Anonymous Referee #2

Thank you very much for your comments to our discussion manuscript. Below we address one by one the comments made during this review. All answers are in blue font.

Overall:

The ms has its focus on regional scale methane dynamic and the modelling of year round dynamics, which is certainly relevant and highly needed. I general there are quite few year round measurements of methane dynamics in the arctic region, which also explains why the modelling studies are even fewer and regional budgets are poorly constrained. Further, the understanding of drivers and exact transport mechanisms in the top soil and soil – snow-atmosphere still in most (not all) cases relies on an interpretation of a net emission, rather that independent quantification of the individual components adding up the net CH4 emission. For that reason the focus of the current ms is important and timely. Despite that the ms is well written and in general well references, I'm a bit reluctant about the qualities of the ms, because I basically find that it tries to accomplish too much and not in a fully convincing way. As pointed out by reviewer 1, also I have a serious problem with the differences in scaling which are used in the different components of the study. In my perspective, the very coarse spatial scale of the model does not compare well with the highly advanced model approach of partitioning the production and transport of CH4 in the soil and snow. The ms simultaneously tries to solve the issues of the spatial /temporal methane dynamics of the large Sibirian wetlands, the process pathways and comparing all the modelling output to relatively few and very local measurements near Chersky. I basically don't think that the available measurements are well suited to verify the model output of the processes leading to the net CH4 emission at the surface, and the differentiation of pathways of CH4 during different periods of the year.

In agreement with reviewer 1, we detailed further our approach for the scaling between the model output at grid cell level and the available observations, especially those from Eddy covariance measurements. These are also added in this response below.

We agree with the reviewer 2, and also highlighted by reviewer 1, that the observational data to validate our model output is few. On the other hand, boreal wetlands, especially those in permafrost regions as in far Northeast Siberia are quite understudied due to the difficulty to reach those places and perform measurements all year round. The data presented in this manuscript shows a synergistic and unique study between the first year-round greenhouse gas emission measurements and summer chamber fluxes in a site of the Kolyma (Chersky) floodplain and a process-based methane model embedded in a land surface model. Site level comparisons are achieved by comparing the two years of continuous Eddy covariance methane flux measurements and summer flux chamber measurements to the model grid cell output. Many fluxes simulated by models in other world regions cannot be adequately evaluated due to the lack of measurements on the study site and have to rely in measurements done in other areas, or from data that has been collected during different periods of time, e.g. only summer measurements. In contrast, our study benefits greatly from the simultaneous temporal and spatial (at grid cell scale possible) synergy where the model development has directly benefited from the year-round greenhouse gas fluxes observations. Finally, is not our aim to been able to evaluate all model grid cells but rather demonstrate that a process-based methane model can achieve in an Arctic tundra region and this is already a scientific contribution per se to the scientific community.

I my opinion the ms could benefit from being divided into two; one with focus on the annual budget for Sibirian and one focused on the process modelling of the different pathways for methane through the soil/snow pack. The later one could benefit from some kind of lab or micro cosmos comparison, where processes could be studied more precisely than what is mostly the case in the field.

We appreciate this suggestion made by the reviewer. The reviewer suggests to have one ms focused solely in the annual methane budget for a larger study region, i.e. Siberia. In fact, we plan to work on such a larger scale (but still high-resolution, process-based) study in the future. However, at this point our intention is mainly to provide a first proof-of-concept of the applicability of our modeling framework at a still relatively small regional domain, and we believe that this manuscript needs a strong focus on the background description of the used model configuration. Such description has not been published elsewhere. The reviewer also suggests to have such process modeling description supported by e.g. micro cosmos experiments, in a separate ms. We agree that such a study would be an excellent addition to the study we already completed; however, developing micro cosmos experiments is out of the scope of our current project and this step could be done for future investigations in order to refine the current model configuration. Instead of splitting our work into two separate manuscripts, we therefore suggest instead to improve the flow of the current ms: we will shorten the revised ms and move extra useful information to the supplementary material, and clarify further the issues mentioned through this review in the new revised ms.

Regardless of the approach, the issues of differences in scales should be discussed much more detailed and qualified than it is done in the present version of the ms. From my perspective the output of the model and the assessment of the advances in the new "improved" version is not credible as it appears now, despite that the output is in the same ballpark as the measured data, and a number of other studies.

