We thank Referee 2 for their help comments (in italics below) and provide responses inline.

The manuscript by Wang et al. focuses on methane fluxes measured by eddy covariance from a forest ecosystem. Up to date only few studies reported on continuous methane fluxes from forest ecosystems and therefore the presented results contribute to the currently available knowledge on methane exchange between ecosystems and the atmosphere. In general the authors provide a well structured and analyzed manuscript and I think stating that pressure pumping could actually lead to higher CH4 uptake rates in the soil is a new but also challenging statement. Even though some points are critical: (1) the rather short time period of measurements (5months) which seems to focus on publishing methane CH4 data as soon as possible even though more detailed analysis of methane fluxes in relation to environmental parameters but also on the variation throughout the year could have been accomplished. (2) A more severe point before publication are the uncertainty issues as already mentioned by Reviewer 1, which I absolutely agree with.

General Comments: The authors add further information with supplementary figures, which given the still relatively little available date on CH4 flux post processing etc could be of further interest the scientific community. I therefore encourage the authors to include figure S4 and S5 in the manuscript. This will further strengthen the manuscript, particularly since the currently presented results have already been shown within other CH4 flux studies.

These two figures will be moved into the manuscript.

How did the authors define the detection limit of the FGGA? In order to do so, one could use the full setup above a system, which emits or takes up a defined amount of methane. Otherwise to determine the noise within the measurement one could go to a non-release/non-uptake study often referred to as "carpark"-studies. Figure 1 as presented by the authors does neither show the uncertainty nor the detection limit of the instrument though one might easily tend to think so when seeing the results.

In response to this comment, and some of the concerns of Referee 1, we carried out further analysis of the data used in our approach to estimate uncertainty and will update the manuscript accordingly. An Allan variance plot will be added to the manuscript. We note that the uncertainty does not seem to scale with magnitude of the CH4 mixing ratio nor with the flux values themselves.

In terms of data availability the authors mention the removal of rather vast amounts of data, and it remains unclear whether each percentage given is cumulative to the previously stated number, therefore a stepwise removal and therefore it is unclear how many data remained for this analysis.

Some information about the steps followed for data removal was included on p. 17749. In the revised manuscript, we will include information about the number of points removed in relation to total points, as well as a clarification that these are percentages of the total number of points at the beginning:

Specific Comments:
The inlet of the FGGA was rather far positioned from the Sonic Anemometer, having a sensor displacement of 0.7m. Why?

This was a constraint of the setup, as 0.7 m was the closest we could get the inlet tube of the FGGA without strapping it directly onto the sonic anemometer, which would have led to other more significant problems of flow distortions for both the sonic anemometer as well as the inlet itself.

Why did the authors define outliers as values diverting from the mean by 8 SD? Especially when having methane fluxes and possible peaks caused by ebullition in mind this seems to be a random pick.

This threshold was originally chosen to identify significant outliers, though the reviewer is correct that there is a possibility it could lead to the removal of extreme, but valid flux data. In our case, only four points were removed in this despiking procedure. Given that none of the points were adjacent to other extreme values, and two were negative (implying extreme uptake), they are unlikely to be accurate and were removed to prevent bias in further analysis.

How much data were lost when removing all fluxes having wind directions between 0-93_?

13% of the original data points were removed as a result of flow distortions in 0 - 93°.

On page 17750 the authors refer to the importance of biologically active plants influencing CO_2 fluxes but not methane fluxes. However previous studies, even though not in forests have shown a clear link between productivity and methane fluxes.

The referee is correct that the biological activity of plants is known to influence methane fluxes, though this typically occurs in methane-rich areas that have water at the root level of the plants, which act as a conduit to release methane to the atmosphere. We do not expect this to be a major influence in terms of the Haliburton Forest ecosystem as the low-lying areas make up a smaller percentage of the total area, and the forest soil is not expected to emit significant levels of methane relative to wetlands. We will reword this section in the manuscript to clarify:

"For CH4 fluxes, in addition to biological productivity, the influence of turbulence could potentially be a strong environmental driver in determining variability in CH4 fluxes, as has been proposed in previous studies (Sachs et al., 2008; Wille et al., 2008)."

