Answer to N. Pirk

The comments of the reviewer are in black, our reply is coloured blue.

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.

We thank Norbert Pirk for his constructive comments. According to his remarks we revised our manuscript as described in the following reply.

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.

The reason for these differences in the calculated flux of the event shown in the case study in section 3.4 including Table 1 is directly connected to the mathematical behaviour of the mother wavelet. The Morlet wavelet allows a very good resolution in the frequency domain while its localization in the time domain is not that good (e.g. Collineau and Brunet, 1993). In our manuscript we used an averaging time of 30 minutes, which is widely used for eddy covariance processing. Usually this is not problematic in times of well-developed turbulence, because then the longest period contributing significantly to the flux should be 30 minutes (e.g. Charuchittipan et al., 2014; Foken et al., 2006). As already shown by Schaller et al., 2017, in times of well-developed turbulence, the results of both Mexican hat and Morlet wavelet are comparable and their deviation is within the typical error range of eddy covariance.

In the current manuscript we analyze fluxes also during times with only little or nearly no turbulence. Table 1 as well as Figure 4 (for tower 2) show the differences of the averaged flux magnitude over 30 minutes as mentioned by Norbert Pirk for the discussed event in section 3.4. The two upper panels show the wavelet cross scalograms for both wavelets. Exemplarily discussed for tower 2 and the Morlet wavelet, the area of high energy is sharp in frequency domain but shows a smearing effect in time domain from Aug 2, 23:00 to Aug 3, 0:45. The Mexican hat wavelet, on the other hand, resolves the event exactly in time domain (Aug 2, 23:59 – Aug 3, 0:07) while the resolution in frequency domain is slightly – but negligible – worse, compared to Morlet. These differences in resolution also explain the differences in the averaged flux over 30 minutes from 0:00 to 0:30. Both the Morlet wavelet flux (147 nmol m^{-2} s^{-1}) and the eddy covariance flux (179 nmol m^{-2} s^{-1}) underestimate the flux compared to Mexican hat (213 nmol m^{-2} s^{-1}).

To obtain similar results at least for the two wavelets, it is necessary to extend the averaging period, i.e., the averaging period should be longer than the smallest contributing frequency. For the eddy covariance method it won't be possible in this case, because the steady-state assumption is not fulfilled.

To sum up: as long as we keep the fixed averaging period of 30 minutes, the Morlet wavelet is not able to resolve the event completely while the Mexican hat wavelet delivers exact and authoritative results. We agree to Norbert Pirk that the shortcomings of Morlet compared to Mexican hat wavelet were not described sufficiently in our manuscript. The findings and scalograms of the Morlet wavelet can be used as some kind of a complex measure for stationarity / steady-state-conditions, which extends the stationarity test after Foken and Wichura (1996) by an ogive test – but this might lead to misunderstandings; we also did not mention that clear enough in our manuscript.

As the aim of our paper is on the characterisation of single short-term events and not on the flux over a longer time, **we decided to remove the results of the Morlet wavelet**, because 1) they might confuse the reader and 2) they don't add significant information to the findings and results. Instead we will add a paragraph in section 2 about the choice of a proper mother wavelet (here: Mexican hat).

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.

In fact, in our manuscript we also give a short information on the event seasonality (section 3.2), i.e. the number of occurrence of events during the time. It is known that increasing soil temperature changes the microbial decomposition rates of organic matter (e.g., Anderson, 1992; Valentine et al., 1994) and we could show that there is indeed a positive connection between the number of events and the soil temperature. Of course, the occurrence of meso-scale processes does not depend on soil temperature or other processes in the soil or the ecosystem, but it is an indirect measure for the activity of methanogenesis in soil that leads to a release of CH_4. So, e.g., during the observed maximum mean of the soil temperature in the first half of August, also the amount of methane released per time reached its maximum. E.g., during stable stratification in early August the biggest amount of CH4 gets accumulated per time close to the ground and later suddenly be released, in comparison to other months. **We will revise our manuscript so that this connection is clearly understandable.**

