Author's response file.

All line calls in this file refer to the Author's track change file.
It is presented as follow:

- Referee comments
- Author's response
- Changes to the manuscript

Comments from Anonymous Referee #1

In the Introduction (lines 47-107), several potential temperature proxies in brachiopod calcite are introduced one by one. While these introductions are important and well written, the individual paragraphs are quite detached from each other and disrupt the flow of the manuscript somewhat. I suggest the authors either tie the paragraph a bit better into the rest of the manuscript or place them in a separate "Background" paragraph.

We will rework the introduction to better tie the paragraph into the rest of the MS.

lines 47-107: Several minor additions were made to the paragraphs concerning the different potential proxies, with the aim to better tie them together.

Lines 156-161: Perhaps the authors can provide a citation or reason for why they used this pre-treatment method. There is some literature suggesting that pre-treatment with oxidizing agents might influence the (clumped) isotope or trace element composition of the carbonate. Personally, I am of the opinion that excess pre-treatment with such substances should be avoided in these types of studies. However, if the authors have convincing evidence (either by their own research or from the literature) that this treatment is warranted in this case, I am happy to support it.

The literature appears divided on the subject (Schöne et al., 2017; Key et al., 2020; for the most recent literature). We have chosen to perform an oxidizing pre-treatment as most of the samples still had the animal within their shell. Most of the organic matter was removed manually but an oxidizing pre-treatment was deemed necessary to remove organic matter still attached to the shell before sampling for shell geochemistry. Although, we acknowledge that this could have been avoided for samples without clear evidence for organic material. Among the different pre-treatment method used in previous studies, we adopted a protocol close to the one used by Bajnai et al., (2018) as we preferred NaClO over H_2O_2 for logistical reasons. We note that bleaching is associated with little to no carbonate dissolution as opposed to H_2O_2 (Pingitore et al., 1993; Gaffey and Bronnimann, 1993).

There is no modification associated with this comment.

Section 3.1: It is not clear from this section how the authors dealt with uncertainty on the temperature and salinity/d18Ow value associated with the samples. Since brachiopod samples are large and do not always represent mean annual averages (i.e. due to sampling of less than a full year, see lines 144-146, or due to variations in growth rate over the year) I think the authors should take into account the seasonal cycle in temperature and d18Ow (or salinity) at the sampling locality as uncertainty on their regression. I assume from the text in this section that the authors used a normal linear regression (not including errors on measurements or on the independent variables). To incorporate uncertainty on the independent variable, the authors could use a Deming regression which takes into account measurement error as well as errors on the "known" variable (in this case temperature and d18Ow). Judging from Figure 1 (which I assume shows uncertainties on temperature), these uncertainties are significant.

The uncertainties displayed in Figure 1 corresponds to the seasonal variation for temperature (highest and lowest monthly averages), and for $\delta^{18}O_w$ the uncertainty combines uncertainty of the salinity- $\delta^{18}O_w$ relationship (LeGrande and Schmidt, 2006) and propagated seasonal variation in salinity, although it is very low for most samples. The regression presented in Figure 1 and Table 3 are simple linear regressions which are primarily used to explore the dataset. While the linear model was proposed in the submitted manuscript for the fractionation equation, we take note of the point made here, and will rather display the results of the York regression (York et al., 2004), which account for uncertainties on the dataset, although in this particular case, it is not significantly different than the linear model. Note that this model is introduced and discussed later on in the submitted manuscript. We will also add some details as to how uncertainties were considered.

Line 255-261: The oxygen isotope fractionation equation displayed here and in the abstract (line 28) corresponds to the result of a York regression (York et al., 2004) which considers the uncertainties associated with each data point in the regression model. We also added precision on the parameters we used to set the uncertainties.

