Final author response to referee comments on **"Arctic Regional Methane Fluxes by Ecotope as Derived Using Eddy Covariance from a Low Flying Aircraft" by David S. Sayres et al.**

1. ***Comment from Referee 1:*** *The chosen flux fragment method disregards a fundamental micrometeorological underpinning that permits relaxing what is formally a continuity equation to a single "eddy covariance" term: Atmospheric turbulence needs to be sampled from the high-frequency dissipation range to the first low-frequency spectral gap (e.g., Foken, 2008). As mentioned by the authors, the latter would necessitate the analysis of several kilometres of flight data at once. Nevertheless, the authors revert to the use of 60 meters at a time, thus disembodying a few numbers at a time from their theoretical foundation (here: discarding the spatially varying "base state").*
   Comment from Referee 2: *The authors used a method called flux fragment method to explore the heterogeneity of the fluxes. But this method is questionable, as each flux calculation only consider data points in a very short period (1 s) and low frequency parts of the fluxes are totally ignored in the calculation.*

   1.1. **Author response:** A common misconception about the Flux-Fragment Method (FFM) is that the fragments contain no information on scales larger than their length (FOCAL used 60 m). To be sure, fragments formed using departures from local 60-m averages would jettison all larger-scale contributions, but these fragments use departures from the 3-km base state not local averages. Sections 2.2 and 2.3 as written described the method correctly, but insufficiently emphasized this point. The scale of the base state is determined by ogive analysis (Foken, 2008) to be an upper limit for the turbulence present at the time of measurement. The fragments therefore contain information on all scales from the Nyquist wavelength of the sample rate up to the 3-km scale of the spectral gap determined from the ogive analysis. Yet, the air packets quantified by the fragments are also short enough to have likely interacted with a single class of surface. All fluxes defined in the paper are formed from sums of at least 50 fragments, enough to have a cumulative length of at least 3 km, usually more.

   1.2. **Changes to manuscript:** A statement about how the base state for the fragment is made will be added to Page 6, Line 28. "Departure quantities used to form the fragments are relative to a base-state of 3-km scale or more, a scale determined by ogive analysis (Foken, 2008) to be an upper limit for the turbulence present at the time of measurement. The fragments therefore contain information on all scales from the Nyquist wavelength of the sample rate up to the 3-km scale of the spectral gap determined from the ogive analysis. Yet, the air packets quantified by the fragments are also short enough to have likely interacted with a single class of surface."
   Add Page 7, Line 4. "Fluxes are calculated only for those surface class groups whose total length is greater than 3 km."

2. ***Comment from Referee 1:*** *Superior space-frequency decomposition techniques are widely available and in use (e.g., Barnhart et al., 2012; Strunin and Hiyama, 2004; Thomas and Foken, 2005; van den Kroonenberg and Bange, 2007). These are not only theoretically sound, but provide better spatial resolution down to meters and do not suffer from the loss of low-frequency contributions.*
   Comment from Referee 2: *As pointed by the other reviewer, other promising methods are available for investigate heterogeneous fluxes, such as wavelet analysis.*

   2.1. **Author response:** The space-frequency decomposition techniques mentioned by reviewer 1 are based in well-developed mathematical theory. Such work as Farge (1993) and Torrence and

Compo (1999) have made multi-resolution continuous-wavelet transforms highly useful to the treatment of turbulence in general and atmospheric turbulence in particular. However, eddy covariance is also theoretically sound and has long been treated successfully (Foken (2008)). The eddy covariance approach remains useful in providing a different, more directly intuitive, physical perspective in the space/time domain. The FFM is a modification of the canonical eddy-covariance. It is unorthodox in its use of conditional sampling to pluck individual fragments from the data stream at will to be combined into a mean covariance. This produces gaps not normally tolerated in space/time eddy-covariance work. The traditional analysis takes advantage of the autocorrelation of the data stream. This advantage is to some extent sacrificed in the FFM, but a large-enough random sample of departure quantities, defined as in response 1.1 above, will produce a meaningful estimate of the flux on all scales of turbulence present in the boundary layer.

Procedures exist to estimate the uncertainty in averages computed over a serially correlated, unevenly spaced data stream (eg. Mudelsee, 2010, Chapter 3).

So long as any significant secondary circulations are accounted in the base-state, the turbulent atmosphere on all its scales can be postulated to repeat over the landscape in a fairly random fashion. A contiguous sample (i.e, without gaps) should not therefore be required. The sample only need be sufficiently large to include multiple instances of boundary-layer structures at each scale. An aircraft moving at airspeed 60 m s^{-1} covers 216 km in an hour encountering 72 instances of 3-km turbulence structure. A sufficiently prevalent class of land surface, whether found in large or small patches is very likely to provide a sufficient sample. Samples which are too short can be discovered in confidence intervals developed by bootstrap resampling as was done by Kirby et al (2008). A more sophisticated bootstrap procedure developed in conjunction with analysis of these 2013 data follows Mudelsee (2010, Chapter 3). A manuscript describing the approach in detail has been submitted to the Journal of Atmospheric and Oceanic Technology and is in review.

