Reply to Reviewers

We thank the three reviewers and the contributor for their careful assessment of our manuscript and their valuable feedback. We have done our best to incorporate the suggestions in our manuscript whenever possible. Our detailed reply will show how our revised manuscript will cover these points. Our reply to the reviewers points (in black) are given in blue text below.

Reviewer #1

The manuscript describes extremely high CH4 efflux from a hydropower reserve in Switzerland, and tries to instigate the environmental controls on these peak methane emissions. I found the manuscript interesting and well written and the topic relevant as it shows that the conversion of new land areas into hydropower reservoir could substantially increase greenhouse gas emission. However, there are shortcomings that should be addressed before the manuscript could be accepted for publication:

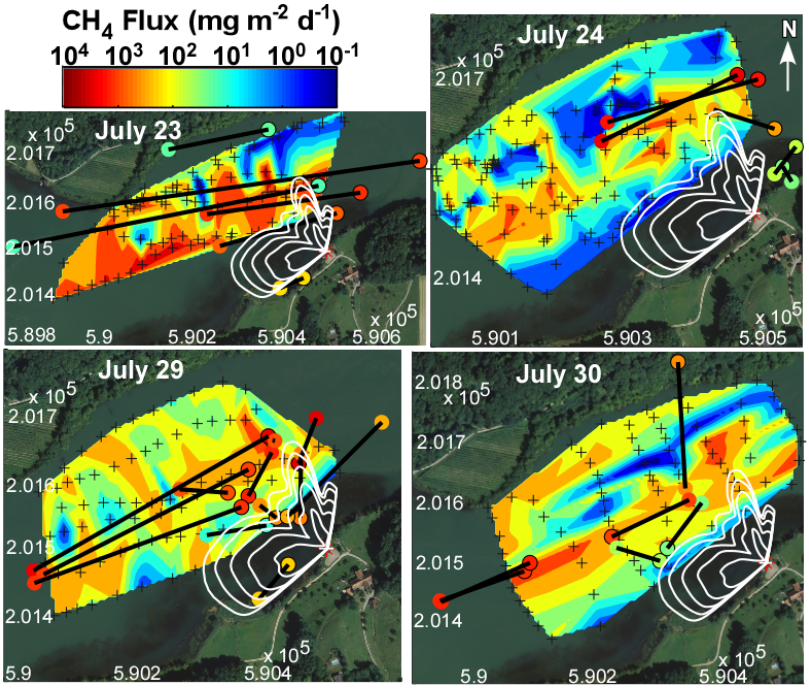
1) CH4 emission are reported, but maybe also CO2 emission are probably very relevant as well, if CO2 fluxes were not measured in this study, an estimate from the literature should be provided to more completely understand the overall GWP of these freshwater areas.

Sebastian Sobek has calculated annual CO_2 emission from alkalinity, pH, temperature, air pressure and wind speed, and arrived at 24 g C m^{-2} yr^{-1}, i.e. much less than annual CH_4 emission. As pH is high (average 8.1), most of the inorganic carbon pool is present as bicarbonate and carbonate, not gaseous CO_2. During the period of our measurements, the pH values in the surface waters entering Lake Wohlen ranged between 8.14 and 8.25 (site AC52, "Eymatt, neuer Steg"; source: Hydrologisches Jahrbuch 2008 des Kantons Bern; downloadable from http://www.wea.bve.be.ch/geoportal/qog/pdf/hydrografisches_jahrbuch_2008.pdf). Similar pH values are found throughout the year and also in other years (minimum pH around 7.7 is typically found in November, and maximum pH of 8.3–8.4 in late spring).

We will add this information and will put it in context with the measured CH_4 fluxes.

2) The authors state that the reason why their chambers measurements were not comparable with the eddy covariance estimate was due to the location of the chamber measurements (outside of the footprint), but did they notice a progressive increase or decrease in CH4 fluxes from the chambers moving from the shore to the centre of the lake? A better description and a figure showing chamber vs eddy covariance measurements should be provided. Also where in the lake these cham- ber measurements were performed? The locations of these measurements should be added in Fig. 6 so that the reader more easily visualizes it. Also, why the near-shore have much different emissions than the centre of the lake? Implement the discussion.

We will add an additional figure showing all chamber deployments from the four dates where chamber flux measurements were carried out (draft figure below):



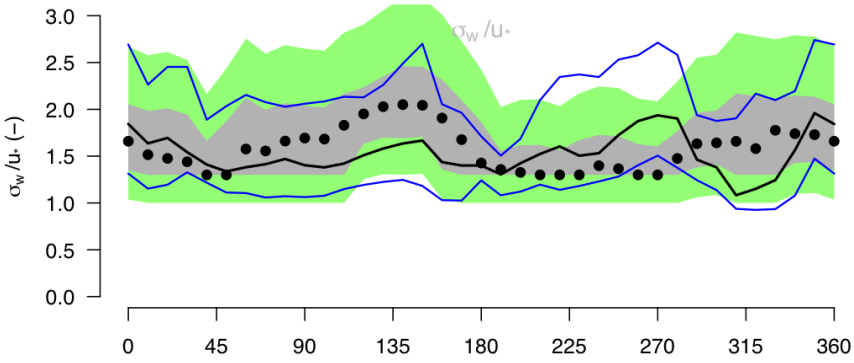
A new figure will be adapted and worked up for our final version and text will be added to indicate that indeed chamber flux measurements were confirming high effluxes where our eddy covariance system picked up higher effluxes as well (northwestern wind direction). Since only very few chambers were near the shore, we will not speculate too much on which is more representative (also since it would be difficult to argue that 4 days of deployment of chambers is more representative than 48 days of eddy flux measurements). The general picture will however become clear that a future study should place the instruments on a float further away from the shoreline.

3) Why emissions are higher when the wind is lower? It seems that the higher wind speed would increase water mixing and gas exchange? From Wanninkhof et al., JGR 1992: "for steady winds, the relationship between gas transfer and wind speed is taken to be k = 0.3 lu2(Sc/660)-1/2 The relationship should be applicable to deduce gas transfer velocities at steady winds [. . .]". Maybe the change in wind speed changed the footprint? Again the chambers measurements across the transect from shore to centre of the lake would help in understanding this result. Also the footprint analysis should be presented in Fig. 6 divide for high and low winds.

The Wanninkhof wind speed dependence is for diffusive flux, not ebullition. Relationships for diffusive fluxes do not directly translate to ebullition fluxes. Therefore, one of our interests was to look at the pressure changes instead, which would be a direct measure for how easily gas bubbles could rise from the lake bottom.

With respect to the footprint model: Kljun et al. (2004) used the Buckingham Π approach and found the following variables to be relevant for the footprint calculations: x, z_m, h, u_*, and σ_w. So horizontal windspeed is no direct driving force, it is however influencing u_* and will influence the footprint via the term σ_w/u_*. We have however not seen a dependence of CH4 fluxes on σ_w/u_* beyond the wind direction dependence that rather indicates that it is the source area where CH_4 is produced and not an atmospheric turbulence that is relevant for atmospheric mixing that leads to high CH_4 fluxes. The figure below shows σ_w/u_* as a function of wind direction. The area with gray and green color with filled circles indicates the range that Thomas Foken expects based on this Integral Turbulence Characteristics (ITC) quality control criterion. Gray shows the area of "highest data quality", green that of "good data quality". The bold black line then shows the median value for each wind direction segment of

our flux measurements, framed by the 25% and 75% quantiles as blue lines. We did not consider this graph for our paper since it basically does not filter out the data from bad sectors.



In fact, in the direction of 135° along the shoreline where shrubs obstruct the turbulence field severely, we only find that measured σ_w/u_* is below Foken's expectation, whereas over the open lake surface (between ~200° and 300°, where we recorded higher wind speeds) it has the tendency to be above Foken's expectation. Unfortunately, however, it also indicates that the ITC test does not really do a great job for assessing flux data quality at our site.

Given the relatively small variability of σ_w/u_* across unobstructed (lake) and obstructed (shrubs) wind directions and the level of simplification of such a footprint model, we do not plan to add two different footprints for high wind and low wind conditions, but we think that the addition of the new figure that was asked for in comment 2 will help readers improve their specific sampling design.

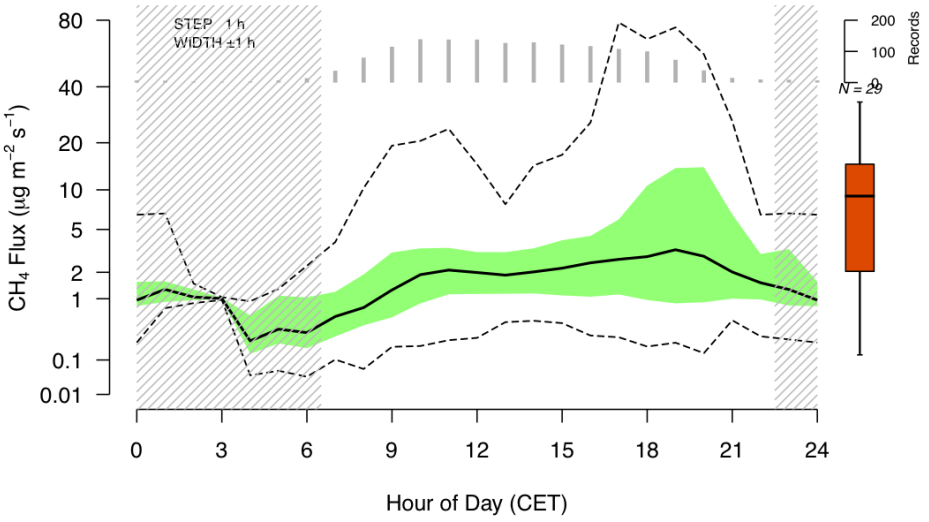
4) Ebullition is a sudden phenomenon; the authors reported nearly continuous higher CH4 concentration from the lake, which is fairly important. More details should be provided: was there a difference between the two measuring periods (June and August)?

With respect to the differences between the two measurement periods we added the sentence: "Methane concentrations and fluxes did not differ significantly between the two periods ($p = 0.7701$ and $p = 0.4651$, respectively; two-sample t-test)."

There was an additional comment by K. Gerilowski concerning the same topic of increased CH_4 concentrations, which will be addressed later in our reply.

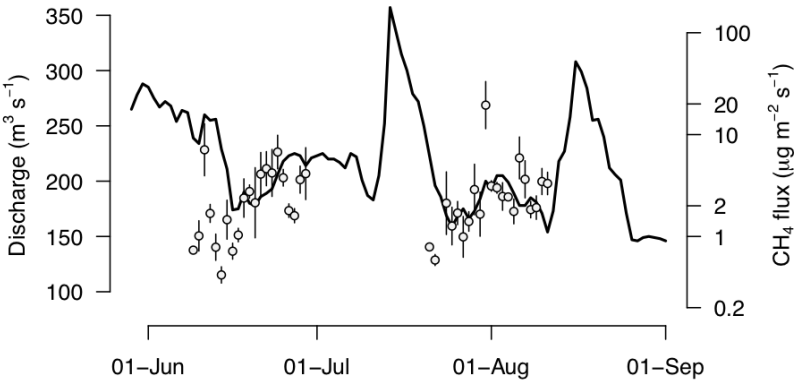
5) Page 5034 lines 5-7: show diurnal cycle in CH4 effluxes;

The diurnal cycle of CH4 effluxes is shown below (again with the range of the chamber flux measurements). The gray shaded areas show the time period where data coverage is poor and values should not be overinterpreted. We can add this figure to our revised manuscript. We will have to add some extra text to explain that lower lake level correlates with higher efflux.