We agree that it is important to distinguish between methane emissions from the footprint of the EC system and emissions from around that were transported to the tower. Especially in the case of horizontal advection being the driver, the methane being vented to the EC system might originate partly from outside of the footprint. For regular flux calculations, especially for long-term balances and the discussion of biosphere-atmosphere interactions, this might definitely be a big issue. On the other hand, our investigations of advection in this study aim to show the impact of this meso-scale

trigger on the flux in a one minute resolution using wavelet analysis. **We will add some sentences to emphasize that the behaviour of the high resolution flux during the discussed advection event is not due to ecosystem dynamics but due to the venting of the accumulated and advected methane.**

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.

Our manuscript explicitly investigates single events, that would usually be removed by gap-filling algorithms, but does not target long-term results. Such an extension of our manuscript by long-term studies including error analysis would be beyond the scope of this article. Nonetheless, we definitely agree that a comparison of wavelet fluxes and EC fluxes as well as EC gap filling approaches over a longer period should be performed to quantify its effect-term balances. This would be an interesting additional paper for future, which is already in preparation in our working group. **We will add this information to the revised manuscript; we will also revise the mentioned note, so that it is clear to the reader that we did not analyse this and that there will be a future paper analyzing the impact on long-term results in detail.**

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.

We agree, that it is much more difficult to detect advection without direct gradient profile measurements. Please see in general also our answer to your specific comment 7 concerning this point. Specifically to your question on advection events, Figure 6 in the manuscript supports this finding, because it directly shows the approaching fog bank. The occurrence and arrival of this fog bank at the towers exactly in time with the observed event in the data clearly indicates advection. Also in literature such kind of fog events are classified directly as advection fog (e.g., Stull, 1988).

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.

Yes, we generally agree, that there are many easier ways to obtain datasets that contain influences of meso-scale processes like advection or weather fronts passing the site, e.g., in Germany instead of going to a very remote and difficultly accessible site in the Russian Far East. However, the original intention of our study was the identification of ebullition events using high frequency data from EC towers. Ebullition is a quite important CH_4 emission pathway in permafrost wetlands and therefore currently a big subject of research in the community. As you know from our manuscript, we could not encounter any signs of ebullition in the data. Instead we found wavelet analysis being capable to resolve periods of the dataset where the steady-state assumption was not fulfilled. While this

manuscript investigates specific events in detail, a follow-up paper will compare the wavelet results with common gap-filling routines, yielding information on its influence to the long-term balances.

Luckily, our manuscript is also not the only study that originates from this indeed extremely valuable dataset. So, based on that dataset, there are already published studies on the impact of a persistently lowered water table on CO_2 and CH_4 emissions (Kittler et al., 2016, 2017) as well as on the energy fluxes (Göckede et al., 2017). Additionally, the dataset was also already useful to compare results of a year-round CH_4 simulation model on CH_4 emissions with real data (Castro-Morales et al., 2018).

We will emphasize the information on our follow-up paper and also add the field site location to the abstract.

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.

Yes, the timescale of ebullition depends on the spatial scale. **We will change this sentence.**

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.

Yes, we generally agree that it is difficult to link the found high flux events to the identified mesoscale processes. Your question is similar to general comment 3 by Anonymous Referee #2, which we both answer as follows:

Based on our meteorological measurements, we conclude that the identified mesoscale processes triggered the observed high-flux events. We agree that it is somehow "a stretch" or "speculative", to identify these mesoscale phenomena without measurements of spatial (vertical / horizontal) profiles. Usually, periods consisting of phenomena like the found mesoscale processes, are replaced by gap filling algorithms during the standard eddy covariance processing. Long-term measurements of the atmospheric boundary layer, including devices like SODAR-RASS or LIDAR as well as arrays of other vertical / horizontal gradient measurements, could fill this gap. Long-term measurements are necessary here, because these phenomena are not detected very often and any statistical analysis is nearly impossible then.