In drawing randomly spaced samples from an autocorrelated data series the FFM does sacrifice some efficiency. A contiguous series (or a multi-scale wavelet reconstruction thereof) can take advantage of whatever coherency is contained in the feature it is sampling, though it still must sample several such features to provide an adequate estimator of the mean turbulence and flux found in the study area.

The FFM in the space/time domain is a statistical approach as opposed to decomposition approaches like the wavelet and Empirical-Mode decompositions. Also, being totally in the space/time domain FFM can in principle provide spatial resolution down to whatever scale is required so long as the small-scale features are repeated sufficiently often. Of course, the range of practically realizable scales will depend on the instrumentation used to make the measurements which is the same for wavelet analysis.

Therefore, we see no justification for the referee's conclusion that wavelet analysis is superior to eddy covariance/FFM. The FFM provides the same spatial resolution, and it does not suffer from the loss of low-frequency contributions suggested by the reviewer.

2.2. **Changes to manuscript:** We do not agree with the reviewers that a change in methodology is needed. While it might be interesting for a future paper to compare the various approaches and their results, that is not the intent of our present manuscript.

3. ***Comment from Referee 1:*** *Next, relating atmospheric fluxes to discrete land cover classes alone neglects intra-class variability (e.g., Beyrich et al., 2006; Ogunjemiyo et al., 2003). This is better expressed with continuous land cover properties such as temperature, vegetation indices etc. (e.g., Glenn et al.,2008; Ogunjemiyo et al., 1997).*

3.1. **Author response:** "Better expressed" is a relative assessment, dependent on the question being asked. One may in fact want to determine the intra-class variability to assess the representativeness of a surface site located in a particular land-use or land-cover class identifiable by remote sensing. Intra-class variability is expressed in our results by the confidence intervals, which as long as the number of fragments is large, mostly represents the variability within that class. One of the goals of the paper is to compare with towers and other published measurements which classify methane flux based on surface classes similar to those used in this paper.

Specifically, the reviewers' suggestion to use NDVI would be inappropriate for methane measurements. It works somewhat well for CO2 flux because CO2 has a known causal relationship with photosynthesis and plant respiration. Methane is not primarily controlled by the physiology of the vegetation. Vegetation type may, however, serve as a proxy reflecting different soil moisture and other properties. Also the roots of sedge are known to act as a passive transport for methane bypassing any oxidation that might otherwise occur in the surface soil. Perhaps a different interval quantity can provide a meaningful correlation to methane flux, e.g. soil moisture, water-table height, or (sub-canopy) soil temperature. These are hard to measure remotely, especially with the accuracy needed. They were not available during the mission, nor do the authors know of a way to do this remotely at the spatial scale necessary. Failing that, we are using surface cover as a proxy for subsurface hydrology. Ignoring any assumptions about subsurface features, our results still show what sort of surface cover is associated with the strongest methane flux. We found wet sedge to dominate CH4 emission when the soil was warm. In particular, it was much more important than open water such as thermokarst lakes, which have garnered much attention based on the work of Walters-Anthony and others.

3.2. **Changes to manuscript:** modify sentence Page 7, Line 26. "These classifications, assigned based on remotely sensed data, are plausible proxies for properties that have been shown to be primary drivers of methane production and emission such as water table height, soil temperature, and emission pathways such as sedge roots. Interval quantities sensible remotely such as NDVI, air temperature, or other vegetative indexes which correlate with carbon dioxide do not correlate with methane (Olefeldt, 2013). Vegetation classifications such as these have been shown to be useful for estimating regional methane emissions from other regions (eg. Schneider, 2009) though those were based on upscaling from ground measurements."

Page 6, line 23. "The Flux Fragment Method (FFM) was conceived to answer questions concerning the homogeneity of land classes defined by some remotely sensible measurement in areas where the land classes vary on lengths short compared to what would be needed for a traditional running flux calculation. "

Page 3, line 14. “How representative were towers’ footprints of the class of land cover, as identified by remote sensing, in which they were placed? In principle a stationary site can measure all manner of properties and state variables in the soil, the vegetation, and the air within and above the canopy. Much can be learned about the bacteria, soil chemistry, canopy storage, and other quantities relevant to the exchange of mass, momentum, and energy with the surface. But all of this is known only at the one site. How representative is that site of other locations that to remote sensors appear similar? Are there land-cover types that are particularly indicative of emission of a given trace gas? Can the class so identified be used as a quantitative predictor of a particular type of soil chemistry. This is relevant in assessing the regional methane emission from remote sensing. Methane in particular has a fairly complex chemistry in the soil involving state quantities such as the (sub-canopy) soil temperature and the height of the water table. These are measurable only in situ so that having a proxy indicator such as vegetation cover would be valuable.

