Atmos. Chem. Phys. Discuss., 14, C11759–C11764, 2015
www.atmos-chem-phys-discuss.net/14/C11759/2015/

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 #2

Received and published: 1 February 2015

General Comments:

The paper describes a method for estimating CO2 emissions from an isolated power plant that includes in-situ and remote sensing measurements, along with forward modeling and linear regression to estimate source emissions. This addresses a relevant scientific question within the scope of ACP and the paper presents novel data. The authors give proper credit to related work, with an appropriate number and quality of references. The title clearly reflects the contents of the paper and the abstract provides a complete summary. The overall presentation is well structured and clear and the language is fluent and precise. However, I have substantial reservations about publishing the paper in its current form.

Scientific methods and assumptions need to be clarified in some sections.

1) In the discussion of the CO2 sources, explain how data on the energy output at NPS is converted to provide the WRF CO2 emissions rates. What are the sources of CO2 in Port Augusta and Stirling North and how are they input into the WRF simulation? Are they treated as area or point sources? Are the steel works emissions assumed to be constant in time as they are input into WRF? What data is used for the Leigh Creek CO2 emissions within WRF? How far away are the bush fires in the Flinders Ranges? A plot of the WRF emissions would be helpful in illustrating the sources in the region. The authors state that the last few days of the experiment that was conducted 7 to 16 May 2012 are not analyzed because of the influence of these fires, yet the figures and discussion do refer to dates through the 16th.

2) Although not explicitly stated as an assumption, the winds at the Port Augusta airport are presented as if they are representative of the winds in the entire area and affecting the NPS emissions. Is there evidence that this is true? It is located only 6 km from the NPS, but it also appears to be located on the opposite side of a body of water. Does this body of water affect the local winds (i.e. sea breezes)? The WRF horizontal wind fields may provide some insight on the spatial variability of the winds, although the narrow water feature at its northern end may not be fully resolved with 1 km horizontal grid spacing. In addition, winds above the surface are not discussed. Wind direction and speed are typically not constant with height and will affect plume transport differently at different altitudes.

Some parts of the paper would benefit from better explanation or clarity. I suggest the following changes:

1) Figure 1 – Add a scale of distance, so that someone unfamiliar with the area can picture the size of the study area. The labels should be larger and darker for better readability. What do the colors represent? If the blue is water, the body should be mentioned, because it will have different surface properties than land and can affect

circulations. Also, topography should be described. If it is relatively flat, a short statement of that fact would be sufficient. Two locations in the text on page 31555 , Stirling North and Miranda, should be located on the map.

2) This is a suggestion, but is left to the authors' discretion. Equation 1 and its description do not add much to the discussion and could be removed from the paper. The first sentence of Section 3 could remain and the last two sentences before Section 3.1 could be modified to name the quantities, instead of referring to terms in equation 1.

My major objection to the current version of the paper is that some statements in the paper are not fully supported by the results.

1) In particular, the repeated statement that the column averaged XCO2 is less sensitive to or unaffected by local sources and sinks was not proven in the study. Lindenmaier et al. (2014) demonstrated significant impact of a near-by power plant plume on column CO2 concentrations and strong correlation of column and in-situ measurements during plume events. It is true that CO2 concentrations measured by in-situ measurements increase by a greater percentage over their background concentrations in response to emissions, but that does not mean that the column concentrations do not respond to the local sources. They register a smaller signal against the background concentration, because of the much greater volume sampled by the remote sensing instrument that includes substantial amounts of relatively clean air at high altitudes. The paper does not present any evidence that this feature of the column measurements is dependent on source location.

2) The paper claims that the model should simulate column-averaged concentrations better than in-situ concentrations because the column-averaged concentrations are more immune to local contamination. This claim neglects a significant feature of the model that calculates concentration as an average within a grid cell and is thus representative of a much larger volume than an in-situ measurement. This feature of Eulerian models is noted in section 7.2 but is not included in the discussion of why the

model better simulated the column-averaged concentrations.

3) Also, the number of days studied (and thus the number of atmospheric conditions sampled) are not sufficient to support a universal claim that column-averaged concentrations are unaffected by local sources. It also provides a very small sample size for computing the statistics in Table 2, casting doubt on the usefulness of those statistics.

4) The authors state that they do not expect to see the NPS plume at the Southern site, because it is too far from the NPS for the plume to touch down (page 31563, lines 7 and 8). The reasoning for this statement is not given, nor do they present evidence to support it. The dropping of an elevated plume toward the surface is dependent on atmospheric conditions, not distance from the source. A more likely explanation of less influence of the NPS at the Southern site is that the plume is diluted more over the larger distance from the source.

5) Similarly, the authors give plume touch down as the reason for coincident peaks in the in-situ and column measurements (page 31564, line 6). The coincident peaks at the Northern site probably result from higher concentrations in a number of layers above the surface or particularly high concentrations in a few layers. A relatively undiluted plume diving to the surface near the measurement site is one, but not the only, possible explanation of the coincident peaks.

6) Section 7.1 discusses model resolution. The authors claim that the 1 km grid horizontal grid resolution used in their innermost domain resolves the 3 km distance between the power plant and measurement site. This grid spacing is not adequate to resolve features less than 4 km in size (assuming the generally accepted minimum of four grid cells to resolve a feature that could influence the plume). They also cite Talbot et al. (2012) to justify not using finer grid spacing, but the conclusion from Talbot et al. (2012) that they reference refers to regionally averaged results. Yet this paper is comparing modeled and observed concentrations at two specific locations, not regionally averaged concentrations. Talbot et al. (2012) also states "Increased resolution

improves the ability of WRF to capture surface variability, which facilitates comparison with measurements and thus improves model validation." that provides an argument for finer resolution to improve model results.

7) Section 7.4 presents results of including biospheric emissions in the WRF simulations. While the calculated contribution to the column enhancement is smaller than the NPS contribution, it is not insignificant and thus sheds doubt on the assumption that NPS is an isolated single source. This assumption is used to calculate the multiplier for the emission rate, using linear regression.

Specific Comments:

Page 31552, line 25 – The reference (IPCC, 2007) does not match the reference and probably should be (Solomon et al., 2007).

Page 31559, line 8 – The term non-hydrostatic in this sentence appears to describe the vertical coordinate, which is incorrect. Non-hydrostatic actually indicates that the model equations do not make a hydrostatic assumption.

Page 31559, line 10 – The Lambert Conformal projection is used to map real data to a sphere.

Page 31559, lines 17-20 – This sentence is redundant with Table 1 and could be eliminated. If it is kept, a comma should be inserted before and after "respectively" and an "and" added before "the NOAH land surface model" in order to appropriately indicate that four different parameterizations are mentioned in this sentence.

Page 31567, lines 11 and 12 – Suggest removing the word unimportant and rephrasing this sentence to "This does not affect the emissions estimates, because the mean level is explicitly calculated by the inversion."

Page 31569, lines 9 and 10– There are two power plants, the Four Corners Generating Station and the San Juan power plant.

Page 31570, line 9 – Explain what is meant by recirculation.

Page 31576, line 12 – The year should be 2013.

All Figures – Be consistent with the case of the letters used to label the individual panels and used in the figure captions, by using all lower case letters.

Figure 3 - caption should include the concentration measurements location.

Figures 8 and 9 – Blue and green lines are difficult to tell apart. Perhaps one could be dashed.

Figure 14 – Labels need to be explained in the caption (e.g. PP is NPS).

A number of articles in the List of References do not appear to be cited in the paper. These include Chevallier et al. (2011), Cockede et al. (2006), House et al. (2003), Rannik et al. (2000), and Rayner et al. (2009).

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