Line 348-351: I suggest the authors also compare the clumped isotope-temperature relationship in brachiopod calcite with the values obtained by applying the Meinicke et al. (2020) calibration, which was recently updated to the I-CDES scale (Meinicke et al., 2021). A recent study by de Winter et al. (2022) demonstrated that the Anderson et al. equation likely induces a cold bias on shallow water carbonates which might explain part of the observed offset in clumped isotope values in this study. I wonder if the brachiopod specific clumped isotope equation proposed by the authors is significantly different from Meinicke et al. if projected on the I-CDES scale. If so, it might not be warranted to propose a new clumped isotope calibration as brachiopods might be calibrated with general calcite calibrations.

Meinicke, N., Ho, S. L., Hannisdal, B., Nürnberg, D., Tripati, A., Schiebel, R., and Meckler, A. N.: A robust calibration of the clumped isotopes to temperature relationship for foraminifers, Geochimica et Cosmochimica Acta, 270, 160–183, https://doi.org/10.1016/j.gca.2019.11.022, 2020.

Meinicke, N., Reimi, M. A., Ravelo, A. C., and Meckler, A. N.: Coupled Mg/Ca and Clumped Isotope Measurements Indicate Lack of Substantial Mixed Layer Cooling in the Western Pacific Warm Pool During the Last ~5 Million Years, Paleoceanography and Paleoclimatology, 36, e2020PA004115, https://doi.org/10.1029/2020PA004115, 2021.

de Winter, N. J., Witbaard, R., Kocken, I. J., Müller, I. A., Guo, J., Goudsmit, B., and Ziegler, M.: Temperature Dependence of Clumped Isotopes (âˆ†47) in Aragonite, Geophysical Research Letters, 49, e2022GL099479, https://doi.org/10.1029/2022GL099479, 2022.

For a detailed response to this comment we refer the reader to "Reply on RC1" of the interactive discussion. To make things short, there is a scientific disagreement between the referee and us (the authors). For reasons detailed in "Reply on RC1", we do not consider that the Anderson et al. (2021) equation induces a cold bias. Rather we consider that significant deviation of Δ_{47} values from the Anderson et al. (2021) equation reflect other processes than temperature that controls Δ_{47} values in brachiopod shell calcite (Bajnai et al., 2018) and other carbonates (Bajnai et al., 2020). However, we agree that to propose a brachiopod specific equation is not a good way to circumvent the observed discrepancies especially as the deviations from the Anderson et al. (2021) equation do not concern all of the specimens studied here.

Lines 435-464: The discussion over the clumped isotope results was significantly reworked. Especially, we highlight that the deviation from the Anderson et al. (2021) equation are not of the same magnitude in different brachiopod groups, which likely relate to biologic processes yet to confidently identified.

Minor comments

Line 31: "but is significantly offset" rephrase to "are significantly offset" ("D47 values" is plural)

Line 31: rephrased as "(our data) confirm significant offsets from …"

Line 102-104: Perhaps the authors could add here that current empirical clumped isotope temperature calibrations also show good agreement with *ab initio* models of the carbonate isotope system, as was demonstrated in Jautzy et al. (2020). This is another argument in favor of the use of common clumped isotope thermometers in a variety of carbonate materials.

Jautzy, J. J., Savard, M. M., Dhillon, R. S., Bernasconi, S. M., and Smirnoff, A.: Clumped isotope temperature calibration for calcite: Bridging theory and experimentation, Geochemical Perspectives Letters, 14, 36–41, 2020.

This precision and reference was added lines 104-105

Line 167: “graving bit” should this read “engraving bit”?

Corrected accordingly

Line 302-305: “At temperate and polar temperatures (20 to 0°C) our equation has a steeper slope than that of Brand et al. (2019)” The authors need to explain this in more detail and/or refer to a figure where the reader can spot this effect. The authors refer to figure 2A later, but it is not clear which line in this figure represents the equation by Brand. I do not understand how the slope can be different with temperature if a linear equation (with a constant slope) is compared, but I might misunderstand what the authors are trying to say. Later on, the authors mention that the Brand et al. equation is non-linear, but I still have trouble following the description of the comparison in this section.

