

## ***Interactive comment on “Estimating CO<sub>2</sub> emissions from point sources: a case study of an isolated power station” by S. R. Utembe et al.***

**S. R. Utembe et al.**

steven.utembe@unimelb.edu.au

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### **Response to Referees' Comments**

S.R. Utembe et al.

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We would like to thank the two anonymous referees for their comments which have allowed us to clarify various points in the paper. Since the referees make different points we will respond to each in turn. Below we place the referees' comments in typewriter font and our responses in Roman.

#### **Anonymous Referee 1**

##### **General Comments**

The authors present observations from in-situ measurements of CO<sub>2</sub> and CO, and ground-based column measurements of XCO<sub>2</sub>, 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

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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. 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.

The question of how much data is required before publication is always difficult. We agree that "it works" is too strong a conclusion to draw from this data set. It is also important to be clear always about the sizes of uncertainties. We have revised particularly the abstract and conclusion to ensure that the uncertainties and their implications are clear. This, of course, doesn't answer the question whether the work should be published at all. That depends on other considerations such as timeliness vs certainty and the danger of preliminary results leading the field astray. We believe that the growing interest in regional inverse studies and the growing list of column-integrated measurement capabilities (e.g. the recent GHOST instrument and the funding of the ACT-America campaign) do make this a timely contribution. We also believe that clarity about the uncertainty leaves little risk of over-interpretation.

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 sea breeze or smaller errors in the sea breeze simulation, ...).

The reviewer makes a valid point that the usability of the in-situ data depend on pre-

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vailing synoptic conditions which may be modelled differently under different synoptic regimes. As stated above, our next task will be to collect a larger dataset in order to evaluate this more deeply. We have therefore, added text in section 5.2.1 that states: "...This improves the correlation somewhat to 0.42 which is still poor. This underscores the deficiencies of the model to simulate the in-situ tracer under the given synoptic conditions."

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.

The reviewer is correct that the uncertainties are large (see discussion above). We have been deliberately conservative in calculating uncertainties on the emission factors. We have used an observational error of 1 ppm while the posterior simulation error is 0.44 ppm. Rigorous propagation of this error would halve the posterior uncertainty of the emission factor. We think this conservatism is warranted at this stage. The most important difference, between in situ and column-integrated measurement, is not affected by this consideration.

#### 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.

The motivation for adding background to the individual tracers is to avoid negative concentrations in the WRF-VPRM model. Since WRF-Chem does not allow negative

concentrations, the advected tracer fields 'disappear' in cases where concentrations become negative even when the positive definite advection scheme is used. This is thus avoided by initialising with a global tracer and also force them with lateral boundary conditions. This global field is later subtracted in order to account for sources inside the model domain.

I 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.

Adding the background improves the fit to the observations for the column data (correlation coefficient from 0.67 to 0.72). It does not improve the fit for the in-situ data (from 0.42 to 0.30 in the case of the time-matched peak scenario) but this is because of the model's inability to model plume touch down at the power point site.

However, for the column data, other influences and their uncertainties are nonnegligible, 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?

The biospheric source matches closely with the background so that it can be assumed to a good approximation to be the background. The nature of the region is semi-arid so that the biospheric source is expected to make a smaller contribution to the CO<sub>2</sub> signal than the power plant ( which is about 800 times larger). Thus to a good approximation

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the prevailing signal in the model domain is dominated by the power plant whose signal we have subtracted from the background.

### 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 XCO<sub>2</sub> should be small, but it could make a big difference for the in-situ values.

The reviewer is correct in saying that the injection height does indeed make a big difference to the simulated in-situ signal than for the column. However, our injection height sensitivity study did not lead to improvement in simulated in-situ data and because this paper is already large, we thought it wise to present only the column-injection height data as our conclusion in this paper shows the column-data to be the more reliable signal (for given synoptic conditions under which the data was collected and analysed).

### 4. Discussion of Results

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.

The reviewer has misunderstood sections 4.1 and 4.2. Here the discussion is purely on the measurement data and not modelled data. Its a qualitative discussion of the

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observed in-situ (section 4.1) and column averaged (section 4.2) time series with respect to the measured winds. We seek to explain the individual observed signals at the Northern and Southern sites with respect to the observed wind directions.

#### Detailed comments

1. p. 31555, line 7: measurements are described for CH<sub>4</sub>, N<sub>2</sub>O, and 13CO<sub>2</sub>, but not used in the rest of the paper. Please explain which data were used and why.

This is a valid point. We have modified the text in section 4 in order to make clear what data was used and why. The text now reads:

"Concentrations of CO<sub>2</sub>, CH<sub>4</sub>, CO, N<sub>2</sub>O and 13CO<sub>2</sub> in near-surface air and XCO<sub>2</sub> were measured during the Port Augusta measurement campaign. In this paper, we use concentrations of CO<sub>2</sub> and CO in near-surface air and column-averaged XCO<sub>2</sub> in order to disaggregate the influence of local and regional sources."

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  $q_0$  is really a separate issue.

