

Interactive comment on “Decoupling salinity and carbonate chemistry: Low calcium ion concentration rather than salinity limits calcification in Baltic Sea mussels” by Trystan Sanders et al.

Anonymous Referee #2

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This is a very nice manuscript from Sanders and collaborators dealing with the impact of seawater chemistry on mussel growth rates. The two experiments have been well designed in order to discriminate first the impact of salinity vs. carbonate chemistry changes and second salinity vs. calcium concentration changes. These experiments have been complemented by a field survey covering over 3 years. Monitoring of seawater physico-chemistry and mussel growth have been performed at 3 sites along a decreasing salinity gradient towards the Central Baltic. The study is well introduced although I agree with Reviewer#1 that paragraph L98-112 should be put up front. The

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methods are most of the time well explained and the results properly discussed. I have no doubts that this will be a nice contribution to the Biogeosciences journal. Congratulations to the authors!

That being said, I have a few concerns and questions that I would like the authors to answer:

1) I have to say that I was impressed on how many individuals you could fit in 2 L containers (1600 animals, small but still. . .). Since you did not consider a flow-through system and changed the water “only” 2 to 3 times weekly, I am really wondering how would change carbonate chemistry but also ammonium and oxygen concentrations between two water changes. Table 1 and 2 are not clear to me. Do these tables show the conditions in the experimental plastic aquaria and/or in the stock seawater? If measured in the aquaria, when were the samples taken? Before and/or after water changes? Were your aquaria aerated? I apologize in case I missed that in the text.

2) In Table S2, you report on a >50% mortality during the 70 days bicarbonate experiment, as well as an important range (10-75%) across treatments. Did you check whether you had some relationships between mortality rates and the imposed chemical changes? Did you replace the dead organisms? If not, what would be the effect on the amount of food available for each individual? Table S2 is not clear to me, what are these biomass data? At the start of the experiment? At the end? You mention on L173 that biomass per litre was comparable between the 2 experiments while I can read that it was 13.2 mg/L during the Ca²⁺ exp and 51.5 mg/L during the HCO₃⁻ exp, it does not seem comparable to me.

3) I believe there is one aspect (maybe related to the point above) that should be discussed. During the first experiment (bicarbonate), mussels at salinity 6 did not grow much (maybe 5 microg/d; Fig. 2a). What is the reason why they grew much better during the second experiment (Fig. 2b) even when Ca²⁺ concentrations are below ambient levels (2.5 mmol/kg), reaching rates of 20 microg/d)? Is it due to the

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differences in terms of experimental design?

4) As such, I do not believe that trying to fit any model to all data points (pooled from the two experiments) makes much sense (Fig. S4 and S5, but also Fig. 3). At least for a better view on the data, you should identify the dots depending on the experiment and salinity levels.

5) It seems that you over-determined carbonate chemistry during the field survey by measuring pH, CT and AT. It is not clear to me if AT data showed (i.e. Fig. 7) are the ones measured or derived from pH and CT, maybe to clarify. Finally, how do computed AT and measured AT compare? This could be a nice way to identify DOC contribution no?

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