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Biogeosciences Discussions

Interactive comment on "Intense pCO_2 and [O_2] Oscillations in a Mussel-Seagrass Habitat: Implications for Calcification" *by* Vincent Saderne et al.

Anonymous Referee #1

Received and published: 3 November 2017

Summary:

Saderne et al. present pCO2 and O2 measurements from a seagrass meadow in Kiel Bay in order to gain insight into coastal carbonate system variability with implications for marine calcifiers (especially the blue mussel Mytilus edulis). They present high-resolution measurements from 7 weeks in the summer of 2013 and use these measurements to calculate other carbonate system parameters of interest. Among this paper's strengths are its error analysis on the measurements from the autonomous pCO2 sensor and its explicit treatment of the role of organic alkalinity in effecting carbonate system calculations. However, I also have some serious reservations about

the carbonate system calculations, especially the result that the mussels experience aragonite undersaturation nearly every day for 6-9 hrs. If this is the case, how do the mussels survive? The fact that the photo of the instruments during their deployment clearly shows mussels suggests that the authors need to either re-visit their carbonate system calculations and/or more critically consider the question they posed in the paper's title, "what are the implications of these measurements for calcifiers?".

This paper has the potential to contribute to the scientific literature on carbonate system variability in nearshore seagrass habitats, but must address some serious shortcomings before I can recommend that it be published.

Below are some general comments for specific sections followed by a list of grammatical and technical comments.

General Comments:

Section 2.2: I have read and re-read your description for the procedures to recover data during the time interval after pCO2 re-zeroing (lines 127-131 + Appendix A). I am still unclear about how you fit your first order kinetics model to the data. I think a simple schematic here would greater improve reader comprehension. I think you do a really nice job with the error analysis here by partitioning the error into the instrumental precision (1%) + the response time uncertainty (1.5%). You add the two components up for a stated uncertainty of 2.5%. However this is only accurate if the errors in the instrumental precision are independent of those from the response time. Have you tested this? If they are not independent, the error will be larger.

Section 2.4: (Please remove the "2.4.1" from the section heading since there is no section "2.4.2" in the paper). I found this section interesting. Given that your model for TA_org plays such an important role in this section (and that the manuscript is not very long in its current form), I think you need to move the full description of your TA_org model from Appendix B and into this section of the main paper. In doing so, I think you will need to include a much more complete error analysis in your model. In Appendix

B, you state that the model provides “only a qualitative approximation of the carbonate system in high DOC waters.” Why are you using the model quantitatively then (Fig. 2)? A proper error analysis on the terms of this model would result in some error bounds in addition to the solid black line on Fig. 2. I also do not understand how your calculation of a TA_org contribution of 0.84% from your 2015 bottle samples results in an 8-30 umol/kg contribution range (since 8 umol/kg out of 2000 umol/kg amounts to an error of 0.4% and 30 umol/kg out of 2000 umol/kg amounts to an error of 1.5%). You need to further explain how you arrived at this result. Also, what would it mean for all of your TA_inorg calculations if you adopted the 2013 TA_org contribution value of 0.48% as opposed to the 2015 value of 0.84%.

Section 2.5: This needs to move ahead of Section 2.4. Otherwise, readers do not understand which TA time series you are correcting for the TA_org contribution.

Section 2.6: This does not seem critical to the comprehension of the paper. I recommend relegating this section to an appendix.

Section 2.7: Why are you using Mann-Whitney U tests? What are these tests telling you? Are the samples independent? If not (and I suspect the time series will be highly auto-correlated), these statistical tests are likely inappropriate.

Section 3.2: It would be nice to see the daily means, medians, maximums, minimums, and daily ranges presented as distributions (histograms) that would allow readers to assess measures of central tendency and variance. If your distributions are not normally distributed, reporting your variance as standard deviations is inappropriate.

Section 3.3: Given your significant sources of uncertainty in your TA-S relationship, as well as the uncertainty regarding the contribution of TA_org to the carbonate system calculations, I am very skeptical that your observations of aragonite undersaturation for 6-9 hrs per day are accurate if mussels are living in the seagrass meadow (as you show in Fig. 1B). Overall, I think you need a full error propagation (e.g. Monte Carlo) through all of your carbonate system calculations (including the TA-S relationship) to

better bound your (and the reader’s) confidence in your aragonite and calcite saturation state numbers. I would like to see you cross-check your numbers by using the oxygen time series and your DIC bottle samples to calculate a net photosynthetic quotient, which, along with some additional information about O2 advective fluxes, could help you recreate a DIC time series. You could then use this DIC time series (along with its associated errors) + your pCO2 observations to re-calculate aragonite and calcite saturation states. Would they agree with your TA-derived numbers?

Section 4: Your deployment photo clearly shows that your instruments are deployed in the boundary layer, where chemistry can deviate substantially from bulk water conditions. I think this is an important point to consider, as well as an opportunity to highlight the need to co-locate instruments at appropriate depths in the water column if you are trying to infer biological implications from the chemical measurements. Separately, your use of 60 umol/kg as a hypoxic threshold is a very crude measure of hypoxic stress. What does the literature say about hypoxic thresholds appropriate for Mytilus edulis? Overall, I think you need to dive much deeper into the implications of your measurements for mussels and to discuss the next steps for integrating the insights from your chemical measurements into an understanding of how the mussels survive and/or thrive in these conditions. Upon completing the Discussion section, I still have a feeling of “so what?”, meaning that I feel unsatisfied in your attempts to link the chemical measurements with an improved understanding of coastal calcifier responses.

Technical Comments:

Line 74: What do you mean by “extension”? Do you mean that it was a circular patch with a radius of less than 2 meters? Or something else? Either way, please clarify

Lines 90-97: Do you have any information about the pCO2 of incoming riverine and/or upwelled water that you could include which would help readers better understand the advective drivers of pCO2 variability in Kiel Bay?

Line 109: What about details on the calibration and precision of your oxygen mea-

surements? You do a great job discussing the quality control and data processing on your pCO2 measurements, but I don't see any additional info for your oxygen measurements.

Lines 151-157: Precision needs to be defined as some recognized statistical measure of variance (e.g. standard deviation, standard error of the mean, etc.).

Line 327: Suggest rewording "However, we note that at not point of our survey the threshold of hypoxia. . ." to "However we did not observe dissolved oxygen concentrations below the hypoxic threshold during our survey. . ."

Line 330: "shouts" should be "shoots"

Line 331: replace "auto-" with "autotrophic"

Fig. 2: The y-axis label color scheme (grey circles) does not match the Fig. 2 caption. Please correct.

Fig. 3: I would like to see a plot of O2 percent saturation below the O2 concentration plot

Figs. 6 and 7: Why are these separated into two figures? If no convincing reason, combine into 1 figure.

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