

# ***Interactive comment on “Coccolithophore productivity at the western Iberian Margin during the middle Pleistocene (310–455 ka) – evidence from coccolith Sr/Ca data” by Catarina Cavaleiro et al.***

## **Anonymous Referee #2**

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The paper by Catarina Cavaleiro and collaborators entitled ‘Coccolithophore productivity at the western Iberian Margin during the middle Pleistocene (310 – 455 ka) – evidence from coccolith Sr/Ca data’ examines the geochemical response (coccolith Sr/Ca elemental data) across the MIS12 – MIS9 time slice offshore Portugal. Based on published coccolithophorid culture finding, the Authors use the abundance of strontium relative to calcite in fossil coccoliths measure by ICP-AES to derive a palaeoproductivity index during the rapid climatic oscillations of the Pleistocene. The region of interest typified by the Portugal Current System was previously documented

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in terms of changes in the courantology, sea surface temperatures (among other key climate-sensitive data) in a bunch of publications (cited in the paper). The authors used this well-established framework to interpret fluctuations in Sr/Ca ratios and productivity in the sunlit waters. They also discuss their data at the level of the phytoplanktonic ecosystem as they argue that coccolithophorid growth (and productivity) is dictated by macro and micronutrient availability and the competition with diatoms. They mainly focus their biogeochemical discussion on MIS 12-10 showing higher productivity at the beginning of these climate transitions. Playing at different timescales, they ultimately compare their coccolithophorid productivity indices to the available *i)* alkenone fluxes and *ii)* nannofossils accumulation rates in published literature and found some coherencies and discrepancies.

I am generally supportive of publication of this work in *Climate of the Past*. I have, however, a number of comments and questions, which I hope the Authors will find fair and useful to prepare their revisions.

### General comments

- It would be good to state what was measured exactly. 'Coccolith fraction' is not sufficient as the less than 20 micron filtrate may contain many non-coccolith particles. Some photos will be welcome from key samples to illustrate this.

- There was this nice paper by Omta *et al.*) that came out a few years back (On the potential role of marine calcifiers in glacial-interglacial dynamics - doi:10.1002/gbc.20060) in which an elegant model linking ocean alkalinity and the flourishing of coccolithophores at the inception of deglacial periods (with a possible role on the deglaciation). This paper has been omitted in the present study. I urge the Authors to explore such a control on their productivity data. Even if the periods are not necessarily the same, another useful related paper is that by Duchamp-Alphonse developing the carbonate counter-pump aspects (Enhanced ocean-atmosphere carbon partitioning via the carbonate counter pump during the last deglacial – doi:10.1038/s41467-018-

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04625-7). What I am trying to say is that the Authors did a pretty good job in integrating local and regional data but quantitatively understanding pelagic calcification requires a bigger biogeochemical picture.

- Sentence line 283 '*We would like to stress that our study focuses on the qualitative characteristics of the coccolithophore paleoproductivity record, rather than quantitatively estimating the productivity of coccolithophores.*' is misleading and made me doubt about my understanding of the paper. If the Authors interpret Sr/Ca ratios, they intrinsically develop a quantitative approach pertaining productivity in the surface waters.

- Removing the temperature effect from Sr/Ca data to derive productivity component only. I am still debating with myself to be honest. When I read the paper for the first time, I found that it was a good idea. But the more I think, the more I believe that this is not. Both calcification rates and temperatures (and the control of the latter on the former) synergistically dictate Sr/Ca coccolith ratios. Thus dissecting the proxy may induce an artificial bias. I leave these thoughts to the Authors for their revisions. . .

