

Interactive comment on “Impact of CO₂-driven ocean acidification on invertebrates early life-history – What we know, what we need to know and what we can do” by S. Dupont and M. C. Thorndyke

Anonymous Referee #2

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Dupont and co-authors review in this manuscript the current knowledge on the effect of ocean acidification on invertebrates early life-history. Although I am convinced that this is a very important topic, I do not recommend this paper for publication in Biogeosciences in its present version. Indeed, this is the second review in few months focusing on this topic after Kurihara's paper in MEPS (Kurihara, H. Effects of CO₂-driven ocean acidification on the early developmental stages of invertebrates. Mar. Ecol. Prog. Ser. 373, 275-284). The authors explain the need for a new review by the fact that Kurihara considered data that have been obtained under unrealistic pH

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levels (by unrealistic the authors consider pH levels that are under the levels projected for the end of the present century, -0.3/-0.4 pH unit) and those obtained by wrong manipulations of the carbonate system (addition of acid/base instead of CO₂ bubbling). While Kurihara compiled data from less than 10 publications, the authors of the present review had access apparently to more data. The problem is that most of these data have not been peer-reviewed and therefore should not be included. If we remove these data, only data from 6 publications focusing on 6 different species can legitimately be used. More importantly, the authors emphasize the strong variability in the responses to a decrease of pH of 0.3/0.4 pH unit and predict that there will be winners and losers. The studies that show increased survival and dynamics in tunicates and some echinoderms have not been unfortunately published yet. If unpublished data are removed for the analysis, I am not convinced the present manuscript is worth a publication.

The authors make a point that we should not focus only on calcification, I totally agree on that. I absolutely disagree when they say that OA “may” not impact calcifying larvae too strongly. As these organisms are mostly found in the coastal zone, it neglects the fact that many coastal areas are already more acidic than the open ocean and that the projected CO₂ increase in the next decades can bring many coastal areas below aragonite saturation (See Salisbury, J., Green, M., Hunt, C. & Campbell, J. Coastal acidification by rivers: a new threat to shellfish? Eos Trans AGU 89, 513 (2008)) for more information. What we don't know, and what the authors should develop in their discussion, is what is causing developmental dynamics to be altered at low pH, is it pH per se (disruption of many physiological processes), is it the associated decrease in the saturation state of seawater with respect to aragonite/calcite, a combination of both?

Although the paper is well written, the structure must be reconsidered and/or the titles of the different sections modified. For instance, the paragraph “What do we need to know ?” would rather be “How do we have to design future experiments ?”. See also comments from Referee#1.

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Specific comments: P3110, L6: unit and not units P3113, L12: Correct this sentence

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