A more detailed information on the chambers would clearly improve understanding the presented results – even though the authors refer to the chamber measurements, considerable little information is provided on this – except the quick comparison of the general pattern of fluxes across the 5 months of measurements.

The relationship between methane concentrations and fluxes in the chamber data will be explored in a forthcoming manuscript by Winsborough *et al.*

In page 17752 the authors state a higher mixing ratio might be occurring due to the 200km distant Greater Torronto area, which I believe is rather far and why would the larger values be caused be the often mentioned patchiness of the site? Please comment.

"A back trajectory analysis (not shown here) indicated that the highest mixing ratios corresponded to airmasses that had passed over the Greater Toronto Area, approximately 200 km to the southwest."
Will be replaced by
"While local biogenic methane emission sources may be located to the south of the site, a back trajectory analysis (not shown here) indicates that the higher mixing ratios corresponded to airmasses that had passed over the populated areas 200 km to the south with anthropogenic emissions on the order of tens of nmol m^{-2} s^{-1}).

How was the comparison and the averaging done when comparing canister with eddy flux measurements? Were potential gaps in the data filled in order to derive a two day average? Please explain.

We have re-written this section by replacing:
"Mixing ratio measurements from the FGGA agreed with canister measurements at the top of the tower to within 5 nmol mol (0.3%) when averaged over the two-day measurement period."

With revised text:

"The average mixing ratios from the canister samples collected at the top of the tower were compared with the mixing ratios measured by the FGGA by calculating a weighted average of the canister samples based on their collection period."

The authors mention the slope rather often. Have the authors checked whether advection of methane occurs and possible sources are resembled in the actual flux measurements?

With a single sensor, we were unable to assess the importance of advection or storage. There was no evidence of a strong wind direction influence on the flux measurements.

The end of paragraph 4.2.: Even though I appreciate the statement of enhanced methane uptake caused by higher windspeeds the things written are contradicting, particularly when looking at the October data, where the uptake rates decreased even though wind speeds were higher. Please comment.

The reviewer raises an important point, which prompted us to revisit the October data. We found strong evidence that the disappearance of leaves during October contributed to the higher wind speeds measured at the mid-canopy. But given that the leaves were then covering the ground, this may not correspond to higher rates of wind-assisted ventilation. We redid the analysis in Fig 6 excluding October data and found a slightly steeper slope. The last paragraph of Section 4.2 will be rewritten accordingly.

Technical Corrections:

Page 17747,l.20: masl –> m a.s.l. Page 17748,l.21: lpm -> l m-1

Corrected.

Page 17750,l.19: every 2.5weeks (on eight separate days) – unclear, please clarify, also I suggest adding a figure which presents the study site including the location of the tower, of the chambers and if possible the reliefs in order to understand the topography would be really helpful

This section will be reworded to state:
"Static chamber measurements of CH4 were conducted on eight separate days throughout the EC measurement period using a distributed network of soil flux chamber stations established within the footprint of the tower following Basiliko et al. (2009)."

A figure of the site topography will be added in the revised manuscript in the supplementary information.

Page 17751,l.5-11: The authors first state about measurements of Tsoil and Theta adjacent t each chamber but than only two sites are mentioned which measured theta and there only one site is used in the analysis.

"Soil moisture data was collected at two sites, but only data from the drier site was used in the analysis."

Will be replaced by

"Out of two sites measuring soil moisture, only one provided representative measurements, as the second site was situated in a low slope area that was predominantly wet. With a small percentage of the study area containing such low slope sites, this was deemed unrepresentative and the data from the higher slope chamber site was used."