5 cont) Page 5036 lines 10-14: if these are considered to be an important phenomenon the data should be shown.

The point referred to on page 5036, lines 10–14 was considered not conclusive enough and will not be elaborated more in our revision. We however can show you the figure that we had prepared but decided not to use. It is clear to us that a specific field study focussing on POM loads would be required to come up with defendable arguments about this link.



Caption: Comparison of mean daily discharge (Bern Schönau gauging station; solid line, left axis) and daily average CH_4 efflux (daily mean ± SE of log transformed fluxes; symbols and whiskers, right axis).

6) The discussion should be improved: in the results the authors state that ebullition is an important phenomenon but then they say that water temperature is the main driver: ebullition is probably not driven by water temperature, explain this better. Also, the authors state that the extreme fluxes are mainly driven by water temperature but temperature was only able to explain a minor percentage of the variability (up to about 35%); probably this result is important as well (especially if ebullition is an important component of the fluxes) but should be better discussed.

We will improve the discussion with respect to clarifying the role of temperature and ebullition. There is however a direct link that we will have to emphasize: if substrate is not limiting, then methanogens in the anoxic lake sediments will be more productive at higher temperatures than at lower temperatures (Kelly and Chynoweth (1981) Limnol. & Oceanogr.; Nozhevnikova et al. (1997) Water Science Technology; Gudasz et al. (2010) Nature (OC mineralization in general)). We do not have direct measurement of sediment temperature, but the temperature of surface sediments is related to overlying water temperature (Kirillin et al. (2009) Aquat.Ecol; Bryant et al. (2010) Limnol. & Oceanogr.).

7) Page 5037 line 22, Fig. 8 panel (e) is missing

We appologize for this; panel (e) was now added to Fig. 8

8) Page 5039 line 13: this is not true, there has been significant research done on boreal lakes and lakes in Alaska, better revise the literature and compare the rates observed in this study to previous studies.

This relates to the statement "So far CH_4 effluxes from inland waters have been largely ignored by the terrestrial ecosystem flux community", where our opinion obviously differs from that of the reviewer. The first author, having been involved in limnologic work in Alaska (with George Kling) since 1995, and his co-authors feel that so far CH_4 effluxes from inland waters was mostly of interest to limnologic scientists, but not to terrestrial ecosystems scientists (which are not limnologists). From this comment, however, we see that our wording can be misinterpreted – of course there was and is quite some limnologic work going on in the boreal and arctic Alaska, but this is not the point we wanted to make. We think that this

reviewer does not make the same distinction between aquatic ecologists (including the limnologists) and terrestrial ecologists (not including the limnologists) that we make, and hence we will reword this statement for the revised version: "The terrestrial ecosystem flux community has largely ignored CH_4 effluxes from inland waters in terrestrial C budgets; therefore, it is of interest to make a rough estimate of how the CH_4 fluxes from Lake Wohlen relate to typical C uptake rates of the surrounding landscape."

Reviewer #2 (G. Abril)

In the present debate on greenhouse gas budgets of hydropower, this is an important contribution to the topic. As the first eddy covariance measurements of CH4 fluxes in an aquatic system the study is particularly innovative. The dataset is well presented, analysed and validated. These data should be published in BG. However I find the ms much too long, the statistical analysis with physical drivers quiet heavy, difficult to follow and not that convincing: correlations are weak even after log transformation of the CH4 flux.

We agree that with higher correlations it would have been more easy to write the text. We however find it important to address the problems of short timescales and the lack of very clear correlations.

I could not understand Figure 7, is this correlation of the CH4 FLUX with the other variable?

Reviewer #3 also had problems with it, hence we see that there is a need for an easier-to-understand caption. We considered removing Figure 7, but it would complicate the explanation of how lagged correlation analysis works, and hence we decided to keep Figure 7 but will expand the caption to make it more understandable. The reviewer is right: it is correlation with CH_4 flux (which we did not explicitly mention in the caption, but which will need to be done in the revisions, we agree).

How do you analyse the effect of level change, when the eddy fluxes are data coming from water bodies with different depths?

This is a standard correlation analysis in which the effects of all variation covered by the eddy covariance flux footprint at the respective 30-minute averaging interval is included implicitly. While water level changes may impact different parts of the lake differently, the water level change still affects each part of the lake. Since the eddy measurements are basically an integration of several parts of the lake, it is correct to use the overall water level change in the regression.

The fact that ebullition dominates could be demonstrated in a much simpler way if concentrations of CH4 in surface water have been measured, diffusion could be calculated.

The methane dynamics of Lake Wohlen have already been reported by DelSontro et al. (2010). On page 5025, lines 26–27 we wrote: "In the case of Lake Wohlen, the dominant CH4 emission pathway during summer is ebullition (DelSontro et al., 2010)." Obviously we should have added ", which means that diffusive flux is rather small.". Additionally, in the Methods section under Site Description we will reword the text to read "It has been shown in Lake Wohlen that seasonal water temperature changes (from… to …) best described and perhaps influenced the variability in CH4 emissions from the reservoir, of which ebullition was dominant and more variable and diffusive fluxes were low and relatively constant (DelSontro et al. 2010)."

If not, the wind speed and flux data allow calculate the theoretical surface water CH4 concentration necessary for diffusion to account for all the flux. This computed concentration should be unrealistically high in oxic water, proving that ebullition dominates.

The DelSontro et al. (2010) paper covers exactly this aspect and hence we guide the reader interested in this to that other paper.

I agree on the fact that allochtounous POC input alone can fuel the CH4 flux. The MS would gain by strengthening the last part of the discussion, in comparison with other younger reservoirs where the flooded biomass is believed to be the major C source.

> We agree that our manuscript would increase in strength, but, as mentioned in our reply to Reviewer #1's point (5), we did not measure POC/POM specifically to address this point. We are hence convinced that a special study that focuses specifically on this aspect would be needed to come up with more defendable arguments. Given the length of Lake Wohlen, it is not that easy to link the inflow directly to sedimentation rates etc. at the locality closer to the dam where we carried out the flux measurements. We hence suggest that this would be the topic of another future study, which certainly would be of great interest.

Explain what variables affect the calculated footprint

> We added a paragraph to the Methods section: "The flux footprint area was computed with the Kljun et al. (2004) model. This simple parametric model estimates the cross-wind integrated flux footprint area in the upwind direction from the flux tower. The governing variables for flux footprint calculations are the upwind distance x (m), the measurement height above local ground z_m (m), the height of the atmospheric boundary layer h (m), the friction velocity for mechanical turbulence u_* (m s^{-1}), and the square-root of the variance of the vertical wind speed component σ_w (m s^{-1})."

P5021L15-19 Schulze et al 2009 did not include waters but Schulze et al 2010 GCB did

> Thank you for this information. Although it is still difficult to put into context, we added the sentence: "In Schulze et al. (2010) the gross estimate for CH_4 and N_2O emissions from all European surface waters was quantified at 147 Tg CO_2 equivalents per year, which is roughly 10% of all non-CO_2 gas sources considered by Schulze et al. (2010)". Please note that we converted the unconventional CO_2-C equivalents (which is wrong at least for N_2O, but also for CH_4 since radiative forcing is not related to a component in a molecule, but to its geometric structure) to the IPCC-standard CO_2-equivalents here.

Fig2 %time for each wind direction could be added for better understanding

> This will be done; the information is currently only included in Figure 5 (top of figure), but we accept that it would also be helpful in Fig. 2.

Contributor #1 (K. Gerilowski)

I would like to ask, if the authors have paid attention to the fact, that the eddy covariance measurements are performed only ~1000 m from the Teuftal landfill, which potentially is a strong local source for Methane emissions. Organic waste has been stored in that landfill until the year 2000 and perhaps beyond (see also: www.teuftal.ch/Geschichte.html, www.teuftal.ch/Portrait.html).

The landfill is equipped with an outgassing system (see also: www.teuftal.ch/Technik.html) and is not (or only temporally) covered. Nevertheless CH4 outgassing is expected to decline, the outgassing system will be active till 2030. It is known from literature, that similar types of landfills can produce emissions up to several kilotons of CH4/Yr (see also: http://prtr.ec.europa.eu).

> The considerations about the Teuftal landfill site is of course a valuable input. We know the site, but we were under the impression that the gas produced by the depony is used by the gas power plant of BKW, the Bernese company producing electrical energy. This plant started its operation in 1989 (see http://www.teuftal.ch/Geschichte.html) and was expanded from 1800 kW to 2700 kW in 1993. This document also reports that the production of methane gas by 2000 has declined, and so did the productivity of the electrical power plant. Finally, in 2007 (the year before we performed our measurements over Lake Wohlen) the power plant was

replaced by a much smaller 180 kW combined power plant (electricity and heating) as an adaptation to the much lower methane gas production of the site.

As the authors themselves mentioned that "Methane emissions from the lake (and from other potential sources in the valley) are strongly contained in the atmospheric boundary layer above the lake surface due to the relatively cold surface water (Fig. 3a; summer maximum 20 C), which limits convection during daytime, but enhances turbulent mixing during nighttime." my question is, how the authors can exclude interference of the eddy covariance measurements by the nearby landfill methane emissions. Can such interference be also a possible explanation for the effect, that higher CH4 fluxes are observed at lower wind speeds (see also comments of reviewer #1) ?

As we mentioned in the author comment (and as Reviewer #3 explicitly confirmed) the footprint area of mean quantities (CH4 concentration) is much larger than the footprint area of fluxes. As proposed in our author comment we have made an attempt to test Gerilowski's hypothesis that the high CH4 concentrations could potentially be from the landfill site and not from the lake. To do so, we made the following considerations: a flux (Fc) and a concentration (c) can be related via an emission velocity (v_e), similar to the established concept for deposition of trace gases where a deposition velocity (v_d) is used instead, with

$$Fc = -v_d\, c$$

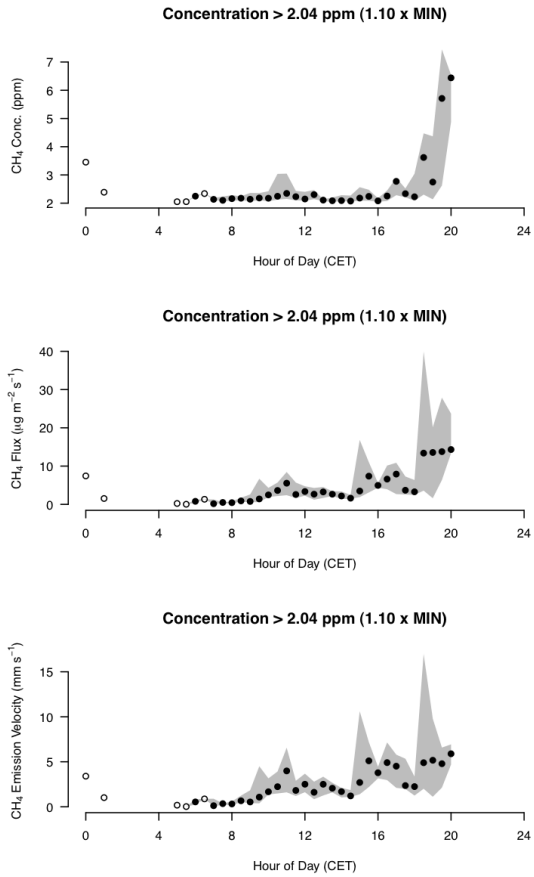
for deposition, and here consequently

$$c = Fc/v_e \qquad \text{or} \qquad v_e = Fc/c$$

in case that c would primarily be a function of local methane emissions Fc. Hence if a high concentration is found during times with low Fc, we would argue that c is not primarily goverened by local emissions from the lake but via advective influences, e.g. from a nearby landfill site. Or in other words, if we take the ratio Fc/c as an estimate for v_e, we would argue that v_e would need to be a small value if c is not increasing due to Fc from the lake, whereas a larger value should result if Fc and c are more closely related (and hence advective influences are less important).