As mentioned earlier, we refined the analysis for the comparison between the methane fluxes from eddy covariance and those from the model, regarding the difference in scales, this also helps to sustain better our results presented in the ms. The difference in scales between EC data and model output is also a comment made by reviewer #1. Our answer to this concern is (same as for Rev. #1):

We agree that the comparison between model methane fluxes and those from observations, specifically from eddy covariance, is a challenge. In our manuscript, we use a scaling factor for the chamber data by considering chamber measurements that were done under exclusively wet and under exclusively dry summer conditions. We then make use of the total fraction of inundated areas in the model grid cell (IF) modeled with the TOPMODEL approach to scale the total chamber fluxes. This scaling approach takes into consideration that the model methane fluxes represent the emissions from only the portion of the grid cell that is inundated, i.e. with water at or above the soils surface.

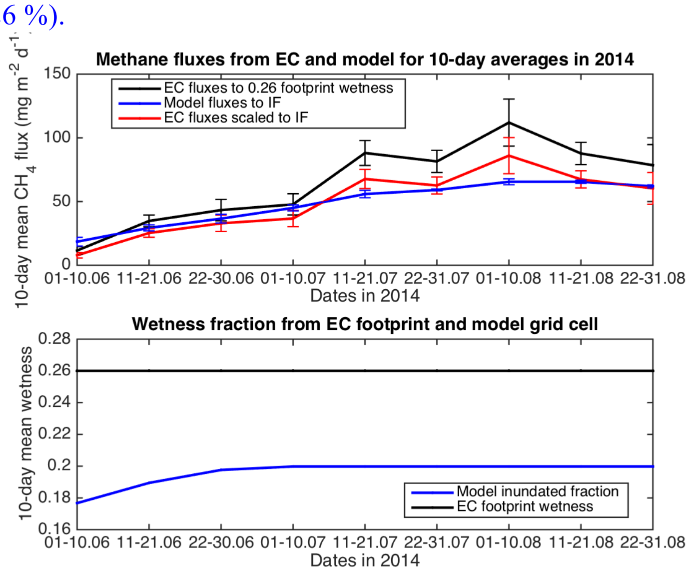
In the case of the eddy covariance fluxes, following the concerns of the reviewer, we re-evaluated our approach for this comparison. In the revised version of this manuscript we include now a thorough analysis of the footprint area of the eddy covariance fluxes as part of a new Appendix B on "Details on in-situ flux observations". This appendix also includes details on the eddy covariance flux data uncertainty assessment and more detailed results on the chamber measurements, as requested below also by the reviewer. This appendix will be part of the revised manuscript and is attached at the end of this response.

In this new appendix, we analyze the type of vegetation and its coverage in the footprint area of the EC tower, from remote sensing images as a metric to identify wet and dry areas. Areas with dominant cotton grasses, specifically *Eriophorum angustifolium* in our study area, are indicators of predominant wet soils, while tussocks, specifically *Carex appendiculata* in our study area, and shrubs are indicators of predominant dry soil conditions. It is important noting that *C. appendiculata*, can be also found in wet areas, but is predominant in dry areas.

For the model, the vegetation distribution per grid cell is too coarse to consider this metric similar as that for the remote sensing data in the EC footprint area, however the total abundance of C3 grasses in the grid cell A is 33.3 % as given for the model (with the rest of the grid cell dominated by deciduous shrubs and extra tropical evergreen trees), but there is no discrimination between cotton grasses and tussocks.

The footprint of the eddy covariance tower in the Chersky floodplain covers an approximate area of 400 m x 400 m, similar to that one depicted in Fig. 1 of Kittler et al. 2016 (cited in discussion ms) (see new Appendix B at the end of this response for footprint area for the EC tower used in this manuscript). The remote sensing analysis revealed that cotton grasses are present in about 26 % of the footprint area, which would translate into the same portion of the footprint area as fully wet zones during the "wet months": after spring melt in June and until

