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

This is a very nice piece of work, presenting new high-resolution coccolith Sr/Ca records from the mid Pleistocene of the Iberian Margin. Coccolith Sr/Ca is an underused but potentially powerful proxy for understanding the cellular growth and calcification rates of the dominant marine calcifying phytoplankton, the coccolithophore algae.

Of particular value in this manuscript, is the integration of these records of growth rates, with other records of export flux to sediments of both the organic (alkenones) and inorganic (coccolith) carbon fixed by these phytoplankton. Together these allow a nuanced interpretation – including the coupling / decoupling - of surface ocean growth conditions and aggregated net export. I would recommend publication with revisions. In particular I would recommend shortening the manuscript and focusing on the strongest signals within the data in order to generate the impact that this work deserves.
We agree with the referee and we will shorten and re-structure the manuscript accordingly.

Comments (in order of the text): Line 17 – instead of "climate models" better to say "Earth System Models" as the common understanding of a "climate model" is one that doesn't include biogeochemistry. And again at Line 48 (and anywhere else) – "climate models" including coccolithophore productivity – better to talk about Earth System and/or biogeochemical and/or carbon cycle models.
This will be changed accordingly.

Line 18 – "coccolithophore paleoproductivity past reconstructions" doesn't make sense
It will be changed to "coccolithophore paleoproductivity reconstructions".

Line 23 – define SST at first usage
This will be changed accordingly.

Lines 38-42 long sentence that jams together two concepts – split.
We agree with the referee 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)."

INTRODUCTION In both the introduction and the discussion, I feel the absence of a clearly articulated question – what is the "knowledge gap" and how does this paper address that gap? There are suggestions of problems in the representation of coccolithophore production and export in biogeochemical models, but no sense of what the specifics of these are, or how they might be addressed by this study. When I first read this section, I was not convinced that models could be informed by new coccolith Sr/Ca records (but see below). Then there is the time period studied – there is a general overview of the significant changes going on through this interval, but the rationale for looking at the coccolithophore response is so broad that it loses meaning: "to evaluate this phytoplankton group's behaviour and gain a better understanding of its response to climate conditions during glacials, interglacials, deglaciations and the transition from interglacial to glacial conditions, at both orbital and sub-orbital time scales." (line 85 on). Or: "We aim to characterise long-term changes in coccolithophore productivity in such a system, where their behaviour in the past remains unknown." (line 74) or: "...and evaluate the main factors influencing coccolithophore productivity." (line 77) I would really like specifics of: 1) the dynamics / processes that you seek to investigate and 2) why these intervals.
We agree with the referee's comments and we will further clarify and narrow the main purpose and goal of this research. We have chosen the Iberian margin because this area is subjected to seasonal upwelling but particularly sensitive to climate change. The behavior and long-term response of coccolithophores in an area subjected to seasonal upwelling remains unknown. Therefore, we aim to characterize coccolithophore's response in an upwelling area subjected to significant climatic changes, such as glacials, interglacials and the transitions between interglacial and glacial substages.

I think part of your struggle is related to: 1) setting up the Sr/Ca as something that is a "better" measure of coccolithophore "productivity" than other approaches – e.g.

NAR/alkenone accumulation; and 2) equating coccolith Sr/Ca with quite a loose concept of "productivity". Together these end up setting off your "productivity" records "against" one another, rather than being mutually informative about different components of the growth – export – accumulation system. This seems to lead to a discussion which is phrased in terms of "mismatch" rather than one that allows the complexity of the system response to be seen, because, you've got the advantage of multi-proxy data covering different aspects of the same system.
We will narrow the purpose and aim of the paper and re-structure the manuscript focusing on the processes and on the advantages of the multiproxy approach.

My recommendation is that you recast this introduction somewhere along these lines: 1) There are significant uncertainties about the complex interactions between coccolithophore growth rates, nutrient dynamics, seasonality, export (carbonate / organic carbon), dissolution and final accumulation / burial rates. These uncertainties make modelling the responses of this system to modern environmental change problematic. 2) These questions can only be addressed with: a) records that test the dynamic response of various components of the system over a reasonable range of change (i.e. palaeo records); and b) through multi-proxy studies of growth environment (Sr/Ca) and export of both organic (alkenones) and inorganic (NAR) carbon. 3) Then make the argument for the particular time period studied providing the chance to test a range of particular environmental conditions – and make your introduction to the time period outline what these might be – e.g. upwelling, seasonality, temperature... With this set-up, hopefully you'll then be able to circle round in the discussion and answer these questions.
We greatly appreciate the referee's suggestions and we intend to include them in the introduction.

