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Interactive comment on "Sensitivity towards elevated pCO_2 in great scallop (*Pecten maximus* Lamarck) embryos and fed larvae" *by* Sissel Andersen et al.

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The manuscript from Andersen and collaborators deals with an important issue related to the impact of ocean acidification on the early larval development of a commercially important species, the great Scallop. This is a concise and well written manuscript that clearly presents the results and provide clear interpretations without over-extrapolating these data based on a controlled laboratory experiment. For all these reasons, I would like to congratulate the authors and will definitely recommend this manuscript for publication in Biogeosciences.

That being said, I have few comments and suggestions I would like the authors to consider before potential acceptance by the editor.

I know the experimental procedure has been published in previous papers but I would strongly recommend the authors to provide more details on 1) how pH was controlled (this appears on Figure 1, but a small paragraph in the text would be useful for the reader), 2) how were pH and total alkalinity measured (I believe these are the 2 "measured" parameters as opposed to pCO2 that is computed) and 3) how did you compute the non measured parameters, CO2-sys? which constants were used? Please also explain how "live" larvae were identified in your formalin preserved samples.

I deeply regret that the authors did not measure and did not report pH on the total scale. For marine environments (their salinity level is 35.1), this is the recommended scale to use. This makes it harder to compare to other studies performed in marine waters as there are no easy way to convert between the 2 scales. Please have a look at the "Best practices guide" that has been released already some years ago: https://www.iaea.org/ocean-acidification/act7/Guide%20best%20practices%20low%20res.pdf. and please consider using this scale for future studies. Anyway, what is done is done, therefore I would strongly recommend to refer whenever possible to their treatments as offsets from the control: i.e. ∼-0.2 and -0.4 pH units, this would be easier for the reader to quickly understand what are the imposed perturbations.

The authors mention that these species is "commercially important" but do not provide justifications for it. Please clarify why this is a commercially important species and provide numbers (yield, income) for it.

As a concluding remark, the authors rightly recommend to conduct future studies considering variable levels of pH/pCO2 as it is the case in many coastal areas, they should further indicate that not only the magnitude of pH variability is potentially different as compared to the open sea but also average levels (especially for these epibenthic species) are certainly far from offshore levels. Just an occasion to insist on the fact that coastal monitoring stations are cruelly missing!

Minor comments: - Please notel that a space is always required between a number and its unit. I will not list them, but many spaces are missing. - P1L27: Not sure this is the right study to cite here, as this is limited to the Arctic. You could cite the meta-analysis of Kroecker et al. (2013) in GCB for instance. - P2L1: Same as above. This will look as self-publicity but you could consider citing Gazeau et al. (2013) in Marine Biology here. - P2L31: correct to: "in 38 L exposure tanks" - P2L33: the authors should mention the levels projected by 2100: -0.06 to -0.32 (by heart, check the right values) according to IPCC 2013. - P6L25: in the standard range of what? - P7L7: survival at day 7 seems higher than what is mentioned here, please check. - Table 1: What is CO2 in ppm ? This is still a partial pressure to me (or a fugacity?) - Figure 1: same comment as Reviewer#1 - Please consider presenting Figure 3 as Figure 2 is presented, i.e. Days in x-axis and pCO2 with different colours.

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