

**Comments on
'Continuous In Situ Measurement of Dissolved Methane in Lake Kivu
Using a Membrane Inlet Laser Spectrometer' by Grilli et al., GI-2019-29**

The manuscript describes the deployment of a new sensor for the in-situ measurements of dissolved methane concentrations in Lake Kivu, known for its very high concentration of CH₄, especially in the deep waters. The sensor is based on a membrane inlet to extract the gas from the water, which is then analysed using a laser spectrometer. This technology has been already described in other papers (Grilli et al 2018 (ES&T), Jansson et al 2019 (OS)) and has proven reliable for low concentration measurements (down to the nM) mainly in oceans.

The challenge here is to evaluate the performance of the technology in very high concentrations (several mmol/l). Lake Kivu offers indeed the perfect conditions for this. It appears that some tuning of the instrument was necessary and yet, the authors concluded that the upper limit for concentration measurement was 3.5 mmol/l even after reducing the sensitivity, while concentrations in the lake can reach 18 mmol/l.

One can see the interest of monitoring the dissolved methane concentrations in Lake Kivu for safety reasons but, it may not be necessary to use a too sensitive technique. As presented, the technique based on laser spectrometer cannot respond to the (important) question of the accumulation rate of CH₄ in the deep layers (highlighted in the introduction). Maybe the same technique could be used in a 'high-concentration' mode by using a less sensitive laser spectrometer. Is it possible to use the near IR absorption bands of CH₄? i.e. 1.3/1.6 μm? Another way of development could be to reduce the exchange surface of the membrane. I also wonder if going for a Teflon membrane was a good change as Teflon is known for a better permeability to methane (although there are different types of Teflon but the authors do not give any precision on this). In fact, I don't think I properly understood this modification of the instrument. A scheme of the membrane block would help the reader.

Regarding the surface measurements, I would have liked some GC measurements as reference (from samples taken in the same time as MILS measurements). I don't think we can use the data from a commercial sensor as reference.

One thing we can conclude from this deployment is that in situ sensors must be carefully chosen according to the environmental conditions and the scientific question, and adapted accordingly. Because this paper is a good illustration of the constraints of in situ sensor development and also because the technique is promising despite its limitations when deployed in high concentrations, I would recommend its publication in Geoscientific Instrumentation.

However, I have some comments on the form of the manuscript. My main problem is the presentation of the results and the discussion that follows. I would go for a 'results and discussion' section followed by a conclusion instead of the current structure. This would avoid the discussion of some of the results in the results section (line 240-255) and repetition in the Discussion section (line 270). Otherwise, the manuscript is clearly written, figures are well described and clear although I would put the units into brackets, e.g. CH₄/‰ changed to CH₄ (‰) to avoid any confusion.

Finally, I have only a few typo corrections:

Line 232: 'therefore not...' therefore no...

Line 263: 'not spatial variability...' no spatial variability