Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-52-RC2, 2020

Interactive comment on "Glider-Based Observations of CO_2 in the Labrador Sea" *by* Nicolai von Oppeln-Bronikowski et al.

Anonymous Referee #2

Received and published: 12 September 2020

Review of Glider-Based Observations of CO2 in the Labrador Sea by: Nicolai von Oppeln-Bronikowski, Brad de Young, Dariia Atamanchuk, and Douglas Wallace

General Comments: The paper is well written and covers a topic of significant interest – testing sensors for different applications and validations against different platforms. In addition, the foray into looking at sub-surface layers in the Labrador Sea is timely and a good demonstration of the usefulness of having pCO2 measurements on gliders. The sensor under test is a CO2 optode sensor previously deployed on static moorings in a variety of locations (by Atamanchuk) and now tested on a glider. The sensor is compared to an infra-red based sensor deployed on an observing platform. The determination of response time of the optode under different glider scenarios (step changes vs profiling) is an interesting and new avenue of investigation and will help

progress the development of the optodes for different platforms. Ultimately the paper shows that the CO2 optode is currently not suitable for glider-based applications, but the use of multiple autonomous platforms can be used to validate sensor performance and provide additional inside into observed processes in the water column. In addition, the temporal and spatial variability observed by the study are evaluated. In general, the author should refer more to the figures in the text, particularly during the discussion sections, as I believe this will help emphasise their conclusions. And use numbers in place of vague terms/adjectives such as somewhat bigger to improve the readability. I think this paper should be published with corrections, which hopefully my queries below can help address. I look forward to seeing this in print.

Specific Comments:

Abstract: I would recommend the addition of some key figures from the text such as optode performance (precision/accuracy), response time, or length of deployment time. These are well utilised in the conclusions so could be used to entice readers within the abstract. The justification of the IR sensors only being used on the SeaCycler is not needed in the abstract, as it is not the focus of the paper and it is not relevant to the abstract to refer to Jiang et al's paper. The few lines discussing the ProCV also adds to the confusion in the last few sentences when referring to "this" and "the" sensor. I also think that the questions posed in the introduction could be summarised more clearly in the abstract.

Introduction I would also suggest the author look at the weather vs climate objectives for sensor performance as defined by GOAON (2nd edition). Line 63 – perhaps the author could make reference to the term "foil". Line 76 – the term "extended" with respect to the trinity bay work implies that it was continuous from the VITALS mission- however further on I understood there was some additional testing between the missions – Am I mistaken? If not perhaps the author could use re-deployed.

Data and Methods Figure 1 – the caption could clarify the importance of red vs. blue boxes around the profile data. I would also request the profiles on the right hand side have consistent axis (x axis on the top or on the bottom). Are the profiles from the SeaCycler as assuming the blue axis links them to the VITALS work, are there any shipboard CTD's to provide background for the trinity bay? What is the PCO2 sensor CO2 Prototype 4797? Is this the optode? Or is it the SN57? IS this 4797 on the Sea-Cycler – is any data from this presented? Perhaps the authors could clarify why not. With the ProCV can we have some details on how it performs detailed? (e.g. the stability and accuracy calculated from the measurements?). Given that the Jiang study was based aboard a vessel using an underway water supply, are there more references that apply the sensor in situ? If the CO2 optode underwent testing in Dalhousie University before deployment, what was the accuracy and precision also determined prior to deployment? And based on this, the optode should have been partially conditioned under the correct conditions to limit the initial drift of the optode on the glider. Is the SN57 the Aanderaa optode? The Pre-mission testing that was undertaken for the trinity bay work – was this similar undertaken for the VITALS mission? If not this might explain the conditioning timescale difference observed between VITALS and trinity bay. I was also wondering with the inconsistent drift behaviour if there was any other odd responses from other optodes (oxygen) or anything noted on the optode on the post deployment calibration? Perhaps the authors could posit some theories on the behaviour for future investigations. Line 155 – Winkler titrations to not to my knowledge allow you to determine DIC/TA only oxygen. Please can you clarify the instruments used to measure DIC/TA as this may influence the precision/accuracy of these data. In addition, any information on the collection of DIC/TA (e.g. poisoning, storage medium etc). Line 156 – please quote the constants you used for CO2SYS and the errors associated with the calculated pCO2. These will compound any instrument specific offsets.

Glider data processing I like the idea of using the ascent/descent as the ZM from Fiedler. I believe Fiedler also used ZM's to reduced drift of the response – were the authors able to do similar? Was the calibration curved used to calculate pCO2 from the foils from the pre-trinity bay mission testing or from Dariia's paper? Was the same

correction used for both VITALS and trinity bay? Line 179 – remove Also. Line 198 – correct to " the sensor began to display inconsistent behaviour…" (or similar) Line 198 – I would also change the word last to final, as this is clearer as the end of the experiment, rather than a relative statement.