For this we produced a series of graphs of which we include one representative example below. Each graph has 3 panels which show the diurnal course of the CH_4 concentration (c, top panel), the CH_4 flux (Fc, center panel; note that here we used the measured values, not the log transformed) and, the ratio between CH_4 flux and CH_4 concentration, which we labeled "CH_4 Emission velocity" (v_e, bottom panel; note that we converted ppm values to µg CH_4/m^3 and at the end multiplied the ratio by 1000 to yield millimeters for the emission velocity). We produced graphs with a selection of data that exceed the minimum CH_4 concentration in our data set by a factor 1.0, 1.01, 1.05, 1.1, 1.2, 1.3, 1.4, 1.5, 2, 3, 4, 5 (this information is found in the title of each panel). It is clear that the larger the factor the smaller the number of remaining records that are available. We combined our half-hourly fluxes into hourly fluxes and show the median value of each hour with data with an open symbol if one single record was available only, and with a closed symbol if at least 2 records were available. Each gray band shows the interquartile range of the values.

In this way we should be able to test the relationship between Fc and c as described above. The pattern seen in v_e (lowest panel) is basically in agreement with what we expect if c is primarily influenced by Fc from the lake (and not by advection from other sources such as e.g. a nearby landfill site). So we are confident that methane concentrations measured in the air above the lake are primarily governed by effluxes from the lake and not by nocturnal drainage flow from a landfill site (in that alternative case we would see low v_e with high c in the late afternoon and early evening).



Example graph for cases with CH_4 concentrations 10% or more above minimum concentration.

In these graphs we see a steady increase of the emission velocity over the day, which we attributed to the typical diurnal cycle of water level as a result of hydropower operations. Even after 16 hours when CH4 concentrations tend to increase exponentially, there does not seem to be a discontinuity in emission velocities. A similar picture is found in all other graphs (see separate PDF file), from which we conclude that we do not see clear evidence that CH4 advection from the Teuftal landfill site is a large contributor to the CH4 concentrations that we measured over the lake.

We'll clarify this point in our revised version.

The general issue of CH4 emissions from the Teuftal site was discussed with Prof. Dr. Josef Zeyer from ETH Zurich, and he confirmed that the political understanding is that all possible measures should have been taken to minimize potential CH4 emissions (namely via power generation of collected methane gas). Under absence of own independent measurements we would not feel confortable to add this to our manuscript, since we fully agree with Dr. Gerilowski that there is a potential for gaseous CH4 losses from that site. At the same time we maintain that this does not influence our flux measurements from the lake, and that we do not have clear evidence that it has a strong influence on the concentration measurements at our site either (we however do not say that it has no influence, to be precise).

Note that it is well known, that very high atmospheric CH4 concentration values of up to some ppm (as reported in Fig. 4) can often be observed over and downwind of landfills (see also: A. Babilotte, Field Comparison of Methods for Landfill Fugitive Methane Emission Measurements, Veolia Final Report, Convention ADEME 07-74-C0034, 2008, www2.ademe.fr/servlet/getBin?name=103DFE1EB5CAB99FC11CE0008D08F6E51274800491082.pdf, and reviewer comments

therein). My question to the authors is, if such concentrations
have also been observed over and downwind of other hydropower reservoirs ?

> We are not aware of any continuous concentration measurements in the air above such a reservoir that would qualify for such a comparison. However, in the meantime we have started eddy covariance flux measurements over another hydropower reservoir (Klöntalersee) and also find a clear diurnal cycle of CH4 concentrations, but with a different timing, which will need to be investigated. Maximum CH4 concentrations are however never as high as at Lake Wohlen (Klöntalersee is also a much colder reservoir at higher altitude), and it will be of interest to us to find out whether this is related to the cold water temperatures, the lower CH4 flux or any other confounding influence that we currently cannot think of.

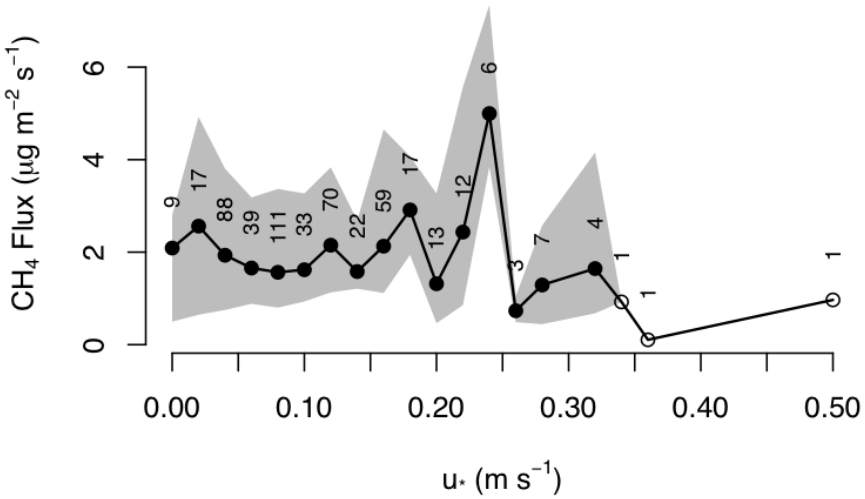
Reviewer #3 (T. Vesala)

The paper reports the first direct eddy covariance measurements of CH4 from a freshwater ecosystem, and as such it is very valuable and important contribution, to be publiched in BGD. Technically the paper is in quite good shape but the following points should be concerned before the publication.

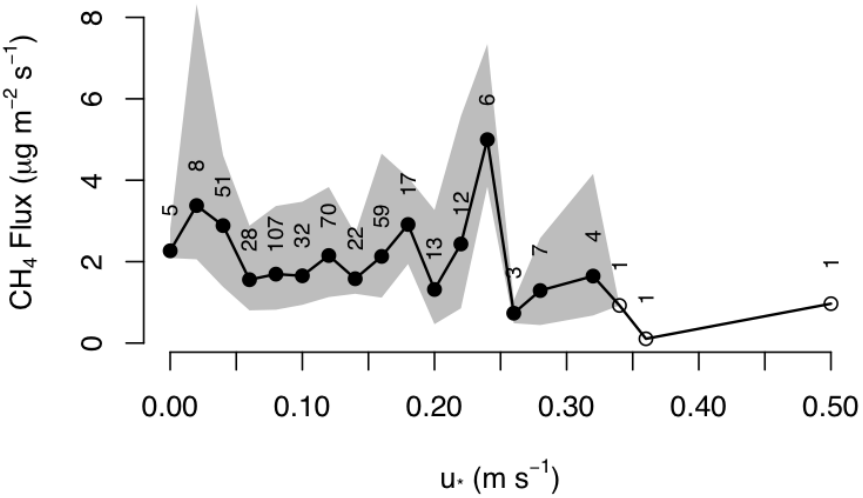
More Major:

1. p. 5026, l. 20-21: In the case of lakes, could u* filtering approach still be used but it is not sufficient and some extra filtering criteria are required? Or is it so that u* is misleading, irrelevant or whatever and if yes, why?

> The graph below shows the u_* dependency of the measured fluxes from the lake. Data were grouped in 0.2 m/s classes, and the gray band shows the interquartile range, with the median as a circle. The numbers show the number of records in each statistical band.



> In the low u_* range there is no decline of fluxes compared to higher u_*, and at u_* above 0.2 m/s the number of records is so small that it would be arbitrary to filter off these values.
>
> We know that this reviewer was a coauthor of the Gu et al. paper and that our opinion on this issue may differ. Here we just wanted to indicate to the reader that they should not expect the u_* filter to solve any issues. In fact, it may be just an error: if we produce the same graph above but exclude the cases with local free convection ($z/L < -1$) where u_* vanishes and is no longer a relevant scaling parameter, we actually find higher fluxes at low $u_* < 0.05$ m/s:



2. I am not sure whether I understood correctly the point of mislocated chambers and posterior computations of footprints. I understand that footprints were calculated much afterwards and then realised that the best guess for chamber locations were in fact wrong. Well, nobody is perfect. However, I would like to ask few things on footprints. How are the footprints estimated for SW direction, along the shore? The footprint has always some cross-wind range including heterogeneity of water-land area and the model by Kljun et al. cannot take that account (only Andrey Sogachev's model if any).
In addition, the footprints are weighted by the respective effluxes so they are not classical footprints by definition but show the footprint/flux climatology. This should be stressed.

> You are right – originally there was no objective to run the chambers exactly across the eddy footprint, also because we did not know the size of the footprint before the measurements were made, and we did not have sufficient knowledge on spatial variability as we now have. We think that adding the new figure according to the suggestion of Reviewer #1 helps to clarify this point.
>
> The Kljun model is a crosswind integrated model approach which does not explicitly include small-scale heterogenities in the lateral direction of the prevailing wind.
>
> As we explained in our reply to Reviewer #1's point 3 the Kljun footprint model uses σ_w/u_* as one of the independent variable groups. In our case this variable shows less directional variation than what we found when the airflow was from the open water (namely NW wind directions), and hence the footprint calculations do not dramatically change. This may be an oversimplification of the Kljun model, and it would be nice if you would be interested in using our data set with the Sogachev model, we'll be happy to make all data available to you.
>
> In our revised manuscript we will add a sentence to clarify why we used weighted footprints after line 2 on page 5036: "Note that we weighted the footprint calculations to show more clearly where the large fluxes come from, an information that is normally not included in traditional footprint displays."

3. p. 5037, l. 13-15: The relation to the surrounding ecosystems is discussed in Conclusions and not in Discussion, although lines 13-15 refer to Discussion. Indeed, it should be in Discussion and Conclusions should be totally rewritten. It should reflect the whole paper and not only the surrounding ecosystem analysis.

> We agree with the reviewer and will reorganize this part of the Discussion and rewrite the Conclusions.

Minor:

1. Abstract: the accuracy of the values may be too big, for example, I would replace 3.76 +- 0.39 (line 11) by 3.8 +- 0.4.

Done.

2. Abstract: It would be good to explain what is meant by "larger terrestrial area". Mention that it is the European-scale compilation of grasslands, croplands and forests.

Done.

3. p. 5024, l. 12: explain how was the ebullition detected. As the sampling site was located on the shore, how well does it represent the reservoir overall?

We added the text "in the form of clusters of bubbles rising in the water column and dissipating at the surface" to further clarify how visible detection of ebullition works.

With the addition of the figure that Reviewer #1 asked for we think that the spatial variability becomes more clear. Given the large day-to-day variability in the spatial pattern detected by chamber deployments it appears unlikely that there is a strong along-reservoir gradient that would question the representativity of our flux measurements.

4. It would be good to introduce/mention the sand box in Site description and define exactly its location. In my opinion, it would be good to have a map of the area showing the reservoir, some part of the land, EC location, chamber locations and the sand box. Instead of a new figure, this information could be implemented also to Fig. 6.

We will draw the outline of the sandbox in Fig. 6, place a capital "S" in its center and refer to it explicitly in the caption. The point about the chamber deployments will be addressed with the additional figure that Reviewer #1 asked for.

5. It would be good to stress in Introduction or in Flux data processing that not only wind directions representing the reservoir but all directions are analysed and included.