Sr/Ca VARIATIONS AND ASSEMBLAGE CHANGES Line 68 – 69: I'd like some more justification for the assumption that "assemblage changes don't matter". My reading of the Fink et al. 2010 paper was that the abundance of the larger *Calcidiscus lep-*

toporus did have a significant impact on CF Sr/Ca. I'm also suspicious of using the logic that in some instances in the modern oceans CF Sr/Ca changes coincide with productivity changes, therefore, it must be productivity, when coincident assemblage changes haven't been properly considered. Given expertise of Baumann and Stoll, I would like to see some more justification of this point, that CF Sr/Ca are really dominated by changes in growth rate, rather than assemblages, especially with respect to *Calcidiscus*, which I suspect can contribute strongly to some records that show large Sr/Ca variations.
We thank the referee's comment and we will make this clearer in the revised manuscript. In summary, in our research, given the vast majority of gephyrocapsids (97 % average, 60 % of Gephyrocapsa caribbeanica*) and very low abundance of* Calcidiscus leptoporus *and* Helicosphaera *sp., it seems unlikely that changes in the range of their average relative or absolute abundance would have a significant effect on the coccolith fraction Sr/Ca ratio.*

Later – Line 179 – you talk about being in the Gephyrocapsa acme and that this makes assemblage variability less of an issue for CF Sr/Ca, but this somewhat admits that assemblage change can be an issue under other circumstances. A clear delineation – even without hard and fast data / rules – between when assemblage changes are likely and not-likely a problem with CF Sr/Ca would be better than trying to imply there is no issue. Please be precise with the logic and transparent with the reader as to if and where assemblage change might be playing a significant role.
The referee is correct when stating that indeed changes in the coccolith assemblage might bias the interpretation of the coccolith fraction Sr/Ca proxy. However, we believe that to delineate a threshold above which changes in coccolith assemblage would significantly bias the coccolith fraction Sr/Ca ratio is out of the scope of this research. As mentioned previously, in our research it is very unlikely that the changes in Calcidiscus leptoporus *abundance could bias the CF Sr/Ca results, given the Gephyrocapsids dominance. Plus, statistically, we find that there is no significant relationship between the relative abundance of* Calcidiscus *and our coccolith fraction Sr/Ca ratio or coccol-*

ithophore productivity proxy (see correlation charts and Pearson's correlation results below – Figure 1 and Table 1).

Line 184 – gephyrocapsids's – lower case I believe.
Correct, this will be changed accordingly.

Lines 255- bias from other carbonate phases – from Figure 5 it looks like the trend starts before the cut-off used, from more like 30 mmol/mol Mg/Ca. And could you please clarify which "cut off" you are using, whether this is Sr/Ca below 1.8 mmol/mol or higher values of Mg/Ca? If tracing contamination from other carbonates, would it make more sense to use the Mg/Ca values for the cut-off? For example you could cut off more stringently, at 30 mmol/mol Mg/Ca and yet maintain what look like more robust / primary signals of lower Sr/Ca within data that would pass this criteria. You would lose a few more data points in total, but I think this would be a more defensible cut-off point and rationale.
We aim to clarify this in the revised version of the manuscript. By combining the data from CF Sr/Ca and CF Mg/Ca ratios we defined a threshold of 1.8 mmol/mol because until such level most of the samples showed quite low CF Mg/Ca results (on the same range of the major cluster of the data). However, as suggested by the reviewer, we will consider changing the CF Mg/Ca ratio from 50 mmol/mol to 30 mmol/mol, to more clearly highlight the samples where some biasing by other carbonate phases might have existed.

Line – 272 – "This interval represents 39% of the whole sampling variation. . .", doesn't make sense to me, please clarify. Do you mean something like the clipped data represents 39% of the dynamic range in Sr/Ca of the full sample set?
We agree with the referee and the wording will be changed in the revised manuscript to make sure that this information is clearly given to the reader. We will substitute that paragraph by the following one: "The coccolith fraction Sr/Ca ratio results varied between 1.2 and 2.4 mmol/mol (sampling range of 1.16 mmol/mol) but 85 % of the samples returned results between 1.8 and 2.3 mmol/mol. These 85 % of the sam-

ples only represent 39 % of the total sampling range." This means that only 15 % of the samples fall on 70% of the sampling range. And, most of them coincide with the samples with higher bias likelihood from other carbonate phases.

Lines 288-289: is significance level of 80% really enough to be confident that the 6ka peak is real? I'm not really convinced. I don't think this spectral analysis reveals anything and is a distraction for the reader - you have a nice tight coupling to well-resolved climate records (Uk37) and a good age model for making 104-year correlations to N. Atlantic climate records, so I don't think there's a need to try to resolve periodicities independently within this record. I would leave this analysis out.
We agree that we should focus the purpose of this research in long-term processes and not so much on shorter scale changes. Hence, 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.

