

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

Anonymous Referee #1

Received and published: 13 January 2016

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,

C11511

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



presents 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.

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? 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 CH₄)”. 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?

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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.

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 CH₄ 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 CH₄ 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 CH₄ emissions from fossil fuel extraction in Alaska?

A recent study has indicated that high Arctic soils may represent a net sink of CH₄ (Jørgensen et al., Nature Geoscience, 2015, doi:10.1038/ngeo2305). How does that relate to the results presented here? As opposed to CO₂, the modelling of CH₄ was much less successful, suggesting that the sources are not well represented by the two flux maps and/or that temporal variability of CH₄ emissions is high. This poor understanding of the CH₄ fluxes is briefly mentioned at the end of Section 4.5 but should also be emphasized in the conclusions.

4. Autumn bursts of CH₄ and CO₂ 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,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



5139-5158, doi:10.5194/bg-10-5139-2013 (2013). It would be good to place the results in context with these studies.

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?
- P9, L27: Are the STILT sensitivities in terms of dry air mole fractions?
- 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)?
- P12, line19-20: I didn’t understand what “the RMS residuals of the boundary curtain” are.
- P13, L11: What is the source of the NOAA NGDC elevation data?
- P15, L19: “diurnal cycles of the CO2” -> “diurnal cycles of CO2”
- 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.
- 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).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper