Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-52-AC2, 2020

Ocean Science
Discussions

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

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Received and published: 30 October 2020

This the Author's response to Reviewer 2 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.

- Thank you – results from text have been added to 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. paper.

- Some details will be omitted to add clarity. Also some suggestions from Reviewer 1 will be adapted here.

The few lines discussing the Pro CV 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

- Thank you, abstract has been clarified with regards to the questions from the introduction and to avoid the confusion between mentioned sensors.

Introduction:

I would also suggest the author look at the weather vs climate objectives for sensor performance as defined by GOAON (2nd edition).

- Thank you for the additional source. Additional mention and short sentence to be added into the text.

Line 63 – perhaps the author could make reference to the term "foil"

- Reference added.

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

- Corrected in text.

Data and Methods:

Figure 1 – the caption could clarify the importance of red vs. blue boxes around the profile data.

- Figure clarified

I would also request the profiles on the right hand side have consistent axis (x axis on the top or on the bottom).

- Done.

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?

- Clarified in caption. Profiles are from glider. As per Reviewer 1, Trinity T-S structure added to the text to Figure 2.

What is the PCO2 sensorCO2 Prototype 4797? Is this the optode? Or is it the SN57? IS this 4797 on the Sea-Cycler – is any data from this presented? Clarify why not

- 4797 is the CO2 Optode Prototype sensor which had the manufacturers serial number 57. Clarified text as per Reviewer 1 suggestion

With the Pro CV can we have some details on how it performs detailed? (e.g. the stability and accuracy calculated from the measurements?).

- The Pro CV had a zero-referencing routine that corrected the drift of the zero point of the sensor (Atamanchuk et al., 2019, supplement section.) Accuracy was given by prior calibration from the manufacturer. Additional references to explain this will be added to the text.

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?

- We are not aware of many uses of this sensor in-situ. I would gladly include them.

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.

- The CO2 optode deployment in VITALS was a first test and we expected stabilization issues. The factory sensor foil calibrations indicated that the sensor met accuracy specifications. In this paper we are reporting on the actual in situ behaviour of the sensor.

Is the SN57 the Aanderaa optode?

- Yes. Clarified in text.

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.

- No, there was no prior comparisons done in VITALS that could be used to estimate instrument offsets. This was a motivation for the subsequent tests in Trinity Bay and the lab experiments done with the glider at Fisheries and Oceans. Clarification was added to the text.

I was also wondering with the inconsistent drift behaviour if there was any other odd responses from other optode (oxygen) or anything noted on the optode on the post de-

ployment calibration? Perhaps the authors could posit some theories on the behaviour for future investigations.

- There was no biofouling or other obstruction found on the sensors during recovery of the gliders. We believe that cold environment, small signal changes (low CO2 gradients) in the VITALS mission made the sensor response slow and stability low. Also the sensor foils have a large range in performance based on the foil batch. It could be that one foil performs better than another foil calibrated at the same time and same conditions. The oxygen optode meanwhile performed really well and no discernable drift behaviour greater than the accuracy (5 umol/L) was found, although no measurements were collected upon recovery.

Line 155 – Winkler titrations to not to my knowledge allow you to determine DIC/TA only oxygen

- Fixed in text. TA and DIC are estimated from coulometry (Johnson et al., 1993) and potentiometric titration (Mintrop et al., 2000).

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).

- Instruments used include: VINDTA 3D (Versatile INstrument for the Determination of Total Alkalinity; manufactured by Marianda, Kiel, Germany) DIC analyzer connected to a coulometer (UIC, USA, model 5015O), VINDTA 3S (TA) analyzer using open cell differential potentiometry equipped with a reference (Metrohm, Canada, model 6.0729.100) and pH glass (Thermo-Orion, Canada, model 8101BNWP Ross half-cell) electrode, which were both referenced against a grounded platinum electrode. Samples were collected in the lab in 500 mL

BOD bottles and were poisoned 100 uL of saturated Mercuric-Chloride (HgCl2) and allowed to warm in a temperature controlled bath (25C +/- 0.1 C) before analysis.

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.

- We regret an error in the text. CO2calc (Robbins et al., 1999) and not CO2sys (Lewis and Wallace, 1998) was used in the determining pCO2. In the calculation they used the CO2 equilibrium constants from (Mehrbach et al. 1973 refit. by Dickson and Millero 1987), total boron constant (Lee et al., 2010), and KHSO4 constants (Dickson 1990). The samples were analyzed in the lab. of Fisheries and Oceans Canada and at the moment they are not setup to measure the uncertainty of the pCO2 estimate in CO2calc from DIC and TA. Reported uncertainty in the procedure for DIC and TA were 3 and 4 umol/kg respectively. I do not have access to CO2calc. Using CO2sys with above constants and to repeat the calculations with the uncertainty in TA and DIC, I arrive at an uncertainty of 4.48 uatm for lab experiment pCO2 estimates we reference in the text.

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?

- This is an interesting idea and could be tested in a future tank experiment. Using in-situ data it is hard to reach a conclusion on sensor drift and ways to mitigate the effect.

Was the calibration curve used to calculate pCO2 from the foils from the pre-trinity bay mission testing or from Dariia's paper?

- Separate foil coefficients were used for both missions which were both determined in the CERC laboratory at Dalhousie. Clarified in the text.

Was the same correction used for both VITALS and trinity bay?

- The conditioning offset from VITALS was estimated by comparison with SeaCycler. The Trinity Bay offsets were not applied as the drift was non linear and a single offset to deal with the conditioning affect was not possible to apply. The same CO2 Optode SN57 was used in both tests. Clarified in text.

Glider CO2 Optode Performance:

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.

- Figure 4 will be improved in a variety of ways:
  - We will display a variety of fits (linear least-squares, robust bi-square method as the distribution of the data is not normal but heavily tailed, median and mean responses
  - Colorbars labels will be fixed as per your suggestion

I am also not clear on the fitted t95 – is this from the equation listed in the text in line 233?

- Yes. Will be clarified.

The authors normalise by dividing the temperature change by 900s. I assume delta T is the minimum and maximum temperatures observed during the 900s intervals?

- Yes, will be clarified.

I am not sure what value figure 4a brings as it is not discussed in the text in any detail.

- We understand our initial version was a bit confusing. Figure 4a shows high scatter of response time (or lack of response) at low gradients. Figure 4b shows a slight bias in initial sensor temperature. However, the color coding is perhaps not required.

The authors then discuss the difference observed between VITALS and trinity bay data – are these both represented in figures 4?

- Yes both are in the figure. Again this will be stated more clearly in the new draft.

If so perhaps a different shape could be used to identify the two cruises while maintaining the figures.

- We will color code the different data sets with colors rather than using colorbars for the RMSE and temperature gradient as no additional information is conveyed.

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.

- Yes, this will be added to the text.

Perhaps the author could expand the sentence that refers to the VITALS glider profiles vs. 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.)

- Sentence will be moved and expanded.

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.

- The RMSE is larger for these on average because this data is from VITALS during weak gradients. The good fits come from occasional staircase profiles in that mission that allowed the response to equilibrate. Response times were more scattered as the sensor is not as responsive to weak gradients in temperature (CO2) leading to large tau values for weak gradients.

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.

- Figure 4b. Will be referenced.

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.

- Sentence will be rephrased to talk about gradients. Also added fits will help discuss the significance of these results in the context of a variety of fits. A table will be also added to summarize the results.

It would also be good to evaluate this in comparison to 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 0.5 C

- It is a great idea to have a lab experiment and see my earlier comment on understanding the sensor response in the Glider Processing Section. If we have another opportunity to work with the sensor your point would definitely be included in future comparisons.

Perhaps the authors could clarify the inference from line 252 – whileCO2 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 rephrase to focus on the sensor response (as a whole) rather than the time taken to respond (as I think this may be their intention).

- Thank you. Good point! Text will be clarified to focus on sensor response to avoid confusion. We mention the linear CO2-T relationship for completeness as another argument to validate the sensor response when true CO2 concentrations are not known.

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

- Good point and I see the argument against having panels a and b. We will play around with the binning. It is maybe best to not include them.

I also noted the low R2 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.

- Good point. Will be added and perhaps panels a, b removed from the Figure.

I note the difference in the dphase range between the VITALS and Trinity bay data, yet a not dissimilar CO2 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.

- This is an excellent point and question. We are speculating at this point and not sure for certain what changes the range in the foil angles. This is something we can mention more clearly in the text.

O2-CO2 Observations:

Please rephrase line 276, as "weak in an average sense" doesn't make sense to me.

- Numbers added to clarify sentence

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 O2 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

- Figure will be modified for legibility and clarity.

Spatial and Temporal Variability Line 310 – please remove the word "somewhat". I would also advise using numbers to make your point clearer.

- Done and numbers added

Take care as figure 9/10the 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.

- The legend was placed to avoid masking observations by the glider, but we are happy to move the legend outside the figure for clarity

The following sentence is also a bit vague – potential CO2 cycling? Perhaps the authors could clarify what they mean by this.

- Will be clarified. In the text we discuss fine-scale temperature variability, which can contribute to fine-scale variability of CO2 sinks. The complex dynamics that drive ML and below MLD CO2 variability are unfortunately out of scope for this paper.

Conclusions:

I would also suggest the author clarify the timescale of the temperature change in line 392 (is it0.5 degrees over the 123.59 seconds?)

- Yes 123.59 seconds is the average time constant we determined from the in-situ data which corresponds to gradients of 0.5 deg C. We will pay attention to the text to clear up any confusing bits.

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?)

- Excellent suggestion and the comments provided to the Methods and Sensor Response sections will serve as a good starting point.

Following editorial changes were directly implemented. Thank you so much for pointing them out.

Line 179 – remove Also.

Line 198 – correct to " the sensor began to display inconsistent behaviour..." (or similar)

Line198 – I would also change the word last to final, as this is clearer as the end of the experiment, rather than a relative statement.

Line 360 – I think it should read "highly variable changes" not "highly varying".

Line 363 – I don't think you mean CO2 solubility - do you mean strength of uptake? Or are you referring to the changing T&S increasing or decreasing the solubility?

Perhaps the term "staircase missions" could be used in the sections where the authors refer to step profiling to maintain consistency with the conclusion.

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