Response to comments by Referee #2

Referee comments are provided in grey and author responses in *blue italic style text*.

The technical note describes comprehensive laboratory tests of a low-cost methane sensor for potential application in flux chamber measurements in aquatic systems. The results are of high relevance for enhancing the spatial and temporal resolution of methane flux measurements and for improving our understanding of their environmental controls. The results and conclusion are comprehensible and well backed-up by data. Moreover, the note provides detailed instructions and procedures for implementing and calibrating such sensor in future applications. It is generally well written and presented. I have a few detailed, minor suggestions for improving the clarity of the note, which are listed below:

We thank Referee #2 for all work and are glad that the manuscript is found relevant and of value in several ways. The detailed comments are addressed below.

- the authors generally refer to "methane levels", which are reported in ppm. I suggest to clarify at some point what exactly is meant by this – mol fraction, mixing ratio?

This will be clarified in the revised manuscript. In many cases, mol fraction could replace levels, but levels are sometime useful when discussing relative changes rather than absolute mol fraction numbers.

- similarly, the authors state that the tested sensor is measuring "methane concentration", however, all results are reported in ppm. Could it be that the sensor responds to the abundance of CH_4 molecules, rather than the mixing ratio (as it has been found for humidity in the present manuscript)? The difference between a concentration (as mols or mass per volume) and mol fraction is the temperature and pressure dependence (as it could be described by the ideal gas law).

Referee #2 is correct that the sensors respond to the abundance of the molecules interacting with the sensor surface which is proportional to the mol fraction. The conversion from mol fraction to concentration (mol or mass per volume) via the ideal gas law correct for some of the temperature and pressure effects. On top of this, there can be extra temperature effects on sensor responses if the temperature influence the electronic component performance or the sensor surface characteristics. In this case water vapor also interact with the sensor surface, which explains the need for extra measures to correct for humidity. When revising the manuscript we will try to clarify these aspects.

- the range of relative humidity used in the experiments was 18-70%. Depending on deployment time, humidity in a flux chamber can become much higher than this. Could you add a remark on sensor performance at higher humidity?

The highest absolute humidity reached in our experiments were in the order of 35 000 ppm H_2O. This corresponds to approximately 100% humidity at 26 °C which is valid for many conditions. Unfortunately we do not dare to make any statements regarding sensor performance outside our test range, so there will be a need for tests at higher humidity in the future, in e.g. tropical conditions. We will clarify this in the revised manuscript.

- the proposed Arduino-based logger includes a humidity/temperature sensor in addition to the CO2 sensor, which already includes sensors for both parameters. What is the reason for adding this additional sensor? Does it have higher accuracy?

We added this extra RH/T sensor just to have a backup to the RH and T sensors at the CO2 sensor. This was done because the CH4 sensor data evaluation (in contrast to the CO2 sensor data) is completely dependent on reliable RH and T information to calculate absolute humidity. We will clarify this in the revised manuscript.

- line 195: "However, results indicate that the relative change of CH4 levels over time, which is the core of flux chamber measurements, can be assessed efficiently with the sensors if calibrated properly." I do not see how this conclusion can be made is this point. Consider adding a justification.

We agree that this sentence needs clarification. The justification is based on data in Table 2 where the SE and R2 for the slope versus the SE for the intercept of the calibration equations indicated that the relative change in sensor response to increasing CH4 mol fraction was rather consistent and accurate (more accurate than the absolute mol fraction derived). We will try to clarify this better when revising the manuscript.

- An important conclusion of the study was that "Sensor-specific calibration is required". Could you add some information about the stability of the calibration obtained for a specific sensor over extended time periods? What calibration intervals would you suggest?

This is a very important question. We do not yet have data from long-term studies to present, but other studies have addressed this. van den Bossche et al., (2017; cited in manuscript) found no drift over 31 days. Eugster et al (2019) studied results from a very similar type of sensor used for outdoor measurements over 7 years and concluded that the drift was in the order of 4–6 ppb/yr and the variability drifted by –0.24%/yr. We will add this information in the revised study to address the drift question.

Reference:
Eugster, W., Laundre, J., Eugster, J., and Kling, G. W.: Long-term reliability of the Figaro TGS 2600 solid-state methane sensor under low Arctic conditions at Toolik lake, Alaska, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2019-402, in review, 2019.

- Table 1: the symbol n is used as a coefficient in equation V1 and also for number of samples. I suggest to use separate symbols. The parameter h (listed in the caption) seems not to be used in the table.

Thanks for such detailed reading! We will change to "N" for number of samples. "h" is used once in Model V2 in Table 1.