

Anonymous Referee #1

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We thank the reviewer for the thoughtful comments and careful reading of our manuscript. We address the points below. The reviewer's comments are italicized and the responses are in plain text.

*This is a very well written and relevant study that should be published in ACP. Both the measurement and modelling approaches are appropriate and well described, and the results are presented in a (mostly) convincing way.*

*I have only a few points which I would like the authors to consider, separated into main and minor points.*

*Main points:*

*1. The local surroundings of the site should be described in more detail, e.g. by complementing Figure 1 with another zoom onto the site showing both topography and land use in an area of say 50-100 km around the tower. Figure 4, for example, resents wind roses for two seasons but it is unclear how strongly these winds might be influenced by the local topography. Some short description of the site is given later in the results section (p 16, lines 21-24), but this kind of information should be presented earlier in Section 2.*

We have now replaced the right panel of the original figure with a more zoomed-in version, which is 44.6 km high (0.4 degrees latitude) and 47.5 km wide (1 degree longitude), and has a different elevation scale than the left panel. This shows the immediate vicinity of the tower more clearly than the previous version. The land use for the area in the second figure is mostly deciduous and evergreen forest, with some woody wetlands and urban (Fairbanks), which is now mentioned in the text.

*2. A "Pacific boundary" is used as background for the measurements which only works for air masses advected from the west. Data with more than 25% of particles originating from a position east of 160° have been discarded. Since winds are primarily from the east between October and April (wind rose in Figure 4), much of the data in this period has to be excluded from the analysis when applying this filter. This is clearly not ideal, especially given the fact that (according to Fig 1a) the site has significant sensitivity to the eastern parts of Alaska. How does the average footprint change as compared to Fig. 1a when this filter is applied? Is the eastern part of Alaska still covered? Is this part not relevant for methane fluxes?*

The reviewer makes a good point here, in that the average influence shown in Fig. 1a is for all the observations at the tower. We now show the new influence contours for both the full data set (as before) and the filtered data set on the figure. As the reviewer predicted, it shows that there is less influence over eastern Alaska and Canada once the filters are applied.

*Is there not better alternative than the “Pacific boundary” that would allow preserving more data during wintertime? Section 3.3. should mention what fraction of data has to be discarded due to this procedure. The consequences of this choice of background are not very clear to me. As stated on P18 line 12, “the choice of background is crucial to any analysis of the measurements (of CH<sub>4</sub>)”. This background appears to be only rarely/poorly defined in winter but at the same time the conclusion is drawn that significant fluxes of methane persist through fall and winter. How robust is this conclusion given the uncertainties in the wintertime background?*

We have now conducted a sensitivity analysis of the background calculation on the CH<sub>4</sub> flux estimates in response to the comments from this and other reviewers. Please see the response to Reviewer 2 for figures showing the effect of the background filter. We discuss the results of the analysis in the text in Section 3.3, and we also note that 50% of the observations are eliminated because of this background filter. Although this is a large percentage of the observations, we believe it is a necessary choice given that this method of calculating the background at least for this subset of observations is the best, based on its agreement with the observations; it captures synoptic scale variability in the observations which would otherwise be interpreted as a regional signal. We also agree that given the few observations retained in winter, we have added language to Section 4.6 to emphasize the large uncertainty in the winter fluxes and emphasize the need for further study in winter.

*Furthermore, it is not always entirely clear which analysis is based on the filtered or the full data set. Section 4.1 mentions explicitly that no filtering is applied, but what about Section 4.2? Figure 5 presents measured time series for the whole observation period together with background values derived from the “Pacific background”. Obviously, background values are only shown where possible (i.e. filtered), whereas observations represent the complete time series. This should be stated explicitly in the caption. From this figure it actually appears that even in winter there are only few gaps in the background, which seems to be incompatible with Figure 4 showing that winds between October and April were mainly from the east and with the statement on P13 - L23 that “in many winter months, fewer than 6 days of observations remained after the data filtering.*

We have clarified this in section 4.2, that our analysis uses all the data, as we are characterizing the site itself. The filters were only applied when enhancements above background were needed. We also have added clarification in the Figure 5 caption. We changed the background to dots instead of a line, which shows the gaps more clearly.