Aircraft, though more limited in what they can measure than fixed sites, are very mobile providing the opportunity to sample many instances of the same remotely sensed class over the landscape. From this multi-instance sample one can assess how representative the single fixed site is. One can also assess the strength of the variability within the given land-surface class for later investigation from the surface. In remote parts of the earth, in particular, a determination of near homogeneity of emission properties from many instances of a recognizably similar surface class can save considerable effort over a surface-based survey. Alternatively, large variation within a class that is not well predicted by some practically measurable interval quantity will be seen as requiring additional effort for in-situ measurements to find an effective monitoring program for methane emission from that surface class.”

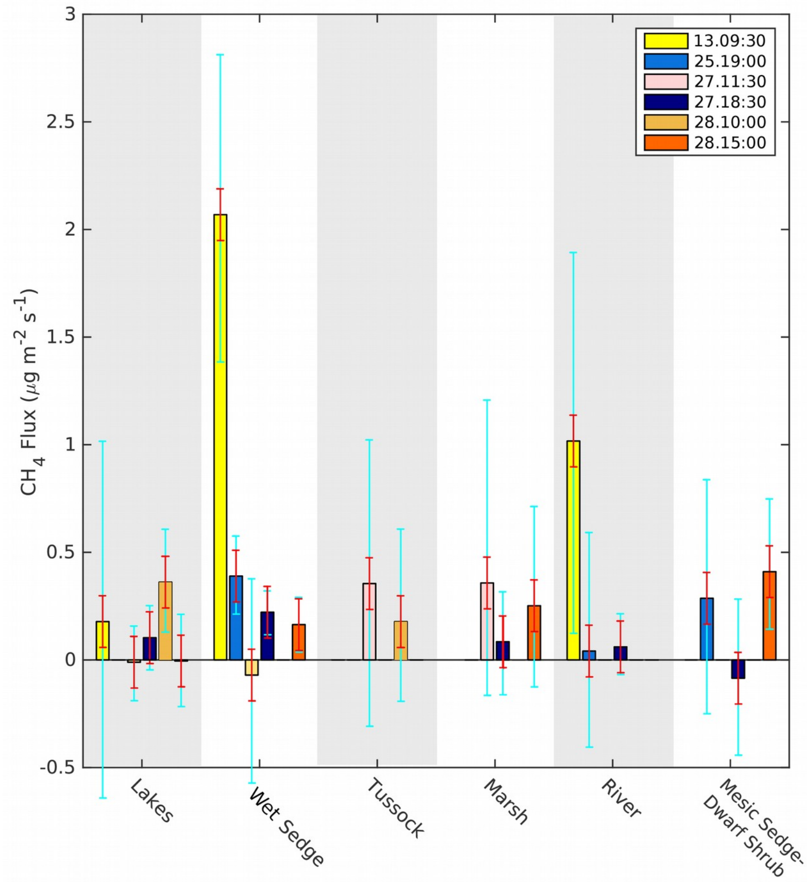
Page 7, Line 21 “The questions to be answered by the FFM, using a fuzzy-logic approach (Nguyen and Walker, 2000) to assign surface classes to fragments and then to conditionally sample them based on those classes include:
a) What is the mean flux over all measured instances of each surface class?
b) What surface classes dominate the methane emission, and by how much?
c) How much does the flux over each class vary? Is there a spatial pattern to the variation. The variability will come both from the prevailing atmospheric environment and the heterogeneity of the emission within the same class.
d) How representative is a particular instance of all similar instances over the landscape?”

4. ***Comment from Referee 1:*** *In addition, FFM results for individual land covers are not comparable across flight days, as day-to-day synoptic variations and different flight times within the diurnal cycle are not taken into account.*

4.1. **Author response:** This comment has nothing to do specifically with FFM because the same question could be asked about eddy covariance in general from a tower or aircraft. Again, FFM is a specific implementation of eddy covariance. Synoptic variations are ideally removed by the base state, except as they affect the turbulence. Unlike CO2, methane has a weak to non-existent diurnal cycle. Our tower data do show a weak cycle, most likely caused by near surface soil temperature changes through the day. However, this diurnal cycle is an order of magnitude less than the variation due to other causes including deeper-soil temperature. It is also smaller than the difference observed between surface classes and therefore comparing flights even though they were at different times of day is justified.