Nonetheless we think, that it will be worth to publish observations of such relatively rare phenomena if it is possible to observe them. The detection, identification and differentiation of such mesoscale phenomena requires considerable experience. Such experience is fortunately available in our working group, considering, e.g., the publications by Foken et al. (2012) or Serafimovich et al. (2018), where surface flux measurements and boundary-layer measurements were available. Of course, we carefully discussed the identified processes within our working group, to be as sure as possible in our statements regarding the available data. For the identification of these mesoscale processes we used the data from both towers (distance: 600 m). Here it is important to know that the identified phenomena are always visible at both tower 1 and 2, which supports our findings. **This was not mentioned clearly in the manuscript, so we will emphasize this information.**

To sum up, the classification of the different mesoscale processes is still not completely satisfying, but also not fully speculative. We will carefully revise the manuscript, so that it is clear for the reader why the identification is not just speculative.

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?

Your question is quite similar to general comment 2) by Anonymous Referee #2. Thus, we answer both as follows: using a mother wavelet with an exact resolution in time domain like the Mexican hat wavelet yields the exact flux by integrating over a short time interval, e.g., 1 min. In this case also all parts in the frequency domain that contribute to the flux are included and considered (Percival and Walden, 2000). Summing up these 1 min fluxes to the typical EC averaging interval of 30 min, the results of both EC and wavelet method must be equal as long as the lowest contributing periods < 30 min. That is the case during steady-state conditions (Foken and Wichura, 1996; Foken et al., 2004, 2012) and Schaller et al. (2017) could prove that comparing both wavelet analysis and EC.

In the case of non steady-state conditions with contributing periods > 30 min, the EC quality control tests should flag those cases to be excluded (Foken et al., 2012). Additionally, in those cases also the ogive test (Desjardins et al., 1989; Foken et al., 1995; Oncley et al., 1990) yields contributions to the flux for periods > 30 min. Besides that, the Mexican hat wavelet will yield nonetheless in every case correct and trustworthy fluxes, also for periods > 30 min, if the integration interval in the period domain is chosen big enough (Percival and Walden, 2000; Torrence and Compo, 1998).

We will add this information to the revised manuscript and modify the mentioned sentence.

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

The term "exact flux" in this context means that the result obtained by wavelet analysis offers an excellent resolution of the flux in time and frequency domain, depending on the chosen wavelet. **We will revise this sentence.**

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?

Please see the first paragraph of our answer to your comment 9. The maximum possible time resolution is restricted by the length of the timeseries and the cone of influence. Concerning the minimum resolution, we followed already available studies, because the exact calculation of the wavelet coefficients requires a certain number of values. A study on the lower limit of the resolution in time would be a specific mathematical study.

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).

In our calculations we use the complex valued Morlet wavelet, but nonetheless the cross-wavelet scalogram only shows the real part of the complex number, which is also used by our flux calculations. As we will remove the Morlet wavelet investigations (see our answer to your general comment 1) from the revised manuscript, changes in our manuscript concerning this comment are not necessary.

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.

Yes, the MAD test in general is a robust estimator for outliers / extreme values, but for some of the detected events, the method did not mark all non-extreme parts of the coherent event completely. In those cases we have made an additional visual inspection. **We will change the sentence to clarify that.**

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?