Another referee also stated that this part was unclear. We will address this issue in the revised manuscript and provide further and clearer discussion regarding the difference in “slope” of different equations.

Lines 302-303: This part was rephrased.
This aspect is further illustrated by the comparison of the fractionation equations added in the revised version (lines 378-386; new Figure 4)

Line 408: “In consequence” rephrase to “As a consequence”

Corrected

Line 514: “constrain” should read “constraint”

Corrected

Line 515: “should be privileged for trace-element-based paleotemperatures reconstructions” consider rephrasing to “should be selected/prioritized for trace element based paleotemperature reconstructions”

Changed to “preferred”

Line 550-567: This clear grouping based on trace element content is an interesting observation. I wonder if the authors considered whether there might be a relationship with growth rate. Does the “high” or the “low” group show significantly faster growth than the other? If so, this could be an explanation for the difference in shell composition, as trace element concentrations in calcifiers often show a correlation with growth rate. In addition, in this section about grouping of specimens based on trace element content, adding a figure showing the differences in concentration would be helpful.

This trend may be related to growth rates. Unfortunately, our new dataset comprises only 5 species for which we have constraints on their growth rate, limiting quantitative comparisons. This hypothesis could be mentioned into the discussion. A figure will be added to illustrate this dichotomy. We must also note that this difference is confined to the inner shell layers. The trends described here between Terebratellidina and Terebratulidina disappear while looking at the outer shell layers.

The discussion was reworked and the grouping is highlighted in the new figure 6.
Because we have growth rate constraints for only 5 species we did not relate this directly to growth rate, but we discuss this grouping in section 4.2 in relation to the kinetic effects highlighted in the oxygen isotopes and clumped isotopes (lines 657-695).

Line 606: The authors might consider rephrasing the title of this section to: "Precipitation of brachiopod shell calcite out of equilibrium with seawater"

Title rephrased accordingly.

Line 626-630: The authors might consider citing the recent study by Garbelli et al. (2022) here who also interpret changes in isotopic composition of (fossil) brachiopod shells as seasonal variability.

Garbelli, C., Angiolini, L., Posenato, R., Harper, E. M., Lamare, M. D., Shi, G. R., and Shen, S.: Isotopic time-series (δ13C and δ18O) obtained from the columnar layer of Permian brachiopod shells are a reliable archive of seasonal variations, Palaeogeography, Palaeoclimatology, Palaeoecology, 607, 111264, https://doi.org/10.1016/j.palaeo.2022.111264, 2022.

We agree that the addition of literature regarding seasonal variability registered in brachiopod shells would be pertinent here. Although very interesting and convincing, the work of Garbelli et al. (2022) may not be the most pertinent reference here, when other papers studying modern brachiopods in monitored environment unambiguously highlight seasonal variability (Yamamoto et al., 2011; Takayanagi et al., 2015).

Added a call to the references suggested in response.

Lines 653-673: It seems that some of the discussion of kinetic (growth rate-related) effects in trace element composition could be a useful addition to the section above where the observation of differences in trace element composition between brachiopod groups is discussed (see my comment on lines 443-466).

This trend may be related to growth rates. Unfortunately, our new dataset comprises only 5 species for which we have constraints on their growth rate, limiting quantitative comparisons. This hypothesis could be mentioned into the discussion. A figure will be added to illustrate this dichotomy. We must also note that this difference is confined to the inner shell layers. The trends described here between Terebratellidina and Terebratulidina disappear while looking at the outer shell layers.

The discussion over kinetic effects and their likely impact on trace element content was reworked. The hypothesis of a kinetic control on trace element incorporation is more clearly stated and element/Ca ratios are further compared with isotopic deviation from equilibrium which we associated at first order to kinetic effects.

Line 681: "isotopic" should read "isotopic"

Section rewritten

Line 688-689: "Given the highly…may be coincidental." I think the authors should explain this line of reasoning in a bit more detail.

