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Climate of the Past Discussions

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.

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We appreciate the referee's effort and comments and believe they will improve the quality of the paper. Please find our answers below (italicized).

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 pub-

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lished 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 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 that 20 micron filtrate may contain many non-coccolith particles. Some photos will be welcome from key samples to illustrate this.
We thank the referee's comment and we will further clarify the composition of the "coccolith fraction" in the revised manuscript because indeed, it may contain non-coccolith particles. However, as explained in the methods section, all samples were treated to avoid Sr contamination from non-carbonate particles (assuming that foraminifera and foraminifera fragments were extracted during sieving). Plus, the only existing picture (see below) of site MD03-2699 is from sample 1898 (referring to the depth in the core – 1898 cm) with a corresponding age of 485 kyr (not covered in our research). Note that this photo was taken during a master class exercise and consequently it tried to gather as many different coccoliths in picture as possible.

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Discussion paper

- 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 flourishment 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- 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.
We thank the referee for mentioning these papers, we will include them and take them into account, with an approach centered in the processes, also in the line of referee #1's comment.

- 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.
We thank the referee's comment and we would like to stress that this sentence was included in the manuscript to clarify the reader that the CF Sr/Ca ratio is not an absolute productivity proxy neither does it allow for the calculation of absolute marine productivity in terms of production of organic carbon or calcium carbonate by coccolithophores.

- 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 Emiliania 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.

We thank the referee's comment and we will further clarify these points in the revised manuscript. We further acknowledge the extent work already done in correlating coccolith Sr/Ca ratio with coccolithophore productivity. Stoll and Schrag, 2000 initially suggested that the CF Sr/Ca ratios are strongly controlled by coccolithophorid growth and calcification rate. Stoll et al., 2002a (Potencial and limitations of proxy) and Stoll et al., 2002b (E. huxleyi cultures), Stoll et al., 2002c (multi species cultures), Stoll et al., 2007a (Arabian and Sargassum seas) and Stoll et al., 2007b (bay of Bengal) used culture records, sediment traps and sediment samples to confirm the relationship between coccolith Sr/Ca ratios and coccolithophore productivity (coccolithophore growth rate and coccosphere export). Furthermore, Stoll et al., 2002a and Mejia et al., 2013 clearly stated that the temperature effect on the CF Sr/Ca must be addressed when reconstructing past coccolithophore productivity. Indeed, in our research the extraction of the temperature effect does not represent a major change of the original curve. However, Cavaleiro et al., 2018, show a final coccolithophore productivity record notably different from the original coccolith fraction Sr/Ca curve due to the large influence of temperature in that area. Our temperature correction in the Iberian margin site re-

inforces that, contrary to the open ocean mid North Atlantic, the temperature changes in the Iberian margin do not seem to have had affected the coccolith fraction Sr/Ca and consequent coccolithophore productivity. Plus, the possibility to use a proxy that is independent of accumulation rates allows comparison with commonly used "coccolithophore productivity proxies" such as nannofossil accumulation ratios and alkenone export from which coccolithophore productivity, in the ancient photic layer, is commonly inferred from. Finally, the term productivity is thus in this research used as a coccolithophore productivity proxy directly associated with coccolith calcification rate and generally associated with increased cell division and growth of coccolithophores that could lead to increased particulate organic matter and calcium carbonate export.

- 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 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 synthesized 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.
We thank the referee's comment and we hope to clarify these doubts, also in line with some of referee #1's comments, by re-structuring the paper focusing on processes and on the advantages of the multiproxy approach.

- 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?
We greatly appreciate the referee's comment and based on comments of both reviewers we will verify our assumptions and either clarify or delete them from the revised manuscript.

- 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?
We thank the referee's comment and we will further explain the definition of phenology and why changes in climate could represent changes in the coccolithophore phenology, i.e., how warmer or colder conditions (and consequent climatic changes) could actually lead to changes in the productive regime of coccolithophores throughout the year.

- I found the statistics very poorly treated in the manuscript.
We thank the referee's comment and, if referring to the spectral analysis, in line with the comments from referee #,1 we will decide whether to keep or delete the spectral and cross-spectral analysis.

Specific comments Pg 1 Line 16. Perhaps use Carbonate Counter-Pump instead?
This will be changed accordingly.