August when most annual precipitation in the region takes place, covering most of the growing season. As will be shown below in this response, CH_4 fluxes measured by chambers (footprint of 60 cm x 60 cm) revealed that during the growing season in dry soil areas of the Chersky floodplain that are characterized by a water table below the surface, the emission of methane during the growing season is negligible with even some atm. CH_4 uptake by soil (i.e. negative CH_4 flux rates) (data shown in new Appendix B). Under this consideration, and as confirmed recently by Helbig et al., 2017, the majority of the CH_4 fluxes measured by the EC tower would represent fluxes from fraction of wetland in the footprint area, i.e. 26 %.
In case of the model grid cell where the location of the EC tower falls (grid cell A in Fig. 1 of the discussion ms), the IF for June-July-August during 2014 shows growing inundation values from 17.7 % to 19.9 % (for 10-day mean values for those three months) representing the percentage of total wet areas in the grid cell area. These values are slightly smaller than the 26 % wetness area in the EC footprint, and denote the area of the grid cell where the model methane emissions take place (i.e., no emissions in dry areas, in agreement to the chamber measurements).
With this basis and to make a closer comparison between EC flux measurements and model data for the growing season months, we scaled linearly the 10-day mean EC methane fluxes to the IF from the model, and calculated the standard deviation of the 10-day mean. In the next figure, we show: TOP panel, the original 10-day mean EC methane flux measurements that would represent the emissions of a 26 % wet area between June and August 2014 (black line), the 10-day mean EC methane fluxes scaled to the 10-day mean IF from the model for the same period of time (red line) and 10-day mean model methane emissions for grid cell A, which imply emissions from the IF from the model (blue line). Error bars in all lines are one standard deviation of the 10-day mean flux values. The BOTTOM panel shows the 10-day mean IF from the model used to scale the EC fluxes (blue line), and the constant wetness percentage of the footprint area calculated from the vegetation coverage remote sensing images (i.e., 26 %).



We observe that the scaled EC methane fluxes decreased as a lower IF is considered within the footprint, and those new calculated fluxes become closer to those from the model, and in most cases the latter fall within the 10-day standard deviation of the EC fluxes.
Unfortunately, it is not possible to obtain a temporal varying wetness area for the EC footprint all year, based on our approach of only considering the vegetation cover, thus wouldn't be appropriate to scale all of the EC fluxes for 2014 and 2015 to the IF from the model without any reference for spring and winter wet footprint areas. However, from this analysis we learn that: 1) considering the vegetation cover as indicator of soil wetness, the EC footprint area holds a very similar area to that of the model grid cell through which the majority of the methane is emitted to the atmosphere and 2) the net offsets between methane flux model and EC data can largely be attributed to differences in wetness levels.

Summarizing, we assume that for both the model grid cell and the eddy covariance footprint, methane emissions are not spatially homogeneous, but bound to the distribution of wet (inundated) areas. Accordingly, a meaningful agreement between model and observations can only be obtained if two factors are fulfilled: (i) the fraction of wet surfaces agrees between both data sets, and (ii) the flux rates from wet surfaces agree between both datasets. Through correcting the offsets in inundated fraction, we could demonstrate that the flux rates between model and eddy covariance observations agree very well, emphasizing the sound setup of the model algorithms and parameter settings. We will add the analysis presented here into the new Appendix to complement the discussion on scaling fluxes for comparison between EC and model data.

Specific:

L48 -> 66: Maybe a matter of taste, but I'm in general against using these "horror scenarios" which draw lines between the carbon pool of the Arctic soils and potential increase of GHGs. I think we now know that no indications are found that something very dramatic is happening in foreseeable future, and it doesn't add to the understanding of the ms. Consider rephrasing.

Thank you for this suggestion. We will consider rephrasing these lines. However, we think is still important to mention them, such as that changes in air temperature, soil topography and projected shifts in precipitation in Arctic tundra ecosystems will influence in the future the soil hydrologic regime in permafrost areas which in turn will affect future emissions of CO2 and CH4 into the atmosphere from Arctic terrestrial ecosystems. This projected scenario is part of current literature discussions, which draw the framework of studies like ours presented in this manuscript.

L187: Despite that you are obviously aware of the complications of the comparison between scale I'll encourage you to address specifically how the scaling issue between 0,5_ modelling grid and EC footprint or chambers is dealt with.

We have added the response to this important suggestion above.

