Dear Aninda Mazumbar,

On behalf of my co-authors, I would like to thank dr. Johan Vellekoop and dr. Andrew Johnson for their insightful constructive comments on our manuscript. Below, I will provide a point-by-point reply (in **bold**) to these comments (in *italics*) and state which changes we will make to the manuscript to take away the reviewers' concerns and prepare the text for publication. I hope that the suggested changes below will be sufficient to allow us to revise our manuscript for publication in Biogeosciences, and I look forward to hearing from you concerning your decision.

Sincerely,

Niels J. de Winter

Interactive comment on "Shell chemistry of the Boreal Campanian bivalve Rastellum diluvianum (Linnaeus, 1767) reveals temperature seasonality, growth rates and life cycle of an extinct Cretaceous oyster" by Niels J. de Winter et al.

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The study of de Winter et al. presents interesting trace elemental and stable isotopic data from a set of Rastellum diluvianum specimens from the famous Campanian locality of Ivo Klack. The new datasets highlight both the potential of these kind of studies, and the complexity of interpreting trace elemental data. The authors have generated a wealth of data, providing valuable insights in the age of the Ivo Klack deposits (Sr isotopes), the local temperature seasonality (oxygen isotopes) and in the physiology of the studied oysters (carbon and oxygen istopes and elemental data). At the same time, the complexity of the incorporation of trace elements in mollusk shells limits the usability of large parts of their data. The authors do a good job in highlighting this complexity, and show that, while sometimes elemental records of mollusk show cycling patterns, we are a long way away from successfully developing truly applicable proxies based on this time of data.

While the text is a bit lengthy, and some of the figures are rather complex, overall, this is a well-written manuscript. The authors have generated a substantial dataset, convincingly show that the studied specimens are well-preserved and provide interesting insights in the local climatic conditions at Ivo Klack. Their arguments are solid and their conclusions are sound.

Content wise, my only comments would be on the limited discussion on the possibility of a seasonal variability in d18O of seawater at Ivo Klack. They pass over this issue a bit too hastily, in my opinion. How is the assumption of a constant d18O of seawater justified? Wouldn't such a coastal site be susceptible for seasonal changes in riverine input? Particularly since the fennoscandian shield is usually placed in a wet/temperate climate belt, in Late Cretaceous climate reconstructions. The reference provided by the authors (Thibault et al., 2016) concerns a study on the chalks of the Stevns-1 core, which represents a much more distal site than Ivo Klack. Now, I realize that the authors are limited here, because *constraining d18O of seawater is not easy, and I don't disagree with most of their general conclusions, but it would behoove them to acknowledge their uncertainties in this issue.*

We acknowledge that the reconstruction of sea surface temperatures from stable oxygen isotopes suffers from assumptions about water oxygen isotope composition. We realize that we may not have given this fact the proper attention in our manuscript. In the revised version, we will therefore update our discussion of stable oxygen isotopes where these are translated to temperatures and make clear that these conversions are based on assumptions. We will add a paragraph at the beginning of the discussion of our stable oxygen isotope results in which we more clearly explain what assumptions we make about sea water composition. Finally, at the end our discussion of temperature seasonality we will discuss how the type of seasonal changes in sea water isotope composition that may be expected in a rocky shore setting may influence our conclusions.

Apart from this, all my comments and suggestions are relatively minor. Therefore, I recommend this manuscript to be accepted with minor revisions. Please also note the supplement to this comment:

https://www.biogeosciences-discuss.net/bg-2019-74/bg-2019-74-RC1-supplement.pdf

Comments in PDF supplement:

Comments & suggestions:

P2, L54: What does the "it" in this sentence exactly refer to? The Late Cretaceous cooling trend?

Yes, this refers to the cooling trend, we will replace "It" by "The cooling trend" for clarity.

P2, L57-59: In the 90's chalk was still considered to record sea surface conditions faithfully. Over the last decades, this viewpoint has changed. Most chalk consists of recrystallized material. As a result, d18O values usually result in much lower temperatures, e.g. resulting in the (apparent) Cool Tropics Paradox. I advise the authors to read up on this. The values recorded by Jenkyns et al. are in all likelihood a large underestimation of SSTs (with even Cenomanian-Turonian values still below 28 degrees…). In reality, Cretaceous SST's were probably much higher. See for example the review paper of O'Brien et al., 2017.