4.2. **Changes to manuscript:** Page 10, Line 4 add "Though some of the flights were in the evening (1800 – 1900 local time) and some in the morning, these data are still comparable. Unlike CO2, methane has a weak to non-existent diurnal cycle (Figure 4). Based on our tower data we do show a very weak cycle, most likely caused by near-surface soil-temperature changes through the day. However, this diurnal cycle is much weaker (<0.2 ug m-2 s-1) than the class to class variations, seasonal variations, or variations due to other factors. Therefore, comparing flights even though they were at different times of day is justified. The sharp feature in the tower trace on August 13 (DOY 225) probably has a diurnal component, The important comparison, however, is between the strong methane flux in the summer regime of first half of August and the much weaker flux in the autumn regime of later August after the major reduction in soil temperature."

Figure 7 has been altered to give better evidence of which flights occurred in the daytime and which in the evening.



5. ***Comment from Referee 1:*** *Moreover, FFM acts as a filter reducing the use of available data by order 50%, i.e. it is wasteful with respect to data use efficiency.*

5.1. **Author response:** The FFM retains all data suitable for flux calculation. The data are simply stored and used as 60-s sums of cross products, a convenient form flexible enough to allow many different treatments. The particular approach used in this paper selects a subset of these fragments to address the question being asked, which is to identify discrete land-cover classes that stand out in their contribution to landscape-wide emission of methane. The focus of the current analysis is to examine the spatially dominant land classes in their "pure" form, so rather stringent criteria were applied which, it is true, removed about half of the fragments from the analysis. The FFM was conceived to answer this question: how representative is a single fixed site of other locations on a heterogeneous surface that to remote sensors appear similar? How good is the land-cover class occupied by that site as a proxy for methane flux? The more representative of a single land class the fragment is, the more significant the differences between land classes becomes.

The FFM, however, is not limited to addressing this question alone. Fragments could just as well be associated with values of some interval quantity such as a carbon-isotope ratio, NDVI, or the fraction of footprint occupied by each of several land classes. For this study we wanted to compare to other published measurements and assess the intra-class variability. Limiting the results to a few well sampled classes was better suited to that purpose.

5.2. **Changes to manuscript:** No change.

6. ***Comment from Referee 1:*** *Figures 4, 7 show that at a 5% significance level the FFM-derived fluxes do not actually differ between land cover classes, i.e. there is more unexplained variation in the error bars than there is explained variation in the land cover means. Also here, techniques overcoming these systemic deficiencies are available and in use (Jung et al., 2011; Yang et al., 2007)*

6.1. **Author response:** 1. Not all land-surface types emit significantly different amount of methane. This is not a problem given the questions we are asking. And while many land classes have similar methane emissions, others have significant difference, e.g. mesic sedge and wet sedge on August 13, or lakes and wet sedge on August 13. For the most part, after the soil cooling, the various land classes are not distinguished in their methane release. Keep in mind the goal is not to come up with a criterion that distinguishes land class by its methane emission (or to predict land class based on methane emissions), but to measure regionally aggregated methane emissions from each of a limited number of land classes. It is reasonable that some land classes will have similar methane emissions, especially for land classes that emit little methane.

2. Broader confidence intervals reveal lower statistical power. Typically for our data set, the broader confidence intervals are associated with the shorter samples (which reduces the power). A statistical sample, to the extent that it is independent and identically distributed is a repeated drawing from the population. If a particular outcome happens only 5% of the time, then at each drawing it has a 5% chance of being realized. But with repeated drawing, the chance increases of getting at least once some outcome having a 5% chance or less. In a very large sample, each outcome having a 5% chance will occur 5% of the time. But more than 5% of a small sample will comprise some outcomes individually having a 5% chance. If one uses a bootstrap method, which assumes the realized sample to be the entire population, a disproportionate number of population members will be outcomes that in the full population would be much less likely to occur. Of course, a new measurement set will contain a comparable number of unlikely outcomes, but they will be different from those in the earlier set of measurements. Adding new

data thus reduces the overall likelihood of all low-probability events and increases the power. Unfortunately, getting a new set of measurements is **expensive.** So the tails of the distribution developed using a relatively short sample of actual measurements will be biased toward greater probability than the true population. It will therefore have wider confidence intervals (which depend on the weakness of the tails) than would the true population. Techniques have been developed to address this issue, but their implementation is not trivial. They belong to the next generation of the FFM.

3. Our measurement of wet sedge has the greatest power, Second greatest is often lakes, but may be another land-cover type. Sedge is a strong emitter, but its confidence interval is shorter in part because we have a longer sample from it.

6.2. **Changes to manuscript:** Page 10, Line 23 add "Wet sedge, followed by the Sag river, had the largest observed flux of any of the land classes sampled during the first half of August. The other land classes have smaller, more variable fluxes on most flights so that surface class alone does not distinguish them. Most likely the true variability, contributing to the large confidence intervals, is caused by heterogeneity within the surface class in sub-surface soil temperature and water table height. However, within that we can still derive a mean flux based on a large regional sample. Once the soil cools, wet sedge shows reduced, though still positive, flux of methane consistent with the other surface classes measured such as mesic sedge and lakes. The Sag river shows close to zero methane flux.

