Dear Alberto Reyes and Andrew Johnson,

Thank you both for your comments and suggestions, which have improved our manuscript in this second review round. We are glad to read that you both agree with our revisions in the previous review round and are happy to follow your suggestions for the final minor revisions before the manuscript is published in Climate of the Past.

Please find below our point-by-point rebuttal (in **black**) to the last round of comments (in **red**), as they were provided in the online submission system. In addition to the comments below, both of you provided (digitally or manually) annotated PDFs with in-line comments. Since these comments were quite minor and specific and since we followed all the suggestions in these PDFs, we have decided to summarize the major points in these annotated files into our line-by-line rebuttal here.

Comments to the Author:

Dear Dr. de Winter,

Your manuscript has now been assessed by a third independent reviewer. Their review is positive and points to the importance of this comprehensive manuscript. You'll see that there are still some suggestions for increased clarity through some minor revision. I concur with this assessment, and add my own comments for improved clarity and some potential organizational changes in the attached annotated PDF. Accordingly, I'm recommending that the manuscript be accepted for publication pending minor revisions, to address my comments and those of the third reviewer.

Sincerely,

Alberto Reyes

We would like to specifically thank the editor for taking the time to provide detailed feedback on our manuscript in both review rounds. We implement his comments provided in the annotated PDF in an attempt to improve the structure of our manuscript and clarity of our formulation where necessary.

In reply to the editor's in-line comments, we modified the order in which our test cases are presented in the Methods section. We decided to keep the order of presentation in the Results because reversing the order would compromise the structure of presentation there.

We also followed the editor's suggestion to present our three types of cases in terms of measured vs. virtual environmental and proxy data to further clarify the difference between the three categories.

Throughout the manuscript, we revised all references to "precision error". We agree that this formulation is confusing and chose to only refer to low or high precision or directly to precision standard deviations if the reference is to the numeric result specifically. We also removed a few instances where "reproducibility" was used and rephrased this to "precision" to be consistent on terminology.

The introduction now includes a few sentences introducing the issue of age modelling within seasonally resolved records, as suggested.

Comments by Andrew Johnson

This is a thorough analysis of the factors affecting the accuracy and precision of environmental information determined from stable isotope data. It shows how their effects may be mitigated by combining/aggregating data in various ways and concludes with recommendations for the best approaches to adopt in particular circumstances and to achieve particular ends.

This is a complex paper and the previous version was evidently difficult to follow (see the comments of Reviewer 2 especially). The intended changes signalled in the authors' response to the comments of reviewers 1 and 2 have been implemented such that all the specific points have been addressed and clarity improved. The inclusion of the heatmaps (Figs. 6 and 7) suggested by Reviewer 1 is a good example of a very positive response to comments on clarity. Nevertheless, the text is still difficult to follow in places due to insufficient explanation, small errors, lack of reference to figure parts, etc. I have made comments and suggestions at the relevant points (highlighted text – sometimes very short sections) in the attached annotated version of the manuscript. These should all be considered and changes implemented as appropriate.

We thank the reviewer for his detailed comments in the annotated PDF and did our best to implement these in our revised manuscript in an attempt to make our discussion easier to follow.

In identifying statistical 'solutions' the paper points up data 'problems'. The latter are being reduced by analytical improvements (e.g. the sample size needed for Δ47 has reduced in recent years and the analytical precision has, I think, increased) and, as the authors briefly indicate (e.g. the larger number of samples obtainable from the early, rapid stage of shell growth), data quality can be improved by appropriate targeting. Given the confounding effect of δ18Ow variation on estimates of temperature from δ18Oc, which the authors amply demonstrate, I really think they should take the opportunity to recommend that δ18Oc-based estimates of marine temperature are not conducted on euryhaline organisms. There are plenty of stenohaline marine organisms (including some that grow fast and continuously in early ontogeny) that are far better targets than the euryhaline oysters investigated in many previous works!

We agree with the reviewer that the effect of $\delta^{18}O_w$ variability (especially on the seasonal scale) on $\delta^{18}O_c$-based reconstructions is one of the main issues our study addresses. Indeed, the growth locality of the carbonate producer (e.g. estuarine vs. full marine conditions) strongly controls the size of this problem. However, while euryhaline organisms (such as oysters) generally grow in environments with more salinity (and thus $\delta^{18}O_w$) variability than stenohaline species, this does not, in our opinion, disqualify $\delta^{18}O_c$-based reconstructions from euryhaline species, nor does it entail that $\delta^{18}O_c$-based reconstructions from stenohaline organisms are not affected by this $\delta^{18}O_w$ variability. In reply to this comment, we discuss the difference between euryhaline and stenohaline organisms in a more nuanced way, stating that the problem of $\delta^{18}O_w$ variability plays a larger role in euryhaline organisms while the effect of $\delta^{18}O_w$ variability should be considered in every $\delta^{18}O_c$-based reconstruction.

Identifying suitable targets, referring to ongoing analytical improvements, and mentioning the possibility of constraining δ18Ow by modelling (see comment at line 505), would enable a slightly more positive/optimistic tone in the conclusions.

This is a good comment, and we use the reviewer's comment on line 505 as an opportunity to mention some of these techniques as recommendations for constraining $\delta^{18}O_w$ variability in $\delta^{18}O_c$-based reconstructions (lines 582-589 of the revised manuscript). In the end, our recommendation

remains that actual independent temperature or $\delta^{18}O_w$ reconstructions (e.g. using the presented Δ_{47}-based approach) should be favoured over these methods of approximating $\delta^{18}O_w$.