This is a valid comment, and we will briefly discuss this later insight in our introduction. However, we do note that the introduction of previous work on chalk here mostly serves to introduce the reader into climate reconstructions from successions in the Boreal Chalk Sea. We would therefore rather add some discussion about the validity of SST reconstructions from such successions in the discussion section, where we compare different temperature estimates.

P2 L67-68: With a Tethys ocean still present, a Panama corridor still present and closed-off Tasman and Drake Gateways, I wouldn't state that the continental configuration is "relatively modern". Yes: apart from India, most continents were already close to their present-day position, but from a climatological and paleoceanographical perspective, the Late Cretaceous continental configuration was widely different. Of course, this does not mean that the Campanian could be considered an interesting analogue. I just would not play the continental configuration card.

Valid point, we will rephrase this and nuance our introduction of the Campanian as a reference for future climate, removing the notion of "relatively modern" continental configuration.

P2 L73-75: Does the data represent a fundamental component of the climate system? Or the seasonality? Please rephrase.

We will rephrase this to ", although seasonality constitutes a fundamental component of the climate system"

P3 L93 "The incorporation of these chemical proxies into bivalve shells…": This is a confusing sentence. Are the authors discussing the application of proxies on bivalve shells? Or are they concerned with the incorporation of chemical signals into bivalve shells?

Agreed, we will rephrase this sentence stating that the application of trace element proxies in bivalve shell records is complicated by vital effects.

P3 L109-126 "The Kristianstad Basin….": This paragraph feels a bit misplaced. There is a large jump from the previous paragraph (on the value of mollusks as archives of seasonality) to this one (on the Kristianstad Basin). I think this paragraph would better fit directly after the first paragraph of the Introduction. The first paragraph of the section ends with the notion that Late Cretaceous seasonality records from high latitudes are scares. This could very easily be followed by "The Kristianstad Basin in Sweden provides a great potential for such a high latitude seasonality records. Particularly the Ivo Klack site, located on the southeastern Baltic…. Etcetera).

We thank the reviewer for this suggestion and indeed agree that this paragraph fits better straight after the introduction into the Boreal Chalk Sea reconstructions. We will move the paragraph to this location in the revised version and introduce bivalve shells as climate archives after the site description.

P3 L110-112: suggestion: "The coarsely latest early Campanian shallow marine sediments deposited at Ivö Klack consist of sandy and silty nearshore deposits containing carbonate gravel (Christensen, 1975; 1984; Surlyk and Sørensen, 2010; Sørensen et al., 2015)." (to avoid a confusing "and are.." construction.

We like this suggestion by the reviewer and will implement it with a minor change: "The coarsely uppermost lower Campanian shallow marine sediments deposited at Ivö Klack consist of sandy and silty nearshore deposits containing carbonate gravel (Christensen, 1975; 1984; Surlyk and Sørensen, 2010; Sørensen et al., 2015)."

P3 L114: maybe start a new sentence on the paleolatitude.

Agreed, we will rephrase this to "Late Cretaceous transgression. The paleolatitude of the site is 50°N."

P3 L115: no glaciotectonic movements in this region?

Post-glacial vertical crustal motion of the Kristianstad Basin is very limited (between -1 and +1 mm/yr), because the area is situated in the neutral uplift zone between compressed crust that is rebounding (most of the Scandinavian peninsula) and the glacial bulge (more to the south; Vestøl et al., 2019). The quiet tectonic history of this area is also documented in a report by Paulamäki & Kuivamäki (2006). Similar observations about the lack of glacio-eustatic rebound and other tectonic activity in the area have been documented by Surlyk and Sørensen (2010) and Christensen (1984).