Here we intend to provide a critical look at our dataset as the differences in both trace elements and clumped isotopes may be explained by two kinds of groupings in our dataset: 1)High latitudes vs low latitudes with difference in seasonality of the ecosystem that may induce different growth rate dynamics and 2) taxonomic grouping which may involve differences in shell formation processes including growth rate. However, as the Terebratulidina are mostly associated with low latitudes and Terebratellidina with mid-high latitudes in the dataset, we lack strong arguments to prefer a hypothesis over the other.
We will rephrase this part to make it easier to understand.

This was rephrased (lines 679-680).
The issue of a latitudinal among the different Terebratulida suborders is clearly highlighted in the new Figure 6 C and D.

Line 736: Please add the missing "delta" before "$_{18}O_{sw}$".

Section removed

Line 740: "central values" is a bit of a cryptic term, perhaps the authors mean median or mean/average values?

Section removed

Lines 710-746: I think the addition of a fossil case study is nice, but it is not essential for the study. If the authors would like to keep their manuscript more concise, this is a section that could be significantly shortened or removed in my opinion.

Another referee has done a similar comment. This section will be removed from the revised version.

Section removed

Table 4: As mentioned in one of my previous comments, it might be worthwhile to add the clumped isotope-based temperature reconstructions based on the Meinicke et al. calibration, since these are likely more accurate and more closely in line with the modern brachiopod data in this study as well.

Section removed

Line 796-797: The temperature underestimation by ~3 degrees is very similar to the offset found in de Winter et al. (2022; see comment above) and this nice corroboration between multiple datasets is worth mentioning.

Line 797-798: As mentioned above, I tend towards disagreeing with this call for a brachiopod-specific clumped isotope calibration, since most of the offset in clumped isotope values may be explained by the Anderson et al. equation underestimating temperatures in general (not just for brachiopods). The authors should consider this explanation before suggesting a taxon-specific calibration is needed.

Response to both comments: We retracted from the brachiopod specific equation and rather highlight that brachiopod shells Δ_{47} values can significantly depart from isotopic equilibrium and propose likely explanations.

Comments from: Adrian Immenhauser (Referee)

Abstract:

Poorly written. Many deficits in the logic and precision of the language. I list several points that caught my attention; there are others.

Ln 13. Please use `seawater´ rather than `ocean'. Delta$_{18}$O measurements. That is jargon. A `measurement' is the analytical step(s) we perform to generate the data, the analysis so to speak. The reconstruction of past seawater properties is based on oxygen isotope data (not the measurement thereof).

Corrected

Ln. 18. Missing word: commonly used `archives' in studies….

Corrected

Ln. 19. Unclear wording: …resistant to diagenetic alteration for decades. Do you mean brachiopod shells do not alter over the time span of several decades? Or do you mean that over the past decades, scientists have considered brachiopod shells to be resistant to diagenetic alteration?

Rephrased

Ln. 21. I am not sure what a `growing temperature' is (also referred to as `living temperature' elsewhere)? I did google the term to make sure I did not miss something. The only paper that matched is yours (this discussion version) on the EGU sphere webpage. Do you mean the ambient seawater's temperature during the brachiopod's lifetime? Use `ambient seawater temperature', I suggest. Other than that, please use proper terminology: seawater d$_{18}$O `values' or similar.

'Growing temperature' which may be corrected as 'growth temperature' or 'shell growth temperature', refers to the ambient temperature when the animal grows its shell. This formulation aims to point that the temperature that may be registered in the carbonate archive, will correspond to the temperature of the environment when the carbonate formed. This exclude the periods during which the animal live but does not grow its shell. We acknowledge that in this specific occurrence the use of 'seawater temperature' is better suited.

Replaced by "seawater temperature". Correct mentions to "δ^{18}O values" has been checked throughout the text.

Ln. 23. Again, I can only guess what a `supposed´ carbonate-based palaeothermometre is. Do you mean `novel´ or `less well established´? Moreover, the palaeothermometre is NOT based on carbonate but uses the archive data (geochemical properties) recorded in carbonate (note the difference between the terms archive and proxy).