In this section of our manuscript we refer to the statistics of detected events, i.e., the number of detected event-minutes. The statistics is based on the result of wavelet analysis, i.e., the 1-min Mexican hat wavelet flux.
At tower 1 7.7 % of all data were out of the range from Q_1-1.5(Q_3-Q_1) to Q_3+1.5(Q_3-Q_1), where Q_1 denotes the 25%- quantile and Q_3 denotes the 75%- quantile. At tower 2 5.4 % of the data are out of this range, i.e., 2.3 % less "outliers" than at tower 1.
As the MAD test is a robust estimator, it is quite resilient against such outliers. In consequence, due to the statistical properties of the data, the number of outliers (event minutes) detected by the MAD test is greater at tower 1. **We will modify the text in lines 24 – 27 to clarify that.**

Seen from ecology, the artifical drainage also has an impact on the measured flux, which leads to a reduction of the CH_4 emissions (Kittler et al., 2017). So the median flux at tower 1 (drained, 0.17 nmol mol^{-1} m s^{-1}) was significantly smaller than at tower 2 (undrained, 0.60 nmol mol^{-1} m s^{-1}). On the other hand, as written in our manuscript, the percentage of values exceeding the interquartile range by at least 1.5 times was 2.3 % (tower 1: 7.7 %, tower 2: 5.4 %). There are some possible explanations, but is difficult to prove them: 1) during stable or neutral stratification, advective processes transport methane from outside the drainage ditch into the footprint, which leads to a substantial increase of the CH_4 concentration. When flushing these accumulated CH_4 from the ground to the sensors, greater fluxes are caused, relatively to tower 2. 2) The drainage works quite good and generally lowers the water table up to 0.3 m in summer (Kittler et al., 2016; Kwon et al., 2017, 2016), but there are still some smaller patches within the drainage ditch and the footprint, where the water table was near the ground level. There the rate of methanogenesis might be greater than in other parts of the footprint, where it decreased. In times, where the footprint covers these wet patches, the flux might be greater in comparison to other wind directions. **In our manuscript, we did not include these ideas, because we could not prove them, so they are somehow speculative.**

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.

As mentioned above, this paper exclusively focuses on the capability of our wavelet-based flux processing tool to detect and characterize events. Long-term statistics on event occurrence, including their net implication for the calculation of flux budgets, will be referred to the follow up paper mentioned before. We agree with the reviewer that it is somewhat disappointing to exclude the ecological implications of these event fluxes entirely within the context of this manuscript; however, we already know that those statistics cannot be explained with a short additional paragraph, so their proper interpretation would clearly exceed the appropriate length of this manuscript.

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

In the context of p. 8, l. 16, "trend" just means that the values monotonically decrease, i.e. there is always are smaller value following the current value. Maybe, the word "trend" misleads the reader here, who might think that there is some kind of statistical trend modelling behind. **We will change this sentence and remove the word "trend" here.**

16. Page 9, line 34 I think it would be worthwhile to check if there is a relation be-
tween 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.

We had a look into this while developing the results for the original manuscript, but didn't find any conclusive correlation that would be worth to include into the paper.

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?

The Morlet cross-scalogram, which allows a high resolution in the frequency domain, does not show any signs of a cospectral gap (Fig. 5 of the original manuscript, bottom panel).
During well-mixed, stationary conditions, there are no significant differences observable between the flux for an upper period limit of each 30 min and 190 min, there is a co-spectral gap. Also ogive analysis supports the finding, that there are no flux contributions for periods > 30 min.
It should be noted that in most cases the error of the EC method and the additional flux contribution after calculating the ogives doesn't show significant differences (Charuchittipan et al., 2014).

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?

Yes, usually those times should not pass the tests on steady-state and/or turbulent conditions, so those times were filtered out. **We will change this sentence to clarify that.**
Under low wind velocities it is a difficult problem to identify if the momentum flux is upward or downward. The reason are flow distortion effects, as studied e.g. by Li et al. (2013).

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.

We will revise the abstract, so that ebullition does not play a dominant role any more and change the focus more to non-turbulent mixing.

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?

Yes, the time since decoupling should be the most important parameter, together with the time since the last event. We will add this to the manuscript.

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.

