, line 7: measurements are described for CH4, N2O, and 13CO2, but not used in the rest of the paper. Please explain which data were used and why.

2. Eq. 1: This is a minor point, but the 3rd term is also represented in WRF-Chem, is it not (advected initial conditions)? The bias correction q0 is really a separate issue.

3. p. 13559, line 11: please provide more detail on level placement (approximate heights AGL), at least in the lower part of the domain.

4. p. 31561, lines 8-17: it was not clear which sources were modeled as individual tracers, and which were neglected. Please clarify.

Please also add a description here of the emission height (I found myself wondering how the plume height was represented until I reached the description of the sensitivity tests).

5. Eq. 2: how good an assumption is this for cases where the NPS signal is concentrated at low levels? Can you estimate how big an effect this has on your simulations?

6. p. 31567, line 1-2: I agree it is tempting, but I am not sure it is defensible. Moving individual simulated peaks in time by hand is not a feasible approach for source estimation, and the "improved" correlation coefficient is not meaningful.

6a. It might be helpful to include an indication of the modeled PBL height relative to the emission height in Figure 8, to help identify plume touchdown peak concentrations.

6b. In Figures 10 and 11, which tracer is shown?

7. p. 31570, line 11: The term "local contamination" is not really appropriate here: the NPS is a very local effect, and that is the desired signal. More to the point is the argument made elsewhere in the paper that changes (and hence errors) in the PBL depth do not affect the measured or simulated value.

7. p. 31570, line 23-14: I am not sure exactly what is meant by "such a calculation", but there have been other studies using a simple linear rescaling of source strength (e.g., McKain et al. 2012: PNAS, 109, 8423-8428, doi: 10.1073/pnas.1116645109).

8. p. 31575, line 1-16: The results presented do not support the conclusion that the accurate description of the emission profiles is important: agreement with the observations is better with the simpler source description, and the derived emission factor is within the uncertainty interval of the original estimate. If anything, these results point to deficiencies in the modeling.

9. p. 31575, line 23 - p. 31576, line 2: As explained in my general comments above, the conclusions are not supported by the results: the uncertainty is much larger than the stated 5% accuracy, and data sample is too small to draw any fiorm conclusions about the usefulness of either data source for source estimation.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31551, 2014.