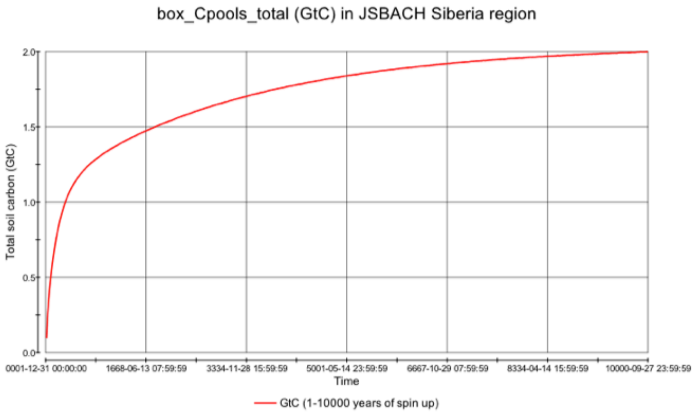
L218: Again please justify, why 11 soil layers are needed, when the horizontal scale is this coarse.

The coarse horizontal resolution in the model does not influence the need for a refined vertical discretization of soil processes. In particular, a fine vertical resolution is required to find numerically stable solutions of the gas diffusion equation.

L323: Spun up for 10.000 years? Please justify further, climate (or C – pools) can not be assumed to have remained constant for this period of time.

Thank you for this question. The idea behind the so called spin up approach is to initialize state variables, such as temperature, moisture or carbon content based on the process representation (the differential equations) and environmental conditions during a pre-industrial time when we can neglect a human-induced disturbance of ecosystems and climate. This is important in prognostic modelling in order to reliably isolate effects of anthropogenic actions and related climate change on ecosystems. Usually, soil carbon pools have a mean residence time of less than 1000 years in the aerobic case and hence, this slowest carbon pool will reach a steady state with pre-industrial climate after 1000 years of iteration. For example, for a temperate terrestrial ecosystem we would assume a stable climate over 1000 years round 1700-1800 and substitute the pre-industrial climate by an observation-based climatology from 1901-1930. Climate variations in the past (e.g. little ice age) are usually neglected because future climate change will be much stronger.

Soil organic matter in permafrost regions is additionally stabilized by soil freezing, even in the active layer, i.e. the OM is either frozen over long time periods in permafrost or the decomposition season is reduced to a few months. That is an additional stabilization which leads to much higher effective mean residence times and hence we need to spin-up the model longer to reach the pre-industrial steady state, usually 10000 years are valid (McGuire et al., 2016; Chadburn et al., 2017). In Chadburn et al. (2017) it was shown that such approach leads to soil organic matter pools comparable to observations at several Arctic stations. At the Cherskii site, we unfortunately do not count with observed carbon stocks. In the following figure however, we show the total carbon (sum of woody, green and reserve) in the soil at the end of this spin up period for the entire model domain and it can be seen that these have reached equilibrium after this period.



L403: I understand that the numbers can be compared, but please argue why the field site measurements can be assumed to be averaging the full 0,5x0,5_ modelling pixel.

In this context, we approach a scaling factor considering the wet and dry areas of the grid cell vs. the wet and dry plots in the chamber sites (model grid cell and chamber plots heterogeneity). This was documented in the discussion manuscript. Briefly, to obtain the total flux from chamber measurements, the values measured from fully wet sites and fully dry sites were scaled to the daily-inundated fractions as given from the model, leading to Eq. 8 of the discussion manuscript. Taking this approach in consideration, the final fluxes from chamber measurements represent the CH_4 emissions per m^2 per day under heterogeneous (wet and dry) soil conditions, similar to those at grid cell scale.

L445: Differences seems to be substantial please comment.

