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Atmospheric Chemistry and Physics
Discussions

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.

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The manuscript presents land-atmosphere methane fluxes from a permafrost-underlain wetland in NE Siberia, with a special focus on short-term fluctuations caused by non-turbulent conditions. The analysis uses wavelet transforms to calculate fluxes with higher time resolution than conventional eddy covariance calculations, also in non-turbulent conditions when the ground surface is (or has been) decoupled from the sensor level. Based on the wavelet flux time series, high-flux events are identified and classified according to their temporal structure. Some specific events are described in detail to distinguish the active mesoscale processes. The work should be interesting

for many in the eddy covariance community and is relevant for the scope of ACP. The language and figures are of good quality. I therefore recommend the publication of this manuscript after minor revision considering my comments below.

1. Generally speaking, it is important to have studies that present alternatives to the conventional eddy covariance flux calculations. After reading this manuscript, however, I am left with the impression that wavelet flux estimations give arbitrary flux results. You used two different wavelets, the Mexican Hat and the Morlet, and get flux differences by about 30% on average (cf. Table 1). Other wavelets might have given even larger differences. I think this makes it difficult for the wider EC community to apply the wavelet analysis approach. While I understand that you cannot resolve this arbitrariness in your manuscript, I think you should be clearer about the potential and the shortcomings of wavelet flux calculations.

2. You imply on several occasions that the high-flux events you identified are related to methane emissions from the ecosystem. For example, when you relate event occurrences to soil temperatures in the abstract (page 1, lines 9ff, "We demonstrate..."). Also many other parts of your manuscript are written as if this study is about bio-physical processes, and not just about a different flux calculation method. At the same time, I think you are aware that your events are probably not ecosystem emissions, but merely a venting of previously accumulated methane. The fluxes you present, e.g. in Figure 1, indicate fast shifts between methane emission and uptake, which are unlikely to have anything to do with the ecosystem dynamics. There is a tendency throughout your manuscript to smear out the distinction between the ecosystem methane exchange and the flux you calculate at sensor level.

3. On a related note, you mention that conventional EC processing would give biased budgets due to events of non-turbulent mixing, even if filtering and gap-filling is applied. However, you don't perform the comparison to a commonly-used filtering and gap-filling routine to make such a statement. If you want to say anything about such a possible bias, your analysis needs to show this.

4. You attribute parts of the high-flux events to methane that entered your footprint by horizontal advection. But since you have no direct measurements of horizontal methane advection, this attribution remains speculative in this study and should be phrased accordingly.

5. I think your dataset of methane EC measurements from NE Siberia is impressive and extremely valuable, but in this manuscript you don't use this potential very much: you say and conclude little about this ecosystem or the methane dynamics of it. I understand that you want to focus on wavelet analysis and short-term events, but you could probably have done this for much easier field studies (something like CO2 exchange above a central European farmland). You don't even mention your field site location in the abstract. I think there is room for improvement to integrate and connect your findings to methane flux studies from permafrost wetlands.

Specific comments:

6. Page 1, line 5 You mention that ebullition events last for only a few minutes, but I think the timescale of ebullition depends on the spatial scale. On a small spatial scale, maybe comprising a single bubble only, an ebullition event would probably only take seconds to be mixed into the ambient atmosphere.

7. Page 1, lines 12ff. "By investigating..." This sentence is unclear. You say you identified mesoscale processes as the dominating processes. But for what? You mean as the trigger for high-flux events? But then this is quite a stretch given your rather descriptive analysis of the mesoscale conditions.

8. Page 1, lines 15f. "It is a reliable..." Please elaborate and clarify this statement. How exactly can I evaluate the flux quality using wavelets? And did you show that this works reliably?

9. Page 4, line 22 What do you mean by "exact" fluxes?

10. Page 4, line 22 You mention the 1-min resolution of the flux results. But what is

the limit for the time resolution of wavelet flux calculations? Why can you not resolve 1 sec, for example?

11. Page 4, line 25 Is the Morlet wavelet you used a real or complex function? I'm asking because I think in the PyWavelets Python package, the "Morlet" wavelet is real-valued, which might be unexpected. At the same time, a real-valued wavelet might have the advantage that you can show the flux direction (uptake/release) in your cross-scalograms (cf. Figure 3).

12. Page 6, line 16 Here you mention that some event-minutes needed to be manually added. Later, in the last sentence of section 3.1, you describe the MAD test as a robust estimator. I wouldn't expect a robust test to need manual intervention.

13. Page 7, line 24 You explain the large difference between the two towers by the percentage of outliers. But what is the explanation for this difference in outliers? Could the real explanation be that tower 1's footprint was artificially drained?

14. Page 7, line 28 Here you describe the event seasonality, and I think it would be nice to see a plot of the event-percentages over time. You can show the three classes of events as separate lines, and the two towers as separate subplots. Maybe you can even add another subplot with the friction velocity.

15. Page 8, line 16 How did you quantify or identify a trend?

16. Page 9, line 34 I think it would be worthwhile to check if there is a relation between the length of the calm period preceding an event and the event's total emission. This test could give you a needed insight to separate local emissions from horizontal advection.

17. Page 10, lines 14f If an extension of the upper period limit changes the flux so much, does this mean there is no co-spectral gap? How does this problem look like during well-mixed, stationary conditions? Have you looked at the ordinary co-spectrum and ogive?

18. Page 11, lines 4f Wouldn't the regular EC processing filter out and gap-fill this period? If so, it doesn't seem right to say "regular EC data processing yielded biased results". And have you checked that the momentum flux is downwards for all these events you discuss here?

19. Page 11, lines 19ff This whole paragraph hits the nail on the head. You should focus on this finding in your abstract, instead of ebullition, which you probably didn't observe.

20. Page 14, line 20 Isn't it more the time since decoupling that determines how much methane can have accumulated, rather than the time since the last event?

21. Page 15, lines 5f Methane budgets with the ecosystem as a reference should not include such high-flux events, because the ecosystem did not emit these large amounts of methane in this period. So I fail to see that filtering and gap-fill these periods would lead to a systematic underestimation of net emission, as you state here.

22. Page 16, line 1 Why was this classification not possible here?

23. Page 16, line 7 I'm not sure EC really "failed to resolve the events correctly". It is not designed to resolve them in the first place.

24. Page 16, line 16 How did you rule out sudden sources from the soil?

25. Page 24, Figure 2 Your w-measurements seem to have a mean value of about 0.1 m/s, so this is data before the tilt correction? But your wavelet cross-scalograms use w after the tilt correction, right?

26. Page 25, Figure 3 The cross-scalograms don't seem to show a co-spectral peak, or an intensity decrease at the lowest and highest frequencies. Is this expected? Are these coefficients pre-multiplied by the frequency? Maybe a legend would help to read these plots. And did you define ITC and RNcov anywhere?

Technical corrections:

Page 4, line 28 Missing full stop.

Page 8, line 22 You probably mean +0.67 % min-1

Page 10, line 5 Please add units to the fluxes given in parentheses

Best wishes, Norbert Pirk

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