As stated in the text the structure of the initial condition will wash out provided the spin-up time is long enough. This is the case here. But we still need a representation of the amount of CO<sub>2</sub> in the atmosphere. We also need something to represent potential biases of the instrument. We could have carried these as two separate constants with their own uncertainties but their impact on the inversion is impossible to separate (they have equal projection onto the observations) so, unless we wanted to learn about them for their own sake, there is no point carrying them separately.

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

This is a good point and we have now added text in section 3.1 that describes the AGL height of the first few levels in the PBL.

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).

We appreciate the reviewers comment and we have now clarified which tracers were modelled and which ones were neglected so that text now reads (in section 3.3): "With the exception of bushfires, we have assigned point source emissions from the NPS, area source emissions from Port Augusta town and the biosphere in the model as different tracers in order to account for their relative contributions to the observed signal in Port Augusta. We have neglected emissions from Whyalla Steelworks, Leigh Creek and Stirling North for the reasons suggested above. . "

We have also added text to indicate the model level height which these emissions are introduced into the model:

"The NPS emissions are input into the model at level 6 representing stack height at 200m above ground. The other tracers are emitted at the first model level at about 10m above ground"

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?

The instrument is always measuring the total column along the slant path to the Sun.

The amount of XCO<sub>2</sub> up to the model top is above 90% of the total column XCO<sub>2</sub>. Daytime convecting mixing ensures that the air is always well mixed so this assumption is valid to a good approximation.

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.

We agree with the reviewer that this 'hand-waving' approach is not defensible. Neither are we suggesting that this should be used for source-estimation. We were only trying to illustrate a point, that the timing in the observed and simulated plumes could be due to sea-breeze. Nevertheless, as the reviewer points out, the improved correlation of 0.42 was still too low to fully account for the limitation. This underscores our point that under the given synoptic conditions the model was unable to adequately simulate the in-situ signals. We have added a sentence in section 5.2.1 that states:

"...This improves the correlation somewhat to 0.42 which is still poor. This underscores the deficiencies of the model to simulate the in-situ tracer under the given synoptic conditions."

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.

At the moment the paper concentrates on interpreting the column-integrated concentrations. So, while we could add such information to a plot, it would only be useful if we discussed it in detail. We don't think a more detailed discussion of why the in situ data is handled badly by the model is within the scope of an already long paper.

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

This has now been made clear in the text and the Figure 10 caption that this refers to

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the tracer from the NPS.

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.

Although the NPS signal is local effect, there is another source that is more local than that and that is the Port Augusta town itself. We are saying that the in-situ signal is influenced by the town itself where as the column signal is more immune to that. Having said that we agree the term 'local contamination' is a misnomer and we have amended the text to read:

"The picture is, however, different for column-averaged concentrations, which are more immune to local sources (such as traffic) in the immediate vicinity as they are averaged over a larger body of air than localised in-situ concentrations."

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).

We thank the reviewer for pointing this to us. We have amended the text accordingly by removing the sentence:

"To our knowledge, this is the first time such a calculation has been attempted"

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

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emission factor is within the uncertainty interval of the original estimate. If anything, these results point to deficiencies in the modeling.

It is true that the simple source profile (constant emissions) gives slightly better correlations and rmse values than that using AEMO diurnal profile. The fact that this gives a slightly worse emission factor (despite being within the uncertainty of the one calculated from AEMO with diurnal profile) indeed points to deficiencies in the modelling and lack of sufficient data. We have amended the text to take this view into account. We have removed the sentence:

"This underscores the importance of using an accurate description of the given emission profiles and shows the risks of using a fixed emission profile in a situation where in reality, the emissions have diurnal profile."

and have replaced it with: "However, this emission factor is still within the interval of the original estimate, which points to deficiencies in the modelling and/or lack of sufficient data."

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 firm conclusions about the usefulness of either data source for source estimation.

We have amended our conclusions to take into account the large uncertainties associated with insufficient data. So our conclusions now read: ".....Using column-integrated measurements in a simple inverse model we can estimate the power-plant emissions with an error of 6% using 6 days of measurements. However, the small size of the dataset used in this study has led to large uncertainties. Future work will address these uncertainties by collecting measurement data over a longer period. Nevertheless, the

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preliminary results suggest that column-integrated measurements offer a good compromise between sensitivity and the capability of current mesoscale models for estimating emissions from point sources."

## **Anonymous Referee 2**

### **General Comments**

The paper describes a method for estimating CO<sub>2</sub> 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 CO<sub>2</sub> sources, explain how data on the energy output at NPS is converted to provide the WRF CO<sub>2</sub> emissions rates. What are the sources of CO<sub>2</sub> in Port Augusta and Stirling North and how are they input into the WRF simulation? Are they treated as area or point sources?

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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.

We have added text to explain how power plant energy output is converted to CO2 emissions. Sentence reads: "The emissions from the power plant are calculated as the product of recorded despatch power of the power plant and the Carbon Dioxide Equivalent Intensity index (0.948 in the case of the NPS)."