The differences in the wetland extent from the model compared to those from the high-resolution remote sensing data might seem substantial (~ 6 %), however, it is important noting the following:

- No other remote sensing data exclusive for this area at such high resolution (150 m) has been available for the Boreal Arctic region such as that one used for our study (Reschke et al., 2012; cited in discussion ms). Other available wetland extent remote sensing products are only at global scale with spatial resolutions in the order of 25 km. By having a better-resolved reference wetland extent, ensures that the uncertainties in the model wetland extent are limited mostly to the model technique and spatial resolution used to simulate wetlands. In addition, high latitude wetlands still pose a challenge in remote sensing, due to the long periods of darkness during the year.
- The addition of the TOPMODEL scheme in the land surface model JSBACH allows the representation of standing water following the topographic profile of the region of interest. Without this scheme, it is not possible at all with this model to allow accumulation of water at the soil surface. Furthermore, this is not an exclusive characteristic of the JSBACH model, as most land surface models lack of an explicit and fully functional hydrology model where also the dynamics of inland waters, such as runoff, are possible to be simulated. From the modeling point of view and within the inherent limitations of the model structure, the possibility of simulating wetland extent in a land surface model and having a remote sensing data with sufficient resolution is an excellent combination of first steps achievements to improve process-based modeling for greenhouse gases at high latitudes. For this reasons, we argue that a 6 % difference between the remote sensing wetland data and the modeled data provides a first good approximation.

L470: I basically don't understand how a threshold can be set for proportion of flooded area in a pixel – what is the rational? Theoretically the whole pixel could be inundated –I assume?

The proportion of flooded area in a pixel follows exclusively the topographic profile and the location of the water table, in a way that if there is enough water content in the soil (basically close to saturation) it will be possible to accumulate water at the surface and only if the topography structure allows it. For this reason, not all the pixel could be inundated. The sequence of the TOPMODEL scheme in our model configuration follows the next steps: 1) selection of soil layer where the water table will be positioned according to the soil water content, 2) by defining a water table threshold, the model locates the position of the water table, 3) a fixed TOPMODEL parameter defines the dependence of flooding on the water table variation in a way that the lower the value of this parameter means that larger areas with the same water table will be flooded (parameter *f* of equation 1, exponential decay of transmissivity with depth, in the discussion manuscript), 4) a fixed threshold in the TOPMODEL scheme limits the area of floodability (χ_{min_cti}, given in L242-245 in the discussion ms), this is used to avoid the occurrence of running water and is dependent on soil types. This TOPMODEL parameter is used in the general TOPMODEL scheme to allow runoff, which in our model configuration should not be taken into account. The lower the value the larger the flooded area e.g., limits of the horizontal extent of the inundated area. We find this level of detail on the model configuration can only be added in the supplementary material of our manuscript and we will consider doing so.

L532: What effect of the snow would you have expected in this context?

As shown in the sensitivities exercise, by describing thinner snow layers (3 and 1 cm) than the value in the control simulation (5 cm) allows only in a temporal shift of the emissions without affecting the magnitude of the total annual CH_4 emissions. The intention of doing this test is to analyze the response of the model exclusively to this parameter and our hypothesis (thinner snow layers allows faster diffusion of gas than thicker layers with constant density values) has been confirmed.

L630: there seems to be significant difference in measured and modelled soil temperatures, please comment.

The effect of having higher soil moisture in the soil pores influences the soil thermal regime in organic-rich soils both during summer and winter. When the soil pores are predominantly filled with water, the water promotes a high thermal capacity, and when pores are predominantly filled with air, the thermal soil capacity decreases and more energy is required to heat the soil. Also, near-surface vegetation in these tundra environments, such as mosses and lichens (Porada et al., 2016) plays an important role as effective thermal insulator but also would help to insulate the surface soil layers from the warm surface temperatures from atmospheric influence during summer. As well, snow cover serves as thermal insulator, and a further snow layer evaluation from the model and measurements in the site needs to be done as measurements become available.

Besides the need to consider the previous factors, our results evidence the effect of the soil moisture variation, which in general is quite low, to the soil thermal regime. The soil hydrology, as mentioned extensively in the manuscript, poses still limitations in our current model configuration and it requires further improvements, also hopefully based, on available soil moisture measurements in this study region, which at the moment are unavailable.

L665: probably why also both absolute values and seasonal pattern seems distinctly different

It is unclear to us what this comment from the reviewer is referring to. We ask for a further clarification in a way that a suitable response can be given from our side.

L710 -723: that differentiation between ebullition and diffusion seems unfounded, and it is hard to see how you verify the different pathways, please elaborate.

