*COMMENT: Title should be rewritten. You do not provide evidence that the small increase in % of shelled larvae at elevated pCO2 is a compensatory mechanism, neither that the observed effects are a consequence of changes in calcium carbonate availability. This is pure speculation (and as you will see, I am very skeptical that this is what you see in your experiment). What you can conclude is that: Pacific oyster is experiencing a delay in development under short term exposure to elevated pCO2; (ii) this effect in only visible after 3 days (after they start feeding?) This should be what your title should be about.*

**REPLY:** Title has been changed to “Elevated pCO2 causes developmental delay in early larval Pacific oysters, Crassostrea gigas”

*COMMENT: Line 43, You need a sentence between 2nd and 3rd to make the link between OA and Pacific oysters (e.g. you could mention that oysters show negative impact to OA and that some crash in aquaculture industry is believed to be related to episodes of elevated pCO2 (the new paper by Dick Feely and colleagues).*

**REPLY:** Added the following: “This process, ocean acidification, is a future and contemporary concern for aquatic organisms, causing failures in Pacific oyster (Crassostrea gigas) aquaculture.” (lines 42-44 in revised version)

*COMMENT: Line 45, before explaining that you have exposed your larvae to different treatment, you should explain the aim of your experiment: test the impact of OA on C. gigas, and what is new in your experiment (from California where they already experience fluctuation in PCO2 due to upwelling) and what is your hypothesis (more resilient? More sensitive? E.g. pre-adapted to elevated pCO2?)*

**REPLY:** Added the following: “This experiment determines the effect of elevated pCO2 on the early development of C. gigas larvae from a wild Pacific Northwest population” (lines 44-46 in revised version). This sentence encompasses the aim of my experiment and how it differs from others. Due to the word number restrictions on the abstract for this journal, I don’t have space to include an explicit hypothesis, but the previous sentence (lines 42-44) implies that I expect these larvae to be negatively impacted.

*COMMENT: Line 46, replace "ppm" by "microatm" (using "symbol")*

**REPLY:** Replaced (lines 47-48 in revised version).

*COMMENT: Try to better describe the results (and avoid use "calcification", see below).*

**REPLY:** I have replaced “calcification” with other terms throughout the manuscript, such as “shell” or “calcified shell”

*COMMENT: Abstract should end with a conclusion (similar info that what I suggest for the title).*

**REPLY:** The last sentences now read, “This unexpected observation is attributed to increased metabolic rate coupled with sufficient energy resources. Oyster larvae raised at HighCO2 shoed evidence of a developmental delay by 3 days post-fertilization, which resulted in smaller larvae that were less calcified.” (lines 49-53 in revised version)

*COMMENT: Line 89, "the highest concentration?" This is probably true for open ocean but I doubt that this is true for coastal waters (e.g. tidal zones) and some specific environment (such as vents). Rephrase.*

**REPLY:** Rephrased to reflect nearshore processes: “During the spring and summer off the U.S. west coast, upwelling of waters rich in CO2 and respiration from nearshore biological activity can cause undersaturation of nearshore waters with respect to aragonite (Feely et al. 2008, 2010; Fassbender et al. 2011).  These processes occur in the same area where planktonic invertebrate larvae congregate” (lines 95-99 in revised version)

*COMMENT: Line 93, Remove ", R" in the reference*

**REPLY:** I have 2 different papers in the references published in the same year by authors with the last name Byrne so I need to keep the initial to indicate which one it is. (line 91 in revised version)

*COMMENT: Line 97, "larval C. gigas". This is the first reference to C. gigas, so it does sounds strange here. So remove this reference and just be general at this stage (planktonic species, including larval stages of many invertebrates?)*

**REPLY:** Changed to “invertebrate larvae” (line 99 in revised version)

*COMMENT: Line 99, I suggest to remove the sentence starting with "Aragonite?" See below for discussion on the proxy for animal physiology.*

**REPLY:** Sentence has been removed.

*COMMENT: Moreover: Line 103-111, I don't think this is needed. You are not studying fertilization and some of the text is not accurate (there is no evidence that OA impact pHi in eggs and prevent fertilization, this is just speculation in a review paper). If all this is removed, it links better with the previous sentence on planktonic larval stages.*

**REPLY:** I agree and have removed it.

*COMMENT: Line 112, this literature review is quite anecdotic. There are dozens of manuscripts dealing with larval stages, including many bivalves. Maybe focus on the bivalves here and precise duration of the experiments and pCO2 tested to help the reader.*

**REPLY:** I have deleted references to non-bivalves and refocused the summary on studies of bivalve larvae. (lines 102-121 in the revised version)

*COMMENT: Line 127, New paragraph on the model species, starting with "Pacific oyster".*

**REPLY:** Changed as suggested. (line 122 in revised version)

*COMMENT: Line 134, "empirical studies?" agreed but on the other hand, it is impossible to test ALL populations in ALL relevant environmental conditions (not mentioning that based on your argument, you should include other changing environmental conditions). So it is better to understand mechanisms.*

