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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Characterisation of short-term extreme methane fluxes related to non-turbulent mixing above an Arctic permafrost ecosystem" *by* Carsten Schaller et al.

Anonymous Referee #2

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Review of "Characterisation of short-term extreme methane fluxes related to non-turbulent mixing above an Arctic permafrost ecosystem" by Schaller et al. acp-2018-277

This manuscript presents an analysis of methane (CH_4) eddy covariance (EC) data measured above a wetland in NE Siberia. The manuscript focuses on CH_4 fluxes during night time in non-turbulent and low-mixing conditions when the EC measurement level is decoupled from the surface. Wavelet methods developed in a companion paper are used to estimate fluxes with 1 min time resolution over one summer and this high frequency flux time series is used to identify and classify high CH_4 flux events during the analyzed period. These events are then speculated to be linked with atmospheric mesoscale circulation taking place in these nocturnal low-mixing conditions.

However, large part of the abstract, introduction and some other sections are discussing ebullition and other non-related topics, whereas results and conclusions are all about nocturnal low-mixing conditions. The authors should modify the beginning of the manuscript so that it matches with the end, so that the text forms one coherent entity. There are also other shortcomings in the text and description of data processing. Please see below.

As it stands the manuscript is interesting and shows promise but requires major revisions (see below) before publication. Once revised, it should be of interest also for the wider community working with micrometeorological flux measurements and hence the study is within the scope of ACP. Besides the shortcomings mentioned above, the presentation quality is good, although some figures need adjustment. I recommend the publication of this manuscript after major revision based on the comments below.

GENERAL COMMENTS

1) Please modify the abstract and introduction so that they match with the results. In my opinion these sections should be largely rewritten since now they are quite disconnected from the rest of the manuscript. The results are about gas fluxes under nocturnal low-mixing conditions and the abstract and introduction should be written about this topic, not about arctic wetland CH4 emission dynamics. As you know, these problems related to low-mixing conditions are universal, not only related to arctic wetlands.

2) The wavelet method is presented in the manuscript as more accurate than EC and reliable reference for the EC fluxes. This is a strong statement, which should be supported by convincing evidence. In order to make that kind of statement you should show that when the data is processed using standard EC methodologies spurious data points are left in the flux time series, yet with the wavelet methods these problematic periods are handled better. I suspect that most of the low-mixing conditions would have

been filtered out by quality screening and friction velocity filtering the data. Hence CH4 budgets derived using standard EC processing are likely not affected by these spurious fluxes during low-mixing. Please show a comparison of fluxes (e.g. monthly CH4 budgets) derived with standard EC processing (including quality screening and friction velocity filtering) and fluxes calculated with your wavelet methods to support your statement. Alternatively, you should phrase the text differently so that it is clear for the reader that it is not possible to say that the wavelet method is more accurate or reliable than standard EC.

3) In many occasions the observed high flux events are linked with mesoscale motions (gravity waves, low-level jets etc.), yet the connections are quite speculative. This is understandable since these mesoscale motions are difficult to quantify with just one flux tower and the authors also clearly state this in the discussion section of the manuscript. However in contrast to the discussion, the abstract and the conclusions are written in such way that the connections are obvious based on the data. Please rephrase the text so that it is clear for the reader that the role of mesoscale motions is quite speculative and additional instrumentation would be needed for a proper identification of these flow patterns.

4) More details about data processing are needed. Did you do coordinate rotation and how did you do it? The regular 2D-coordinate rotation (align u with mean wind and nullify mean w for each 30 min period) does not necessarily work well during low-turbulence since mean w is not necessarily zero when mesoscale motions are at play. Hence I hope that you used planar fitting (Wilczak et al., 2001) and defined the plane used in the coordinate rotation using high quality data. On the other hand if you did not do any coordinate rotation (like in Schaller et al., 2017) then the fluxes might be seriously compromised, since sonic anemometers are always slightly tilted no matter how carefully they are aligned with the surface below. Also did you correct for the time lag between gas analyzer data and sonic data? Lag time determination is always difficult for periods with low and intermittent turbulence. Therefore, please

provide additional details about EC processing. Related to the wavelet analysis, how did you take into account the time series edges and their effect on the results? Did you zero-pad the data and then estimate the cone of influence (Torrence and Compo, 1998)? This is important especially for the low frequencies. Please add details, since they are missing also from the companion paper (Schaller et al., 2017). As it is, it is difficult to judge whether the data were processed in a proper way.

SPECIFIC COMMENTS

page 7, line 24-27 This part is unclear. Do you mean extreme outliers in the 20 Hz data or in the 1-min flux data? It is difficult to understand why the outliers could explain the difference in the amount of events observed with the two towers. Please clarify and rephrase.