Sources of CO2 in Port Augusta and Stirling North are mainly from traffic. These are really small towns (Port Augusta has a population of 13500 in 2013) and Stirling North is even much smaller. The Port Augusta source is treated as an area source and the NPS source is a point source. We have also added text to explain how they are input in the model.

The steel works emissions were treated as constant in time. We did not use Leigh Creek as an emission source as it was deemed to be too far away to make an impact. The Flinders bush fires are about 100km east of the NPS.

We considered including a plot of WRF emissions in the paper but decided against it as the paper is already too large. And besides we have a map of the area showing locations of the emissions that have been considered in this paper. We do not show the location of Whyalla Steelworks and Leigh Creek as they are so far (to the South and North of the map, respectively) that including them increases the scale of the map and details on power plant and measurement sites locations are obscured.

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The measured data is shown up to the 16th for completeness. However, we do not use the data in the analysis when comparing with the modelled data.

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.

We thank the reviewer for these insightful comments. The winds from the airport are presented as representative of the winds from the model innermost domain because the area is flat terrain. If required, we can include some photos in a Supplementary material to illustrate this. There are sea breeze effects although to the northern end of the gulf it is very shallow water (perhaps only a few metres deep).

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

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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.

We are grateful to the reviewer for the helpful suggestions which we have now implemented in the paper. We have added a scale of distance for the map. Miranda is at the Southern site, Stirling North is just about 2 km directly east of the Northern site. We have added text to explain this in the caption. Also added text naming the water body as Spencer Gulf.

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.

We thank the reviewer for the suggestion but respectfully disagree. In general, if it can be done without large investment of space, we think it is useful to start readers from a common point. We believe this text does this fairly efficiently.

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 XCO<sub>2</sub> 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 CO<sub>2</sub> concentrations and strong

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correlation of column and in-situ measurements during plume events. It is true that CO<sub>2</sub> 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.

We apologize for a lack of clarity on this point. The major result of the paper is that it worked better to estimate emissions from the power-plant using column rather than in situ measurements. This requires that, put roughly, the signal/noise ratio for the power-plant-induced concentration signals is better in the column than the in situ measurement. So we agree completely with the reviewer that the column measurements can see the power-plant while seeing less, in relative terms, of other signals.

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.

The reviewer makes a good point about the representativeness of different measure-

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ments and we have added text accordingly. Our fundamental point is not, though, whether column or in situ measurements *should* work better but rather which *does* work better, at least when comparing with available observations.

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.

See response to reviewer 1 above.

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.

This was a poor choice of words on our part. The plume is not seen at the Southern site not because it does not touch down but because the distance is so far the plume is diluted by the time it reaches the Southern site. A plot of the time series of the CO<sub>2</sub> measured at the Northern and Southern sites shows clear peaks for the Northern site but no clear peaks for the Southern site. We have amended the text to read:

"We do not expect to see much of the NPS plume at the Southern site as it is so far from the NPS that the plume is diluted by the time it arrives at the Southern site."

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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.

The reviewer is correct in saying that plume touch down is not the only reason for the peaks. We have amended the text to reflect this:

"This may likely be attributed to plume touch down at the Northern site."

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.

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Both 3 and 4 seem to be quoted as rules of thumb for resolving features. The more important limitation on resolution for us is probably the quality of the model and the driving meteorology. At this stage we have used fairly coarse averaging in time because we think we cannot resolve finer structures.

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.

The reviewer is correct that there are some advantages to running a multiple fit. There are some disadvantages too. With the limited data we are unlikely to see good separation by the inversion between the two sources. This would require a prior estimate which would detract from the clean experiment of calculating one emission factor. The small residual error also suggests adding more parameters to the fit risked over-fitting the data.

#### Specific Comments:

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

We thank the reviewer for pointing out the error. It is IPCC report edited by Solomon et al. This has now been rectified.

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

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equations do not make a hydrostatic assumption.

The sentence was indeed incorrectly phrased. We have now modified to read: "The model, which has a hydrostatic pressure, terrain-following vertical eta-coordinate system, computes meteorological and tracer fields and it conserves mass, momentum and entropy."

Page 31559, line 10 - The Lambert Conformal projection is used to map real data to a sphere. Sentence amended to read: "In this study, the Lambert conformal projection has been used to map real data to the surface of the Earth"

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.

We agree with the reviewer and have updated the sentence accordingly.

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."

We agree and have updated the sentence to read as advised by the reviewer.

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

We have changed the text to read: "...there are two large power plants."

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

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By 'recirculation' we mean plumes of CO<sub>2</sub> that are advected over longer distance before reaching the measurement site.

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

We thank the reviewer for pointing this error which has now been rectified.

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.

This has now been amended to use lower case letters for individual panels as is the case with figure captions.

Figure 3 - caption should include the concentration measurements location.

We have now added text to the caption that states: "The concentrations were measured at the Northern site 3 km north of the NPS."

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

We have now replaced the green solid line with green dashed line.

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

Corrected.

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).

We thank the reviewer for pointing these out. We have edited the document accord-

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ingly.

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