Page 11, Line 9 add "The mean methane flux from lakes sampled on a flight by flight basis shows little flux on average, except for the lakes sampled on 130828.3, which are in a different area 250 km west of the tower. Those lakes show an aggregate mean of 0.36 ug m-2 s-1 (Figure 7)"

7. ***Comment from Referee 1:*** *I suggest the authors to consider a combination of above methodologies. In fact in their introduction the authors cite Metzger et al. (2013), who demonstrate such combination specifically for the use case of airborne flux measurements.*
Comment from Referee 2: *A few recent papers have used the wavelet analysis method to determine fluxes of air pollutants in urban and oil/gas regions (Karl et al., 2009;Vaughan et al., 2015;Yuan et al., 2015). The authors are encouraged to try this method.*

7.1. **Author response:** These papers look promising as discussions of how one can operate in urban and fracking regions. As explained in the first few responses, the FFM is a reasonable and sound method for analyzing these data. Its value derives from its position as an alternate approach from a different perspective (space/time domain). A comparison of the different methodologies is an activity we hope to pursue, but that is outside the scope of this present paper.

7.2. **Changes to manuscript:** No change.

8. ***Comment from Referee 1:*** *This hemispherical model requires calibration, in the case of very low-level flight in particular to offset dynamic upwash and ground effect which otherwise affect the covariance calculation (e.g., Crawford et al., 1996; Garman et al., 2008). Have these calibrations and corrections been performed, and if so to within which residual error?*

8.1. **Author response:** We flew multiple calibration maneuvers both in preparing for and during the Alaska campaign. Before assembling the FOCAL system, we characterized the BAT (gust) probe in a wind tunnel (Dobosy et al., 2013). We also tested a similar BAT probe in flight on a different aircraft (Vellinga et al., 2013, hereafter V2013). After calibration derived from a flight taken on the evening of August 27 in Alaska, we performed the yaw maneuver described by V2013 and obtained a residual contamination within 10%, as described there. A pitch maneuver described by V2013 was performed resulting in contamination of 10% for the high-frequency pitching (1.6 s period), which was the best executed of the pitch test's three parts and is the severest test.

8.2. **Changes to manuscript:** Page 4, Line 9 insert new paragraph "Before assembling the FOCAL system, we characterized the BAT probe in a wind tunnel (Dobosy et al., 2013). We also tested a similar BAT probe in flight on a different aircraft (Vellinga et al., 2013, hereafter V2013). After the FOCAL system was assembled, similar calibration maneuvers were flown in preparation for and during the Alaska campaign. As part of a calibration flight on the evening of August 27 in Alaska, we performed the yaw maneuver described by V2013 and obtained a residual contamination within 10%, as described there. A pitch maneuver described by V2013 was performed resulting in contamination of 10% for the high-frequency pitching (1.6 s period), which was the best executed of the pitch test's three parts and is the severest test.

9. ***Comment from Referee 1:*** *Please confirm that you use CH4 dry mole fraction for the covariance / flux calculation. •In case your calculation is based on partial density, how do you correct for density variations due to temperature and humidity fluctuations (WPL), as well as variations in pressure-altitude and corresponding changes in temperature and pressure, and thus partial density (Poisson equation)?*

9.1. **Author response:** We had provided this confirmation in the manuscript, page 4, line 25, and also on page 5, line 13 citing both Webb et al. (1980) and its update, Gu et al., (2012). We will move this citation back to the first mention of the gas measurements.

9.2. **Changes to manuscript:** Page 4, Line 25, add (Webb et al., 1980; Gu et al., 2012).

10. ***Comment from Referee 1:*** *At 5 m above ground this is approximately the eddy wavelength contributing most to the turbulent vertical transport. •Using the power law of spectral decay, for this platform the need for high-frequency spectral correction of the vertical turbulent flux would be minimal only at measurement heights of 50 m above ground and higher. •As you are focusing on measurements below 25 m above ground, which high-frequency spectral correction did you use, and how large was the correction?*

10.1. **Author response:** Plots of the spectra and cospectra of the data streams of vertical air motion and the dry-air mixing ratios of the trace gases were prepared and presented in a paper that was submitted to J. Ocean. Atmos. Tech. We have not used high-frequency spectral corrections as long as the highest wavenumber for vertical wind was clearly in the inertial subrange, i.e. following the -5/3 power of the wavenumber, and clearly above the wavenumber of the maximum spectral density. A data-starvation test using the flux runs from the evening of August 25 yielded an estimated loss of about 10% in fluxes computed with a coarser sample rate. Presenting a long discussion of the the spectra and cospectra seemed out of scope for the current paper. A discussion is included in a separate paper submitted to the Journal of Atmospheric and

Oceanic Technology (JTECH).