In our methane module, emissions of methane via ebullition and diffusion are explicitly modeled and are based in fundamental principles of gas motion. Diffusion of a gas is a molecular motion process and its speed relies on the medium where it takes place: it is slow in water and faster in air. It works independently of a water table with the net movement of molecules following a concentration gradient from high to low concentrations in order to achieve equilibrium. In the case of ebullition, this takes place when a certain volume of water gets saturated with a specific gas and oversaturation allows the formation of bubbles that, due to pressure effects, are released through available pathways, such as interstitial water in the soil. While diffusion is a continuous but rather slow molecular process, ebullition is fast and highly sporadic. These are well known physical processes in gas dynamics, and an excellent review on the explicit diffusion and ebullition processes for methane in soils is provided in Le Mer and Roger (2001), Eur. J. Soil Biol. Vol. 37, doi: 10.1016/S1164-5563(01)01067-6.

Para 3.4.3: could this be merged with the sensitivity study in 3.2? seems to be fundamentally alike.

This recommendation by the reviewer is unclear. Section 3.2 is about presenting the results of the sensitivity experiments, while section 3.4.3 contain results on the environmental controls related to the methane fluxes and their temporal variation. Thus, these sections are not alike and therefore cannot be merged as suggested. Perhaps there was some confusion in the number of sections that the reviewer is referring. We ask for a clarification on this comment to better assess a response.

Fig. S5b: legend does not seem to match.

Ok, panels a and b were inverted and this is now corrected. Thank you for identifying this mistake.

L916-920: the conclusions here seem somewhat unfounded due to the previously mentioned scaling issues.

The lines the reviewer here is referring to are part of the discussion and not of the conclusion section (starting in L1098 of discussion ms). Related to the lines referred here from the discussion, we wrote: “We simulated for the first time year-round methane emissions in a Northeast Siberian region centered on the city of Chersky, including emissions during the non-growing season. Our results showcase the ability of the improved JSBACH-methane model to reproduce seasonality in the CH_4 emissions when compared to fluxes measured by eddy covariance and chambers in a study site near Chersky.”

In these lines of the discussion, we refer also explicitly to the ability of the model to reproduce the seasonality of the methane emissions (lower in winter, higher in summer months) independent of their magnitude, which we presented and discussed accordingly in the discussion ms. Regarding the scaling between EC measurements and model results for the comparison of their magnitude, we hope that with the clarification above this argument is adequately answered.

References.

Chadburn, S.E., et al., (2017), Carbon stocks and fluxes in the high latitudes: using site-level data to evaluate Earth system models, Biogeosciences, 14, pag. 5143-5169, doi:10.5194/bg-14-5143-2017.

Helbig, M., Quinton, W.L., Sonnentag O., (2017) Warmer spring conditions increase annual methane emissions from a boreal peat landscape with sporadic permafrost. Environmental Research Letters, 12, 115009, doi: 10.1088/1748-9326/aa8c85.

McGuire, A.D. et al., (2016), Variability in the sensitivity among model simulations of permafrost and carbon dynamics in the permafrost region between 1960 and 2009. Global Biogeochemical Cycles, 30(7), pag. 1015-1037, doi:10.1002/2016GB005405.

Porada, P., A. Ekici and C. Beer (2016), Effects of bryophyte and lichen cover on permafrost soil temperature at large scale, The Cryosphere, 10, 2291-2315, 10.5194/tc-10-2291-2016.

Appendix B: Details on in-situ flux observation program

Eddy-covariance flux data uncertainty assessment

Following well-established procedures in literature (Aubinet et al., 2012), our uncertainty analysis for the eddy-covariance flux data has been split up into random and systematic errors. The major sources for random errors, associated with the turbulent sampling and instrument issues, have been quantified for each 30 min flux value through the flux processing software TK3 (Mauder and Foken, 2015). Errors related to footprint uncertainties were not quantified, since there are no major transitions in biome types within the core areas of the flux footprints.

