

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

**Anonymous Referee #2**

Received and published: 7 March 2017

Hartery et al. presents CH<sub>4</sub> 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 CH<sub>4</sub> 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 calcu-

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lated fluxes. However, I am not sure how appropriate the CH<sub>4</sub> 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 a “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).

### Specific comments

Page 1 line 1. “emissions from northern regions is still poorly constrained”. Change “is” to “are”.

Page 1, line 10. Change “flux” to “fluxes”.

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?

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

Page 5, line 31. You refer to a 5 day footprints here, but on page5, line 17 you say that

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footprints were calculated over 10 days. Have I misunderstood something?

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.

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

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

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.

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?

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?

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

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

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

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

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.

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