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Interactive comment on "Seagrass beds as ocean acidification refuges for mussels? High resolution measurements of pCO_2 and O_2 in a *Zostera marina* and *Mytilus edulis* mosaic habitat" *by* V. Saderne et al.

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We are very thankful to the two reviewers for offering generous recommendations on how to improve the manuscript and figures.

Reviewer 2 main criticisms are that the title does not match the article, that the article lack clear aim and foci and a discussion reaching far from what is warranted from the results. We will modify the title for a revised version and especially remove the first part of it. This oratory question was used to highlight context in which our study is inscribed

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and indeed the article has neither the ambition nor the wealth of data to answer it. Using the interrogative form here could mislead potential readers. The article in the present form is torn between the primary aim as announced in introduction and the methodological issues linked to an over-determination of carbonate chemistry in our case. We will change the introduction towards a clearer explanation of the context motivating the article i.e.

1- The lack of carbonate chemistry data in nearshore benthic habitats

2- What are the carbonate chemistry dynamics experienced by calcifyers living in close interaction with primary producers.

To answer this, we present a case study in which we measured carbonate chemistry with a new sensor set-up directly on a mussel patch within a seagrass bed. Additionally, we discuss O2 data measured simultaneously. We will therefore remove references in introduction referring to upwelling, since no upwelling was observed during our deployment, and greatly reduce the part about O2 in introduction and discussion. The methodological considerations from p11434 l4 to p11436 l19 will be moved to the appendix, which will clarify the structure and by that improve the flow of the article. The discussion will focus on the interpretation of the data obtained and will be rewritten based on p11436, l20-30; p11437 l1-30 and p11438 l1-10. The parts, that we guess have been judged speculative, from p11438 l11-l17 will be removed. We will conclude the discussion with a parallel between our observation and what is known in the literature about the effect CO32- and saturation states for CaCO3 isomorphs on mussel biomineralization p11439 l17-30 and p11440 l1-6, by that addressing precisely the objectives of the article. This last being no speculation but a valid exercise of comparison, all due precaution taken.

All through the text there is reflections of an over-belief in the accuracy of the new equipment, like “The sensors were recently purchased and their specs are supposed to meet the manufacturer’s data” (11428, line 12)

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The cited sentence refers to the O2 optode and the conductivity cell for the measurement of salinity and the corresponding temperature probe. These (Aanderaa O2 optodes and SeaBird CT-sensors) are proven standard technologies in oceanography and we therefore have no reason to doubt their suitability.

The authors then report that the measurements are quite inconsistent, and show large discrepancies between measured and calculated (from DIC and TA) pCO2. The authors then reasons that: "The observed discrepancy at elevated pCO2's (Fig. 8) would correspond to an unrealistic measurement uncertainty of above 50 %. This is extremely unlikely since the sensor successfully passed calibration"

The cited sentence regarding the quality of the measured pCO2 time series data marks the final concluding sentence of a detailed discussion addressing the encountered carbonate system inconsistencies (p. 11434 l.4 – p. 11436 l.1). Within this discussion, enabled through the over-determination of the CO2 system within our case study, we thoroughly address all to our knowledge potential and reasonable effects. TA is excluded as the source of the inconsistencies through qualitative and quantitative discussion of low salinity effects within alkalinity titration measurements and of a potential TAorg influence within our discrete sample data. Therefore the deviation must originate from the DIC and pCO2 data. Since we have not encountered any peculiarities within the pCO2 sensor measurements, the calibrations of the very unit showed expected quality, the drift of the sensor throughout the deployment was tracked via the repeated zero gas measurements and we addressed response time aspects in a detailed manner, we can conclude that a measurement uncertainty of around 50% can be excluded for the sensor. Within a revised version of the manuscript we will rewrite the cited sentences and add additional quantitative information regarding the sensor calibrations to the text. The material and methods part could be extended as follows: "The sensor was calibrated at a water temperature of 17.5°C at 6 different CO2 levels across a measurements range of 200–2200 μatm before (June) and after (November) the measurements. The corresponding calibration polynomials had a quality of R2=0.999998

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and 0.99998 with root mean square errors of 1.15 and 3.98 ppm for the June and the November calibrations respectively."

"We therefore attribute the observed discrepancies between measured and calculated pCO2 to strong pCO2 gradients on small spatial scale near the mussel bed" While it might be true that there are real differences in water chemistry that could explain the reported discrepancies, it is a mere speculation from the author's side, and should be tested properly with additional measurements.

As said by reviewer, gradients in the benthic boundary layer and at the vicinity of the ground are extreme and realistic; see e.g. the review of Hurd et al., 2000. We indeed do not have proper tests of fluxes at the water column / sediment boundary as it could be achieved by means of an eddi-covariance system. However, we have excluded one by one all possible sources of error we could find in the literature, taking into account e.g. organic TA, and carefully investigated the CO2 sensor performance, before and after the deployment by means of the successful calibrations as well as throughout the deployment by the zero gas measurements and response time considerations. These arguments and the method of elimination finally only left the mentioned spatial heterogeneity as the most likely explanation.

Hurd, C. L.: Water motion, marine macroalgal physiology, and production, J. Phycol., 36, 453–472 [online] Available from: http://onlinelibrary.wiley.com/doi/10.1046/j.1529-8817.2000.99139.x/full (Accessed 10 July 2012), 2000.

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