Yes, we definitely agree if you refer to advection as source for the measured event. The paragraph including p. 15, l. 5f starts on p. 14, l. 33 saying "In the absence of advection...". **To clarify for the reader that also the statement in p. 15, l. 5f is made under that assumption, we include "...provided that the event was not caused by advection..." into the sentence.** With advection being absent, the methane being vented during an event must have originated from the site itself, so these fluxes need to be included into the flux budget. Assuming rather constant local emissions, this would imply the tower 'sees' low-biased fluxes for a while before the event occurs, then the 'missing' methane is vented out at the onset of turbulence. Accordingly, filtering out the event would mean missing an important part of the long-term flux budget, even though not all the methane was produced at the time of venting.

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

Your question is quite similar to the specific comment on p. 16 l 1-2 by Anonymous Referee #2. We answer both as follows: We decided to postpone this analysis to a follow-up study, because the complete, detailed investigation would be beyond the scope of this manuscript. It is quite difficult and due to the lack of additional boundary layer measurements maybe even impossible to get reliable evidence on the exact meso-scale processes triggering these events. Another major reason for our decision to exclude these events is the scope of the manuscript, which focuses on events that occur at short time scales, i.e., last only for minutes or some tens of minutes. **We agree, that these reasons were not stated clearly yet, and we will modify section 4.1.2 to make clear why the analysis might be possible, but is beyond our scope.**

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.

The EC method failed to resolve the events correctly, because it is not able to do that from design. **We will modify this sentence to clarify that.**

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

All of the observed events were triggered by meso-scale processes, mostly under stable or neutral stratification. Here the emitted methane was accumulated near the ground over longer times before the event started. During the occurrence of the meso-scale process, the accumulated methane was flushed up to the inlet of the measuring system tube causing the event flux. A differentiation whether the gas was accumulated over a longer time (very likely) or suddenly released directly from

the soil, is difficult. If it is a sudden release directly from soil, it is quite unlikely that it mostly happens during stable or neutral stratification as in our study.

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?

Anonymous Reviewer #2 also addressed this topic in his general comment #4. So we answer both your and his question as follows: Due to a sloped terrain, a non-exact alignment of the sonic anemometer or flow distortion effects, the streamlines of the wind might be tilted. In this case, usually a coordinate rotation is conducted to move the coordinate system into the streamlines and to solve this problem. For our study, we carefully inspected the measured wind vectors during times with well-developed turbulence and good flow conditions, i.e., sufficiently high wind velocities. We could not detect any disturbance of the streamlines due to the terrain or other influences, i.e., we are sure that the assumption of a negligible mean vertical wind component ($<w> = 0$) is valid. Due to that finding, the complete study is based on w-data without tilt correction.
Especially due to the fact that the found phenomena were connected to a distinct vertical wind component, double rotation might lead to irregularly high rotation angles (this was investigated in an master thesis by Matthias Mauder in 2002 for the EBEX-2000 experiment, flat terrain). Instead, planar fitting (Wilczak et al., 2001) is a proper choice in such situations, since here the rotation angles are based on a long term averaging period, so that short time periods, where $<w> \neq 0$, does not affect them. In our study we still did not apply planar fitting, because 1) it is not a long term study to make a careful analysis of the optimal interval for planar-fit rotation (Siebicke et al., 2012) and 2) at our towers fortunately the assumption of $<w> = 0$ was proven as valid for well-developed turbulence. **We will add this information to the revised manuscript.**

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?

A co-spectral peak is not expected / visible in the cross-scalograms of this case study due to existent flux contributing periods > 30 min, which were shown in Fig. 5, where the upper period limit was set to 184 min.
The coefficients shown in all figures are not pre-multiplied by the frequency and show directly the wavelet coefficients. **We will change the colours of the plot, to make it possible to see directly the sign, i.e., the direction of flux.**
ITC and RNCov describe the integral turbulence characteristics and the steady state parameter, respectively (Foken et al., 2012). Both are very common and very well known in the EC community, so we did not define them in our manuscript.
We will remove the panel for ITC in both Figures 3 and 4, because ITC < 30 % was never reached in that time.

Technical corrections:
Thanks for your correction remarks, we will consider them in the revised manuscript.

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

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