Comment on

“Expanding seawater carbon dioxide and methane measuring capabilities with a Seaglider “ by Hauri et al.

Summary:

The manuscript by Hauri et al. presents the development and comprehensive testing of a glider-based sensor package for measuring water column partial pressures of CO2 and CH4. This is a timely task because it directly addresses the need of increasing observations of the spatial and temporal variability of these major greenhouse gases.

Overall assessment:

This contribution is significant for the field in that paves the way for large-scale surveys of greenhouse gases during process studies ranging from mid-water to the deep (1000 m) ocean, which in turn could be useful for the combined investigation of physical and chemical oceanographic variability. The manuscript is well written, figures and tables are mostly of adequate quality and the literature choice is appropriate. However, a major caveat I see in the study is the applicability of this approach given the different “readiness” level of the sensors for in-situ measurements of CO2 and CH4. While for the former the authors clearly show that the data obtained during both laboratory and at-sea conditions matches the desired accuracy and resolution (albeit uncertainties), for the latter both aspects rise questions on whether gliders are actually the right platform to be used (which is also an issue raised by Dr. Atamanchuk on her comment to this manuscript).

The authors indicate that their pCH4 measurements have an uncertainty of +/- 2 µatm, which is problematic because it would not allow fully distinguishing between under- and supersaturated conditions in the water column. Although I would expect this to be a more serious issue in open rather than in near-coastal settings, I would still expect that in the latter part of the seasonal variability (and possibly a large fraction of the water column) could not be adequately resolved. Recent intercomparison efforts from different groups (see Wilson et al., 2020; https://doi.org/10.5194/bg-17-5809-2020) came to the conclusion that although there is no consensus on the threshold for “high-quality” seawater CH4 measurements, the achieved accuracy should be such that it allows tracking the ocean’s response to increasing tropospheric CH4 inn time scales of 5 years (which translates into an analytical agreement of <1 % between independent observations). I would argue that, at this point, this should be the target for marine water column measurements. Furthermore, the slow response time of the CH4 sensor poses a practical constraint for vertical profiling, at least if combined with the CO2 sensor. The CH4 sensor used in this study is therefore rather suitable for stand-alone (and potentially shorter) deployments in gliders, or long-term applications in, for instance, moored observatories where seasonal and longer time scales of variability are the target.

I noticed that the description of the validation conducted by the authors is mostly centered around CO2 and my impression is that, at least in parts of the manuscript, the work done for CH4 was addressed somewhat qualitatively. Since the authors do have data to substantiate their thorough tests, I would kindly invite them to discuss the above-mentioned caveats in their manuscript.

In the following, I list general and specific comments not only to support my general assessment, but also in the hope that this is useful for the authors in view of a potential revision.

General comments:

Introduction:

- The manuscript is strongly biased towards CO2 and this is reflected also in the introduction. If the platform is to be presented as relevant for both gases, the key processes for CH4 cycling in the ocean should at least be briefly mentioned. This is important because for a reader not familiar with trace gases, it will be hard to grasp where CH4 comes from in the ocean and why processes such as seepage and permafrost thawing (as mentioned by the authors) are so relevant.

- The motivation of the study (beyond the notorious technological advances) is not clearly specified. A line of thought which – in my opinion- would help introducing that motivation would be to first mention the extent of the ocean contribution to the natural sources/sinks of these two gases, see e.g.:

  Global Carbon Budget (https://doi.org/10.5194/essd-15-5301-2023)

  The global Methane Budget 2000 – 2017 (https://essd.copernicus.org/articles/12/1561/2020/)

  After which the issue of undersampling would underpin why extending the usage of autonomous platforms to climate-relevant trace gases is urgent. Overall, I would recommend revising the structure of this section to show more clearly why this is great progress that needs to be further developed and implemented. While I appreciate that the focus of the manuscript is more technical, doing this would increase the impact of this work.