A regression of 3-km running flux (see Section 2.3.1) against the height above ground for flight 13.09:30 was run to assess the correlation of flux with altitude. A quadratic regression was required yielding significant positive slope but significant negative curvature. The regression line reached a maximum at an intermediate point before the maximum height above ground. Furthermore, the regression explained only 10% of the variance.

10.2. **Changes to manuscript:** add above to Page 4, line 9 and after additions from 8.2 above.

11. ***Comment from Referee 1:*** *Correct, this method neglects low-frequency contributions to the vertical turbulent flux. •As mentioned above, Ogive analysis typically saturates at 100 - 1000 x measurement height. •How did you correct low-frequency loss and how large were the contributions to the flux*

11.1. **Author response:** This comment was addressed in the response the reviewers' objection 1 above.

11.2. **Changes to manuscript:** See changes from comment one above.

12. ***Comment from Referee 1:*** *How was the turbulence statistics for a robust application of the flux footprint model calculated? A 1 s flux fragment has far too large random error to assume upstream isotropy of the wind field.*

12.1. **Author response:** The turbulent statistics required to parameterize the model of Kljun et al. (2004) were computed from averages taken over the length of each flight leg, where the flight leg was defined as the straight segment between turns over which the collected data were used. The detrending (subtracting the base state from the original series) was done over each flight leg. Typically the flight legs were 15 km to 20 km.

12.2. **Changes to manuscript:** Page 7, Line 17 [Start in 13.2] "We use the parameterization scheme described in Kljun et al. (2004) which uses a backward Lagrangian model (Kljun et al., 2002) for a range of heights, stability measures and other turbulence quantities that are measured from the aircraft. The turbulence quantities are computed from averages taken over the length of each flight leg, where the flight leg is defined as the straight segment, between turns, over which the collected data were used. [continue at 13.2, second part]

13. ***Comment from Referee 1:*** *This model is 1-D and does not resolve the cross-wind distribution of the influence area – how did you take this into account? •An updated 2-D version of this model is available (Kljun et al., 2015). Why was this model not used?*

13.1. **Author response:**
Since we use the surface class as a categorical quantity the crosswind-integrated form of the footprint model of Kljun et al. (2004, KCRS04) was considered appropriate for our use as a membership function for the fuzzy set (Nguyen and Walker, 2000) of a particular surface class. The selected 85% membership criterion is strict so as to admit only particularly representative instances of the surfaces encountered.

The more recent work of Kljun & Co. (2015, KCRS15) became known to us late in our investigation. In providing an explicit crosswind distribution to the footprint it represents significant advance over KCRS04. However, the crosswind-integrated footprint of KCRS04, fundamentally unchanged, provides the backbone for the two-dimensional footprint of KCRS15. The crosswind spread may be important, for example, where an interval quantity, such as NDVI is to be calculated from the footprint of each unit of flux in order to train a regression or machine-learning model, such as done by Metzger & Co, (2013), or Ogunjemiyo & Co (2003).

The present study was not intended to produce a regression scheme. It is about the role of each surface class (as a category) in the emission of methane. Since the footprint is computed every 60 m the procedure will identify all instances of the surface classes present except for the very smallest. Expanding the footprints to two dimensions does not appear to add sufficient value to justify recalculation. The results would be unlikely to produce any changes in the results.

13.2. **Changes to manuscript:** Page 7, Line 14 “Finally, a footprint model is applied to estimate the level of influence of each surface type on each fragment. This provides a measure of membership of that fragment in the fuzzy set (Nguyen and Walker, 2000) associated with each surface type, treated as a categorical variable. Fragments having a sufficient level of membership for a particular surface class are assigned to that class. A membership level above 0.5 restricts all fragments to no more than one class. Fragments can thus be grouped into sets all members of which have a measure greater than a prespecified level of the probability that they came from the same surface type (see sec. 3.2 for examples of how FFM is used to interpret these data)” [continue at 12.2]

[second part, continued from 12.2] The more recent two-dimensional version (Kljun et al., 2015) was not considered necessary because of the footprint’s restricted use as a membership criterion to assign a selected subset of fragments to the surface categories.

14. ***Comment from Referee 1:*** *There is no such website. Where can the data (incl. raw data) be accessed?*

14.1. **Author response:** The URL was missing an 's'. Should have been https://. Thanks for pointing this out.