- **Christensen, W. K.: The Albian to Maastrichtian of southern Sweden and Bornholm, Denmark: a review, Cretaceous Research, 5(4), 313–327, 1984.**

- **Paulamaeki, S. and Kuivamaeki, A.: Depositional history and tectonic regimes within and in the margins of the Fennoscandian shield during the last 1300 million years, Posiva Oy. [online] Available from: http://inis.iaea.org/Search/search.aspx?orig_q=RN:43061185 (Accessed 12 February 2020), 2006.**
- **Surlyk, F. and Sørensen, A. M.: An early Campanian rocky shore at Ivö Klack, southern Sweden, Cretaceous Research, 31(6), 567–576, 2010.**
- **Vestøl, O., Ågren, J., Steffen, H., Kierulf, H. and Tarasov, L.: NKG2016LU: a new land uplift model for Fennoscandia and the Baltic Region, J Geod, 93(9), 1759–1779, doi:10.1007/s00190-019-01280-8, 2019.**

P3 L124: I presume "original shell material " only refers to the calcitic material? Or is aragonite also preserved?

The oyster shells we describe contain very little original aragonitic shell structures (oysters only build thin aragonite structures at the resilium and the adductor muscle scar), so we only investigated calcite preservation in our study. The same holds true for the cited studies into macrofossils at this site. To clarify this, we will specifically refer to "calcite shell preservation" in the revised manuscript text.

P7 L195: TSR and TSA are not specified. What do there abbreviations stand for?

These stand for Time of Stable Accuracy and Time of Stable Reproducibility, terms which are defined in de Winter et al., 2017b. We will revise this section by writing out the full names of these terms and briefly defining what is meant by them in the context of microXRF measurement quality.

P8 L243-244: what percentage of samples were run in duplicate?

Duplicates were measured during every run of ~30 samples. We will mention this in the revised manuscript.

P11 L340-346: There are a lot of 'allows' in this paragraph. Maybe rephrase a sentence or two?

Good point, we rephrase the sentences on lines 341-346 to: "From this extrapolation we could estimate the total shell height from microstructural growth markers (Fig. 3; following Zimt et al., 2018), linking growth to changes in shell chemistry. This way, chemical changes in the shell can be interpreted in terms of environmental changes by applying calibration curves for trace element proxies that were previously established for modern oyster species (e.g. Surge and Lohmann, 2008; Ullmann et al., 2013; Mouchi et al., 2013; Dellinger et al., 2018)."

P11 L358-360: This sentence is slightly confusing because the "(deeper waters)" directly follows the "bivalves". This reads as if the bivalves live in deeper waters, rather than the belemnites. Please rephrase.

We rephrased this to: "This suggests that $\delta^{13}C$ in belemnite rostra are affected by vital effects while heavier $\delta^{18}O$ values of the belemnites suggest that belemnites lived most of their life away from the coastal environment (in deeper waters),"

P13 L388-389: How is the assumption of a constant d18O of seawater justified? Wouldn't such a coastal site be susceptible for seasonal changes in riverine input? Particularly since the fennoscandian shield is usually placed in a wet/temperate climate belt, in Late Cretaceous climate reconstructions. The reference *provided by the authors (Thibault et al., 2016) concerns a study on the chalks of the Stevns-1 core, which represents a much more distal site than Ivo Klack.*

This comment reflects the major criticism of the reviewer. We hope that by more thoroughly discussing the stable oxygen isotope composition of sea water we can acknowledge the shortcomings of this assumption of constant seawater $\delta^{18}O$ values.

P13 L394-396 "Superimposed on these changes, a statistically significant ontogenetic trend can be discerned in the d13C records of 10 out of 12 shells. However, the scale and direction of these trends do not seem consistent between shells.":

(1) I understood that only 5 of the 12 specimens were measured for isotopes? How can the authors have d13C data on all 12 shells? In table 1, only the 5 specimens are mentions, of which 3 out of 5 seem to have a statistically significant trend? It looks like something got mixed up here

(2) Please insert a reference to Table 1 here. This was not immediately clear from the text.

(3) I am intrigued by the difference in the direction of supposed ontogenetic trends. On the other hand, the only shell with a negative trend doesn't show a statistically significant trend….

We fully agree with all the reviewer's points of critique here, something must have gotten mixed up here and we apologize for the mistake. We will rephrase this sentence as follows: "Superimposed on these changes, a statistically significant ontogenetic trend can be discerned in the d13C records of 3 out of 5 shells. In specimens that show a statistically significant ontogenetic trend $\delta^{13}C$ increases with age (see Table 1)".