- In line 57 and following, the authors mention how key parameters are under-sampled. Most of the variables measured by the platform presented in this new application including gases are actually classified as essential ocean variables of the Global Ocean Observing System (see https://goosocean.org/what-we-do/framework/essential-ocean-variables/). I would suggest the authors to mention this framework in their manuscript in order to emphasize how scientific and technical developments in this direction do address a timely task at an international level.

- With regards to undersampling, it would be important to mention that this does not only refer to spatial, but also temporal coverage. This would emphasize further the potential contribution of the approach presented in this manuscript, as the largest sampling deficits occur in winter.

- Since the authors aim to show the advantages of glider-based, large-scale measurements of CO_2 and CH_4, it would be good to provide the reader examples of the current approaches and their limitations (added as citations). Some suggestions (which include further examples therein) are as follows:

  Behncke et al (2024): A detectable change in the air-sea CO_2 flux estimate from sailboat measurements, https://www.nature.com/articles/s41598-024-53159-0

  Resplandy et al (2024): A Synthesis of Global Coastal Ocean Greenhouse Gas Fluxes, https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2023GB007803

Methods:

- Some statements in this section (mostly regarding CH_4) are vague or require additional explanation in order for the readers to be able to fully understand the methodological approach of the study (and ensure reproducibility).

- As part of the validation discrete samples for both gases were collected and analysed. While it is clear that the glider sensors provide CH_4 values as partial pressures, the discrete samples were measured as dissolved gas concentration in an aqueous matrix. This means that partial pressures needed to be calculated. I would suggest the authors to clarify this aspect in the manuscript.

- In lines 301–302 the authors mention that the response times of their gas sensors do not have a temperature dependency. I think this is an important advance, which should be shown with data in the manuscript.

- I suggest the authors to make sure they write this section consistently in past tense.

Results and discussion:

- Besides withstanding high pressure and probably the need of extended battery power, I could not not see why differences in the performance of the deep water glider are to be expected (or it is at least not described in the manuscript). Even if there would be any changes, the tests shown on Figure 6 depicts a deployment that was even shallower than the first one, and therefore does not necessarily substantiates the author's argument.

- Section 3.2 (comparison with underway measurements): Neither during the methods, nor the results and discussion sections there is mention of how underway systems were used to cross-check the glider CO2 and CH4 sensors.

- Section 3.3.1 (tank experiments): the description provided here is rather qualitative. I recommend the authors to present the corresponding results on this validation for CH4, as they nicely did for CO2.

Specific comments:

- l. 21–22: “The key parameters to observing and understanding (…)”. Here I recommend revising the syntax.
- l. 31: (…) provides (…). I would write “provided” instead.
- l. 38–41: Spatial variability appears twice. I would check here to avoid redundance.
- l. 67: The abbreviation “BGC” should be inserted here.
- l. 68: This is unclear. Of which variables? In the sentence above only pCO2 and pH are mentioned.
- l. 91: Here it should be indicated what is the measurement standard (and/or reference) that substantiates this statement (e.g. accuracy, detection limits, etc.). Is this referred to Newton et al (2015) as included in the caption of figure 7?
- l. 128: “POM”. This abbreviation should be spelled in full upon first usage.
- l. 154: “headspace” instead of “head space”
- l. 163: “percentage” instead of “%”
- l. 223–224: Here the specifics of the CRM should be indicated (e.g. exact denomination, literature if any, etc).
- l. 238: I would suggest "processing" as more appropriate than “manipulation” here.
- l. 258,259: “CRM” instead of “Certified Reference Material” as the abbreviation was already defined
- l. 292: “required parameter”. This is rather vague. Here it should be stated which parameter was missing to be able to carry out the post-processing.
- l. 450–451: Considering the comparatively low accuracy and long response time, I would have to disagree, unless it is clearly stated for which types of studies this is the case (see also overall assessment above).
- l. 492: There should be a link in the revised version.

Kind regards,

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