- Emerging from the previous point, the heart of the Sr/Ca productivity proxy is poorly approached in this paper. The Authors mix cellular growth rate, primary productivity, and calcification rates. This is only calcification rates that control the substitution of Sr to Ca. Yet, culture data are unable to properly measure calcification rates, as they only document the bulk over the course of the batch experiments (See the Appendices in Stoll *et al.* 'Climate proxies from Sr/Ca of coccolith calcite: Calibrations from continuous culture of *Emiliana huxleyi*' published in 2002 in GCA). Thus, the generalisation of the proxy to productivity is far-fetched, as it implicitly means primary productivity in turn leading to the strength of the biological pump. I think that the Authors should clarify this.

- The Authors spent considerable effort (and space in the manuscript) to try and find a good match between their coccolithophore productivity and the sedimentation of *Point 1* coccolith-derived calcite (NAR) on one hand, and *Point 2* coccolith-derived compound-specific organic matter (alkenones) on the other hand. *Point 1* For the

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reasons outlined above, the Sr/Ca has not to scale with the bulk production (-ity) of calcite. This geochemical proxy has to do with intracellular processes why the production of calcite is also related ecologically with the density of cells in seawater and cellular division rates. *Point 2* We know that alkenones are not only synthesised by the coccolithophores but also by other non-calcifying haptophytes (incl. naked coccolithophores). Furthermore, the export of calcite and organic matter from the top of the water column down to the seafloor obey to different processes (as their on the seafloor and during sedimentary burial diagenesis).

Therefore, I cannot see why all these parameters should scale. I am not aware of any sedimentary succession in which this is the case. I am happy to be wrong though.

- I personally disagree with the fact the Si and Fe concentrations relative to Ca are meaningful in such a sedimentary study nor that they reflect the palaeoconcentrations of these elements. Si and Fe are very tricky to measure and it is unlikely that the measurements reflect the composition of coccolith calcite. Even if it was the case, by which means (proxy) the coccolith Si/Ca ratios would reflect the concentration of silicic acid in ambient waters?

- The Authors have managed to lose me with the concept of phenology they are trying to introduce. This is a black box concept and this is very misleading or at least not clear at all. Could they elaborate?

- I found the statistics very poorly treated in the manuscript.

### Specific comments

Pg 1 Line 16. Perhaps use Carbonate Counter-Pump instead?

Pg 1 Line 30. This what?

Pg 1 Line 33. Not clear to me.

Pg 2 Line 40. Circumvoluted sentence. Consider splitting it.

Pg 2 Line 59. I disagree with this statement (see General points).

Pg 3 Line 68. I wonder whether the changes in size of gephyrocapsid coccoliths could

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influence the Sr/Ca ratio

Pg 4 Line 97. Poorly defined in terms of what?

Pg 5 Line 122. Minimum numbers. Do you mean absolute or relative abundances?

Pg 6 Line 156. The less than 20 micron fraction contain non coccolith particles. The Authors should do a better job in the characterization of the calcite / dolomite particles analysed. This is crucial.

Section 3.4. I don't understand what is the relevance of this.

Section 4.1. belongs to the discussion. Section 4.2 should come first noting that the description if the results is extremality skinny.

Figure 4. Please make the ages more legible.

Figure 5. What is the significance of the anti-correlation between Mg and Sr?

Figure 6 is unnecessary in my opinion.

Pg 14 Line 303. See my general comment on temperature and productivity on Sr/Ca ratios.

Pg 16 Line 333. What do you refer to with 'opportunistic and fast growing species' here?

Pg 16 Line 355. Methodologically unjustified even using 'weak' acid.

Pg 17 Line 361. Sentence not clear and too long.

Pg 17 Line 371. Decrease of the SST.

Pg 18 Line 421. I am not following the logic here. Are the Authors trying to say that the ice coverage reached the studied area?

Pg 19 Line 449. I don't understand the point that the Authors are trying to make here.

Pg 19 Line 454. Visual comparison of what?

Pg 19 Line 457. An illustration of the poor statistical approach here. . .

Pg 19 Line 457. An illustration of the poor statistical approach here. . .

Pg 21 Lines 477- 492 and figure 9 are not necessary.

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