Please clarify. Page 17753,l.12: high emission rates – how high is high, please provide numbers or values for the different chambers

The values for low-slope sites ranged up to nearly 1000 nmol m^{-2} s^{-1}.

Page 17753,l.15: (+/- standard deviation) –> (+/- SD)

Corrected.

Page 17753,l.22: with ranges of 8-100% and 55-100% - this is unclear since the averages including SD have been given before

"The soil percent moisture by mass was measured at two mid-elevation sites with average values of 60.0±14% and 72.1±7.9 %, with ranges of 8–100% and 55–100 %, respectively."

Will be replaced by

"The soil percent moisture by mass varied between 8–100%, with an average value of 60%."

Page 17754,l.2: remove "soil moisture"

Corrected.

Page 17754,l.13-17: Why is this relevant?

The relevance of the diurnal cycle in mixing ratio is elaborated on in Section 4.1 and relates to the comparison of the direct flux measurements versus the surface exchange that can be inferred from the pattern in the mixing ratios. This will be clarified in the revised manuscript.

Page 17755,l.1: how narrow were the bins – be precise

"Each panel is overlaid with the average CH4 flux for narrow bins of the independent variable, and with a line of best fit through the entire dataset."

Will be replaced by

"Each panel is overlaid with a line of best fit through the entire dataset. Additionally, the average CH4 flux for narrow bins of the independent variable limited to where the data density is highest, are used to guide the eye."

Page 17756,l.18: 163nmol m-2 s-1 – This is a very large value and if these are averages what were the maxima and what were the minima?

The average for low slope sites was highly affected by one chamber that registered emissions of 998 nmol m^{-2} s^{-1}. The maximum uptake recorded in a low slope site on that date was -3.1 nmol m^{-2} s^{-1}.

Page 17757,l.24-26: What about possibly deeper installed sensors?

Because it is not clear if methane oxidation takes place throughout the soil or in specific areas (e.g. those containing roots), it is difficult to know what placement of the sensors would be most useful.

Page 17758,l.7: just because of "shallow" soils and commonly occurrence of methantrophs in the upper soil layers I think it is a wild guess to argue this way. Therefore I suggest stating the typical occurrence of methanotrophs in the upper soil layers and leave it at this information.

This sentence will be removed from the manuscript as there was no concrete measurement of microbial community depth at the study site.

Figure 2: does this represent available data from all months? What about data gaps? Might there be a bias towards daytime data?

Figure 2 includes all of the data from the entire measurement period that passed the quality control procedures. We will update Figure 2 to include a panel showing the percent of valud measurements for each half hour, which ranged from 90% at night to ~ 75% at midday. Because there was a more significant removal of points during the day, when the average flux was most negative, there could be a small bias in the campaign-average flux we report, i.e. we may underestimate the average uptake. Because the dependence of the methane flux on the environmental variables is not well-understood, no gap-filling is attempted.

Figure 5a: This is the same as Figure 2 – redundant, therefore I suggest to remove or add the additional variables to Figure 2!

As stated above, we will update Figure 2 to include a panel showing the percent of valid measurements for each half hour.

Figure 5d: no change in soil water content is visible, adjust the scale. Which measurement location is this, dry or wet?

Even with an adjusted scale, there is little variability in soil moisture. The scale will be kept to include the 5^{th} and 95^{th} percentiles. The location is a higher slope, drier site more representative of the study area and will be clarified in the revised manuscript.

Figure 4: bin-averaged slope – please explain

The plot includes averaged mixing ratios within the measurement period in each 45° bin of incident wind angles. This will be clarified in the revision.

Figure 7: I disagree with the statement that chamber fluxes agree well with the results found using EC measurements, particularly on Jun 7th, Jun 21st and Aug 2nd this is absolutely not the case. Please correct.

This is a fair criticism. We will revise Figure 7, and edit the text to identify periods of reasonable vs poor agreement.