Systematic errors can be introduced by unmet theoretical assumptions and methodological challenges, as well as by instrument calibration and data processing issues. In the context of the Chersky observations, instruments were maintained and calibrated in regular intervals, therefore minimizing this potential error component. Moreover, data intercomparisons with a second eddy-covariance tower close by (~ 600 m) yielded no systematic offset in the frequency distributions of wind speed, sonic temperature, and methane mixing ratios between towers. Regarding flux data processing, the TK3 software package contains all the required processing steps, conversions and corrections for the flux data processing, and yielded good agreement in a recent comparison with EddyPro (Fratini and Mauder, 2014). To avoid methodological issues that may bias flux data results, we employed a rigid post-processing quality control and flagging system scheme, with the well-established analyses for stationarity and well-developed turbulence originally proposed by Foken and Wichura (1996) at its core, supplemented by additional tests (absolute range and spikes) to flag implausible data points in the resulting flux time series. Based on the quality assessment and control tools outlined above, we excluded systematic errors from the uncertainty quantification of flux data that were assigned a high to medium data quality (QF 1-6 based on the scheme proposed by Foken et al., 2005; 2012) and subsequently used for assessing long-term CH_4 flux budgets.

No u*-threshold was applied to the flux dataset, since we determined stationarity of the signal and integral turbulence characteristics also for nighttime conditions. This information facilitates identifying datasets with regular turbulent exchange also during stable stratification, therefore producing fewer gaps compared to a bulk exclusion of data during stable nighttime stratification through the u*-filter method. After filtering out low-quality fluxes, the data coverage of methane fluxes was 86 % during the growing season and 67 % during the winter from the original full 30 min flux data set (Kittler et al., 2017). To produce a continuous flux record for quantification of long-term CH_4 budgets, we filled the remaining gaps by averaging existing flux data within a moving window of 10-day length centered on the gap. Uncertainties for gap-filled values were quantified as standard deviation within the corresponding window, similar to the definition of gapfilling uncertainties for the CO_2

flux via the well-established marginal distribution sampling routine by Reichstein et al. (2005).

To produce aggregated uncertainty values for longer time periods, we applied the procedures suggested by Rannik et al. (2016). All random errors were combined by considering them as independent variables, and normally decrease with the length of the averaging period. Averaged over both data years used within the context of this study (2014 and 2015), the CH_4 flux uncertainty based on the 30 min data is 7.4±8.3 nmol m^{-2} s^{-1}, a result comparable to 4.7±3.8 nmol m^{-2} s^{-1} reported for a fen ecosystem by Jammet et al. (2017).

Source weight function of the eddy-covariance flux data

We conducted a source weight analysis, also called footprint analysis, to determine the fractional contribution of different land cover types within the field of view of the eddy-covariance flux tower. Source weight functions for each 30min flux measurement were computed based on the Lagrangian Stochastic footprint model by Rannik et al. (2003). Footprints were accumulated, analyzed and interpreted using an approach presented by Göckede et al. (2006; 2008). We projected these footprints onto a WorldView2 land cover map at 2 m horizontal resolution (see also Figure A1). In the context of the presented study, we aggregated the original 22 land cover classes into 9 classes to concentrate on the dominant elements of the vegetation community structure (see also Table A1).

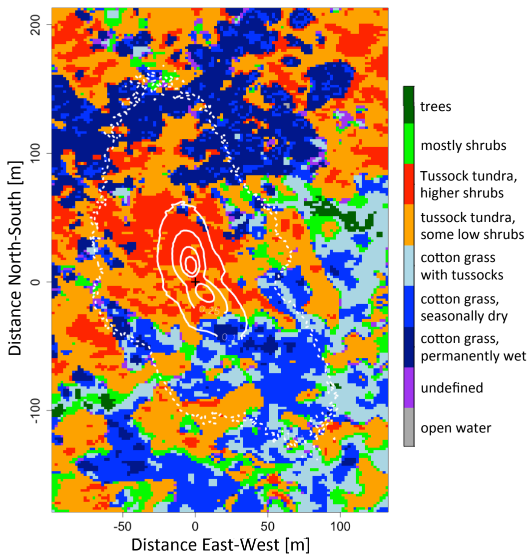


Figure A1: Accumulated source weight function for the control tower within the Chersky study site, based on data from the growing season (mid June – mid September) in 2014. Solid white isolines indicate the 80, 60, 40, and 20% levels, while the dashed line gives the 10% level. Background colors give aggregated land cover classes based on WorldView2 data.