Pg 1 Line 30. This what?
"This" refers to the fact that more nutrient-rich waters decreased the competition with diatoms for nutrients. It will be written more clearly in the revised version of the manuscript.

Pg 1 Line 33. Not clear to me.
We thank the referee's comment and this will be addressed in the body of the paper

to clarify why changes in climate could represent changes in the coccolithophore phenology, i.e., how warmer or colder conditions (and consequent climatic changes) could actually lead to changes in the productive regime of coccolithophores throughout the year.

Pg 2 Line 40. Circumvoluted sentence. Consider splitting it.
We agree with the referee's comment, also in line with referee 1's comment, this will be changed to: "They are the most important unicellular primary producer producing calcite (Brand, 1994) contributing up to 60 % to the total oceanic calcium carbonate (Flores and Sierro, 2007) and sensitive to rapid fluctuations in temperature, salinity, nutrients, and turbidity of surface waters (Baumann et al., 2005; McIntyre and Bé, 1967). Coccolithophores had a peak contribution of >80 % in the interval of Marine Isotope Stage (MIS) 15 to MIS 9, when the assemblages were by far dominated by gephyrocapsids (Baumann and Freitag, 2004; Saavedra-Pellitero et al., 2017)."

Pg 2 Line 59. I disagree with this statement (see General points).
We thank the referee's comment and we will further address and clarify this (see also the reply given above, to the general points' replies).

Pg 3 Line 68. I wonder whether the changes in size of gephyrocapsid coccoliths could influence the Sr/Ca ratio.
We thank the referee's comment but believe this is out of the scope of our research since it is not our intention to better understand how coccolith Sr/Ca varies with the size of gephyrocapsa coccoliths.

Pg 4 Line 97. Poorly defined in terms of what?
We thank the referee's comment but believe this is out of the scope of our research since it is not our intention to better describe the Portugal current system. We can however substitute "poorly defined due to" by "with".

Pg 5 Line 122. Minimum numbers. Do you mean absolute or relative abundances?
We thank the referee's comment and we will address this by clearly stating absolute

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.
We thank the referee's comment and this will be clarified in the revised version of the manuscript.

Section 3.4. I don't understand what is the relevance of this.
We agree that this spectral analysis might deviate the reader from the most important aspects of our research. Therefore, it is very likely that this analysis will be left out of the revised manuscript after the re-structuring and re-focusing of the paper.

Section 4.1. belongs to the discussion. Section 4.2 should come first noting that the description if the results is extremality skinny.
We thank the referee's comment and given the re-structuring of the paper, it is also likely that this section is moved to the discussion.

Figure 4. Please make the ages more legible.
This will be changed accordingly.

Figure 5. What is the significance of the anti-correlation between Mg and Sr?
We thank the referee's comment and this information will be added to the manuscript. The p-value of the Pearson correlation is 0, therefore the relationship between either the coccolith fraction and the coccolithophore productivity and the coccolith fraction Mg/Ca is highly significant, as expected. It is the outliers, or higher values of Mg/Ca that are associated with very low CF Sr/Ca ratios and coccolithophore productivity results, mostly associated to abrupt and cold millennial-scale events.

Figure 6 is unnecessary in my opinion.
We thank the referee's comment and it is very likely that this spectral analysis will be left out of the revised manuscript after the re-structuring and re-focusing of the paper.

Pg 14 Line 303. See my general comment on temperature and productivity on Sr/Ca ratios.
We thank the referee's comment and believe we have already commented this matter above.

Pg 16 Line 333. What do you refer to with 'opportunistic and fast growing species' here?
We thank the referee's comment and clarify that by "opportunistic and fast growing species" we refer to species r-selected. This will be re-written and clarified in the revised manuscript.

Pg 16 Line 355. Methodologically unjustified even using 'weak' acid.
We thank the referee for this comment and we will look carefully and clarify on the revised manuscript or delete the assumption that higher coccolith fraction Si/Ca and Fe/Ca could evidence higher competition with diatoms.

Pg 17 Line 361. Sentence not clear and too long.
We thank the referee for this comment and we will look carefully and clarify on the revised manuscript or delete the assumption that higher coccolith fraction Si/Ca and Fe/Ca could evidence higher competition with diatoms.

