Geoscientific

Model Development

Discussions

[GMDD]

Interactive

comment

Interactive comment on "ShellTrace v1.0 – A new approach for modelling growth and trace element uptake in marine bivalve shells: Model verification on pacific oyster shells (Crassostrea gigas)" *by* Niels J. de Winter

Anonymous Referee #2

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Review of de Winter "ShellTrace v1.0 - A new approach for modelling growth and 1 trace element uptake in marine bivalve shells: Model verification on pacific oyster shells (Crassostrea gigas)"

This manuscript evaluates the trace elemental composition of the shells of the pacific oyster using an integrated proxy and modelling approach. There is currently significant debate in the peer-reviewed literature regarding the use of trace elemental ratios derived from marine bivalve molluscs as palaeoenvironmental proxies. This debate is largely associated to developing an understanding of the contribution of environ-

mental conditions and vital effects have on the trace elemental composition. These effects have been reported to create significant heterogeneity in the trace elemental composition of bivalve calcium carbonate and thus developing a robust quantitative understanding of these effects is required if we are to reach a stage where trace elemental ratios could be applied in a robust palaeoenvironmental application. Therefore a detailed study utilising modelling and actual shell data is very interesting.

This study takes an interesting approach to developing an understanding of the relationships between shell growth metrics, age and trace elemental composition. I think that approaches undertaken in this manuscript could have the potential to significantly develop our understanding of the drivers of trace elemental composition in oyster shells and potentially other long-lived marine bivalves, if more widely applied. However, as the manuscript is currently presented there appear to be significant flaws in the methodology that lead to the stated conclusions being unsupported by the presented data. I have serious misgivings about the assumptions that appear to have been applied in the development of the model and in the manner in which the data are presented. The manuscript therefore needs significant revisions before it reaches a publishable standard.

General concerns:

My main concern revolves around the generation and interpretation of trace element data, and the lack of an independent age model based on the interpretation of growth increment patterns. Firstly, the line used for sampling the trace elements, as shown in Figure 4, does not follow the axis of maximum growth (which is the conventional approach for sampling geochemistry in shells) and is not perpendicular to any of the growth increments sampled. This means that the temporal averaging of the trace elemental ratios in each analyses in each increment is inconsistent. This makes it very difficult to compare data between increments. Other complications such as the potential of non-linear seasonal growth rates are not considered. It is not clear from the data that are presented that there are any consistent seasonal patterns in the elemental

ratios. The author must provide a statistical assessment of the mean seasonal trend for each element analysed. As this is not clear it is near impossible to evaluate if the relationships between elemental ratios and age is robust. I go through each of these points in more detail below.

Detailed comments:

Line 38-39 – This is a strange list of references to use. There are many more papers that arecurrently already published that would be better to cite here. For example (Butler et al., 2013, Butler et al., 2011, Reynolds et al., 2016, Wanamaker et al., 2008a, Wanamaker et al., 2008b, Wanamaker et al., 2011, Schone et al., 2003, Schöne et al., 2011, Jones, 1980, Jones et al., 1989, Witbaard et al., 2005, Witbaard et al., 1997, Witbaard et al., 2003, Witbaard et al., 1994, Swart et al., 2010)

Lines 41-42 – The author needs to cite a better range of literature.

Line 47 – should not cite work that is not published (remove deWinter et al., PPP)

Line 39 – not all bivalves are calcite, change to calcium carbonate. This term bivalve calcite is used throughout the manuscript and should be changed.

Lines 50-52 it would be useful for the author to expand on this statement to say exactly which trace elements have been used to examine which environmental archives and importantly from which bivalve species these studies were conducted. MY experience is that there is a lot of contradictory literature surrounding the application of trace elements as environmental proxies and so a far more detailed discussion around this is needed.

Lines 53-59 – Again the author needs to provide a far more detailed discussion of what these previous studies into oyster shells found. "There is some discussion about the role of these calcite mineral phases, whether their precipitation is controlled by environmental conditions and whether changes in the precipitated mineral phase are paced to regular (solar or lunar) cycles" So what do they actually say in this discussion?

What environmental factors do they suggest are responsible? Were there any issues identified in these studies? The author needs to expand significantly here.

69-71 – The author needs to be more concise in the structuring of sentences throughout the manuscript. As it is currently written it is quite difficult to read or. One example for sentences that could be reduced is: "Furthermore, several studies have shown that rates by which trace elements are incorporated into bivalve shells and the degree to which these rates are controlled by environmental factors can be vastly different between different bivalve species". This could be condensed to, for example, "Studies have shown that the rates of incorporation, and environmental response of, trace elements in bivalve shells varies between species"

77-85 – this section reads far too much like a methods summary and not a set of aims or hypotheses that this study aims to test. This should be rephrased.

88 – I presume not, but please state whether the shells cooked by the restaurant before they were analysed in this study? If so would this have had an impact on the elemental composition of the shell?

90 - from what water depth were the shells collected?

105 – clarify that you mean the polished shell surface.