Since the tower is situated on a slightly elevated patch of tundra, tussocks and shrubs featuring various levels of wetness (red and orange colors in Fig. A1) dominate the immediate surroundings. Even though inundated parts of the study area, in this case identified by the prevalence of the cotton grass *Eriophorum angustifolium* (blue-ish colors in Fig. A1), are dominating the area encircled by the 10% isoline that is used here to mark the boundary of the cumulative footprint area, they are mostly present in the outer reaches, therefore combining just about 26% of the total flux signal sampled by the eddy system. Another 31% is contributed by wet to moist tussock tundra with some shrubs. Overall coverage fractions within the major wetness categories (see also Table A1) remain approximately constant between tower footprint and two larger regions covered by the same WorldView dataset, indicating that this composition of wetness levels is typical for the Kolyma floodplain ecosystems analyzed within the context of this study.

Table A1: Fractional coverage of aggregated WorldView land cover classes within the control tower footprint of the Chersky study site. Background color coding was used to categorize the classes into wetness levels. The rightmost two columns give fractional coverage of these classes within the area immediately surrounding the towers (1.2 x 1.2 km) and within the entire WorldView scene analyzed (5 x 5 km).

Land cover class	category	tower footprint	1.2x1.2km	5x5km
water	open water	0.001	0.035	0.134
cotton grass, wet continuously	wetland	0.111	0.043	0.053
cotton grass, partially dry	wetland	0.067	0.153	0.147
cotton grass with tussocks	wetland	0.081	0.063	0.038
tussocks with some shrubs	wet to moist	0.312	0.418	0.280
tussocks with higher shrubs	moist to dry	0.388	0.165	0.182
higher shrubs, with tussocks	moist to dry	0.031	0.097	0.115
trees	moist to dry	0.001	0.006	0.017
undefined		0.008	0.020	0.035

Flux chamber observations

The Chersky study site features two transects of 10 permanently installed PVC collars for flux chamber measurements. With distances of approximately 25 m between individual microsites, both transects cover a distance of ~225 m within the drained and control sections, of this permafrost site. Site locations were selected quasi-randomly to reflect the dominant microsite characteristics (e.g. vegetation composition, wetness level) found at each of the target locations. With a chamber footprint of 60 cm x 60 cm, this technique allowed studying microsites with rather homogeneous environmental conditions, as compared to the eddy-covariance fluxes with often heterogeneous footprint areas. Details on the chamber program, overall methane flux rates observed, and functional relationships with e.g. soil temperature, vegetation and wetness levels, are provided by Kwon et al. (2016; 2017).

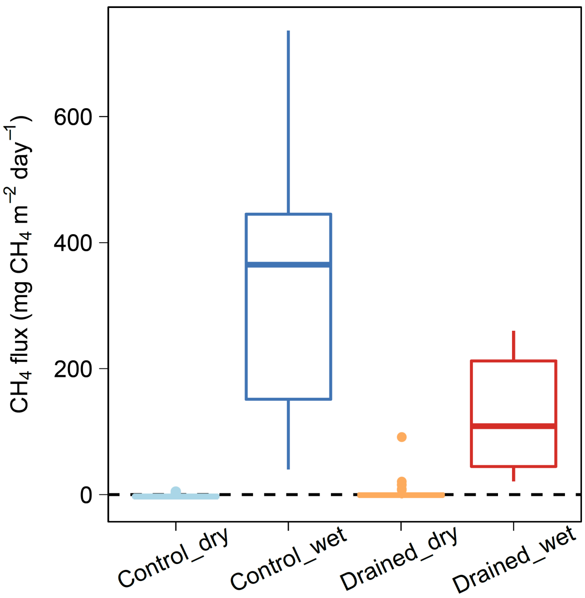


Figure A2: Daily methane flux rates aggregated from flux chamber measurements within the growing season of 2014. Measurements are separated into drained (1 wet microsite, 9 dry microsites) and control (8 wet microsites, 2 dry microsites) transects.

Figure A2 displays average flux rates for wet and dry microsites observed within the drained and control transects during sampling campaigns in summer 2014. These flux chamber results clearly demonstrate that methane release rates were virtually zero in the absence of standing water. At some of the dry microsites (results not shown), even negative CH_4 flux rates were observed, indicating the oxidation of methane under highly aerobic conditions within these predominantly wet tussock tundra ecosystems in Northeastern Siberia.

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