14.2. **Changes to manuscript:** Page 12, Line 2 “https://arcticdata.io”

15. ***Comment from Referee 1:*** *There are more intuitive ways to visualize the footprint influence area. I am wondering why the authors did not use standard contour plots. Also, it is not apparent from the display whether cross-wind dispersion has been taken into consideration – the individual sequences of dots simply extend in the along-wind direction, which is only half the truth.*

15.1. **Author response:** With crosswind integrated footprints, it makes sense to plot them as lines, rather than as 2-D contour plots. However, to show the full footprint area along the flight track we have modified figure 4 to show a ribbon of footprint probabilities for one leg of each flight track for each day. Arrows have been added to show the dominate wind direction, which was observable before from the individual footprints. Hopefully this will be clearer for the reader.

15.2. **Changes to manuscript:** Figure 5 has been modified as described above.

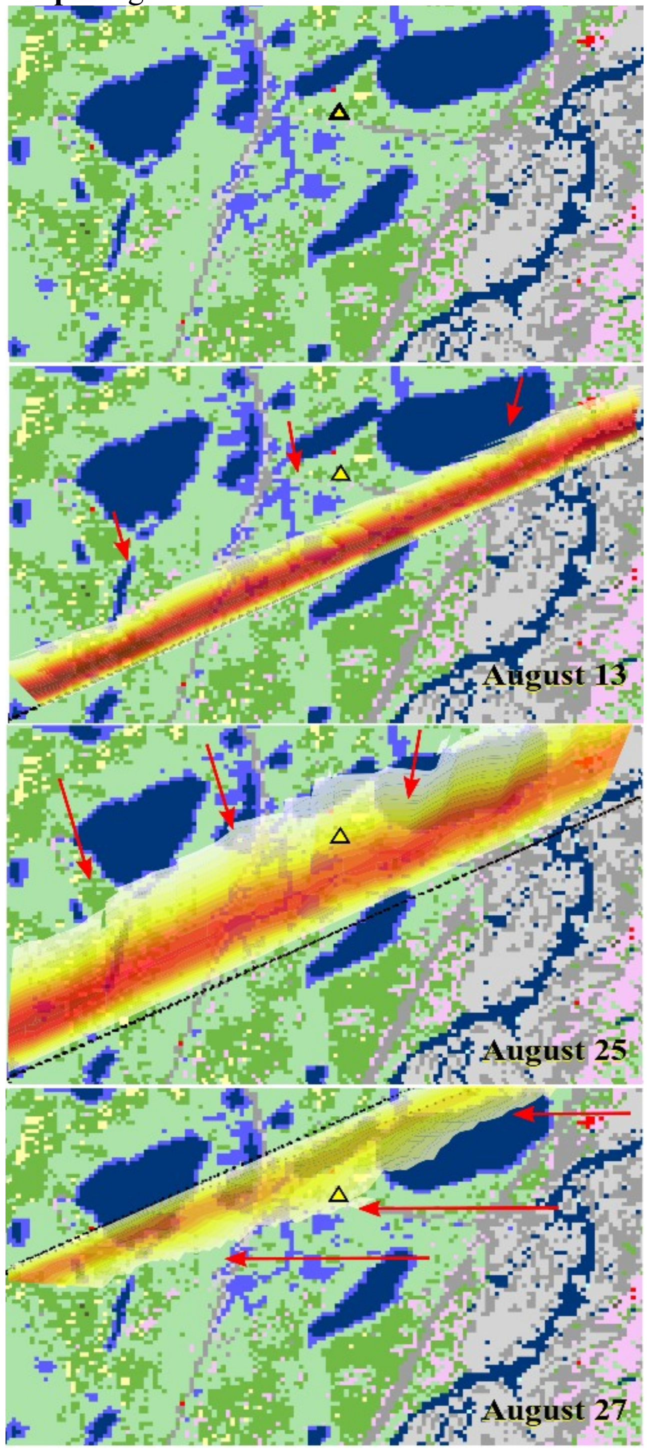


Figure 5. Map of area surrounding the flux tower (yellow triangle) with false color map representing different land classes defined as in Fig. 2. Bottom three plots show three days when data was taken near the tower. The flight track for each flight is shown as black points, where each point is the start position of a flux fragment. Colored ribbon shows the flux footprints along the flight track. The darker and redder color of the ribbon represents larger probability of contribution to the total flux as described in the text. Red arrows indicate the mean direction of the wind.

16. ***Comment from Referee 2:*** *The authors spent some time to introduce the fast measurement system of wind and CH4. Could you add some spectral analysis for measured data.*

16.1. **Author response:** See 10.1

16.2. **Changes to manuscript:** See 10.1

17. ***Comment from Referee 2:*** *Figure 4. Can you show the graph as 2*2 layout? The inserts are somewhat misleading and are hard to follow at present layout.*

17.1. **Author response:** We have modified Figure 4 by breaking it into four panels. One long panel displays the tower data, locating the three near-tower flights as before. Temporal resolution was improved by displaying only the periods when the aircraft was operating. The three insets have been relocated as individual panels underneath the tower data and are labeled by flight day instead of a,b,c. The abscissa of each is now given as (local) time of day to show the actual time of flight.

