

Interactive comment on “Influence of temperature and CO₂ on the strontium and magnesium composition of coccolithophore calcite” by M. N. Müller et al.

Anonymous Referee #2

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This study presents an empirical calibration for partitioning of trace elements (Sr/Ca and Mg/Ca) in coccolithophore calcite. The manuscript is written in clear prose and presents a very thorough account of experimental procedures followed. This study provides an important scientific contribution to both biomineralization and paleoproxy research. The results have implications for partitioning of trace metals into biogenic calcite, and likely apply to multiple clades of calcifying organisms, in addition to coccolithophores.

In general, the manuscript is well-written, but the authors present it as a calibration study and laboratory experiment, with implications for biomineralization pathways. A synthesis of the results within a broader context of oceanographic and paleoceanographic settings where these trace element proxies are applied would significantly strengthen this manuscript and highlight its scientific contribution.

The dependence of Sr/Ca as a proxy for temperature and growth rate is well-established. This study shows that the partitioning of Sr into coccolithophore calcite is also dependent on seawater carbonate chemistry. Importantly, these results challenge interpretations made in previous studies using Sr/Ca as a paleoenvironmental proxy, and complicate the interpretation of coccolithophore Sr/Ca from high-pCO₂ environments. This is a key contribution of this paper, and I think it should be stated more strongly in the abstract.

The introduction is fairly long, and only in the third paragraph does it start to succinctly frame the scientific importance of this particular study. This section should be shortened and focused to better highlight the contributions of this study to paleoproxy and biomineralization research. For example, the authors suggest in the conclusions that there is a need for updating coccolith partition coefficients, and how the application of these equations can fit into a broader scientific context (such as paleoproxy use or ocean acidification). This could also be addressed briefly in the introduction, to better frame the hypotheses this study tests.

Sr/Ca:
The results suggest that Sr/Ca is not a reliable proxy for productivity and temperature at elevated pCO₂ or with modified seawater carbonate chemistry. The implications of these results are substantial. The discussion and conclusion hint at one potential scenario where previous interpretations could be compromised (the Paleocene-Eocene Thermal Maximum). I think the authors should address this in further detail, and explore additional oceanographic or paleoceanographic settings where interpretations from Sr/Ca could be affected by their results.

Sr/Ca:

Data presented here suggest that the effect of pCO₂ on DSr is minor compared to that of nutrient limitation. Are there existing field studies or culture studies that have

addressed the effect of spatial variability of nutrient limitation on coccolith Sr/Ca? If not, could the authors suggest how to test this hypothesis further?

Mg/Ca:

Page 15: The results demonstrate inter-specific differences in measured Mg/Ca ratios. What implications does this have for choices of species in paleo-reconstructions?

On pages 16-17, the authors explore the idea that the mechanisms controlling biological cycling of Mg and Ca are so different that these two cations are effectively decoupled, and act independently. This is a fascinating idea. If the incorporation of Mg into biogenic calcite is tightly controlled by Mg-binding proteins, what are the implications for biomineralization mechanisms in other calcifying organisms? Are there implications for the existing Mg/Ca paleothermometers which have been calibrated for other calcifying organisms such as planktic foraminifera, especially from geologic times with different Mg/Caseawater?

I think that exploring these concepts in the discussion would add valuable insight to this manuscript. At the very least, the idea of decoupling Mg and Ca incorporation mechanisms should be mentioned in the conclusions.

Additional comments:

This manuscript provides a detailed description of the experimental setup used in calibration experiments, which is for the most part exemplary in its completeness. The authors describe two different methods for altering carbonate chemistry: modifying total alkalinity while keeping DIC constant, and modifying DIC while keeping total alkalinity constant. Why were these various methods selected? What are the implications for the dependence of DSr on different carbonate chemistry parameters? The rationale behind this part of the experimental setup, and the implications for interpreting records from high-pCO₂ environments, should be addressed in more detail.

On page 12, the text shows equations DSr. Were there also supposed to be Mg equa-

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tions? I suggest either including all regression equations, for both Sr and Mg, or not list any within the text.

Figure 1b: It would be clearer to list the partition coefficients for individual experiments on the figure (e.g., "DSr = "), although adding regression lines might make the figure illegible.

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