**REPLY:** Changed to “Thus, empirical studies are needed to understand the mechanistic responses of species to a specific environmental stress and how the stress corresponds to the species’ or population’s ecological niche.” (lines 129-131 in revised version)

*COMMENT: Line 135-150. Something that worth mentioning is that many bivalve species are living in very low pH and undersaturated waters, and reproduce with no problem.*

**REPLY:** Added the phrase “but some invertebrates that inhabit naturally CO2-rich environments are able to reproduce and the larvae settle without apparent adverse effects (Thomsen et al. 2010).” (lines 147-148 in revised version)

*KEY POINT 1: For example, Thomsen et al., show that in Kiel fjord experiencing frequent undersaturation, ecosystem is dominated by bivalves and even worse, they settle when the pH is the lowest. In deep sea vents, bivalves dominated communities are living close to vent with pH as low as 5.6 ! As a consequence, bivalves are able to live, grow, calcify and buffer pH changes in very acidic waters and omega below 1 is NOT a problem (and a parameter that does not make any sense from a physiological point of view). So I suggest to be very careful with this idea that things undersaturation is a key parameter. It is probably just a coincidence. What is probably more important is the niche of the species. It is striking that the limit of what your species can cope is the extreme pCO2 natural experienced today. As usual, species are able to cope with what they can predict and are facing stress when out of their niche. This is important to consider and I would not put too much emphasis on this in the paper since you cannot discriminate from your data between the real parameter that matter.*

**REPLY:** Agreed that niche is the most important thing.  I tried to clarify that the problem is that whatever environment the population is experiencing will change and that the focus needs to be on the mechanistic processes that are affected by environmental stress that occurs beyond the normal range.

*COMMENT: Line 166, You mention here that 1000 microatm is the extreme of the natural range today for this species and that it is also a scenario for the coming century (line 176). I see what you mean but that sounds contridactory. Just rephrase to mention at all tested scenarios are within today's range but that the species is expected to spend more than in the elevated pCO2 in the future.*

**REPLY:** Changed the last sentence of the paragraph to “Acidification events are projected to become more frequent and sustained as atmospheric pCO2 continues to rise”. (lines 168-169 in revised version)

*COMMENT: Line 187-189, delete from "These levels" to "(Feely et al. 2008, 2010). This is a repetation from the end of the Intro.*

**REPLY:** Deleted

*COMMENT: Line 217, precise sperm concentration and/or fertilization success.*

**REPLY:** Sperm dilution better quantified by the following: “Sperm was diluted (so as to approximate a 1:1 sperm:egg ratio)” (lines 213-214 in revised version). Unfortunately the sperm counts are not completely reliable so this is as accurate as I can be.

*KEY POINT 2: I have seen your answer to the referee but I still do not agree that a qualitative check of presence/absence of a shell or a fully developed shell is a proxy for calcification. Calcification is a process and you are just scoring stages. So do not use the working "calcification". This is confusing and you will have a lot of critics on this point. I suggest to rather replace by what you are measuring: shell presence, etc. For example, line 222 "presence or absence of a shell"*

**REPLY:** I have gone through and changed the word “calcification” to something more appropriate throughout the manuscript.

*COMMENT: Line 226, "Larvae were fed?" precise that it is ad libitum and level used in hatcheries (I guess) to ensure optimal growth.*

**REPLY:** Added “concentrations for optimal larval growth” (line 224 in revised version)

*COMMENT: However, you should also mention in the Discussion that using high level of food is probably leading to under-estimation of the impact of OA (see Thomsen et al.). Food is a modulating factor that can compensate for the stress.*

**REPLY:** Added sentence in Discussion: “It is also possible that the high level of food available to the larvae modulated the impact of ocean acidification and could have led to an underestimation of its effect in this treatment (Melzner et al. 2011)” (lines 407-408 in revised version)

*COMMENT: Line 230, "survival", it is unclear how survival was measured. Please explain.*

**REPLY:** Explained with the following: “Survival was determined at 20-40x: larvae were counted as dead if there was a complete absence of ciliary movement.” (lines 228-229 in revised version)

*COMMENT: Line 231, "degree of calcification", change (see below).*

**REPLY:** Changed to “shell presence/absence” (line 230 in revised version)

*COMMENT: Line 235, "calcification", change and you can then delete the next sentence "All calcification?"*

**REPLY:** Changed to “shell presence” and next suggested sentence deleted. (line 232 in revised version)

*COMMENT: Line 258, it is unclear how replicates were included in the analyses (e.g. for the morphological parameters).*

**REPLY:** All analyses used allow for keeping replicates within treatments separate so that differences between treatments can be robustly assessed.  Added the following: “At least two replicates within treatments and time points were used for all statistical analyses” (lines 266-267 in revised version)

*COMMENT: Results could be re-organized to be more logical.*

*1. Start with the Survival (lines 315-318) and precise the stat used and real stat values.*

**REPLY:** Moved survival to first paragraph of section and specified values for stats. (lines 287-291 in revised version)