17.2. **Changes to manuscript:**

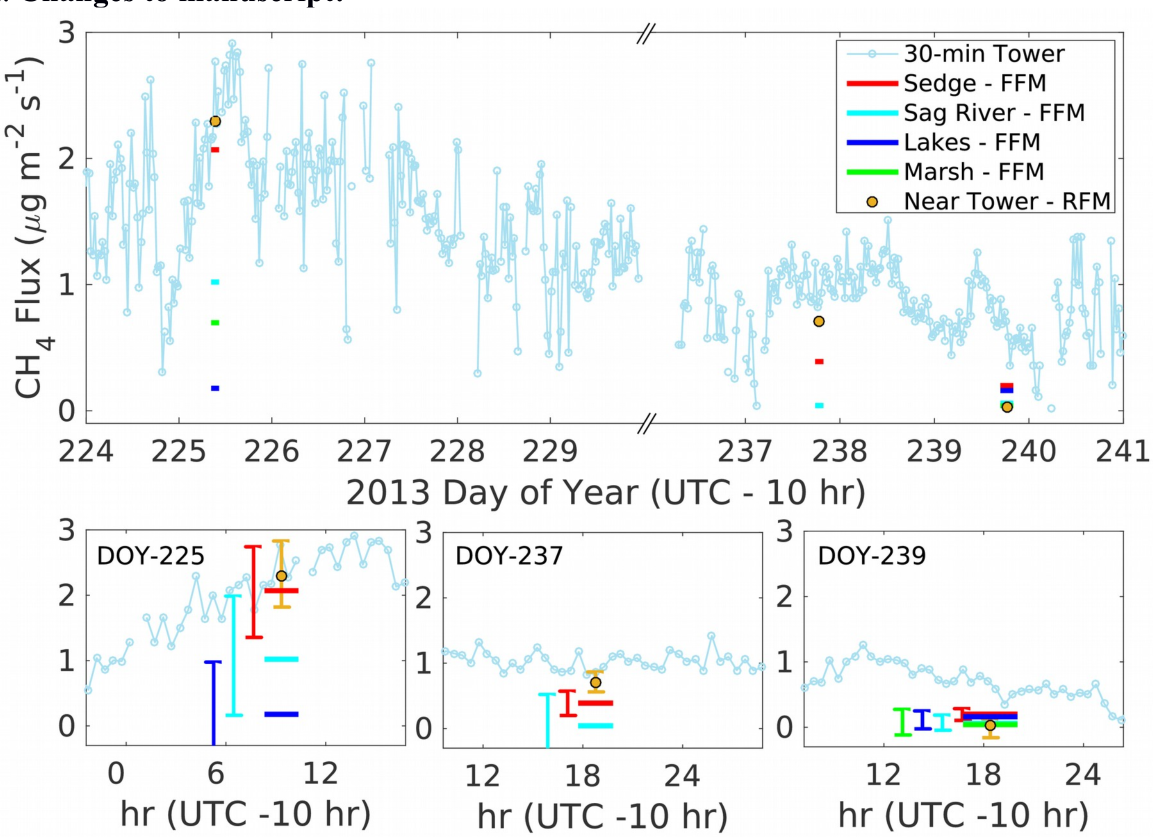


Figure 4. Comparison of methane flux measured by the flux tower with fluxes measured by the FOCAL system. Tower methane fluxes (top plot) are 30-minute means plotted versus day of year. Three flights (Aug. 13, 25, and 27) made repeated flight transects near the tower. A running mean flux, using the nearest 3 km of flight track to the tower for each leg, was calculated and the mean of these fluxes is plotted for each day as an orange circle. Fluxes for wet sedge, marsh, lakes, and the Sag river were calculated using FFM using data from the whole

flight and are plotted for each day, color coded according to the legend, with the length of the line along the time axis representing the time over which the data were taken. Bottom plots show details for each flight day, labeled by day of year (DOY), with bars showing the 95% confidence interval based on bootstrap analysis. Bars are offset along the x-axis for clarity.

18. ***Comment from Referee 2:*** *Figure 2 and Figure 7: Could you use a consistent way to indicate flight numbers conducted at the same days. Please include this information in the figure caption.*

    18.1. **Author response:** It is consistent, Figure 7 just leaves off the common 1308 part, but we can add that back into the figure legend. The information is already included in the captions of fig 2 and 7 and table 1.

    18.2. **Changes to manuscript:** We have modified the date convention to include the flight time and changed, Table 1 and Figures 2 and 7. The new convention uses DD.HH:MM.

References added:

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