We added the following statement at the end of the introduction: "In addition to the fluxes from the hydropower reservoir we will also present contrasting CH4 fluxes from the surrounding landscape for conditions when the wind was not blowing over the water surface."

6. p. 5031, l. 9-12: the information given is important but I would not include under Section 3.2. This sentence could be put together with Fig. 4, maybe on Line 21 on the same page. In addition, when Fig. 4 is introduced, only Fig. 3a from Fig. 3 was introduced/explained. If the sentence is moved, as I am suggesting, maybe the order if Figs. 3 and 4 should changed.

Text was moved accordingly and then the former Figs. 3 and 4 were swapped to correct for the chronology of figures in the revised text.

7. The comparison of chambers and EC is interesting. I would mention something on that and on the result in Abstract.

We added the following sentence to the abstract: "Floating chamber fluxes from four selected days confirmed such high fluxes with 7.4±1.3 µg C m^{-2} s^{-1}." (note that by doing so we found a typo that we corrected: the 7.4 µg C are not µg CH_4 as our previous version showed; this was corrected in the text and in the abstract).

8. Fig. 7 is difficult to follow. The location and meaning of each 3 arrows should be explained in the main text. Why is one arrow located to the residual (water level) curve? I was thinking that they should be associated with mean curves.

This reviewer had the same trouble with Figure 7 as Reviewer #2, so there is a need for improvement. We will modify the caption to make it easier to understand, and we will explain

the meaning of the arrows more carefully in the main text. In order to not overload the graph, we only put arrows for the three most important curves, and one of them is a residual curve as described in the text. We now see that we were not clear enough and will work on this aspect in our revisions.

9. Fig 8 (e) is missing.

We appologize for this; panel (e) was now added to Fig. 8

Other:

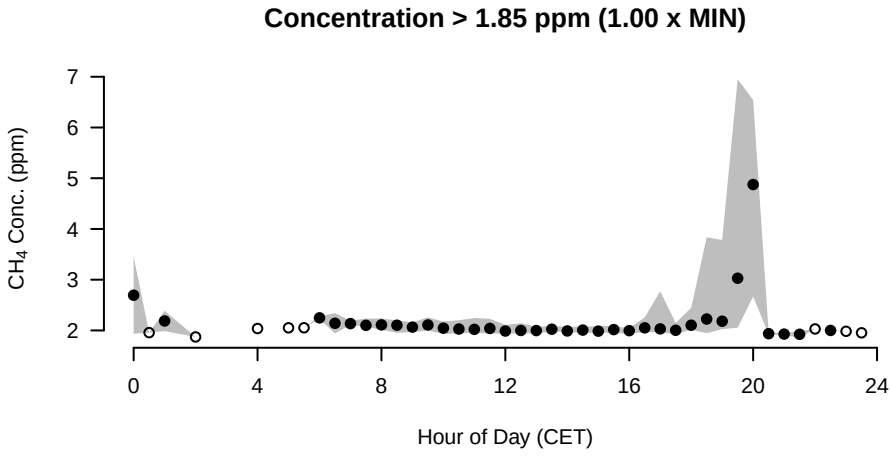
I noticed the comment from Gerilowski and the reply from Eugster. Some flux data from a landfill, as asked by Eugster, can be found in "Lohila, A., T. Laurila, J.-P. Tuovinen, M. Aurela, J. Hatakka, T. Thum, M. Pihlatie, J. Rinne and T. Vesala: Micrometeorological measurements of methane and carbon dioxide fluxes at a municipal landfill. Environ. Sci. and Technol. 41, 2717-2722, 2007".

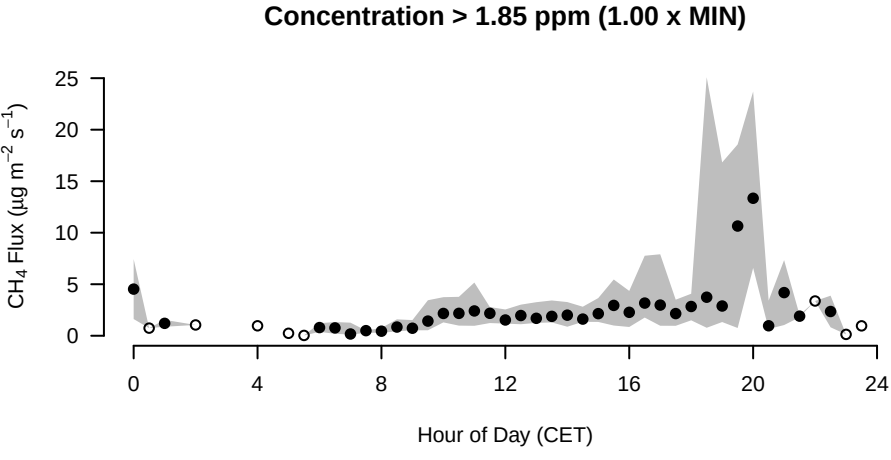
In addition, the concentration footprints are typically much larger than flux footprints, as pointed out by Eugster, see e.g. "Vesala, T., N. Kljun, Ü. Rannik, J. Rinne, A. Sogachev, T. Markkanen, K. Sabelfeld, Th. Foken and M.Y. Leclerc: Flux and concentration footprint modelling: State of the art. Environmental Pollution 152, 653- 666, 2008" for an explanation.

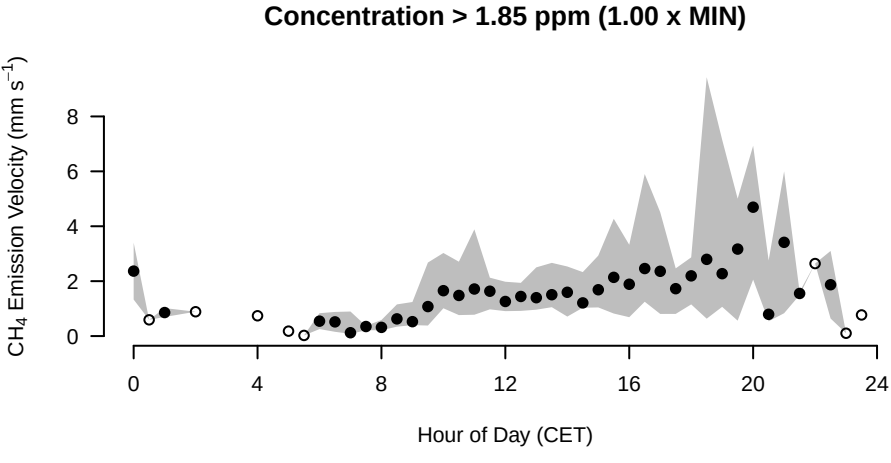
Thank you for this support. We have now added an additional paragraph in the Discussions: "In parallel with high EC fluxes, the CH4 concentration in the air above the lake was often surprisingly high. It is not entirely impossible that some methane leading to high concentrations over the lake actually might stem from the nearby landfill Teuftal. However, the ratio between CH4 efflux from the lake and CH4 concentration in the air above is suggesting a rather consistent emission velocity v_e around 5 mm s^{-1} (median value) during the hours of day with highest concentrations and effluxes (18–20 hours CET), compared to $v_e < 3$ mm s^{-1} during morning hours with moderate fluxes and concentrations. Since footprint areas of turbulent fluxes are typically almost one order of magnitude smaller than footprint areas of concentrations (Schmid 1994; see also Vesala et al. 2008), we would have expected lowest – not highest – v_e during periods with highest CH4 concentrations if these high concentrations had been caused by off-site effluxes from a landfill outside our flux footprint area shown in Figure 6."

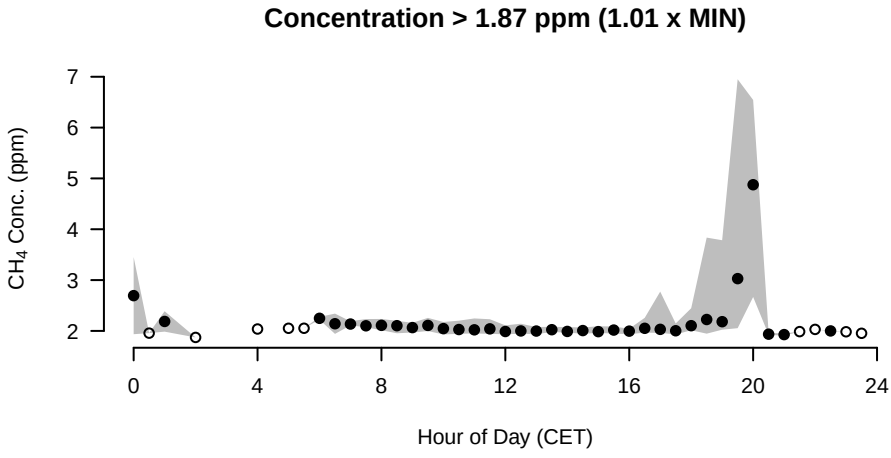
We thank all reviewers and the contributor again for their help in improving and strengthening our manuscript.

