

***Interactive comment on* “The effect of salinity on the biogeochemistry of the coccolithophores with implications for coccolith-based isotopic proxies” by Michaël Hermoso and Marceau Lecasble**

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Dear Editor,

We are very grateful to the Reviewer 1 for his time in reading and reviewing our manuscript. We were very pleased to read his overall good opinion on our study. All his remarks can be relatively easily dealt with in a revised version of our manuscript, should we be invited to submit one. You will find our responses to his comments below.

Sincerely,

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Michael Hermoso

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*Review Hermoso and Lecasble (BG-2018-357)*

*Dear editor,*

*After careful assessment of the manuscript of Hermoso and Lecasble on the effect of salinity on coccolithophore calcite chemistry, I recommend publication of minor revisions. Below, I listed some minor comments that I hope will further improve the text. I have only one serious issue with this work: the statistical basis for the regressions is lacking. There is no explanation as why the authors chose second-order polynomial fits or to what extent they fit the data better than linear functions. The authors often refer to '(in)significant' trends, but it remains unclear how this is determined.  $r^2$  values are themselves no indication of significance, as the authors suggest. This needs to be addressed in the revised version of their manuscript.*

*Sincerely,  
Lennart de Nooijer*

Authors's response: The choice of the second-order polynomial fits was not made *a priori*, but corresponds the best fit function of the dataset. The only implication of these "bell-curves" is that coccolithophores have salinity optima comprised in the range of the examined conditions, and that generally, growth rates (Fig. 3), cell size (Fig. 4), as well as the isotopic composition of certain coccolith species (Figs 6 and 7) changed on either sides of the optima. We will provide a Table as SI Material with the various regression coefficients for the polynomial (quadratic) and linear functions in each case.

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The statistical issue regarding the only  $r^2$  values presented in the text and figures will be addressed by performing suitable *goodness of fit* tests in a new version of the manuscript.

## Methods

*page 2, line 26/27: evaporation of seawater by sub-boiling to a salinity of  $\approx 40$  does not result in precipitation of salts. Preparing media by evaporation, however, also leads to differences in for example, total inorganic carbon concentrations, which might lead to differences in calcification. Perhaps the authors can use this as an alternative reason for preparing the culture media 'de novo'.*

Authors's response: This is a good point. We will add this and modify the rationale behind our choice to use the ESAW medium in the manuscript.

*Which brings me to the question whether the carbonate chemistry (pH, [DIC], TA) of the water was determined/ monitored during the culturing experiment.*

Authors's response: We opted to conduct our cultures using a very dilute (in terms of cell numbers) and semi-continuous batches to avoid artifacts linked to drifts in the composition of the medium. We did not monitor these parameters, as we refreshed the media every two days to ensure that the cells grew in the desired and initial conditions.

*page 3, line 14-17: this can be omitted since it has no further relevance for this manuscript. Since this section will then become a bit short, it may better be combined with the previous or next one.*

Authors's response: We will follow the Reviewer's suggestion.

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*page 4, line 7: should be 'days'. I think in equation (1), the 'number of days between  $d$  and  $d-1$ ' can be replaced by 'n'.*

Authors's response: We will replace this.

## Results

*page 6, line 3: I don't understand the 'sensitivity' in this sentence.*

Authors's response: We will change "sensitivity" for "response".

*page 7, line 13: please avoid 'lighter', but instead use 'more depleted'. See also elsewhere.*

Authors's response: We agree. This will be changed.

*page 7, line 22: use 'equilibrium values' instead of 'inorganic conditions'.*

Authors's response: We will implement this change.

## Discussion

*page 7, line 31/32 and later in the discussion: salinity has no unit. Not even per mille.*

Authors's response: This is correct. We will change this throughout the body text and in the figures.

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*page 9, line 4: but isn't the availability of CO<sub>2</sub> not also determined by the activity of the coccolithophores? E.g. by photosynthesis. Moreover, CO<sub>2</sub> may not be the preferred inorganic carbon species used for calcification (but maybe HCO<sub>3</sub><sup>-</sup>), so I fail to see the logic of this argument.*

Authors's response: Most coccolithophore species acquire DIC predominantly in the form of aqueous CO<sub>2</sub> by passive diffusion through the cell membrane. It is true, however, that the concentration of DIC in the coccolith vesicle, *i.e.*, at the site of calcification, is made by active transport of HCO<sub>3</sub><sup>-</sup> from the cytoplasm - see papers by Kottmeier et al. (2014 - Photosynthesis Research) and McClelland et al. (2017 - Nature Communications) referenced in our manuscript. Our data are not able to resolve these biogeochemical features, as it will require knowledge of more information, as the relative allocation of the carbon resource into photosynthesis and calcification. These mechanistic considerations are interesting, but beyond the scope of this paper that we would like to keep at an empirical level. Nevertheless, we will clarify this point in our revised manuscript, as there is indeed one or two missing sentences required to disambiguate this.

*page 11, line 10 and elsewhere: please italicize the 'p' in 'pCO<sub>2</sub>'.*

Authors's response: We will do that.

## Figures

*I don't see the added value of figure 1. The spatial resolution is too coarse to see the sal/ del-18O of the water at the sampling locations. Otherwise, these maps show known global distributions in sal and del-18O and including them here therefore seems superfluous to me.*

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Authors's response: We can remove figure 1 indeed.

*Figure 3, x-axis title and caption: salinity is unitless, so please remove the 'percent'. I think the upper x-axis title can also be removed. I find the  $r^2$  values not very useful: they are by themselves not indicative of a significant correlation.*

Authors's response: We will follow all these recommendations and add the p-values.

*Figure 4: same as for figure 3. Please remove the trendline for E. hux, morphotype B: a trendline usually suggests a (significant) trend, which there is not in this case.*

Authors's response: We will implement these changes.

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-357>, 2018.

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