*3. The manuscript never spells out clearly what sources of methane are expected in Alaska. The first time the reader learns about the potential importance of wetlands as a CH<sub>4</sub> source is Section 4.1. Up to that point it remains pretty much unclear what sources of methane are expected, and therefore the motivation for producing an elevation-based flux map as described in Section 3.5 remains unclear.*

*There would certainly be better options than surface elevation to describe watershed hydrology (if that was the purpose), but there are also data available for wetland extent in Alaska (see Whitcomb et al., C. J. Remote Sensing, doi:10.5589/m08-080, 2009) that could serve as a proxy for CH<sub>4</sub> source areas. What about wild animals? What about oil, gas and coal mining? Alaska's economy seems to be dominated by the oil and natural gas industry (<http://alaska.gov/kids/learn/economy.htm>)? Fairbanks is mentioned as a potential source of anthropogenic emissions, but what about other CH<sub>4</sub> emissions from fossil fuel extraction in Alaska? A recent study has indicated that high Arctic soils may represent a net sink of CH<sub>4</sub> (Jørgensen et al., Nature Geoscience, 2015, doi:10.1038/ngeo2305). How does that relate to the results presented here? As opposed to CO<sub>2</sub>, the modelling of CH<sub>4</sub> was much less successful, suggesting that the sources are not well represented by the two flux maps and/or that temporal variability of CH<sub>4</sub> emissions is high. This poor understanding of the CH<sub>4</sub> fluxes is briefly mentioned at the end of Section 4.5 but should also be emphasized in the conclusions.*

We have now expanded a paragraph describing the site in the introduction. We have added background information that we were missing before in this section, regarding the expected contribution to the methane fluxes from different sources, including wetlands (and now referring to previous work illustrating late fall emissions), and wild animals and oil and gas exploration. We don't have a way to partition total methane observed at the tower between these different sources. Animals and fossil fuel exploration are not thought to be a large factor in total Alaskan methane emissions, based on inventories and previous studies (Bruhwiler et al., Kirschke et al.). Geologic seepage is a source as well (e.g. Walter et al.) (all the above now cited in the text). The soil sinks estimated in Jørgensen et al are small in magnitude relative to the fluxes shown here, but are now mentioned as a possible additional factor in the text. We also now mention in the conclusion that the spatial and temporal representation of CH<sub>4</sub> fluxes is likely incorrect based on poor agreement with the hourly observations.

*4. Autumn bursts of CH<sub>4</sub> and CO<sub>2</sub> fluxes coinciding with soil freezing in the high-Arctic Tundra have been described by Mastepanov, M. et al. Large tundra methane burst during onset of freezing. Nature 456, 628–630 (2008) and Mastepanov, et al. Revisiting factors controlling methane emissions from high-Arctic tundra, Biogeosciences, 10, 5139-5158, doi:10.5194/bg-10-5139-2013 (2013). It would be good to place the results in context with these studies.*

We have added references to this literature now in 4.6, as our study supports these observations.

*Minor points:*

- P4, L4: "focused on in its" -> "focused on its"
- P8, L20: What does "drift-corrected" mean? Is this a drift of the signal offset or of the span?

These two changes have been made (the drift is the offset).

- - P9, L27: *Are the STILT sensitivities in terms of dry air mole fractions?*  
Yes.

- - Section 3.2: *The three nested domains of WRF are described, but the simulation domain of STILT remains unclear. Is STILT only simulated in the inner domain, or is it run in a nested configuration as well (as is possible e.g. with FLEXPART)?*

STILT is run after WRF fields are generated, and does not require a grid on its own. It can run over the entire WRF domain, using the WRF fields available at the grid point of the particle location. After particle trajectories are calculated, the footprints are gridded in post-processing, which can occur at whatever resolution the user requires. For this work, we used a relatively coarse half-degree grid over the entire WRF domain. We have added a sentence in section 3.2 to clarify.

- - P12, line19-20: *I didn't understand what "the RMS residuals of the boundary curtain" are.*

This is now explained further in the text. They are the residuals that are computed when the boundary curtain is calculated from measurements. A smoothing function is applied and the residuals are the residuals between the data and the fit.

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- - P13, L11: *What is the source of the NOAA NGDC elevation data?*

The caption of Figure 1 now includes more detail and a reference.

- - P15, L19: *"diurnal cycles of the CO<sub>2</sub>" -> "diurnal cycles of CO<sub>2</sub>"*  
- - P19, L23: *Probably it would be clearer to say "more negative" instead of "lower".*  
- - P21, L6: *The lack of correlation may not only be due to a poor spatial representation but could also be due to temporal variation of the fluxes.*  
- - P22, L14: *"also have" -> "also has"*  
- - P23, L23: *As mentioned above, it is quite likely that the soils in some regions act as sinks rather than as sources.*

These changes have been made.

- - Figure 1: *Since Figure 3 shows the influence of different regions, it would be very useful to include in Figure 1 the borders of these regions (at least between Lower Alaska and the North Slope and Canada).*

We believe the description in the text is sufficient and the figure would become too cluttered, so we have chosen not to include this border in the map.