

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 $\delta^{18}\text{O}$ 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 $\delta^{18}\text{O}$ 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 $\delta^{18}\text{O}$ 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.

With respect to the $\delta^{18}\text{O}$ 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.

Another obscure use of the $\delta^{18}\text{O}$ 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.

These two instances where further explanation is required of the use of $\delta^{18}\text{O}$ data only emphasise the need to exclude discursive trace-element data and discussion, especially if (as recommended below) all the $\delta^{18}\text{O}$ profiles are included in the main text.

Some other points:

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

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.

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.

L 258. Some brief justification is required for the choice of value for water $\delta^{18}\text{O}$, even if it repeats Thibault et al. (2016) – this is an important issue in the present context.

L288. The parallelograms are not in ‘different shades of blue’.

L348. Exclude ‘multi-proxy’ (redundant).

L368. Exclude ‘vast’ – there are quite a lot of $\delta^{18}\text{O}$ values associated with a Mn content of more than 100 $\mu\text{g/g}$.

L373. The results for *C. gigas* are not in ‘grey/black’.

Fig. 6. Explain the vertical dashed lines (corresponding to the maxima in the $\delta^{18}\text{O}$ plot); change 1.0 to -1.0 for the water value on the y-axis. I think it would be worth having the $\delta^{18}\text{O}$ 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).

L425. ‘virginica’ in italics.

L437. ‘follows’ rather than ‘shows’

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’.

L457. This is where you need to be able to refer to all the $\delta^{18}\text{O}$ profiles.

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

L583. ‘placed’ rather than ‘replaced’.

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

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.

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

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.