On behalf of all coauthors: Werner Eugster


Concentration > 1.85 ppm (1.00 x MIN)
CH4 Conc. (ppm)
7
6
5
4
3
2
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 1.85 ppm (1.00 x MIN)
CH4 Flux (µg m−2 s−1)
25
20
15
10
5
0
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 1.85 ppm (1.00 x MIN)
CH4 Emission Velocity (mm s−1)
8
6
4
2
0
0
4
8
12
16
20
24
Hour of Day (CET)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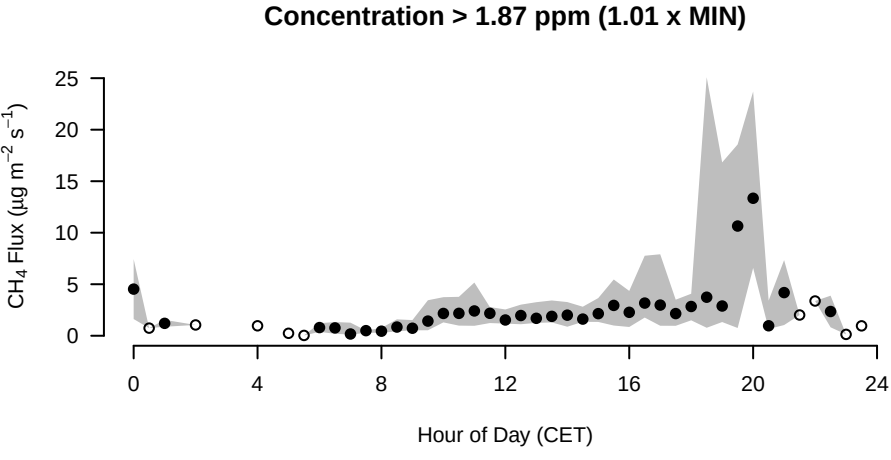
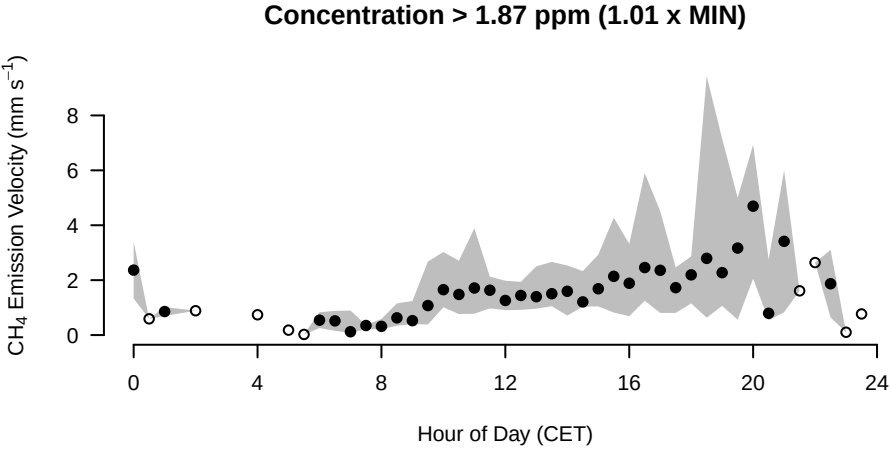
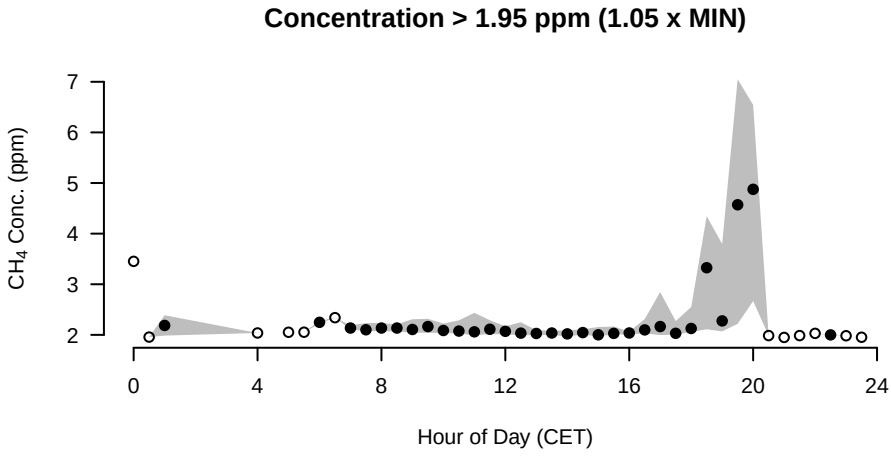
CH4 Conc. (ppm)
7
6
5
4
3
2
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 1.87 ppm (1.01 x MIN)
CH4 Flux (µg m−2 s−1)
25
20
15
10
5
0
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 1.87 ppm (1.01 x MIN)
CH4 Emission Velocity (mm s−1)
8
6
4
2
0
0
4
8
12
16
20
24
Hour of Day (CET)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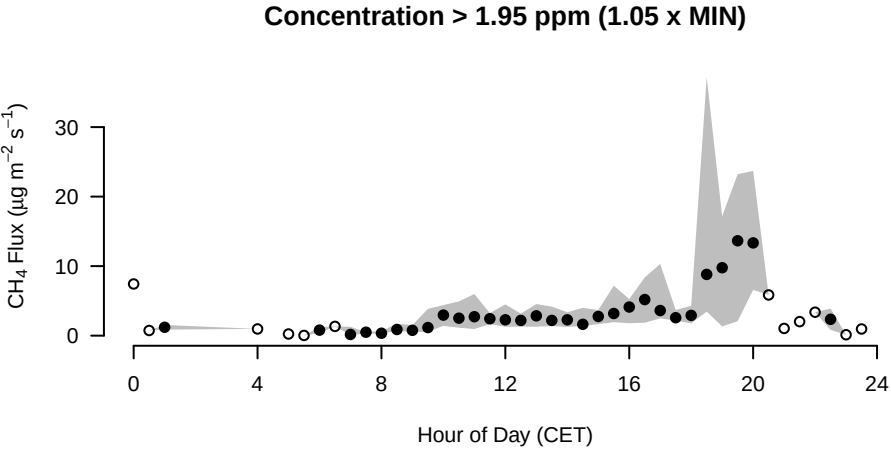
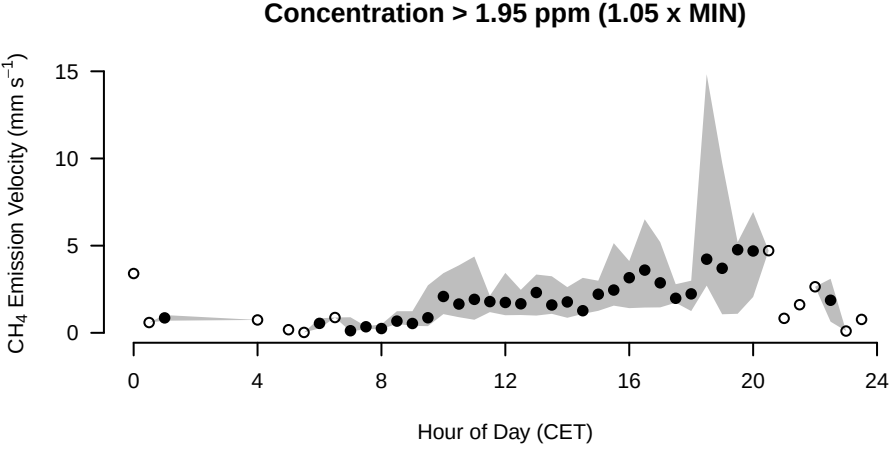
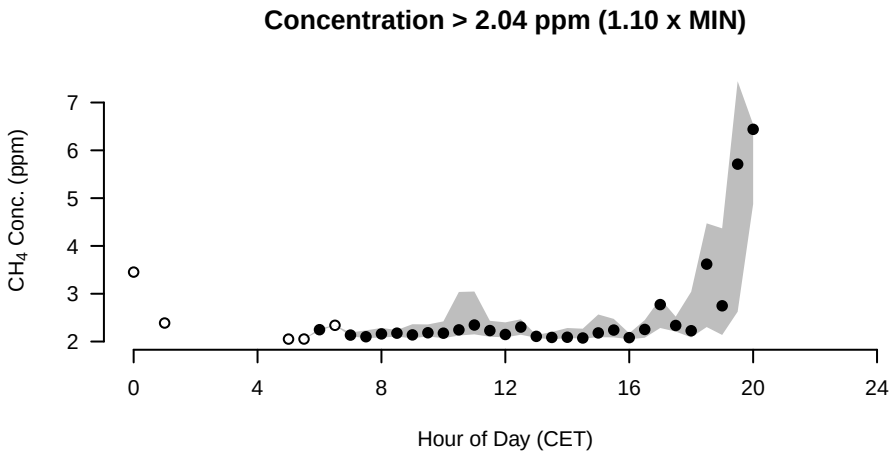
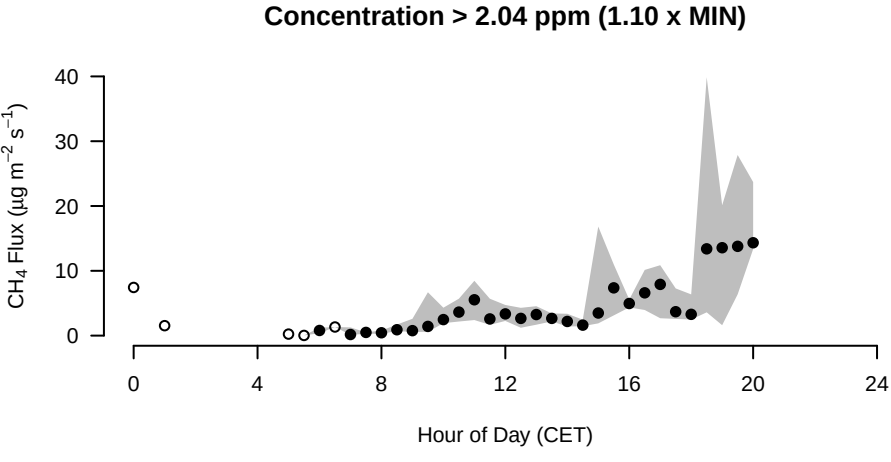
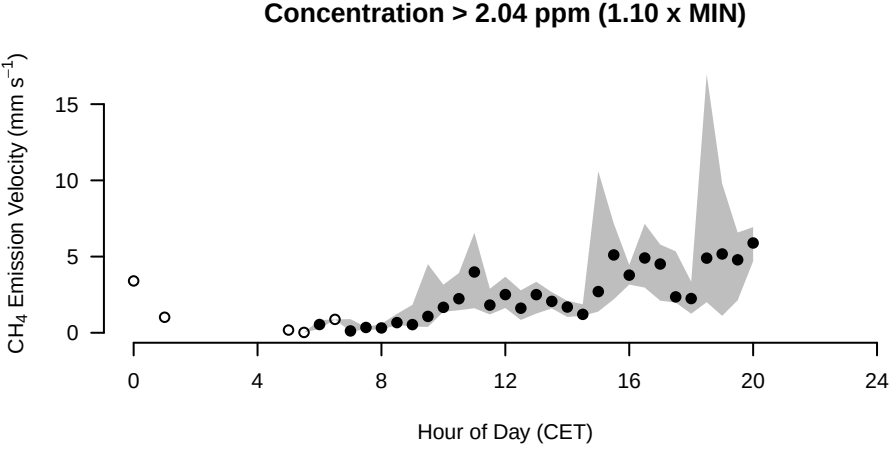
CH4 Conc. (ppm)
7
6
5
4
3
2
0
4
8
12
16
20
24
Hour of Day (CET)