P13 L403: Supplementary file S10 seems to be missing from the supplements

File S10 contains the plots of multiproxy records against age. We regret that these plots did not make it into the supplement and will make sure that they do in the revised version. In response to comments by the second reviewer, we now show $\delta^{18}O$ records of all shells in the main manuscript as well.

P16 L451-453: Is anything known about annual variations in growth rates in modern oysters? Do they respond to food availability? Could this be an early spring phytoplankton bloom? Or some other environmental parameter? Or is there a relationship with something like spawning?

In the revised manuscript, we will add some discussion here about how these findings compare with those in modern oysters. There is some literature on this which suggests indeed that food availability plays a role. We hypothesize the presence of a spring phytoplankton bloom later in the manuscript, but will move this hypothesis forward here, where we can discuss it together with the comparison with modern oyster species.

P20 L568: salinities are usually not indicated in g/kg, but either in psu or in m%

We will convert these values to psu.

P21 L600: "as well as" should be replaced by "including", since bivalves with symbionts are also marine or freshwater bivalves.

Correct, we will rephrase this.

P22 L644 "because not all seasons contributing to the average have long growing seasons": seasons having long growing seasons? This is a confusing sentence. Please rephrase.

We agree that this is a convoluted sentence and will rephrase as follows: "Averaging seasonality (Fig. 8) underestimates the extent of seasonality at Ivö Klack, because not all specimens contributing to the average have long growing seasons, which will reduce the average extent of seasonality."

P23 L696-698: Why would oysters need to compensate for lower ambient Sr concentrations? What is the benefit of building Sr into their shells? How does this help to compensate for lower seawater Sr concentrations?

We agree that "compensate" is not the right term here. We will rephrase as follows: "Therefore, the similarity in absolute calcite Sr/Ca ratios between modern *C. gigas* and Campanian *R. diluvianum* demonstrates that *R. diluvianum* incorporated more Sr into its shell relative to the ambient seawater concentration. This observation may entail that there is a minimum Sr concentration that is favorable for oysters to incorporate, or that there is a fixed physiological limit to oyster's discrimination against building Sr into their shells that is independent of ambient Sr concentrations."

P24 L774-782: Is anything known about the spawning season of modern oysters? Maybe the authors could discuss how similar or dissimilar their results are.

Modern oysters typically spawn at the end of the spring season and spat settles in during summer. This makes our result for *R. diluvianum* different from the modern situation. We will acknowledge this in the revised manuscript and provide references for spawning of modern *C. gigas*.

P25 L819-832: The notion of a spring supply of freshwater, bringing in nutrients, causing a spring phytoplankton bloom, is somewhat conflicting with the assumption of a constant d18O of seawater, discussed in lines 388-389. Note to the authors: at modern day mid- to high latitudes, the spring bloom is often triggered by storm-induced mixing. A spring bloom is not necessarily related to riverine input of nutrients. It could be related to changes in mixed-layer depth as well..

We thank the reviewer for this comment and advice and will add this to the discussion. As mentioned in our reply to his major comment, we will discuss potential changes in seawater composition in more detail in the revised manuscript and specifically add a paragraph detailing how changes in seawater composition can affect our interpretation in terms of temperature seasonality.

Interactive comment on "Shell chemistry of the Boreal Campanian bivalve Rastellum diluvianum (Linnaeus, 1767) reveals temperature seasonality, growth rates and life cycle of an extinct Cretaceous oyster" by Niels J. de Winter et al.

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See attached PDF

Please also note the supplement to this comment:

https://www.biogeosciences-discuss.net/bg-2019-74/bg-2019-74-RC2-supplement.pdf

Comments in PDF supplement:

Comments on de Winter et al. (submitted to Biogeosciences)

This paper contains a great deal of carefully collected data but I think that it suffers from the sheer volume of information, and the attempt to discuss all issues to which the data may relate. Had the authors started with a question rather than with the data they would have developed a clearer line of argument, making the contribution easier to read, more persuasive and (I think, ultimately) more used. The main 'question' is probably seasonality in the Cretaceous, but we are led in various other directions, and certain important issues relating to the δ18O data go undiscussed in the process. By contrast, there is extensive discussion of the meaning of the trace-element information but these data in the end contribute nothing to the seasonality question – temperature variation is determined entirely from the δ18O data. There is a separate paper to be written on why the trace-element data does not help in determining seasonality. I suggest the authors focus here on doing a good job with the δ18O data (its implications for seasonality, together with those for growth) and deal only with trace-element data in so far as it relates to age and preservation.

