The authors wish to thank the anonymous reviewer again for their time and valuable input to the development of the manuscript. We have included the reviewer's comments below in black, with our responses following in blue.

The authors state that it is important that "the concentration gradients are precisely quantified using high-precision gas analysers" to achieve high accuracy flux measurements. Would it be possible to use the information on system performance (lines 107-110) to calculate uncertainties for annual budgets (for example for line 204)? What if there would have been a small bias in concentration gradients between year 1 and 2? Adding an uncertainty range to annual CH4 emission estimates could provide further support that there was indeed a significant difference in CH4 emissions between years.

This is a good point, and we have quantified the uncertainty from our measured null gradients and included these in the manuscript.

Included in Line 111: One-way ANOVA tests performed on the line intercomparison data showed that methane null gradients were not significantly different throughout both years ($p = 0.03$), however this was not the case for carbon dioxide null gradients, with those from the final intercomparison (in December 2015) being significantly lower than the rest. Estimation of the cumulative uncertainty calculated from null gradient data (achieved by substituting ($C_2 - C_1$) in Eq. 1 with ($C_2 - C_1 + \varepsilon$), where ε is the measured null gradient value), gave values of 0.5% and 0.2% for methane in Years 1 and 2, respectively. For carbon dioxide, the respective uncertainties were 8% and 47%.

Would it be possible to use the CH4 concentrations gradients (see Fig. 3) to estimate a ballpark figure for methane emissions (e.g. using some literature-based soil-gas diffusivities)? I think one of the unique aspects of this manuscript is that it pairs ecosystem-scale flux with pore gas concentration measurements. The authors extended the discussion of the pore gas concentration data, but could, in my opinion, use these data to further strengthen the manuscript.

This is certainly a valid desire, and one that we discussed and strove for at length during the initial stages of developing the manuscript. Unfortunately, arriving at suitable literature-based soil-gas diffusivities was prohibitively difficult due to the changing conditions of the substrate. For example, the soil became saturated with water at times (most notably thawing seasons), which could lead to an order-of-magnitude change in the soil-gas diffusivity (e.g. Hu et al., 2018; doi: 10.3390/app8112097). Further, during early freezing and thawing seasons, we noted large increases in soil pore-gas concentrations that we can only hypothesise were due to changes in soil-gas diffusivities related to freezing within the soil. Lastly, the presence of snow above the soil for most of the year, has a very large and uncertain impact on gas diffusivity, depending on crystal type, porosity, lensing, and wind speeds (e.g. Whelsky, 2017; https://search.proquest.com/docview/1989143200).

For these reasons, we decided that a simple model of gas diffusion would not be suitable for providing satisfactory insights, and unfortunately we do not have the capabilities within the group to create a more complex model. We would be thrilled to work with a researcher or group of researchers who do have such capabilities, and will be happy to share our data should anyone be interested.

No changes made.