

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

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Received and published: 22 May 2009

First we would like to thank the 3 referees for their extensive work on our manuscript, their constructive suggestions and interesting ideas. Despite some positive comments (“the paper is well written”, referee 2), the general tone of the reviewers is quite negative based on two key points: - The use of unpublished data - The recent publication in MEPS of another review on the same topic by Kurihara. However, we believe that our new review includes new and important information and ideas, not covered by the recent Kurihara review. In particular we remain convinced that there is an urgent need

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for a new review paper and that our manuscript provides this and, perhaps critically, suggested guidelines for future experiments in a very topical and important field. Indeed this urgency was communicated to us by the organizers in Monaco and was the reason we constructed our ms in this way.

We outline our responses to the referee’s comment below

1. The number of relevant publications doubled since the submission of the manuscript. This manuscript was written not to add one more in a field where there are more reviews than original papers but to provide an up-to-date vision of what is going on and what is needed. Investigations of the impact of OA on early development are very topical, for example, since the submission of our manuscript a few months ago, 7 new articles have been published: Arnold K E, Findlay HS, Spicer J I, Daniels CL, Boothroyd D (2009) Effect of CO₂-related acidification on aspects of the larval development of the European lobster, *Homarus gammarus* (L.). *Biogeosciences Discussions*. 6(2): 3087-3107. Byrne M, Ho M, Selvakumaraswamy P, Nguyen HD, Dworjanyn SA, Davis AR (2009) Temperature, but not pH, compromises sea urchin fertilization and early development under near-future climate change scenarios. *Proceedings of the Royal Society B* doi: 10.1098/rspb.2008.1935. Clark D, Lamare M, Barker M (2009) Response of sea urchin pluteus larvae (Echinodermata: Echinoidea) to reduced seawater pH: a comparison among a tropical, temperate, and a polar species. *Marine Biology* 156: 1432-1793. Ellis RP, Bersey J, Rundle SD, Hall-Spencer JM, Spicer JI (2009) Subtle but significant effects of CO₂ acidified seawater on embryos of the intertidal snail, *Littorina obtusata*. *Aquatic Biology* 5: 41–48. Gutowska MA, Melzner F (2009) Abiotic conditions in cephalopod (*Sepia officinalis*) eggs: embryonic development at low pH and high pCO₂. *Marine Biology* 156(3): 515-519. Munday PL, Dixson DL, Donelson JM, Jones GP, Pratchett MS, Devitsina GV, Døving KB (2009) Ocean acidification impairs olfactory discrimination and homing ability of a marine fish. *PNAS*. doi: 10.1073/pnas.0809996106 O’Donnell MJ, Hammond LM, Hofmann GE (2009) Predicted impact of ocean acidification on a marine invertebrate: elevated CO₂ alters re-

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sponse to thermal stress in sea urchin larvae. *Marine Biology* 156: 439-446. All these papers will be included in the revised version of our manuscript and more focus will be put on critical (but constructive) views of methods and how experimental design and open minded interpretation of results are relevant in the context of the future ocean and predictions. This is the core of our paper.

2. An urgent need for “good practice” The main reason why we decided to write this manuscript is the urgent need for baseline data to help design future experiments. The Impacts of OA on development is at the intersection of a number of disciplines and it is extremely difficult but nevertheless critical for researchers from these different disciplines to understand how to design and execute good and relevant experiments. For example, using “time” of exposure is probably not relevant knowing that OA affects developmental rates (see our manuscript) and may easily lead to the wrong interpretation. This can be observed in recent publications using interesting and original approaches but with a flawed experimental design (e.g. O’Donnell et al. 2009) where gene expression differences were observed at a given exposure time without any consideration for the developmental stages. It is then impossible to discriminate between the effect of pH and the effect of developmental dynamics. Other examples include the importance to consider longer term experiments and not extend conclusions from incomplete observations. For example, Byrne et al. 2009, base their broad and sweeping conclusions on experiments of only 36 hours. Clearly this is not adequate for larvae that might have a larval period of 24 or more days and over which time other phenomena will clearly become important (see our ms). Other shortcomings in published work that should be highlighted include the lack of relevant parameters such as carbonate chemistry (e.g. Clark et al. 2009). In such a rapidly moving field, we aim to provide balanced and constructive critical advice to help to the design of better experiments.