This is a valid point, and agree that our trace element data does, in the end, not contribute as much to the seasonality story as we would have hoped initially. We would therefore largely follow the reviewer's suggestion and strongly limit our discussion of the trace element data. However, we do not fully agree that the trace element data by itself would stand alone in a manuscript. Therefore, we would like to keep discussing (albeit more briefly) the patterns in trace element concentrations we find in our specimens. Moreover, we believe that the comment raised here is also partly a result of the (admittedly somewhat chaotic) structure our manuscript inherited in our attempt to tie together several lines of evidence and reasoning about the species' paleobiology and living environment. Besides shortening the discussion of trace element results, we will also make an attempt to streamline the manuscript as a whole to make these lines of reasoning easier to follow.

With respect to the δ18O data my main query is the authors' abandonment of their initial estimate of seasonal temperature range (5.2°C) in favour of a much higher figure (13.4°C), representing the difference between the maximum and minimum temperatures from all the shells sampled. They then go on to compare this with figures for seasonal temperature range in the North Sea now and at lower latitudes in the Cretaceous, but it is not clear whether these figures are derived from equivalent (extreme) summer and winter values. If they are not the comparisons are worthless, and the conclusions about latitudinal seasonality variation in the Cretaceous compared to now will need to be reformulated. It looks like the figure for the North Sea now is based on extreme values (the stated range of 16–20°C is much higher than the mean range of about 11°C in the southern North Sea) but the authors need to explain this.

This is a valid comment, and we will reevaluate this part of the manuscript where we compare our seasonality results with modern and reconstructed seasonality data. Data such as SST profiles of the present-day North Sea will invariably show differences depending on where these data were sampled from (e.g. from which water depth or locality). We will therefore be more careful in stating how

exactly the data were sourced, whether these are extreme seasonal ranges or (more conventionally) differences between extreme monthly temperatures and how they compare to reconstructed seasonal amplitudes.

Another obscure use of the δ18O data is in Fig. 10. I looked at this, the caption, and the accompanying text for a long time but could not understand how the time of spawning was being inferred. The statement (LL 493–494) 'The onset of the first growth year in each shell at its precise position relative to the seasonal temperature cycle showed in which season spawning occurred (Fig. 10c)' does not mean anything to me – what is 'the first growth year'? The caption of part b added to my confusion since it does not describe what is illustrated—a bivariate plot of minimum growth temperature against mean annual temperature.

We acknowledge that Fig. 10 may not be very clear, and we will attempt to revise this figure to clarify what we would like to show here. The time of spawning could be placed relative to the seasonal stable oxygen isotope cycle by noting during which part of the annual cycle shell growth started. Assuming the regular variations in stable oxygen isotope composition reflect temperature seasonality, the season in which the bivalve started growing can be inferred from phase of the oxygen isotope sinusoid during onset of growth. We will clarify how this is achieved in more detail in the revised version of the manuscript.

These two instances where further explanation is required of the use of δ18O data only emphasise the need to exclude discursive trace-element data and discussion, especially if (as recommended below) all the δ18O profiles are included in the main text.

Some other points:

LL54–55. How is the cooling trend 'recorded in the white chalk successions…'?

The cited references are of studies in which (mostly) oxygen isotope records from these chalks have been used to document this cooling trend. For clarity, we will rephrase this sentence as: "The cooling trend is well documented in stable oxygen isotope records from the white chalk successions of the Chalk Sea, which covered large portions of northwestern Europe during the Late Cretaceous Period…"

L99. The 'vital effects' largely relate to trace element content. A small effect on isotopic composition has been noted in Pecten maximus but little or no effect in other scallop species.

Agreed, we will clarify this in the revised version. "Vital effects" on oxygen isotope composition in bivalves are rare, and most of them are thought to precipitate at or close to isotopic equilibrium.