Rephrased

Ln. 31. Missing word: …with `seawater´ temperatures…

Rephrased

Ln. 37. What do you mean by `relatively good´. In agreement with the measured temperatures within xy degree Celsius?

‘good’ should be here replaced by ‘strong’ relative to the strength of the correlation between the geochemical parameters and the temperature, which is illustrated by the regression coefficient.

Rephrased

Introduction:
Much better written, but still some language deficits similar to those listed for the case of the abstract. Please consider.

Ln. 48. What are past seas, and what is the difference to oceans? Do you mean epeiric seas as opposed to genuine oceanic bluewater?

That is what we mean. This difference is here to imply that the $\delta^{18}O$ values of the seas that may be more or less restricted, can deviate substantially from that of the open ocean. Especially as while $\delta^{18}O$ values of the open ocean mostly reflect global processes (Amount of continental ice, oceanic circulation, global climate), the $\delta^{18}O$ values of the seas are also influenced by more local processes (runoff, evaporation).

This was kept in the corrected version.

Ln. 62. Many inconsistencies concerning technicalities of cited references. See, for example, ln. 52. Brand et al., (2013) should read Brand et al. (2013).

Corrected. Similar mistakes have been checked throughout the manuscript.

Ln. 121. That is a scientific criticism. The authors argue about the question of whether shell carbon (DIC) and oxygen isotope values are in equilibrium with the seawater from which the shell carbonate precipitated or not. Please allow me to clarify that brachiopod biominerals are secreted from bodily fluids, NOT seawater. The problem is threefold: (i) What is the isotopic value of the bodily fluid relative to that of the ambient seawater? (ii) Does the isotopic value of the bodily fluid change during active versus passive cycles in the brachiopod metabolism cycle and during the brachiopods life span? Juvenile brachiopods grow rapidly, mature slow down. (iii) What is the fractionation factor between bodily fluid and brachiopod biomineral, and is it constant during the lifetime of a brachiopod? In some cases, brachiopod bodily fluids are isotopically close to the ambient seawater; in others not. In short, it is complicated. The authors provide text about thermodynamics and kinetics but less so about these metabolic effects and biomineralization pathways. In my oppinion, that is a weakness of the paper. Please see the discussion and references cited in:

Immenhauser, A., Schöne, B., Hoffmann, R. and Niedermayr, A. (2016) Mollusc and brachiopod skeletal hard parts: Intricate archives of their marine environment. Sedimentology 63, 1-59.

I emphasise that you do not need to cite my paper! That is entirely up to you. It simply saves the reviewer time when being able to refer to the text and the cited references in a published paper. Please consider.

We fully agree that rynchonelliform brachiopods shells are not the result of an inorganic precipitation experiments from the seawater, but that they result from biological processes promoting carbonate precipitation from a biologically controlled fluid, forming a structured carbonate shells where calcite crystals are embedded in an organic matrix (Williams, 1968; Curry et al., 1991; Gaspard et al., 2008; Simonet Roda et al., 2019, 2022). We acknowledge that these aspects are not very present in the paper, mostly as the scope of this paper is the use of brachiopod shell geochemistry for paleo-environment reconstruction. But we are fully aware of the differences between biogenic and inorganic carbonate precipitation and will put more emphasis on these aspects in the revised version. From a more methodological point of view, the comparison between the brachiopod shell mineralisation and inorganic calcite precipitation is an approach to highlight the effect of biological processes on shell geochemistry, especially when the chemistry of the mineralizing fluids for brachiopods, remains for now largely unknown (We do not know of any study reporting isotopic values of ionic concentration from the brachiopod body fluids).

We added to the introduction several aspects on brachiopod shell formation to highlight what biological and environmental processes may affect brachiopod calcite precipitation and its chemistry. Line (108-120)

Material and Methods:

No major comments, looks o.k. One exception, please avoid acronyms in titles (2.3) and please refer to `values'. d13C of modern brachiopods is jargon. Please use carbon isotope values of modern….