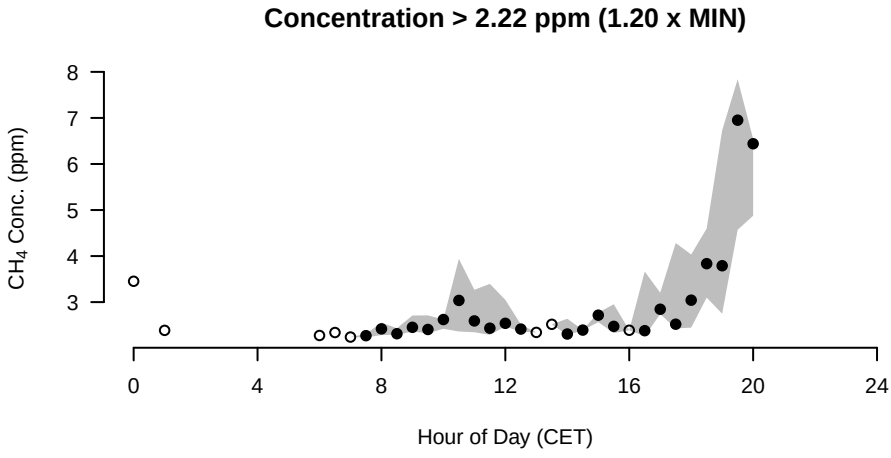

Concentration > 1.95 ppm (1.05 x MIN)
CH4 Flux (µg m−2 s−1)
30
20
10
0
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 1.95 ppm (1.05 x MIN)
CH4 Emission Velocity (mm s−1)
15
10
5
0
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 2.04 ppm (1.10 x MIN)
CH4 Conc. (ppm)
2
3
4
5
6
7
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 2.04 ppm (1.10 x MIN)
CH4 Flux (µg m−2 s−1)
0
10
20
30
40
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 2.04 ppm (1.10 x MIN)
CH4 Emission Velocity (mm s−1)
0
5
10
15
0
4
8
12
16
20
24
Hour of Day (CET)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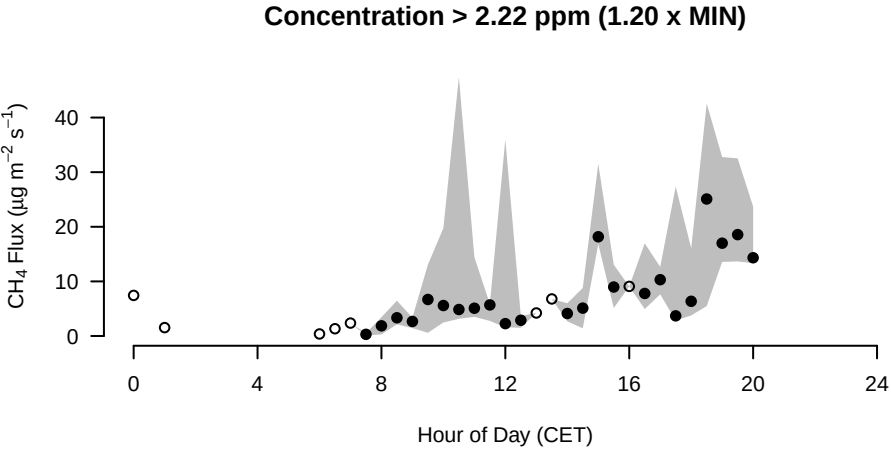
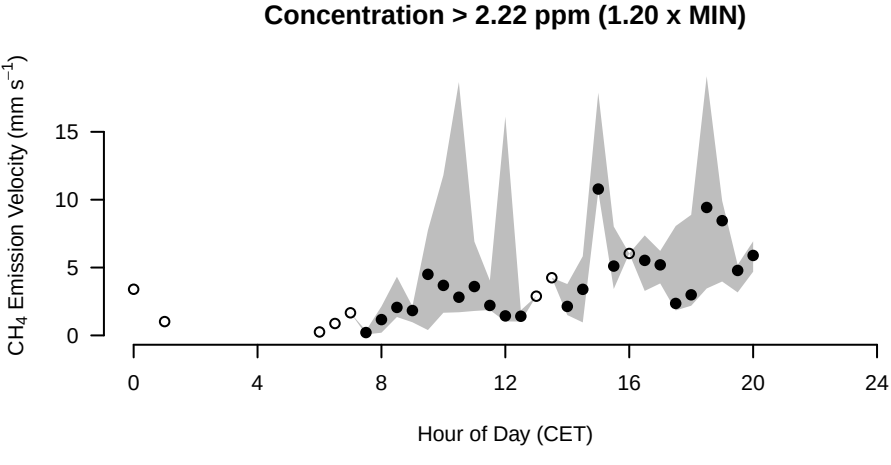
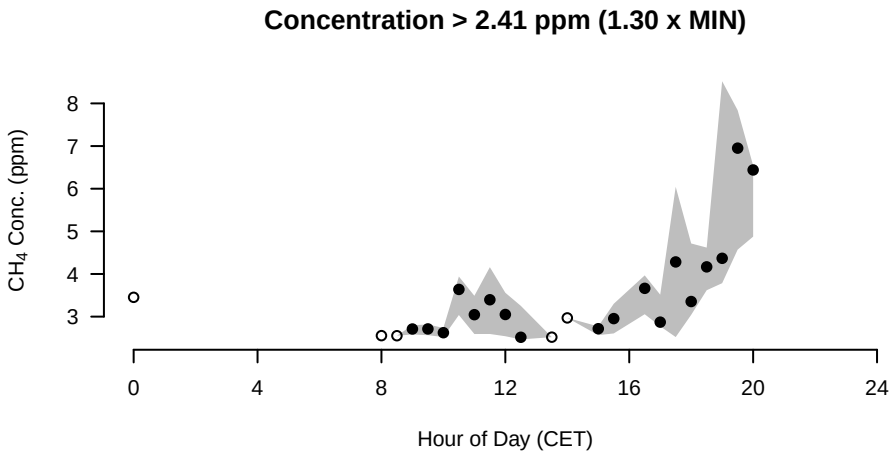
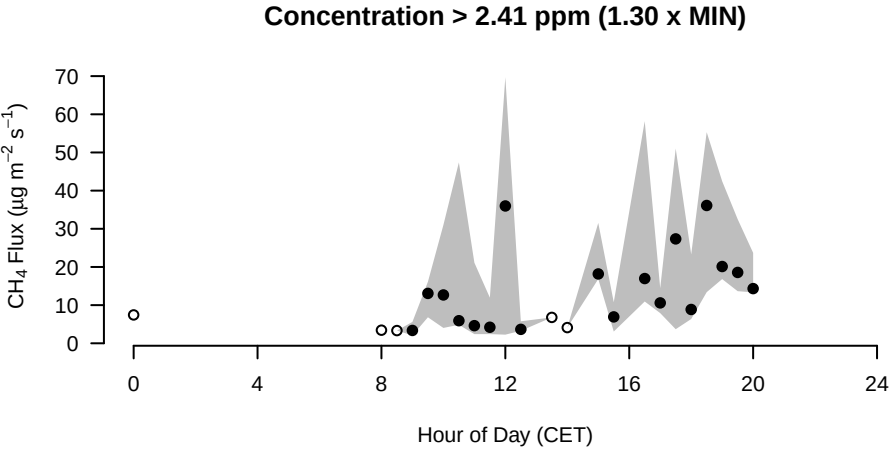
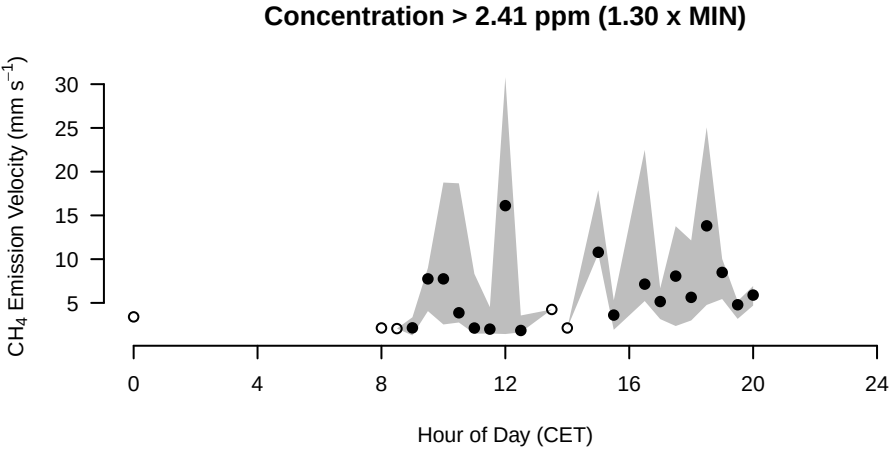
CH4 Conc. (ppm)
8
7
6
5
4
3
0
4
8
12
16
20
24
Hour of Day (CET)

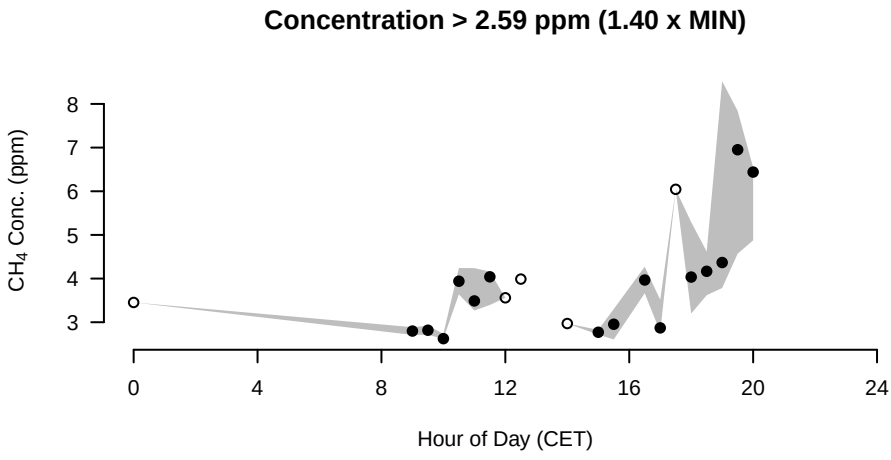

Concentration > 2.22 ppm (1.20 x MIN)
CH4 Flux (µg m−2 s−1)
40
30
20
10
0
0
4
8
12
16
20
24
Hour of Day (CET)

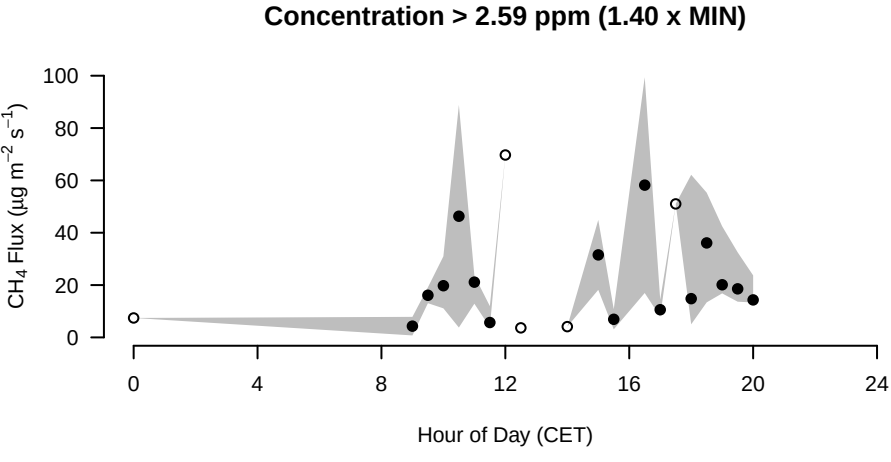

Concentration > 2.22 ppm (1.20 x MIN)
CH4 Emission Velocity (mm s−1)
15
10
5
0
0
4
8
12
16
20
24
Hour of Day (CET)

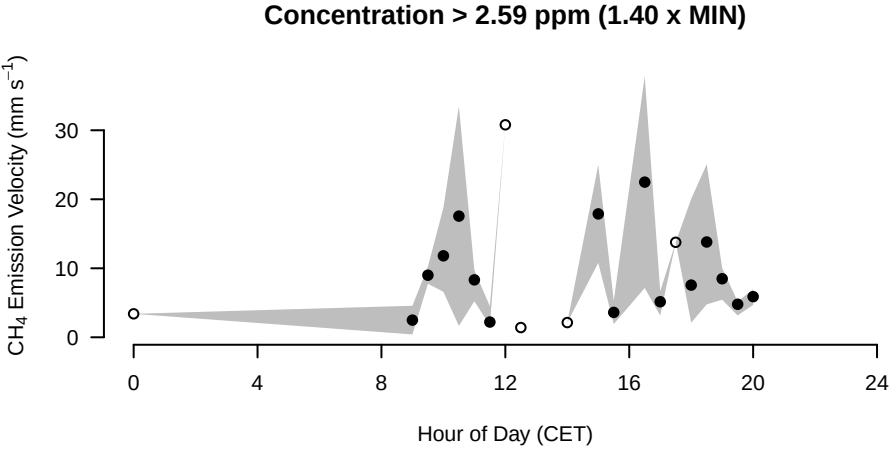

Concentration > 2.41 ppm (1.30 x MIN)
CH4 Conc. (ppm)
8
7
6
5
4
3
0
4
8
12
16
20
24
Hour of Day (CET)

