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Interactive Comment

Interactive comment on "Investigating Alaskan methane and carbon dioxide fluxes using measurements from the CARVE tower" *by* A. Karion et al.

Anonymous Referee #2

Received and published: 18 January 2016

The authors present an analysis of CO2 and CH4 fluxes using three years of data collected from the CARVE tower in interior Alaska. The manuscript is well-written and figures are easy to read. The measurements are of high-quality and made using established methods. There are some methodological issues with the analysis that need to be addressed but if these comments can be resolved, the paper should be published in ACP.

Major comments:

1. The method used for the background identification has some issues. Firstly, the authors use a Pacific curtain as the background for calculating enhancements in the

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measurements. This only applies to air masses that are transported from the West. The authors first discard the subset of particles that did not exit from the West or exited from too far North-West and allow up to 25% of particles to be discarded in this way. Can it be clarified that both the effect on the surface influence as well as on the background are removed in this way? This would lead to up to a 25% error in the analysis, as of course the measurements would still be sensitive to these directions. Secondly, as the authors correctly discuss that the choice of background is critical (especially for CH_4), what is the potential impact of these choices on the results? Can a sensitivity analysis be done? Can the authors use a better method for identifying the background, in the absence of having an additional tower that samples the background (i.e. using model simulations for other directions)?

2. In the absence of being able to do a full quantitative study with an inverse modelling analysis and appropriate treatment of boundary conditions, the authors need to more explicitly that they state that the quantitative results will be subject to larger uncertainties and biases (the qualitative results presented have more weight than the fluxes and uncertainties presented).

3. The prior flux maps for CH_4 need more discussion. Why is a uniform map assumed? What sources is this based on?

4. The method on how CH_4 fluxes were estimated is not clear. Was this not done for CO_2 because it was shown the correlation between modelled and observed CO_2 enhancement lay close to the 1:1 line (i.e. fluxes were correct?). This is a bit confusing because several times, it has also been stated that CO_2 signals are larger than predicted.

In Sec 3.5, the CH_4 flux estimation method is described. Observed CH_4 enhancements are first averaged into daily values and then into monthly values. The modelled CH_4 enhancements were first simulated on hourly timescale but then averaged into daily and then monthly. Why is the flux estimation based on monthly values rather than

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averaging higher resolution fluxes? How is this fit done (there is no information on what method is used to determine the optimal fit)?

Further in section 4.5, the fitted monthly fluxes are then used to drive daily enhancement values using the WRF-STILT footprints. The correlation analysis is then based on observed versus modelled daily enhancements (modelled values based on the fluxes derived from the monthly fit). In effect, the authors are fitting fluxes based on the monthly enhancement values, but then state that the daily modelled enhancements do not match the observations well. Can the authors firstly reorganise these two sections together and secondly, explain why the different timescales are used? What is the implication that fluxes derived based on a fit to monthly data does not lead to a good fit on daily timescales? Why is hourly not used (as was done for the CO2 correlation analysis)? This section needs some more explanation.

5. The flux uncertainties that are presented are likely to be too small. The authors are only including an uncertainty due the background term. In almost all inverse modelling analyses, studies have shown that the overwhelming uncertainties are due to model, representation and aggregation errors. In addition, uncertainties should include uncertainties in the fit. While the authors do mention the simple uncertainty analysis that was done, it should be more explicitly stated that the uncertainties presented are likely to be an underestimate of the true uncertainties for these reasons.

Minor comments:

Abstract lines 19-22: These two sentences seem inconsistent. Firstly that the model does well in predicting magnitudes and distributions, and then that the signals are larger than predicted.

P. 34874 line 4: Typo ‘in’

P.34876 line 16: Are these supplemental flasks to the in situ? Flasks are not discussed until later.

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P. 34876 line 23: How can the authors be sure that there is no local influence at the site? A tower at 32m could still very much sample local fluxes. More discussion is needed on this.

P. 34877 line 11: What are the three heights? Not mentioned until later.

P. 34878 line 6: How long is the flushing time?

P. 34878 line 9: How often is the water correction performed? Is it instrument specific?

P. 34878 line 17: How do the authors know that the nonlinearity has not changed during the four year period? Has it only been measured once prior to deployment?

P. 34880 line 11: What is the size of the domain?

P. 34880 line 13: Has any sensitivity analysis been done to show that 500 particles is sufficient to statistically represent the footprint? If 1000 particles were used, what is the effect? This is an issue with LPDMs in general that is never adequately discussed.

P. 34880 line 17: This could lead to important differences. Can the authors do a sensitivity test to show the effect of using model ground level?

P. 34882 line 4: To clarify, are these particles removed from the footprint calculation as well? As discussed above in the major comments, this artificially changes the footprint. For example if a plume were travelling to the south-west corner, such that 75% exit "west" and 25% exit "south", by removing the southerly particles, the plume will have narrowed by 25%, which is significant.

P. 34883 line 17: What about the lowest height on the tower? Is it used for anything?

P. 34885 line 1: How is this scaling performed? What statistics?

P. 34885 line 3: See major comment about uncertainties and being more explicit that these uncertainties are likely too small.

P. 34886 line 7: As the authors state, the magnitudes of amplitudes are difficult to

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compare as they are very site dependent and depend on sampling height, PBL height etc. This section doesn't add a lot because of all of these additional complexities and any comparison is quite speculative. The section would be better if shortened and condensed to say that the range of measured fluxes is xx and that this is dependent on factors such as. . .

P. 34890 line 20: Following major comment above, this section on the correlation analysis of CH4 enhancements is confusing.

P. 34891 line 4: This could also be due to uncertainties in the background estimation.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34871, 2015.

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