Title rephrased (line 191)

Results:

Header chapter 3.1. Please do not use `stable' as a synonym for carbon and oxygen isotope values; science knows about 120 stable isotopes.

Line (244): Header rephrased

Ln. 245. I always wonder, what is the meaning of the second decimal in a range of isotope values resulting from bulk samples? What is the meaning of -2.24 permil in this context? My opinion, the second decimal is meaningless. You analyse a bulk sample from a brachiopod shell, and I would refer to that as pseudo-precision. I suggest providing one decimal values. Bulk samples and second decimals do not match. Particularly as you mix bulk sample data and data from the inner and outer shells (see Table 2).

$\delta^{13}C$ and $\delta^{18}O$ values reported with only one decimal in text and tables.

Discussion:

General comment: This chapter is longwinded and, in part, difficult to follow. I advise streamlining the text and shortening it by at least 20%. I wonder if the `holy trilogy´ of scientific writing consisting of Data Presentation, Data Interpretation and Discussion is applied here? If so, where is the discussion? Consider rephrasing the header as `Interpretation and Discussion'.

Section 4.3. was removed. Some parts were rewritten to clarify the discussion. However, there is some added discussion in order to answer the comments of other referees. Header was rephrased.

Ln. 333. What are `independent´ brachiopods? Please explain.

Obviously here we do not refer to brachiopods that are independent from anyone or anything. 'Independent' here qualifies the dataset chose to test the fractionation equations. The adjectives associated with dataset here are numerous so we will rephrase it to make it clearer.

Rephrased

Chapter 4.1.2 is poorly written. Quite some problems regarding grammar and formalities (citations etc.). Please clean up.

This section has been significantly reworked (lines 414-464)

Chapter 4.1.4 All good science but very longwinded. Could you streamline that? This is not easy to follow and this is not something you want to hear from the readers.

We will rework the construction of this section to make it easier to follow and more concise.

The last paragraph of this section was reworked in depth (lines 539-589) with more call to the data, and an added figure (Figure 6) as asked by anonymous referee 1, which should help follow this discussion.

Chapter 4.2 Here, we need much more emphasis on metabolism and biomineral secretion from bodily fluids. The authors deal with the topic as if brachiopod biomineralization pathways were an inorganic precipitation experiment. These are super complicated little bio-machines', and they are fascinating since each individual is a case on its own. Please see papers from the marine biology community (mainly aquaria monitoring experiments but also field observations).

Indeed, if the data can be explained by what is known of inorganic precipitation, then we do not need to invoke any biological processes. On the contrary if the data diverge from what we expect from inorganic precipitation, then biological processes may explain these differences.
Regarding the discussion around kinetic effects, we clearly state (ln 689-709) that we expect the extent of the kinetic effects to be directly linked to shell growth rates, which is biologically controlled.

More emphasis was put on biological processes in this sections and others. There is a focus on kinetic effects which can be related to shell growth rates and which we identify here. We invoke other possible sources of biologic or environmental, but that our dataset is unable to resolve.

Conclusion(s):

Please use the plural, I suggest that you list more than one conclusion here.
This chapter is very much written in a discussion style. Please consider coming up with genuine conclusions style text rather than a short (renewed) discussion. The last statement is an anti-climax. First, you present all of these data and text. Then you tell the reader that you advise considering the variability in brachiopod live habitats, environmental conditions, metabolic effects, seasonal effects etc.? I must admit, have read very similar concluding statement in many papers published a decade or more years ago. Please consider.

Header corrected. The conclusions were almost completely rewritten.

Comments from Anonymous Referee #3

Comparison of the new equation of the oxygen isotope fractionation with those of Brand et al. (2019). Both equations are similar in the temperature range between 10-25°C, but differ in the low-temperature field (<10°C). Since the Brand et al brachiopod-data set is by far much better constrained by data points, the authors should provide a more in-depth discussion about causes of the offset. Further, they should strengthen their arguments why it is necessary to introduce the new equation, and why it shall be an improvement.