Glider-based CO2 optode performance Response time – I understand the authors are (quite sensibly) looking for relationships between the response time and temperature changes in situ – which is a challenge using only in situ data. The authors undertake a comparative analysis with two parameters, the temperature gradient, and the initial sensor temperature. However, I find the figure 4 (the authors way to display this) confusing. More specifically on Figure 4 – the legend for the colour bar should be next to the bar (ideally rotated) rather than at the top of the figure which implies it is the figure title. I am also not clear on the fitted t95 – is this from the equation listed in the text in line 233? The authors normalise by dividing the temperature change by 900s. I assume deltaT is the minimum and maximum temperatures observed during the 900s intervals? I am not sure what value figure 4a brings as it is not discussed in the text in any detail. The authors then discuss the difference observed between VITALS and trinity bay data – are these both represented in figures 4? If so perhaps a different shape could be used to identify the two cruises while maintaining the figures. Were the response time data for VITALS collected after the sensor had become suitably conditioned to the environment? If not, this could explain the scatter over the smaller temperature gradient. Perhaps the author could expand the sentence that refers to the VITALS glider profiles v.s Trinity Bay step profiles with reference to the response time (or move the sentence closer to the paragraphs below which discuss this in the context of figure 5.) I am also interested in the data points where the response time is above 500s and the variation in RMSE values for these. Perhaps the author could comment on what this means or speculate on why these response times were longer. Is line 244 referring to the relationship shown in figure 4a? Perhaps referring to it in the text and also modifying the spot colour to be the initial sensor T would be helpful here. I am also concerned by the sentence in 246 which states there was a significant temperature dependence on response time. The previous paragraph does not demonstrate this, nor in my opinion figure 4. It would also be good to evaluate this in comparison to Dr Atamanchuk's lab based experiments -they utilised t63 as opposed to t95, but it would be interesting to see the difference between a lab based experiment and an in situ determination – particularly when it appears in figure 4b you have some data collected at 0.5C. Perhaps the authors could clarify the inference from line 252 – while CO_2 solubility may have a linear relationship with temperature, I am not convinced that is relevant to the optode response time? Perhaps the author is using this to explain the ProCV response time? I would suggest the author rephase to focus on the sensor response (as a whole) rather than the time taken to respond (as I think this may be their intention). I am not sure if figure 5 a and b are useful plots, as the glider profiling would presumably match less precisely to the CTD-style sea cycler profiles where they remain at the same depth for 20 minutes. The scatter in the data (creating the low r2 on the linear fits) I suspect is also due to the binning implemented to try and maintain a match in data records. I also noted the low R^2 values – perhaps the p-values could also be shared to demonstrate that these relationships are indeed significant and provide weight to the "linear trend" statement in line 259. I note the difference in the dphase range between the VITALS and Trinity bay data, yet a not dissimilar CO_2 range. The temperature range is significantly wider in trinity bay yet there is no overlap in the dphase values. I was wondering if the author could additionally comment on this (is it a result of the conditioning to local conditions, indicator bleaching?) as it is mentioned only in passing in line 260.

O_2-CO_2 Observations Please rephrase line 276, as "weak n an average sense" doesn't make sense to me. Figure 7 – the K1 mooring and SeaCycler locations are denoted I think by red and blue lines respectively – these are used within the colour scheme- perhaps white or gray could be considered as alternatives? The O_2 data doesn't have the glider profiles used for plotting on? The oxygen data demonstrates the suitability of the multi-platform approach to in situ calibration

Spatial and Temporal Variability Line 310 – please remove the word "somewhat". I would also advise using numbers to make your point clearer. Take care as figure 9/10 the legend appears to obscure data points at the start of a track. Perhaps the legend would be better suited on the right-hand side, or outside the plot. Line 360 -I think it should read "highly variable changes" not "highly varying". The following sentence is also a bit vague – potential CO_2 cycling? Perhaps the authors could clarify what they mean by this. Line 363 – I don't think you mean CO_2 solubility - do you mean strength of uptake? Or are you referring to the changing T&S increasing or decreasing the solubility?

Conclusion Perhaps the term "staircase missions" could be used in the sections where the authors refer to step profiling to maintain consistency with the conclusion. I would also suggest the author clarify the timescale of the temperature change in line 329 (is it 0.5 degrees over the 123.59 seconds?) I would also suggest that the author summarise some of the extra work, mentioned throughout the rest of the paper as a forward look (e.g. more tests to evaluate the influence of flow field on sensor performance in situ and a response time model?)

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