

Title: Marked recent declines in boron in Baltic Sea cod otoliths – a bellwether of incipient acidification in a vast hypoxic system?

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Summary:

The authors examine B:Ca changes in cod otoliths across a 30+ year period in the Baltic Sea where there is a good record of environmental data that are hypothesized to drive observed ratios. The article was generally well written with appropriate references. With a few minor to moderate clarifications I suggest below in the Concerns section, would make an excellent addition to the wider otolith chemistry literature where researchers are diving much deeper into the meaning of many of the markers used for stock discrimination and uncovering useful relationships with environmental variables. The Comments section are mostly editorial changes that help readability or alternative words that create clarity.

Concerns:

Line 126: The spot size (110 μm) is large and travel speed ($7 \mu\text{m}\cdot\text{sec}^{-1}$) is slow relatively speaking. How many data points is the ICPMS collecting across the full transect? Many of these points are going to overlap, though the repeated measures analysis used does account for this by nesting age within fish. Figures 1 and 3 both indicate much more variation than I would have expected using this combination of spot size and travel speed. My expectation would have been a much more muted record with smaller peaks and valleys.

Line 142: The analysis section could be clarified somewhat. While I understand the mechanics of the comparisons made, I did need to read this section a second time when I got into the results. Line 155-158 are the bigger picture analyses. It would be helpful to highlight this analysis more by reordering the paragraph or adding to these lines so that it leaves a larger impression later in the manuscript.

Line 135: How much does the selection of different water depths to average (pre and post 1995) impact the results? I presume cod were found deeper post 1995. If just cod from overlapping depths (40-60 m in this case) were chosen, would the differences have been as dramatic? While 10-15 m is not much in the open ocean, in the nearshore environment, small changes can dramatically affect chemistries due to terrestrial inputs. Figure S2 illustrates this concern well. While the 40-75 m and 30-60 m annual averages follow similar trends, there are times where the differences are large particularly across the recent 2000-2020 period.

Line 185: Why do the degrees of freedom differ in table 1? I would have expected df to be the same for each comparison. Are some of the annual environmental parameters missing? While an exact accounting of missing data isn't necessary, the existence should be stated in the methods.

Line 247: This paragraph seems to be an orphan in the discussion. It is useful information and would fit better in the paragraph beginning on line 60.

I did not read the Journal guidelines but I do not like a heavy reliance on supplemental information. It's certainly not a deal-breaker and I do understand page limitations for print. My

preference would be for greater inclusion in the manuscript or simply writing with less reliance on supplemental figures.

Comments:

Line 21: anti-correlated should be inversely correlated

Line 39: built could be composed

Line 151: reducing instead of reduced

Line 234-237 should be moved to the discussion