*2. Score of development (lines 298- 302) and delete sentence 296-297. Then add the part of presence/absence of shells in the same paragraph (lines 309-314) but change the wordings (no "calcification", see previous comment).*

**REPLY:** Suggested changes made. (lines 292-301 in revised version)

*3. Part on larval size (Line 289-295 but add “did not increase SIGNIFICANTLY in size” line 294) + the part on allometry (line 302-306). Delete the sentence starting by “The similarity?” (line 306). This is an interpretation, not a result.*

**REPLY:** Suggested changes made*.* (lines 302-312 in revised version)

*KEY POINT 3: For me, the clear story is:*

*- You have a delay in development (shown with the stages, morphometrics and allometry) under elevated pCO2 with no increased mortality after 3d. The decreased number of fully formed shells is also probably a consequence of this delay.*

*- the second interesting observation is the apparent increase in number of  shelled larvae at day 1 in low pH.*

*This is interpreted as a "compensatory mechanism" or evidence that you can have acclimation. I do not believe so. This is for sure NOT an evidence of acclimation since you do have negative effect later in time. Moreover, it does not look at all like a compensatory mechanism for the same reason.*

*To me, it looks more like a "side effect" of a simple metabolic.*

*You have more and more literature showing an increase in metabolism under OA scenarios, including in larvae (e.g. Stumpp et al. 2011). This can lead to either an increase growth rate if energy is not limiting (e.g. Dupont et al. 2010) or a decrease in growth rate if energy is limiting (e.g. Stumpp et al. 2011). If you speculate that something similar happens in your species (increased metabolism), then you can expect a faster growth rate at early stage (d1) when the development is only based on stored energy reserved and problem later (d3) when the larvae is feeding (if pH has an impact on the energy acquisition and processing, as you discuss).*

*For me, that it is main difference between d1 and d3, it is pre- and post- feeding start. It is also important to remember that when larvae start feeding, the metabolism increases (with then more chance to reach tolerance limits).*

*Anyway, you do not have much data and you cannot extrapolate too much. Guessing a compensatory mechanism or evidence of acclimation is pushing too much. Same comment on seeing this as an evidence that omega=1 is a magic tipping point (even if I agree that it is in the air, mostly from the chemists, but has no relevance from a physiological point of view).*

**REPLY:** I have changed the Discussion accordingly. I no longer reference a compensatory mechanism and I focus on the change in metabolic demands between non-feeding and feeding stages.

*COMMENT: Line 321, I would start by explaining that there is a developmental delay (line 344) leading to smaller larvae at a given time, then compare with the literature (line 328). I sugest to remove Line 321-324. Then you can speculate on the cause (line 348-?)*

**REPLY:** Changes made as suggested (lines 315-332 in revised version)

*COMMENT: Line 365-371, not really relevant here. You do not have smaller larvae but delayed development (they will all go through all sizes, it is not miniaturization). Moreover, small size can also have some benefits (e.g. reduced predation).*

**REPLY:** I agree that this paragraph no longer really fits in the discussion and I have removed it.

*COMMENT: Remember to not use "calcification" for the parameter you measured.*

**REPLY:** I have made sure to go through and replace “calcification” with a more physiologically appropriate term

*COMMENT: Line 378-399. This part is not very convincing. I suggest to remove/reduce considerabley. But it does not make sense that you have different processes for calcification and response to acidosis at d1 and d3. See my previous key point. Your view is too anthropomorphic.*

**REPLY:** This paragraph has been changed to focus more on how the difference between days 1 and 3 is due to a change in metabolic demand, not a change in the process of calcification. (lines 354-384 in revised version)

*COMMENT: Line 401-403, NO, see previous point of calcification in under-saturated waters in many bivalve species.*

**REPLY:** Added in the phrase “in this population” to underline that biological niche is essential in determining physiological limits. (lines 404-406 in revised version)

*COMMENT: Line 416, see previous discussion on compensatory mechanisms.*

**REPLY:** Added the sentence: “The compounding negative effects of ocean acidification throughout an experiment may also be due to the species’ decreasing ability to tolerate a specific environmental stress as their metabolic needs change throughout development.” (lines 421-423 in revised version)

*COMMENT: Line 439, "acclimatize and adapt". There is nothing on adaptation (for a real paper on this, see Sunday et al) or acclimation (see Parker et al. 2012 or Dupont et al. 2012). Nothing in your data are suggestions any potential for acclimation or adaptation.*

**REPLY:** Sentence has been deleted and the next sentence has been changed to read, “In order to effectively evaluate the possibility of acclimation or adaptation, future research should focus on characterizing larvae from diverse genotypes and locations as well as assessing any influences that might be experienced later in development.” (lines 434-436 in revised version)

*COMMENT: Figure 2 and 3 and 5, add pCO2 on the graph (easier to read)*

**REPLY:** Added pCO2 onto graphs 2, 3, 4, and 5

*COMMENT: Table1, add variability on relevant parameters.*

**REPLY:** Added variability to temperature, salinity, total alkalinity, pCO2, pH, and carbonate concentration