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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimating regional scale methane flux and budgets using CARVE aircraft measurements over Alaska" *by* Sean Hartery et al.

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Summary

Hartery et al. presents CH4 fluxes from the Alaskan wetlands derived from aircraft measurements. The paper examines the relationship between the fluxes and several variables. The paper uses an extensive dataset, which has been presented by several previous studies. However, it is interesting to compare different techniques/models to derive fluxes. Examining the drivers of the CH4 flux at such a large scale is novel. The paper is interesting and well written with relatively few typos. The paper should be suitable for publication, but I do have several queries first.

My main concern is that the manuscript is missing a detailed description of how the uncertainties associated with the fluxes were calculated? How were the uncertainties for the individual components estimated and propagated.

I would expect that the choice of background would have a large impact on the calculated fluxes. However, I am not sure how appropriate the CH4 above the mixed layer is for the background. You are assuming that it is representative of air 5 days upwind of the measurements. Wouldn't you expect significant exchange between the PBL and free troposphere over a 5-day period? Is it possible that the free troposphere and mixed layer air could have different histories (e.g. due to long range transport)? You compare your background to those from Karion et al. (2016) who use 'pacific curtain', however I imagine this will only be valid if there is a west to east airflow. Also is Poker Flats in interior Alaska really observing background air? The choice of 5 day sensitivity footprints seems a bit arbitrary, does changing the length of time used to derive the footprints e.g. 5 to 10 days have much impact on the calculated fluxes? Would you use the same background if you used a 10 day sensitivity footprint in your flux calculation?

Figures 3 to 7 appear to have a lot of problems with missing units, axis labels, legends, etc. It looks like the figures have been corrupted. I don't know if this is just a problem for the pdf reader I am using, but please check them. This didn't seem to be a problem with the initial submission. But it does make reviewing some parts of this current version difficult (particularly sections 4.4 and 4.5).

We thank the reviewer for their thoughtful and detailed comments on the manuscript.

The reviewer has insightfully pointed out the greatest uncertainty in our analysis - identifying background CH4 levels. As the reviewer summarizes, we assume that CH4 levels in the free troposphere are representative of background levels in the mixed layer. As we cited in the submitted manuscript, this is not unusual for aircraft studies (Chang et al., 2014; Gatti et al., 2014; Chou et al., 2002) and has been conducted successfully for the CO2 analysis from this campaign (Commane et al., 2017, now cited in the main text). As evidence that this is a valid assumption, we compared our free tropospheric levels with surface observations at the Barrow site in Alaska. This is illustrated in a new figure now included in the Supplement. As requested by the reviewer, backgrounds determined for the CRV tower are also included in this new figure. As can be seen, the Barrow observatory tends to observe slightly higher mole fractions of CH4, although more variable, than observed in the free troposphere or from the pacific curtain used by Karion et al. One could conclude that the free tropospheric levels are too low, resulting in a higher flux estimate. However, the background CO2 measured at Barrow and the free tropospheric CO2 measured during CARVE, show very little, if any, bias (Commane et al., 2017). To be more transparent about these issues, we have expanded the text in Sect. 4.3 and direct the reader to the new figure in the Supplement so that they can judge our assumptions for themselves.

Another piece of evidence that air in the mixed layer does not mix up into the free troposphere is that vertical profiles of the cumulative 5-day footprint sensitivity determined by WRF-STILT tend towards zero in the free troposphere (e.g. Fig. 3 in the main text). This would suggest that the free troposphere is not being influenced by the surface in our domain, and is therefore not influenced by air from the mixed layer. As the reviewer suggests, however, it is possible that air in the mixed layer and the free troposphere have different transport histories outside of our domain and we now discuss this in Sect. 4.3.1. Unpublished data from measurements taken on the Alaskan Coast Guard aircrafts off the northern coast of Alaska show that average methane profiles are fairly constant from the surface to 7 km, suggesting that free tropospheric levels do represent surface background levels (measurements described by Karion et al., 2013). However,

this may not be true for air transported from other directions. It should be noted that the majority of the air enters our domain from over the ocean, with only approximately 10% originating from the region east of our domain.

To address both reviewers' concerns about backgrounds, we now include a new section (Sect. 3.7) describing how we determine and propagate the uncertainties in our estimates. As the reviewer surmises, the background CH4 mixing ratio contributes the greatest uncertainty to our calculations, now shown in Table S4.

With regards to 10-day footprints, the observed free tropospheric mixing ratio that we use for our background obviously remains constant. The modelled footprint sensitivity does not significantly change if the run time is extended further back because the air will have moved outside of our study domain (50-75°N and 130-170°W) and only surface influences from inside our domain are included in our analysis of the modelled column enhancement. As such, modelled column enhancements from the 5-day cumulative footprints closely resemble those calculated from the 10-day cumulative footprints.

Finally, we apologize for the figures not displaying properly in the Discussions paper. We will ensure that we double check that there are no more compiling issues in the future.