We will clarify this part and add elements of comparison between the different fractionation equations.

We discuss an example of the possible source of discrepancies between the datasets, and highlight differences in the environmental parameters (Temperature, $\delta^{18}O_{sw}$ values) used to determine the equation (lines 313-322).
The comparison of the different equations by applying them to the dataset of Bajnai et al. (2018) was pushed further by comparing the distribution of the temperature offsets with growing temperature (lines 378-386; new figure 4)

In this context, please also indicate the MAT range covered by the Bajnaj et al. 2018 brachiopod data set (line 335).

MAT ranged added

The supposed sampling procedure avoids specialized parts of the shell as umbo, edges, muscle scars, primary layers. However, Ullmann et al. (2017) observed additional significant taxon-specific ranges in their intra-specific high-resolution oxygen isotope data. How has this observation an effect for the results of this study? Is intra-specific variability smoothed by the sample size? Which degree of uncertainty introduce specimens of the suborder Terebratellidina to the fractionation equations? Please comment on this.

We assume that the sampling area, which spans at least two major growth lines, allows a smoothing of the intra-specimen variability within the collected homogenized powder. While major growth lines are commonly interpreted as annual, that is not necessarily the case, which explains the uncertainty on the time represented by the sampling area. This sampling creates mass-averaged geochemical compositions, which is mathematically different from time-averages which we use for environmental conditions, because of varying shell growth rates through time. The more variable the growth rate, the more biased this sampling will be towards periods of high growth rates (Schöne, 2008; Yamamoto et al., 2011; Takayanagi et al., 2015).
Specimens of the suborder Terebratellidina highlight clumped isotope deviations from equilibrium and trace elements compositions of their inner layer significantly different than specimens form the suborder Terebratulidina. However, deviation from thermodynamic equilibrium in oxygen isotope composition are not significantly different between the two groups, within the dataset tested. So we do not expect the fractionation equation to be biased by any of the groups.

The Jurassic example is not well executed and not the scope of this study. New and very few data points are introduced first time in the discussion. I recommend the removal of this part of the manuscript, since its focus is on modern brachiopod taxa.

As also suggested by another referee, this part will be removed from the revised version of the manuscript

Part removed from the revised version.

Please, explain all parameters and abbreviations in Supplementary Table S1.

All columns are described in more details

Please check the manuscript for spelling errors, here are some I spotted:

“Rhynchonellida” in Fig. 4 – revise spelling two “l”

Figure corrected

Line 103: Delete “previous”

Deleted

Line 129-131: incomplete sentence

The sentence appears complete.

Line 375: Enter a space between “regression derived”

corrected

Line 532: spelling of “isotopic fractionation”

Corrected

Typos in supplementary File S3 (Sheet Description)

Corrected

References:

Bajnai, D., Fiebig, J., Tomašových, A., Milner Garcia, S., Rollion-Bard, C., Raddatz, J., Löffler, N., Primo-Ramos, C., and Brand, U.: Assessing kinetic fractionation in brachiopod calcite using clumped isotopes, Sci. Rep., 8, 533, https://doi.org/10.1038/s41598-017-17353-7, 2018.

Bajnai, D., Guo, W., Spötl, C., Coplen, T. B., Methner, K., Löffler, N., Krsnik, E., Gischler, E., Hansen, M., Henkel, D., Price, G. D., Raddatz, J., Scholz, D., and Fiebig, J.: Dual clumped isotope thermometry resolves kinetic biases in carbonate formation temperatures, Nat. Commun., 11, 4005, https://doi.org/10.1038/s41467-020-17501-0, 2020.

Curry, G. B., Cusack, M., Walton, D., Endo, K., Clegg, H., Abbot, G., and Armstrong, H.: Biogeochemistry of brachiopod intracrystalline molecules, Philos. Trans. R. Soc. Lond. B. Biol. Sci., 333, 359–366, https://doi.org/10.1098/rstb.1991.0085, 1991.