Sect. 3.3 I would like to see an analysis using Richardson number (Ri), since Ri is typically used to indicate dynamic stability of the flow. Moreover, if Ri exceeds so called critical Richardson number (Ric) then the turbulence is strongly dampened or even almost completely wiped out (e.g. Grachev et al., 2013). Ric is typically said to be around 0.25, although this is debated (Galperin et al., 2007). It would be interesting to see how Ri is affected by these events you identified and if Ri>Ric always before the events. The analysis you did on stability parameter (z/L) is somewhat similar, however I would prefer Ri since you cannot determine a turbulence cutoff with z/L the same way as with Ri. I suggest you use gradient Richardson number for the analysis, however in case you are missing the needed vertical gradients, then use flux Richardson number.

p. 8, l. 20-25 Why the analysis with relative humidity? I would guess that it is not relevant for the topics at hand.

p. 8, l. 29-32 Referring to my comment before, did you do coordinate rotation? It should be always done, regardless of how flat the terrain is since anemometers are always at least slightly tilted. If coordinate rotation is not done, then w data is compromised by horizontal wind speed fluctuations.

p. 9, l. 6-8 It is difficult to understand why there would be unstable stratification during night. Could it be because during these events EC is not working properly and hence you have erroneous heat fluxes and therefore also erroneous values for z/L? Did you have also negative vertical gradients in air temperature (decrease with height) during these periods?

p. 9 l. 16 This title should be modified. Based on the evidence shown it is not possible to say that there was advection of CH_4 to the study domain. In order to make such a statement you should have measured also CH_4 concentration horizontal gradients.

Sect. 3.4 The event that is analyzed in this section was already analyzed in the companion paper (Schaller et al., 2017). For instance Fig. 4 here is partly the same as Fig. 5 in Schaller et al. (2017) and also the text is quite similar. It would be better to concentrate on some other event in this study, now this analysis is a bit redundant.

p. 10 l. 12-15 You analyse here a period that lasts for two hours, right? Can you then extent the maximum wavelet period above 120 min? If you can, then how accurate the results are at these very low frequencies, given that your time series does not cover even one whole wavelet when the wavelet period is above 120 min? On a related note, shouldn't you also take into account the cone of influence (those regions of the wavelet spectrum that are significantly affected by the edges; see Torrence and Compo, 1998) in your cross-scalograms and in the corresponding analysis? If you used two hour long time series in this analysis, then wavelet periods above 120 min are definitely within the cone of influence and hence unreliable.

p. 11 l. 4-5 This sentence should be modified. One cannot claim that EC fluxes were systematically overestimated since you do not have an absolutely correct reference. For instance damping of the signal within the cone of influence (Torrence and Compo, 1998) might decrease the wavelet based fluxes. This could partly explain the observed difference.

p. 11 l. 31 Please replace "by an eddy-covariance system" with "with these wavelet algorithms”.

Sect. 3.52 & 3.5.3 & 3.5.4 Difference between these three categories is difficult to see, especially the description of 3.5.3 and 3.5.4 looks similar. Try to emphasize more the differences in meteorological forcings between these event categories. As it reads now, combining the events with different mesoscale flow patterns seems rather subjective.

p. 12 l. 12-13 As you probably know, ebullition is often hypothesized to be connected with falling (Tokida et al., 2007), but sometimes also increasing atmospheric pressure (Chen and Slater, 2015). Could this be somehow connected to this daytime event?

p. 13 l. 20-21 Onset of turbulent mixing in the morning has been shown to cause CH4 flux peaks also in other studies (e.g. Peltola et al., 2015). Did these events that you identified to be connected with the onset of turbulent flow take place in the morning?

p. 14 l. 8 How did you define which events were influenced by advection? These periods discussed here are most likely non-stationary and would have been filtered out from standard EC data.

p. 14 l. 11-18 This is a good point and it would have been nice to see this idea used in the prior analysis.

p. 14 l. 24 How did you determine this 15 min limit for identifying events that are affected by advection?

Sect. 4.1.1 I would add here text about CH4 concentration profiles since large part of this manuscript discusses flushing of previously stored CH4 below the EC level. With detailed concentration profile you could measure this.

p. 15 l. 1-2 Why the analysis on cluster events was not possible?

Figure 2 Mean w is around 0.15 m/s, which is quite high value. Did you do coordinate rotation? You should definitely do it. Another thing: you could add here the Richardson number, like I suggest above.

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Discussion paper

Figures 2 & 3 These two figures are complicated and should be explained better. For instance how did you define “Unstable”, “Stable” and “Neutral”? Where the stability is shown? How can you have EC data in the bottom plot with different quality classes at the same time?

Figures 2, 3 & 4 You most likely have change in flux sign at some certain color in the cross-scalograms (e.g. negative fluxes at blue colors and positive at red colors). Please highlight the zero flux lines in the cross-scalograms with e.g. white contour lines. Also, is the color scale the same in both subplots? If not, then please try to use one color scale per figure. Add also the cone of influence (Torrence and Compo, 1998) to all subplots.

TECHNICAL CORRECTIONS

p. 4, l. 12 You defined the abbreviation EC here, but you defined it already on page 2 line 19. Use the abbreviation everywhere in the text after you define it. Also, you use both “eddy covariance” and “eddy-covariance”, replace both with EC.

p. 7, l. 14 and other places Please give dates in a consistent manner and try to follow the journal recommendations.

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