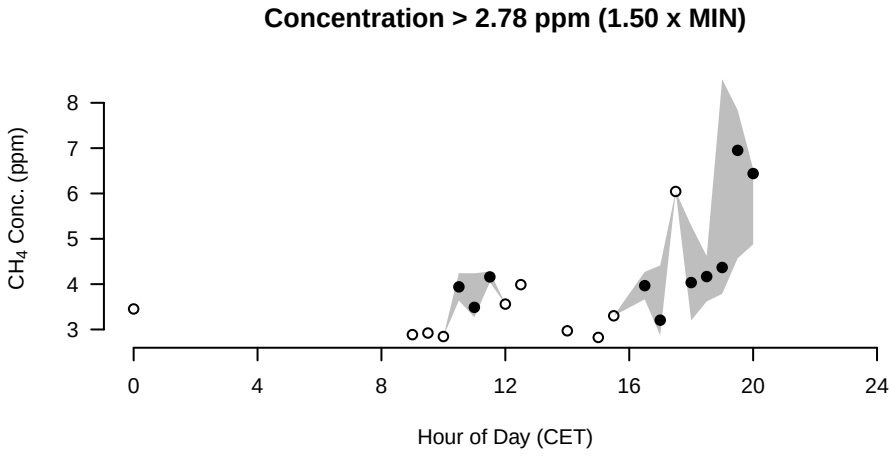

Concentration > 2.41 ppm (1.30 x MIN)
CH4 Flux (µg m−2 s−1)
70
60
50
40
30
20
10
0
0
4
8
12
16
20
24
Hour of Day (CET)

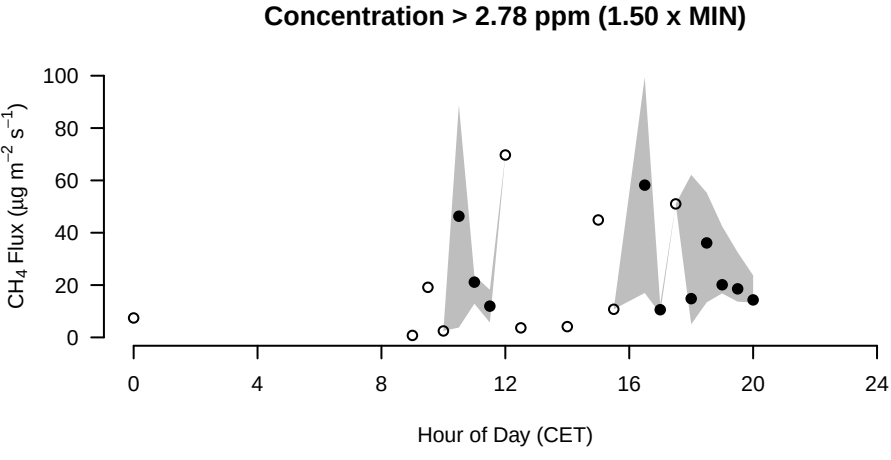

Concentration > 2.41 ppm (1.30 x MIN)
CH4 Emission Velocity (mm s−1)
30
25
20
15
10
5
0
4
8
12
16
20
24
Hour of Day (CET)

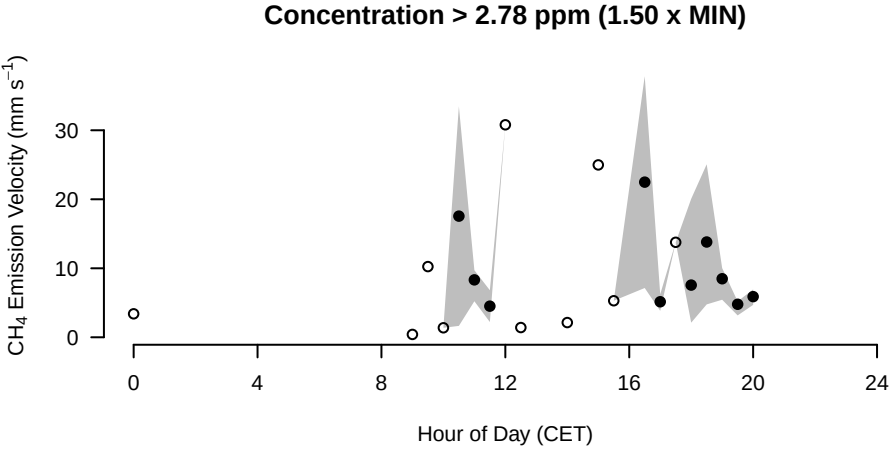

Concentration > 2.59 ppm (1.40 x MIN)
CH4 Conc. (ppm)
8
7
6
5
4
3
0
4
8
12
16
20
24
Hour of Day (CET)

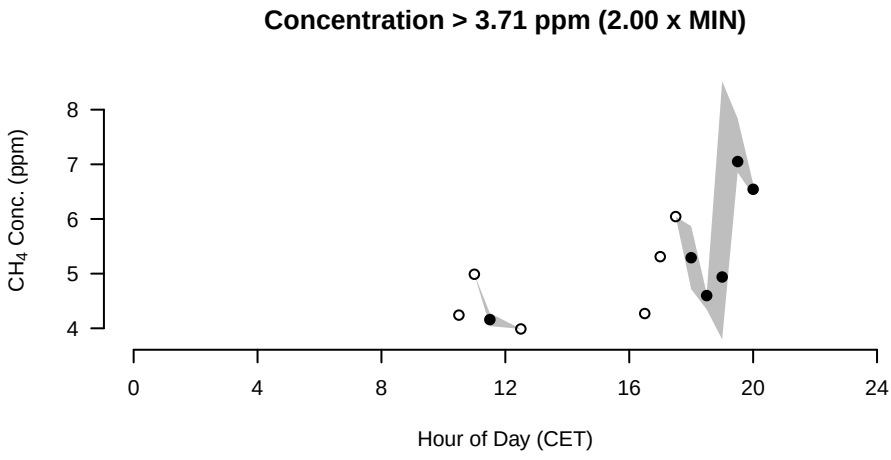

Concentration > 2.59 ppm (1.40 x MIN)
CH4 Flux (µg m−2 s−1)
100
80
60
40
20
0
0
4
8
12
16
20
24
Hour of Day (CET)

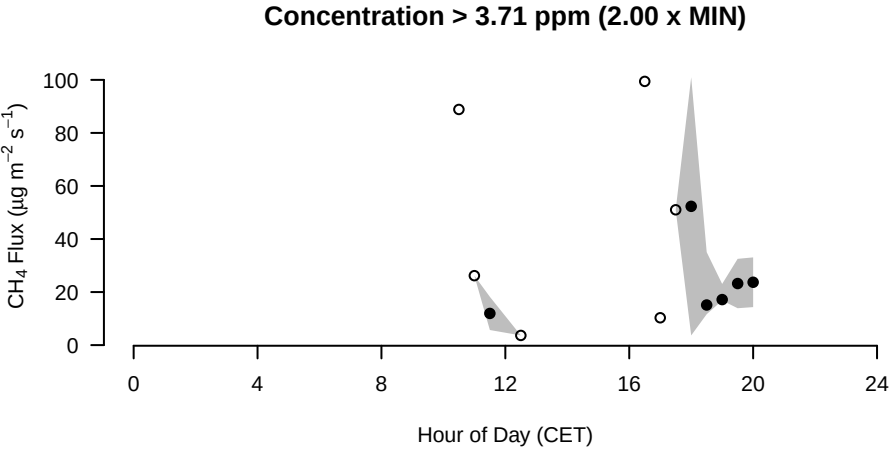

Concentration > 2.59 ppm (1.40 x MIN)
CH4 Emission Velocity (mm s−1)
30
20
10
0
0
4
8
12
16
20
24
Hour of Day (CET)

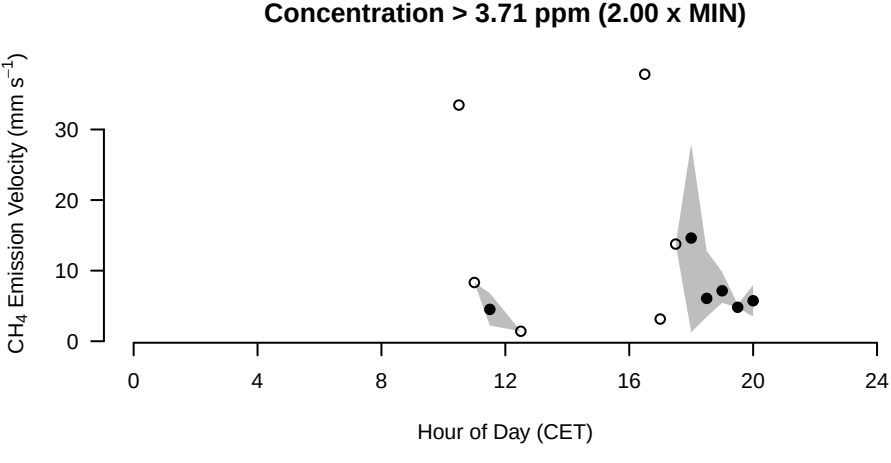

Concentration > 2.78 ppm (1.50 x MIN)
CH4 Conc. (ppm)
3
4
5
6
7
8
0
4
8
12
16
20
24
Hour of Day (CET)

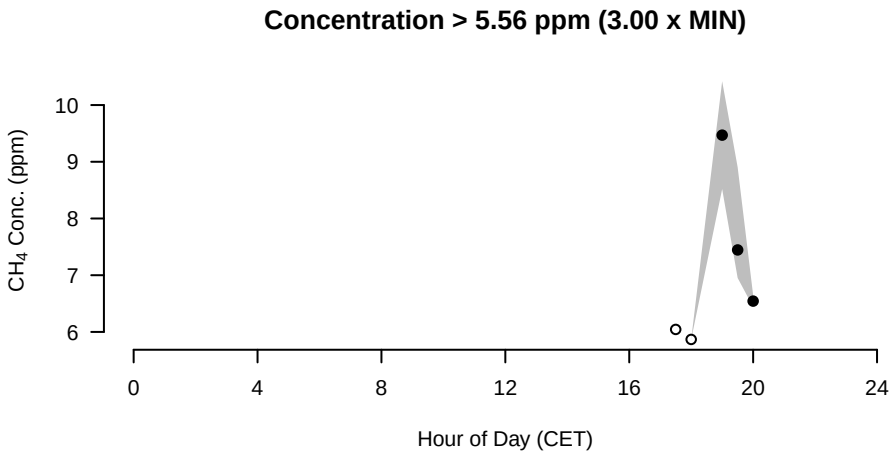

Concentration > 2.78 ppm (1.50 x MIN)
CH4 Flux (µg m−2 s−1)
0
20
40
60
80
100
0
4
8
12
16
20
24
Hour of Day (CET)

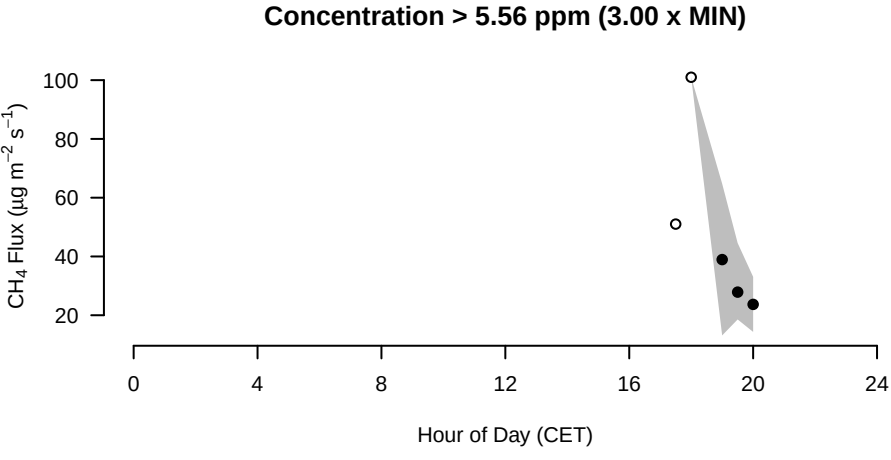

Concentration > 2.78 ppm (1.50 x MIN)
CH4 Emission Velocity (mm s−1)
0
10
20
30
0
4
8
12
16
20
24
Hour of Day (CET)