Gaffey, S. J. and Bronnimann, C. E.: Effects of bleaching on organic and mineral phases in biogenic carbonates, J. Sediment. Res., 63, 752–754, https://doi.org/10.1306/D4267BE0-2B26-11D7-8648000102C1865D, 1993.

Gaspard, D., Marie, B., Luquet, G., and Marin, F.: Biochemical BlackwellPublishingLtd characteristics of the soluble organic matrix from the shell of three Recent terebratulid brachiopod species, 2008.

Key, M. M., Smith, A. M., Phillips, N. J., and Forrester, J. S.: Effect of removal of organic material on stable isotope ratios in skeletal carbonate from taxonomic groups with complex mineralogies, Rapid Commun. Mass Spectrom., 34, https://doi.org/10.1002/rcm.8901, 2020.

LeGrande, A. N. and Schmidt, G. A.: Global gridded data set of the oxygen isotopic composition in seawater, Geophys. Res. Lett., 33, L12604, https://doi.org/10.1029/2006GL026011, 2006.

Pingitore, N. E., Borrego, P. M., and Crawford', G. M.: Dissolution kinetics of CaCO3 in common laboratory solvents, J. Sediment. Petrol., 63, 641–645, 1993.

Schöne, B. R.: The curse of physiology—challenges and opportunities in the interpretation of geochemical data from mollusk shells, Geo-Mar. Lett., 28, 269–285, https://doi.org/10.1007/s00367-008-0114-6, 2008.

Schöne, B. R., Schmitt, K., and Maus, M.: Effects of sample pretreatment and external contamination on bivalve shell and Carrara marble δ18O and δ13C signatures, Palaeogeogr. Palaeoclimatol. Palaeoecol., 484, 22–32, https://doi.org/10.1016/j.palaeo.2016.10.026, 2017.

Simonet Roda, M., Ziegler, A., Griesshaber, E., Yin, X., Rupp, U., Greiner, M., Henkel, D., Häussermann, V., Eisenhauer, A., Laudien, J., and Schmahl, W. W.: Terebratulide brachiopod shell biomineralization by mantle epithelial cells, J. Struct. Biol., 207, 136–157, https://doi.org/10.1016/j.jsb.2019.05.002, 2019.

Simonet Roda, M., Griesshaber, E., Angiolini, L., Rollion-Bard, C., Harper, E. M., Bitner, M. A., Milner Garcia, S., Ye, F., Henkel, D., Häussermann, V., Eisenhauer, A., Gnägi, H., Brand, U., Logan, A., and Schmahl, W. W.: The architecture of Recent brachiopod shells: diversity of biocrystal and biopolymer assemblages in rhynchonellide, terebratulide, thecideide and craniide shells, Mar. Biol., 169, 4, https://doi.org/10.1007/s00227-021-03962-4, 2022.

Takayanagi, H., Asami, R., Otake, T., Abe, O., Miyajima, T., Kitagawa, H., and Iryu, Y.: Quantitative analysis of intraspecific variations in the carbon and oxygen isotope compositions of the modern cool-temperate brachiopod Terebratulina crossei, Geochim. Cosmochim. Acta, 170, 301–320, https://doi.org/10.1016/j.gca.2015.08.006, 2015.

Williams, A.: A history of skeletal secretion among articulate brachiopods, Lethaia, 1, 268–287, https://doi.org/10.1111/j.1502-3931.1968.tb01741.x, 1968.

Yamamoto, K., Asami, R., and Iryu, Y.: Brachiopod taxa and shell portions reliably recording past ocean environments: Toward establishing a robust paleoceanographic proxy: BRACHIOPOD OXYGEN ISOTOPE RECORDS, Geophys. Res. Lett., 38, L13601, https://doi.org/10.1029/2011GL047134, 2011.

York, D., Evensen, N. M., Martínez, M. L., and De Basabe Delgado, J.: Unified equations for the slope, intercept, and standard errors of the best straight line, Am. J. Phys., 72, 367–375, https://doi.org/10.1119/1.1632486, 2004.