112 – "using the point-by-point scanning method outlined in de Winter et al. (in review, PPP)" Firstly you shouldn't cite work that is not published. Secondly we cannot see what this method is as it hasn't been published. We therefore cannot assess if this is a robust methodology. The author should elaborate on the details of this methodology.

281 – why would you estimate age based on proxy records? Growth increment analyses are a far more robust measure of ontogeny.

309-311 - "Shell increments used as tracers for growth modelling are generally characterized by decreased Ca and Mg concentrations and increased concentrations of Fe, Mn, Zn and Sr." This is not obvious from Fig 4 at all. I personally cannot see this inter-

pretation in these data the way they are plotted. If there is a consistent response in the elemental ratios throughout the year, with either peaks or troughs corresponding to the growth lines then the authors should generate a mean seasonal elemental curve using these data and plot with error bars the mean seasonal change in each element. This will then provide a more rigorous assessment of whether the ratios do indeed show a change occurring around the timing of the growth check forming. However, I am concerned by the trajectory that was used to generate these data as shown in Figure 4 (line A-B). The XRF sampling line (line A-B) does not follow the axis of maximum growth through the hinge region of the shell (the maximum growth appears to follow the position of the numbers 0-7). The position of the sampling line as is not perpendicular to any of the growth increments meaning the temporal positioning of each set of analyses through these increments is not consistent. This will lead to large changes in the temporal smoothing of the data through the timeseries and lead to significant challenges in interpreting the data. For instance this means that comparing one year to the next is very difficult as they incorporate a different temporal smoothing. The plotting of these data against a constant time axis is therefore inappropriate. Plotting the data like this makes the assumption that the growth of these shells is constant throughout the growing season and that the growing season is 12 months. Both of these assumptions are likely false as has been found in many other species of bivalve. The author needs to provide a far more robust approach to determining the timing and rate of seasonal growth before these data can be plotted against a time axis.

Fig 4: This figure is extremely complicated to understand. The impression this figure gives me is that the sampling strategy of this study is fundamentally flawed. For example, the black lines, which apparently correspond to the growth increments show there are 7 increments yet these do not appear to line up with annual chronology in any way? Why is this? Also the panels on this figure need to be labelled and referenced more clearly in the fig legend.

312-313 – this needs to be tested.

313-314 – why did the authors use the elemental ratios to determine an age model when the growth increments would have provided a far more robust, and independent age model? Based on the data that is displayed in Figure 4 it is extremely difficult to see any consistent patterns that would facilitate the attribution of an age model with any degree of confidence. As mentioned above, if the elemental ratios are to be used as an age model, the data from each increment should be averaged to generate a mean seasonal curve for each element. These data, with associated error bars could potentially be used to assign ages, providing the data demonstrate a significant seasonal pattern.

315+317 – "Note that line scanning through the hinge of the shell only allows for the sampling of the last three growth years, as the irregular shape of the oyster shell and the occurrence of chalky calcite further up the hinge prevents the measurement of a complete record through the foliated calcite" The author needs to explain why this line was used for sampling and why the line that follows the position of the numbers in Figure 4, lower left panel, which contains 7 growth increments wasn't used. Using this line would have facilitated sampling through 7 increments.

326 – Growth model section – I really don't understand why the author has not used the visible growth increments to develop the age model and instead used the trace elemental records. I am not convinced that the geochemical signatures provide a robust signal for age model development based on the data that has been presented in this manuscript. The author needs to provide significant new data comparing growth increment based age models in relation to the geochemical records before these analyses can be assessed in a robust manor.

344 – How does the Von Vertalanffy R2 statistic (0.60) compare to that of other bivalves?

391- Trace element model section. In this section the author presents "Records of trace element accumulation rates and total shell trace element concentrations" and

discusses these data based on Figure 6 which plots these data against a time axis over the period from 2014-2017. As mentioned with Figure 4, these analyses assume a constant seasonal growth rate and that growth occurs all year round. Yet no evidence or citations are provided to support this claim.

Figure 6 – remove the scan of the shell in the lower panel as it is not needed. Also all the panels should be labelled (A, B etc.)

403-404 - "These differences in total shell concentrations and concentrations in the shell hinge illustrate the value of the proposed trace element modelling approach" To be honest I am not sure why this is important. In the application of sclerochronological trace elemental ratios we are looking to explain the patterns of variability that we can measure along a 1 or 2 dimensional growth axis in the shell. Whilst it is of interest to understand the reasons behind heterogeneity in trace elemental ratios across the sampling plane, we are rarely interested in total shell elemental composition. The big question that needs addressing with trace elemental work is to what degree is the variability in trace elemental ratios, sampled along the axis of maximum growth, due to vital effects and what proportion is due to changes in ambient water chemistry. The author does hint at this in lines 406-411 however, I feel that this has not been achieved by this study.

418-419 – I strongly disagree with this statement. There is no evidence that this is the case presented in this study and should be removed.

422-424 – This statement if wrong. The assumptions made in this study if applied to other marine bivalves such as those commonly used in sclerochronology (e.g. Arctica islandica) would likely not hold. This study assumes constant annual growth which is not the case for A. islandica.

Suggested references

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