Pg 17 Line 371. Decrease of the SST.
This will be changed accordingly.

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?
We thank the referee for this comment and clarify that we have not stated or suggested that ice coverage reached the Iberian margin. We are stating research (Line 338) that has found evidences of the presence of melting icebergs in the western Iberian margin during rapid millennial-scale events.

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

We thank the referee's comment and this will be written more clearly in the revised version of the manuscript.

Pg 19 Line 454. Visual comparison of what?
We thank the referee's comment and further clarify that the records of coccolithophore productivity, nannofossil accumulation rate and alkenone flux were compared visually.

Pg 19 Line 457. An illustration of the poor statistical approach here. . .
We thank the referee's comment and the statistical analysis will be provided in the supplementary material with the respective p-values.

Pg 21 Lines 477- 492 and figure 9 are not necessary.
We agree that this spectral analysis might deviate the reader from the most important aspects of our research. Therefore, it is very likely that this analysis will be left out of the revised manuscript after the re-structuring and re-focusing of the paper.

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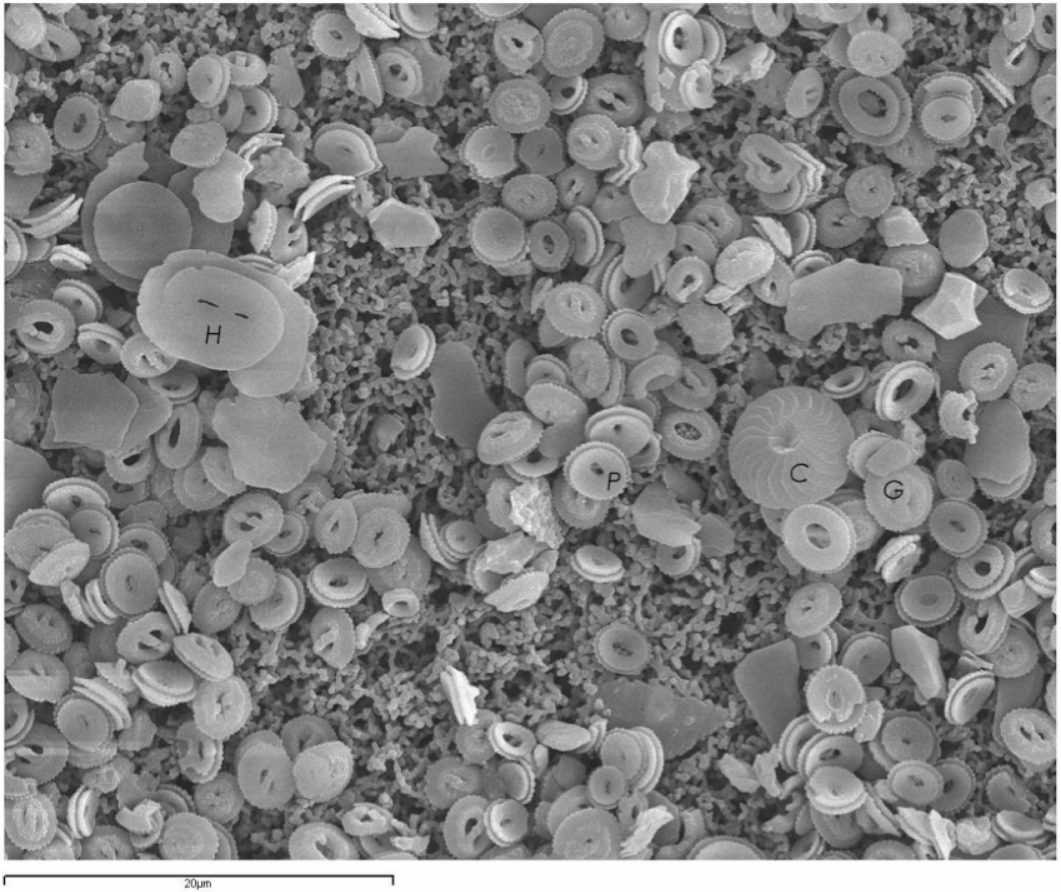

Fig. 1. Figure 1 – SEM picture from site MD03-2699, at 1898 cm with a corresponding age of 485 kyr. The irregular surface background is the filter and several coccoliths from different species can be seen, na