

Interactive comment on “Seagrass beds as ocean acidification refuges for mussels? High resolution measurements of $p\text{CO}_2$ and O_2 in a *Zostera marina* and *Mytilus edulis* mosaic habitat” by V. Saderne et al.

Anonymous Referee #1

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GENERAL COMMENTS

The present paper of Saderne et al. under review for the journal Biogeosciences describes summer oxygen and carbon parameter variations over a *Zostera marina* and *Mytilus edulis* mosaic habitat in the Kiel Bay. Using autonomous in situ sensors, discrete samples and a modeling approach, authors got an interesting and consistent dataset of about seven weeks over such coastal environments where this kind of data is still scarce. My first general comment is objectives of the manuscript are not clearly enounced and targeted that leads to organization and clarity issues (result and dis-

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cussion parts, no explicative subtitles, figure order and presentation). The second associated comment is the manuscript in its present state remains too descriptive with not enough quantitative aspects to explain observed O_2 and carbonate parameter variations (statistical tests for significant variations or not between studied periods; daily plots and correlations between $p\text{CO}_2$ (O_2) and environmental parameters as salinity, etc. . . see for instance Dai et al. 2009, L&O, 54:735-745; computations, i.e. temperature/biological controls on $p\text{CO}_2$, see Takahashi et al. 2002, Deep-Sea Res. II, 49: 1601-1622). In this way, information on the Kiel Bay hydrodynamic (other than upwelling events) are particularly lacking, i.e. water stratification/mixing, water residence time, freshwater (rainfall and river discharge) inputs or/and seawater exchanges, since it is known all these parameters can strongly influence $p\text{CO}_2/\text{O}_2$ variations over coastal areas. Community metabolism estimated over this mosaic habitat could also be better highlighted opening on comparisons with similar or different environments (Champenois and Borges 2012, cited in the manuscript, etc. . . see specific comments below). I then suggest major revisions to allow the publication of the present paper of Saderne et al. for the journal Biogeosciences. Here are some specific and technical comments to help authors in this way.

SPECIFIC COMMENTS

1. Introduction

-P.11425, l.16-23: please in this paragraph emphasize the combined effect of ocean acidification and eutrophication over coastal systems as described in Cai et al. (2011).

-P.11425-26, l.24-10: I am wondering if this information about the effect of upwelling events (high $p\text{CO}_2$ /low O_2) on the benthic metabolism is relevant as no such event occurred in the Kiel Bay in the present study contrarily to what was observed by Saderne et al. (2013) in the Echernförde Bay?

-P.11426, l.8-11: please specify here the location of the study carried out by Frieder et al. (2013).

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-P.11426, l.15-21: please specify the different objectives of the study that come with the temporal description of O₂ and carbon parameter variations. They will have to clearly appear in turn in result and discussion parts. As it stands now, data based on in situ measurements, discrete samples and carbonate estimations remain mostly descriptive but not enough explicative and endorsed by quantitative aspects.

2. Material and methods

2.1. The site

More characteristics of the studied site should appear in this part, i.e. previous studies carried out in this area, estimated seagrass and mussel covers (density, biomass, volume?) and hydrodynamic information (freshwater inputs, seawater exchange, distance from the shore, water residence time, currents, stratification?). A photo of this mosaic habitat if available could also be appreciated.

2.2. In situ sensor suite

A picture with captions of the system should be added. Was biofouling important in this area especially at this season? Please briefly specify advantages/disadvantages of the pCO₂ measurement system used here (HydroC, Fietzek et al. 2014) compared to Equilibrator systems (Frankignoulle et al. 2001, *Water Res.*, 35:1344-1347) or other available CO₂ sensors in terms of precision, range and equilibration time.

2.3. Discrete sampling

Please add the reproducibility (precision, uncertainty) obtained for DIC and nutrient analysis.

2.5. Community metabolism

Calculations following Champenois and Borges (2012) should be detailed with written equations and also water column depth used to integrate O₂ fluxes.

2.6. Calculation of the regional atmospheric pCO₂

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Please add the distance of the station from the study site and also pCO₂ standard deviations (or ranges) obtained for August and September periods.

3. Results

This part along with tables and figures need to be reorganized and modified to clearly open on the discussion. I propose to authors to describe studied parameters for both periods with (i) environmental parameters (water temperature, salinity that is not described in the manuscript, irradiance, wind direction/speed, nutrients. . .) illustrated by a new table and figure and (ii) O₂/carbonate parameters with one table and figures. Tables and Figures as they appeared in the submitted manuscript are not specified neither in the right order and could be merged. For instance Tables 1 and 2 could be associated whereas Table 3 is not necessary. Statistical tests should be added in the result part to characterize each period according to studied parameters (significant differences etc. . .). Figure 5 and associated paragraph could be displaced in the discussion part opening on community metabolism over coastal ecosystems (see previous/next comments).