Specific comments

Page 1 line 1. "emissions from northern regions is still poorly constrained". Change 'is' to 'are.'

This change has been implemented at the reviewer's suggestion.

Page 1, line 10. Change 'flux' to 'fluxes'.

This change has been implemented at the reviewer's suggestion.

Page 4, line 9 to 11. I find this sentence a bit confusing. I presume you interpolate the instrument's calibration curves between calibration times. I am not sure what the additional interpolation is?

The sentence was meant to convey that we interpolate using the calibration curve determined by the low and high spans. The reference to the second interpolation has been removed and the text now reads "`These in-flight calibrations were linearly interpolated between calibration times to generate time-varying calibration curves`".

Page 5, line 15. 'among other methodological improvements' is a bit vague. Either give more detail about these changes or remove.

This line has been removed as the relevant considerations are as already stated.

Page 5, line 31. You refer to a 5 day footprints here, but on page 5, line 17 you say that footprints were calculated over 10 days. Have I misunderstood something?

WRF-STILT footprints were calculated for a total of ten days by Henderson et al. (2015). However, our study only uses the first five days because the air had mostly exited our study domain by then. To clarify this, we now state this clearly in the last

paragraph of Sect. 3.1.

Page 11, line 29. For 2012, are there any differences between this study and Chang et al., (2014)? They appear to use identical data and methods.

This study covers a slightly larger east-west domain (130–170°W vs 140–170°W) and only reports fluxes from non-mountainous land. The primary reason for including the 2012 analysis was so that the results could be compared over all three years using a consistent analysis method. It also served as a means of checking our current method.

Page 11, line 29. Can you also compare your fluxes with those from Karion et al (2016)?

In the submitted manuscript, we compared our fluxes with those from Karion et al. (2016) in the last paragraph of Sect. 4.1. The section that the reviewer listed is in the budget calculations where we do not compare with the study by Karion et al. because they do not report an estimated budget. It would be inappropriate to extrapolate their results to our entire domain since the CRV tower is not sensitive to the same surface types.

Page 12, line 27. Please be more specific about how the background for the CRV tower was calculated.

As discussed by Karion et al. (2016), backgrounds for the CRV tower were calculated by following particle trajectories in WRF-STILT backwards until they crossed a Pacific basin boundary "curtain", which is determined based on an interpolation of observa-

tions. These details have now been included in the text.

Page 12, line 20. How large was the difference between CARVE and BRW? Perhaps you could show a scatter plot showing the different methods used to derive the background. This comparison is a bit vague at the moment.

As recommended by the reviewer, a figure illustrating background CH4 determined from this study, the CRV tower and BRW observations is now included in the Supplement (Fig. S3). The comparison is quite favorable with the exception of those months already mentioned in the text.

Page 14, line 20. It is worth noting that wetland maps can show significant difference (e.g. Melton et al., Biogeosciences, 2013). I wonder if this would impact on your results in section 4?

Since different wetland maps have different spatial distributions, the results are dependent on the map that we use. In earlier iterations of this analysis we did try different maps, including those compared by Melton et al. (2013). However, we decided to only show results from the map by Bergamaschi et al. (2007) because the focus of the study was to quantify the methane budget and not to optimize wetland maps. The study by Miller et al. (2016) compares CH4 emissions estimated using different wetland maps extensively and we now direct the reader to this study in this section of the main text.

Page 15, lines 1-3. What do you mean by "diluted the contributions of other land types?" Does this increase the flux from other land types?

'Dilution' was intended to convey the fact that by averaging over a region where no flux is expected, that the estimated monthly flux will be underestimated in areas where the flux is actually occurring. This does not affect budget calculations and allows comparisons with tower and chamber studies more easily. This sentence has been changed to "as their inclusion would have led to an underestimation of the net CH_4 flux attributed to non-mountainous land surfaces".

Page 15, line 7. Add correlation coefficients to main text.

These have now been added to the main text.

Page 16, line 20. Why was May 2014 suspected of being an overestimate?

As was discussed in the submitted manuscript on page 12, line 29–31, the flux estimates in May 2014 are possibly overestimated based on comparisons of observed free tropospheric mole fractions of CH4 by the CARVE aircraft being significantly lower than those observed at either the CRV Tower or at Barrow. At the reviewers request, this is now illustrated in Fig. S3 in the Supplement.

Page 17, line 33. Suggest you reword e.g. "... regional emissions can be determined by up-scaling local scale studies"

This has been changed to the reviewer's suggestion.

Page 18, line 1-5. Is 3 years really long enough to comment on a lack of inter-annual variability?

We agree with the reviewer here and have removed this entire paragraph.

Figure 3. The legend doesn't explain what the green and grey shading are, the units in the legend are missing the exponent, missing axis labels. Check formatting.

Figures 4 to 7. These appear to have similar problems to Fig 3. Please check.

We apologize that the figures in the discussion paper were not properly formatted. This will be fixed for the final publication.

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