3. The review by Kurihara (2008) needs important updates As previously mentioned, many new articles have become available since the submission of our manuscript. This is even more apparent since the publication of the review by Kurihara in 2008 in

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MEPS. The Kurihara paper was submitted in 2007 and since then many new papers have been published that are not included in her review (e.g. our paper Dupont et al. 2008 in MEPS and many others as noted above and in our ms). The review by Kurihara is mainly a summary of her substantial contribution to the field and an update of her review from 2004. It summarizes her work and includes papers using quite dramatic approaches (e.g. use of acid or unrealistic pH values). We have decided to use another approach and focus only on data from more “realistic” experiments using CO₂ and pH values predicted for the end of this century. Our review is centered on an ecological perspective and experimental design.

4. Unpublished data can be presented as Supplementary material In any other discipline, our strategy should to focus on publishing first our original data and then work on a review. However, in an area such as climate change and ocean acidification where there is an urgent need for good quality data, we decided to write a review to provide a global perspective and important information for future experiments. Our contact with colleagues (for example at the Monaco conference) suggests that this approach would certainly be of general interest and we were in fact encouraged to submit such a manuscript. The consequence is that we have decided to include unpublished material. As with many other publications these days, this can appear as supplementary material, this is quite usual. So if acceptable we can do that. We understand that the three referees feel uncomfortable with the addition of unpublished data in the manuscript: “these non-peer reviewed results should only be included in this review article if (1) they are accompanied by the necessary qualifying information and (2) after reviewing this qualifying information, reviewers of the manuscript deem these studies worthy of publication. This is a very critical issue.” (Referee 1); “If unpublished data are removed for the analysis, I am not convinced the present manuscript is worth a publication” (referee 2); “At this moment, it is impossible to evaluate the effectiveness of this paper as it is unable to confirm the accuracy of the methods” (Referee 3). Our suggestion is to add information on our unpublished data as Supplementary Material: experimental design and basic data (e.g. survival rates, etc.).

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5. Other comments On the top of the two major key points described earlier, referees made other important suggestions that will be taken into account. For example: - Referees 2 & 3: "The manuscript would benefit from a more thorough discussion of the various patterns identified amongst the various taxa listed in Table 1" and "Discussion of adaptation potential In their manuscript, the authors offer only a limited discussion of the potential for and mechanisms of adaptation to ocean acidification. This section should be substantiated" - All the suggestion to improve the quality and the structure of the manuscript will be taken into account.

The referee 3 made two other suggestions: - "Though I agree that to predict "what will happen in marine ecosystem by 2100" is essential to focus on studies evaluating effects at CO₂ value expected for 2100, in terms of physiological or mechanistic or adaptive studies, I could not understand why authors should exclude all published data (there are several interesting data that could be discussed here) evaluating effects of higher CO₂ levels (e.g. 2000ppm; pH > 7.3) expected to occur in the far future (e.g. 200 or 300 years later)." - "Although authors pointed out the importance of [not focused only on calcifiers and more energy should be invested in the potential "winners"] at p. 9 line 20, they had completely ignored a mass of studied evaluating effects of hypercapnia on fish physiology (e.g. Ishimatsu et al. 2008). These studies could be a good example to understand why some species are very "strong" to the ocean acidification" The selection of pH values expected for 2100 is "arbitrary" but based on the fact that further predictions (200 or more) are more speculative and relate to other process (e.g. from an evolutionary point of view). These are of great interest from a physiological perspective . However to emphasize the importance of physiology as one of the key parameters likely to explain impact of OA, physiological process is not the aim of this review (it is very nicely done in the review by Pörtner 2008 in MEPS). There is indeed a lot of fascinating work on hypercapnia in fish physiology. If it is not mentioned in this manuscript, it is because our paper is only focusing on short developmental processes, mainly in invertebrates. For a recent review on adults and resistance to ocean acidification, see the recent review by Melzner et al. in BGD. We will cross reference this in

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our review.

We feel that such an approach is valid and timely. However, before embarking on such a substantial revision and including much new data then we need some assurance that this approach and format will be acceptable to BGD. We point out again that our approach in this article was very much encouraged at the Monaco congress by those involved in seeking manuscripts for this volume. This is why we did it in this way since we thought and were advised that this would be welcomed by BGD. In this respect we are a little disappointed that now, after a lot of work, referees' are saying this is not what is needed. We should very much appreciate some guidance on this. Of course it is quite correct to have articles peer reviewed but when the structure and approach recommended is criticized then we become puzzled. We were only producing what we thought was required.

We should be grateful for your advice on the most efficient way forward with our manuscript.

Interactive comment on Biogeosciences Discuss., 6, 3109, 2009.