

Interactive
Comment

Interactive comment on “Effect of ocean acidification on early life stages of Atlantic herring (<i>Clupea harengus</i> L.)” by A. Franke and C. Clemmesen

Anonymous Referee #1

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This study examines the important question of whether ocean acidification will affect the early life history stages of Atlantic herring, a commercially and ecologically important species of fish. Very few studies have examined the effects of ocean acidification on marine fishes, with nearly all research to date involving tropical reef fish; consequently, this study makes a valuable contribution. Consistent with the work from tropical reefs, rearing eggs in acidified water had no detectable effect on fertilization success, embryonic development, hatch rate, length and weight at hatching and yolk size. Furthermore, there was no clear effect on otolith size, although there was a trend for otoliths to be larger at the highest CO₂ levels. These are novel and important results that will be of broad interest to climate change scientists and fisheries managers.

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The study detected a significant decline in RNA in acidified fish compared with controls, although the ecological relevance of this phenomenon remains unclear given that none of the key life history traits (e.g. size at hatching, yolk area) were affected. A major strength of the study is that a comprehensive suite of early life history traits were examined and were largely unaffected even at CO₂ levels much higher than predicted to occur in most parts of the shallow ocean by the end of this century.

I have a few specific issues that should be considered in a revision. The text also needs a careful edit to improve clarity and to correct some awkward phrases and sentences.

1. Seawater was acidified with strong acid (HCl) in a closed system. Although this replicates the pCO₂ environment expected for a given pH, it causes a decline in total alkalinity (TA) that does not occur with CO₂-acidification. This decline in TA is evident in Table 1. Although the decline in TA is unlikely to have had any effect on the life history traits measured, because the physiological processes of the fish are responding to rising pCO₂, not TA, I think it is important that the authors explicitly mention this issue in the methods, especially for readers who are not expert in the chemistry of ocean acidification manipulations.

2. I assume from Table 1 that bicarbonate and/or carbonate additions were not used to correct TA in the experiment. Although not important to the life history traits measured, this might have some relevance to the otolith analysis. The reduced bicarbonate and carbonate concentrations in the acid-treated water, compared with that predicted for CO₂-treated water, could conceivably influence the concentrations of these ions in the plasma and otolith endolymph. Although any effects are likely to be trivial, because the concentration of bicarbonate in seawater remains high and should not affect the ability of fish to transport bicarbonate from the ocean to plasma for acid-base regulation (the major mechanisms that fish use to compensate for acidosis). This limitation in the experimental methods should at least be mentioned in the discussion.

3. In the abstract, and to a lesser extent in the discussion, the authors make claims

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about how reduced RNA levels could affect somatic growth and survival of larvae that are not supported by the results. They even extrapolate out to ecosystem and fisheries impacts without support from their own data. I feel this inaccurate interpretation must be corrected. Yes, the RNA/DNA ratio was different in fish reared in acidified water compared with controls, but there were absolutely no differences in the key life history traits measures, including length weight and yolk area. Therefore, there is no evidence of a link between RNA levels and early life history growth rates in this species. Variation in RNA/DNA ratios often do not correspond with variation in life history traits and the predictive power of these ratios in an ecological context is highly questionable. I urge greater caution here.

Technical corrections

Page 7099, line 5. Awkward and unclear sentence. “.... a further rise up to 450 respectively 1100 ppm by the end of the century...”

Page 7099, line 18. “used to focus” should be “have focussed on”

Page 7099, line 27. Taxa is confusing in this context. “different groups of organisms” would be better

Page 7100, line 7. Change “Unfortunately, only few” to “Most”

Page 7100, line 9. Delete “most of the focussing”

Page 7100, line 10. Finish sentence after ...Dupont et al. 2010). Start new sentence with “These studies indicate...”

Page 7101, line 3. State units for salinity.

Page 7101, line 25. What do you mean by “aimed pCO₂” in this sentence? Do you mean “pCO₂ was calculated in CO₂SYS based on measured CT and AT”?

Page 7104, line 13. The methods for estimating carbon parameters need to be set out in the methods, not here in the results. Also, if you didn’t use CT as stated here,

then you cannot say in the methods it was used to calculate pCO₂. This needs to be clarified. Seems you used pH and TA to calculate pCO₂.

Page 7104, line 15. Oxygen would be better presented as % saturation. 7.2mg/l will be meaningless to most readers.

Page 7104, line 17. Information on actual temperature must be in the methods, not result.

Page 7106, section 4.1. The first para of the discussion repeats the introduction and can be deleted.

Page 7106, line 21. I suggest the statement “since non-significant results are inconclusive” should be since non-significant results can be inconclusive”

Page 7109, lines 11-18. The conclusions in this para are not supported by the data - there is no evidence that growth is affected by acidification in the study species.

Page 7109, section 4.3, para 2. You need to start this para by reminding the reader that although there was no significant effect of acidification on otolith area, there was a trend for greater area at higher pCO₂, consistent with previous studies by Checkley et al. 2009 and Munday et al. 2011. Otherwise the para has no context.

Page 7109, section 4.3, last para. You need to remind the reader that there was no effect of acidification on sagitta, but an apparent increase in lapillus area.

Table 1. In the text you report there was a problem with CT samples and could not be used (P. 7104, line 13), yet they are reported here. Are these estimated in CO₂SYN. Please clarify.

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