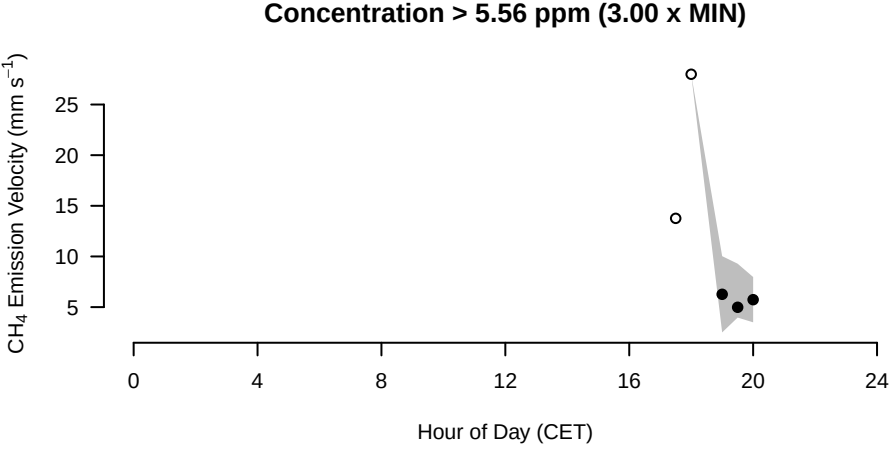

Concentration > 3.71 ppm (2.00 x MIN)
CH4 Conc. (ppm)
8
7
6
5
4
0
4
8
12
16
20
24
Hour of Day (CET)

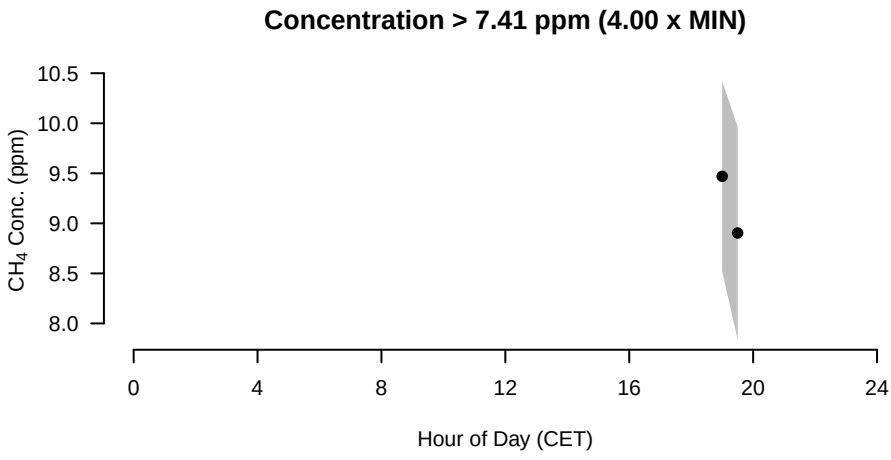

Concentration > 3.71 ppm (2.00 x MIN)
CH4 Flux (µg m−2 s−1)
100
80
60
40
20
0
0
4
8
12
16
20
24
Hour of Day (CET)

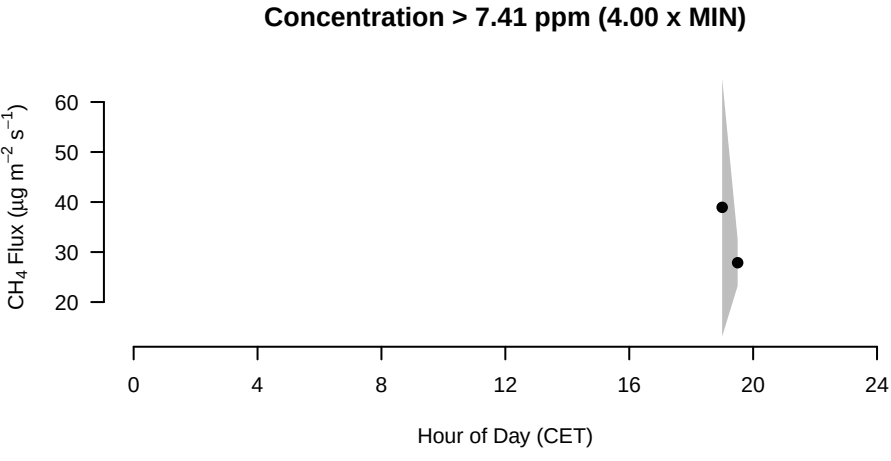

Concentration > 3.71 ppm (2.00 x MIN)
CH4 Emission Velocity (mm s−1)
30
20
10
0
0
4
8
12
16
20
24
Hour of Day (CET)

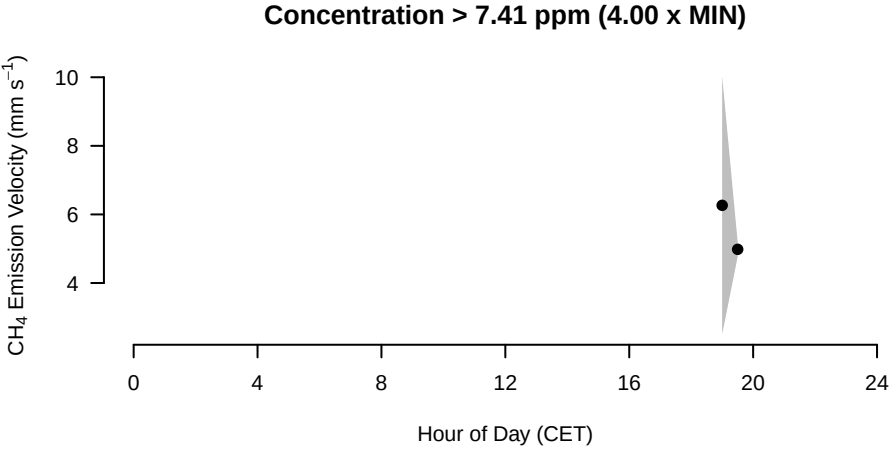

Concentration > 5.56 ppm (3.00 x MIN)
CH4 Conc. (ppm)
10
9
8
7
6
0
4
8
12
16
20
24
Hour of Day (CET)

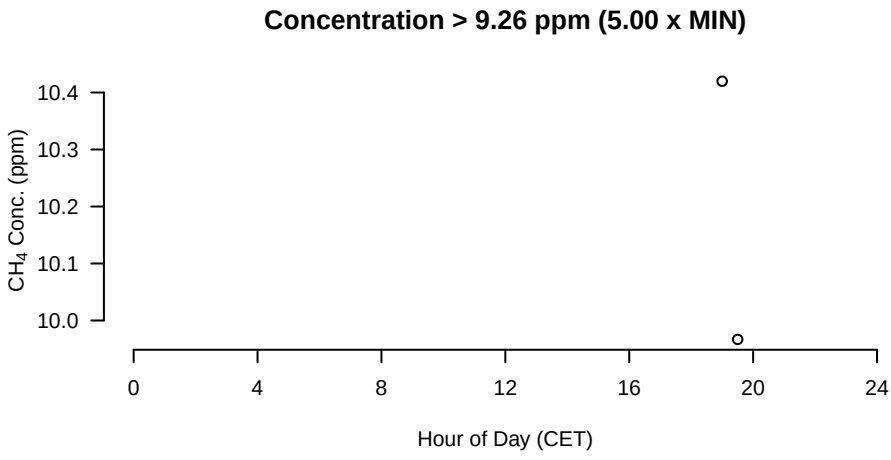

Concentration > 5.56 ppm (3.00 x MIN)
CH4 Flux (µg m−2 s−1)
100
80
60
40
20
0
4
8
12
16
20
24
Hour of Day (CET)

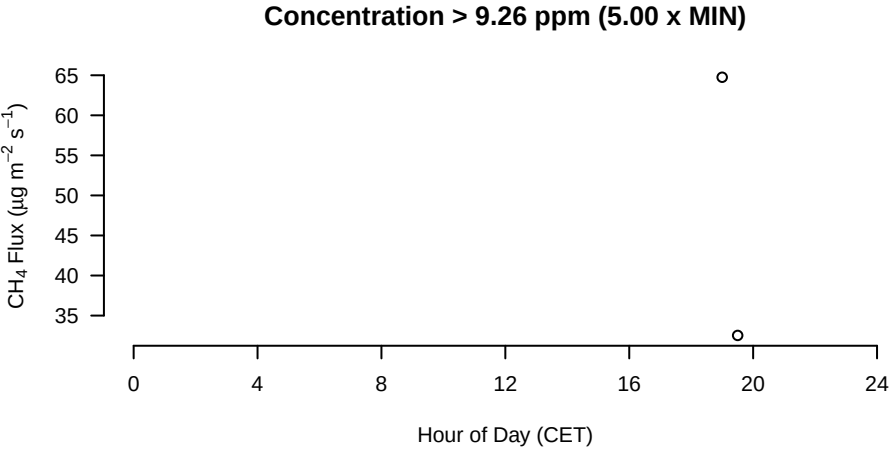

Concentration > 5.56 ppm (3.00 x MIN)
CH4 Emission Velocity (mm s−1)
25
20
15
10
5
0
4
8
12
16
20
24
Hour of Day (CET)

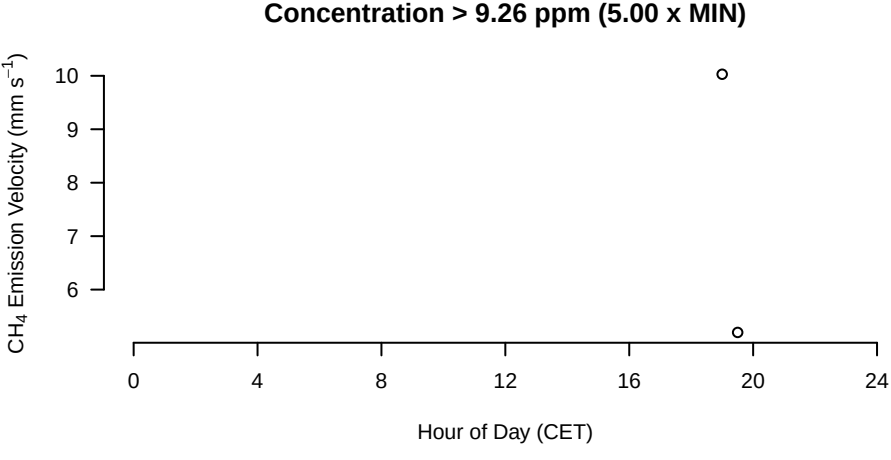

Concentration > 7.41 ppm (4.00 x MIN)
CH4 Conc. (ppm)
10.5
10.0
9.5
9.0
8.5
8.0
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 7.41 ppm (4.00 x MIN)
CH4 Flux (µg m−2 s−1)
60
50
40
30
20
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 7.41 ppm (4.00 x MIN)
CH4 Emission Velocity (mm s−1)
10
8
6
4
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 9.26 ppm (5.00 x MIN)
CH4 Conc. (ppm)
10.4
10.3
10.2
10.1
10.0
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 9.26 ppm (5.00 x MIN)
CH4 Flux (µg m-2 s-1)
65
60
55
50
45
40
35
0
4
8
12
16
20
24
Hour of Day (CET)


Concentration > 9.26 ppm (5.00 x MIN)
CH4 Emission Velocity (mm s-1)
10
9
8
7
6
0
4
8
12
16
20
24
Hour of Day (CET)