Fig. 3a. The use of the false yellow colour needs to be explained in the caption. What is the (non-sediment) yellow-coloured area – maybe altered pallial myostracum? If so, the early ontogenetic samples would be from the inner shell layer – not ideal material (deposited far from the shell edge) and maybe an explanation for some aberrant data.

We will add a description of the yellow color in the figure caption. This is indeed the color we use to highlight highly altered shell material and sediment infilling, as seen in the XRF maps below.

L 258. Some brief justification is required for the choice of value for water δ18O, even if it repeats Thibault et al. (2016) – this is an important issue in the present context.

This comment touches on the major comment posed by our other reviewer (dr. Johan Vellekoop). We hope that the changes we will make in reply to his comment will satisfy this comment as well.

L288. The parallelograms are not in 'different shades of blue'.

Correct, this is a remnant of an earlier version of this figure. We will correct this by stating that the specimens are represented by parallelograms of different colors matching the probability distributions below.

L348. Exclude 'multi-proxy' (redundant).

Agreed, this will be removed.

L368. Exclude 'vast' – there are quite a lot of δ18O values associated with a Mn content of more than 100 µg/g.

Agreed, we will remove "vast"

L373. The results for C. gigas are not in 'grey/black'.

Correct, this again refers to a previous version of the figure. We have overlooked this error and will correct it in the revised version, stating that the results of *C. gigas* are in yellow/brown.

Fig. 6. Explain the vertical dashed lines (corresponding to the maxima in the δ18O plot); change 1.0 to -1.0 for the water value on the y-axis. I think it would be worth having the d18O profiles from all the shells (not just this one) in the main text, so that the reader can get a picture of all the important data (see also comment on L457).

We will add a sentence to the caption stating that the vertical dashed lines separate growth years. In addition, we will correct the typographic error in our assumed $\delta^{18}O_{sw}$ value. We will add a composite figure displaying all $\delta^{18}O$ data used in this study.

L425. 'virginica' in italics.

Certainly, we will change this in the revised text.

L437. 'follows' rather than 'shows'

Correct, this will be rephrased.

LL450–451. You don't mean 'seasonal temperature range … was between 16°C and 21°C'. I suggest you say 'temperature varied between 16°C in winter and 21°C in summer'.

Agreed, we will rephrase this accordingly.

L457. This is where you need to be able to refer to all the δ18O profiles.

Agreed, we will refer to the composite figure we will add showing all $\delta^{18}O$ records here.

Fig. 9. It is not clear to me how ages were derived for the start of the growth curves. Were growth increments used?

For most specimens, $\delta^{18}O$ measurements were possible until very close to the onset of mineralization. For the specimens were this was not the case, we used a combination of annual growth increments and extrapolation of the $\delta^{18}O$-based age model to infer the age of the ontogenetically oldest $\delta^{18}O$ measurements. We will clarify this in the revised version.

L583. 'placed' rather than 'replaced'.

Agreed, we will rephrase this.

LL664–665, 703–704. Repetitions of earlier statements.

Agreed, we will significantly shorten these sections about trace element concentrations and remove these repetitions. This is also in response to the major comments by the reviewer stating (rightfully) that the discussion of trace element patters distracts from the main seasonality discussion in the manuscript which is mostly based on $\delta^{18}O$ records.

LL752–3. 'cemented together in groups' suggests there would have been space competition and a 'high-energy environment' is not obviously something that would reduce space competition – needs explanation.

Here we wanted to refer specifically to the competition with other taxa, which would not thrive in this high-energy environment. In addition, the *in situ* distribution of oysters on the fossil rocky shored of Ivö Klack as documented in Surlyk and Christensen (1974) and Sørensen et al. (2012) shows that there is limited competition for space. We will rephrase "cemented together in groups" into "cemented next to each other in groups" to clarify that the oysters are not cemented on top of each other (as modern *C. gigas* often is) and have less space limitations that modern oysters.

L760. 'deep shelf' for 'deep marine' – Placopecten magellanicus does not occur in anything other than shelf environments.

Correct, we will change this throughout the text.

General point: please refer in the text to relevant parts of figures (where identified by letter) rather than the whole figure, to facilitate rapid appreciation of data.

In the revised manuscript, we will go through all the figure references and specify the parts of figures wherever relevant.