

## ***Interactive comment on “Is the perceived resiliency of fish larvae to ocean acidification masking more subtle effects?” by E. C. Pope et al.***

### **Anonymous Referee #2**

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This paper makes a valuable additional to knowledge on the potential effects of climate change and ocean acidification on marine fishes. The ms is generally well written and easy to follow. Similarly, the experiments appear to have been appropriated conducted. Although there is an issue of pesudoreplication (many fish reared in the same tank), this is unavoidable due to the difficulty and complexity in rearing larval fishes; it would be effectively impossible to do the study any other way. Importantly, the study employs replicate rearing tanks to help overcome “tanks effects”. Overall, this is a very good study that clearly shows that this population of sea bass is resilient to projected future CO<sub>2</sub> levels. I do, however, have several major comments, the most important of which relates to the interpretation of the results, especially the idea that this study somehow shows that there are subtle negative effects being masked in short-term experiments. The major effects of temperature and CO<sub>2</sub> reported here (increased survivorship and

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growth) are anything but subtle, and it is hard to see how they could be interpreted in a negative context. While aerobic scope declined at higher pCO<sub>2</sub> and temperature, and some prominent hypotheses might consider this to be detrimental, it is associated with better survivorship and growth rates in this study, so any argument that a decline in aerobic scope is bad for the fish studies here requires that the other results be ignored. The study overwhelmingly finds that seabass are resilient to elevated pCO<sub>2</sub>. As a result I find the title, and parts of the abstract, to be misleading. The headline here is that these fish appear to do better (increased survival and growth) under elevated CO<sub>2</sub>, as has been reported in some other studies. It nearly seems like the authors are anxious to report a negative effect when none exists. This problem can easily be overcome in revision, and I think this is essential to do this to avoid the findings being misinterpreted.

My other major comment is related to the reporting of pH values and the calculation of pCO<sub>2</sub>. pH is reported on the total scale and pCO<sub>2</sub> has been calculated accordingly in CO<sub>2</sub>SYS, but the methods state that NIST buffers (7.0 & 10.0 from WTW) were used to calibrate the pH electrode. As far as I can tell, this means the pH has been measured on the NBS (NIST) scale. TRIS buffer would be required to measure on the total scale. If the pH values in Table 1 are NBS (about 0.15 lower than total scale on average) then the calculated pCO<sub>2</sub> would be closer to 1000  $\mu$ atm for pH 7.82. Clarification is needed on the pH scale used and the pCO<sub>2</sub> calculations.

Specific comments.

Title: As above, I find the title to be very misleading.

Abstract: More could be done to report the important findings about increased survival and higher growth at high pCO<sub>2</sub>. For example, what % changes were involved at the various key sampling stages. I suggest that less text be wasted on speculating about possible “masking of subtle effects” by other studies. This “masking effect” was not tested in this study, nor is there any strong evidence for it in the results.

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Abstract, line 14. Why does the new sentence here start with a “However”? The implication is that in the juveniles in the high CO<sub>2</sub> treatment do worse than the larvae, but in fact they continue to grow faster than controls. This is a “similarly” statement, not a “however” statement. The finding of reduced factorial aerobic scope at higher temperature and pCO<sub>2</sub> could be separated out and reported by itself, but even then, there is no evidence that this is detrimental to the fish because it is associated with higher survival and faster growth.

17046, line 3. Please change “impact” to “influence”. Impact implies negative effects, but some of the studies you cite report positive effects of ocean acidification on fish life-history traits.

17047, line 2. It is confusing having such a large section of text in parentheses. I suggest you remove them and place this information within the main text.

17047, line 24. What do you mean by pHT? pH<sub>total</sub> (pH measured on the total scale)? If so, how did you measure this with NIST buffers. See comment above.

17048, line 5. While you could “calibrate” the hydrochloric acid used for titration, I don’t think this is what you mean. I assume you mean the TA results were validated using certified reference material from the laboratory of Andrew Dickson.

17049 and elsewhere. Daily mortality seems meaningless for a 42 day study where you only have numbers at the start and end. You have no way of comparing the shape of the mortality curve, which would be the main reason to estimate daily mortality. I suggest you simply stick with survivorship at the relevant time point(s).

17053. I would have liked some detail here on the statistical methods applied, instead of waiting for each result to find out what was done. It would be particularly helpful to explain how tanks were included in the analysis (if they were).

17054, line 3. You say that TA did not vary substantially between tanks. In fact, Table 1 shows that mean TA and standard deviations are identical for all tanks within each

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treatment! This is almost impossible given instrument error in measuring TA. Presumably these are mean values for the treatment. Clarification needed here.

17054. The first para of results is redundant as it simply repeats the values in Table 1. I suggest you refer to Table 1 in the methods (at the end of the seawater chemistry section) and delete lines 6-13 in this section.

17054. "All tanks contained larvae at d42" But the methods say some tanks were empty at this stage. Clarification please.

17056. Please report the results for absolute aerobic scope separately to those for factorial aerobic scope. I found this section very confusing and it was difficult to decipher which treatments were significantly different from which others. Is it only at high temperature that elevated pCO<sub>2</sub> has an effects on scope? A more careful description of the results is required.

17057, first sentence of Discussion. It is very important to have this strong statement of the major findings at the opening of the discussion. My only suggestion is that you insert the caveat "appear" to be resilient, because you have not assessed the whole life history and do not know if these effects will be directly translated to natural populations.

The discussion is generally very good, although it could probably do more to explain the consequences of increased survival and faster growth of larvae and juveniles to individuals and populations. I would also like to see some discussion on why survival and growth were higher in the high CO<sub>2</sub> treatment. What mechanisms could be involved? My only concern is with the second last para where you discuss the decreased aerobic scope in fish raised under chronically elevated CO<sub>2</sub>. You should make it clear this only happened in conjunction with higher temperature. It is also worth pointing out that despite having lower aerobic scope, these fish had better survivorship and growth than fish in the other treatments – lower aerobic scope doesn't seem that bad after all!

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Interactive comment on Biogeosciences Discuss., 10, 17043, 2013.

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**BGD**

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