4. Discussion

The discussion part, as already explained needs to be reorganized, clearer and more consistent based on O₂/carbonate parameter process and control explanations with quantitative aspects.

- The discussion starts with methodological considerations comparing in situ carbon parameters measurements and calculations (p.11434, l.4-p.11436, l.19) before continuing with pCO₂/O₂ controls/mechanisms (p.11436, l.20-p.11438, l.16), mussel/seagrass interactions (p.11438, l.17-p.11439, l.7) and ending by mussel adaptation to OA (p.11439, l.8-29). Despite its interest, this long methodological part is not really expected since it is not clearly enounced in the objectives. It then needs to be better integrated in the manuscript structure. Since authors observed large differences between measured and calculated pCO₂ especially for high values, have they tried other model

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calculations for comparisons, i.e. the CO₂ system calculations from Lewis and Wallace (1998)? I agree about the effects of organic acids contribution to TA and uncertainties associated to pCO₂ calculations (see for instance Abril et al. 2015, Biogeosciences, 12:67-78). Thus I am again wondering about the contribution of freshwater inputs to the Kiel Bay and its influence on carbon parameter variations, i.e. pCO₂ or TA with the decrease observed the 03 September 2013 (Fig. 6.) associated to the salinity decrease (Fig. 4). Similarly, authors partly attributed measured and calculated pCO₂ discrepancies to small-scale gradients (p.11436, l.6) but nothing is explained about the hydrodynamic or spatial heterogeneity of the studied site neither supported by references to address this assumption. Could authors explain TA versus Salinity regression R² differences between August and September in Fig. 6?

- P.11436, l.21: it seems to be even more than 50 %?
- P.114336-11437, l.27-11: this part on O₂/pCO₂ mechanisms/controls/variations need to be developed with for instance daily plots, correlations. . . Authors explained pCO₂ variations with in situ biological activities of seagrasses and mussels without quantitative considerations. Authors could quantify this effect as well as the temperature effect using Takahashi et al. (2002) equations. Dai et al. (2009) were able to distinguish relative contributions from photosynthesis/respiration, calcification/dissolution and temperature to pCO₂ variations over a coral reef ecosystem at Xisha Islands (see Fig. 11). It could be interesting to have a similar approach for the present study. What about the importance of non-autochthonous processes on O₂/pCO₂ variations? Could freshwater inputs or water mass advection also explain high pCO₂ values observed in the studied site?
- P.11437, l.12-15: the described mechanism is not clear, what do authors mean with "shift" and "amplifying"
- P. 11438, l.3-6: O₂ decrease and pCO₂ increase between the two periods need to be support by statistical tests.

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-P. 11438, l.11-16: the comparison with upwelling effect on pCO₂ variations is repetitive in the submitted manuscript whereas no such event occurred during the study. Even if this latter can be cited, Fig. 9 is not justified. In the same way, p.11439, l.8-15, the paragraph about blue mussel adaptation to hypoxia is off-topic in the discussion.

- P.11438, l.27: could author estimate mussel/seagrass density in the present study?
- I suggest to authors to replace these discussion parts by one dealing with community metabolism estimations over coastal systems with comparisons with similar/contrasted ecosystems and methods (Champenois and Borges 2012; Rheuban et al. 2014, L&O, 59:1376-1387; Reidenbach et al. 2013, L&O: Fluids and Environment, 3:225-239; Martin et al. 2005, Aquatic Botany, 83:161-174; . . .). To go further in this way, would it be possible to estimate CO₂ fluxes with the atmosphere choosing a good parameterization of the gas transfer velocity (K₆₀₀) and supposing/proving a good mixing of the water column, i.e. similar benthic and sub-surface pCO₂?

TECHNICAL COMMENTS:

- P.11428, l.21: specify NDIR
- P.11432, l.4-8: remove or rephrase it with a scientific meaning
- P.11432, l.19: $\mu\text{mol kg}^{-1}$
- P.11434, l.6: "between" instead of "of"
- P.11436, l.10 2030 cm -> 20-30 cm?
- Table 1: mmol kg^{-1} -> $\mu\text{mol kg}^{-1}$
- Table 2 and 3: inverted captions
- Fig. 3: add atmospheric pCO₂ equilibrium
- Fig. 7.B: add discrete DIC sample values

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