Lines 355 – 350: are all these paragraphs part of one argument? If so combine.
All paragraphs from current line 335 to line 350 will be one paragraph only, as suggested by the referee.

Line 356: Si/Ca fraction. With the preparation methods and uptake by acetic acid digestion, can the authors please clarify which phase / sedimentary component they think the Si is coming from? Is it likely dominated by biogenic Si?
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.

Lines 360 – 365: I think I get what you're trying to say, but this could be expressed more clearly.
We thank the referee's comment and we will make this clearer in the revised manuscript.

Lines 435 – 437: what has MIS12a got to do with MIS 2 and 6? Not clear what your point is here.
We thank the referee's comment and the reference to MIS 2 and 6 will be either deleted or clarified in the revised manuscript.

DISCUSSION: In general, the discussion feels long and could have more focus. It feels like you are discussing every aspect of the record from a descriptive perspective, rather than focusing on what the data tell you about processes. I've got specific comments below, but I would focus on the broader longer-term trends and behaviour of the coccolithophore productivity and export system during times of distinct oceanographic conditions (i.e. between the stages) rather than the millennial scale lead and lags (dubious as to how robust these are). It would be great to use these different intervals to try frame clearly articulated conceptual models about how and why growth rate is coupled or decoupled from organic and inorganic carbon fluxes at different stages. Such conceptual models would have the potential to genuinely inform the thinking of biogeochemical modellers by providing clear patterns of change that should be reproducible by numerical biogeochemical models of these systems. But I think you need to clearly formulate these, in words and ideally schematics, in order for them to take notice of your data. This is also where the multi-proxy approach you take is a clear ADVANTAGE, it's not about "mismatches" in the data, it's about using multi-proxy data to represent the responses of different components of the primary production to export system. Section 5.2 – looking at the records, I think it's a matter of scale at which they are interrogated. Yes, they are subtly different, but they also preserve some of the same features with reasonable fidelity – for example there are broad trends from MIS 12 to 11, to 10, to 9 that are conserved between the proxies. I would consider taking off the "Mismatch" from your title to this section, to give you space to consider both the agreements and the divergence. This would be more helpful in the communication of the key findings of the study – point out the agreements first and then suggest the mismatches. Following on from this Line 474 – alkenone and n-alkane fluxes mostly reflect conditions of increased export / preservation of organics. Maybe, but I'm not sure that

this can, or should, then be decoupled from “rather than coccolithophore growth”. . .in line 475. The first order coupling with some features of the coccolith accumulation rates (NAR) and the alkenones, and also your Sr/Ca measure – e.g. the transition from MIS 12 to 11 – would suggest a coupled system change, and this could be the case if increase coccolithophore production was part of the driver of increased general MAR and organic export and accumulation rates? I just wouldn’t be so quick to decouple these components. Lines 477 – 485: leads and lags of the alkenone versus Sr/Ca records of <1000 ka – is this getting down to the resolution of sampling uncertainty / offset? Were the records based on the same sample set?
We thank the referee’s comment and confirm that the samples where both CF Sr/Ca ration and alkenone content and accumulation (and nannofossil accumulation rate) were measured are from the same set of samples but the alkenone content and consequent sea surface temperature estimation had a higher sampling resolution.

Figure 8 – useful to have the Uk37 temperatures on this figure as well for reference.
This will be changed accordingly.

Line 498 – “the NAR reveals large amplitude shifts during times of high coccolithophore productivity” – this seems to be missing the point. The Sr/Ca also shows large amplitude shifts within this period, arguably larger than the NAR. The point is that the NAR steps down substantially form MIS 11b to 11a, whereas Sr/Ca remains high (as you say in preceding sentence). This to me is the interesting system change, and there’s a reverse trend in alkenone MAR (gentle rise in values into MIS11a). Could this be a seasonality thing between Sr/Ca and NAR? I.e. growing faster (higher Sr/Ca) but for a shorter growth season (less coccoliths?)? Seasonality could (maybe?) also be coupled with more efficient export and preservation of organics (alkenone MARs)? Ah, yes you come to this in Section 5.2.3. But, (see comment below), I think you could condense, simplify and make your interpretation of the key points more clearly in the discussion; including integrating 5.2.3. with these discussions of the data. Lines 505 – 509: again the question would be about sampling uncertainty between the records –

can you confirm that these leads / lags are meaningful on the sub-millenial scale? I’m just pushing back, because, as a reader, they do not convey a strong argument about process or feature of the data that I would be confident in. This feeds into a general point about the discussion – I think this could be edited down quite considerably, so that your key points are more clearly and forcefully stated (and more easily digested by the reader!). This cross-spectral analysis doesn’t add anything for me.
We greatly appreciate the referee’s comments on the Discussion and, accordingly, we will shorten and re-structure it, focusing on processes and on the advantages of the multiproxy approach.

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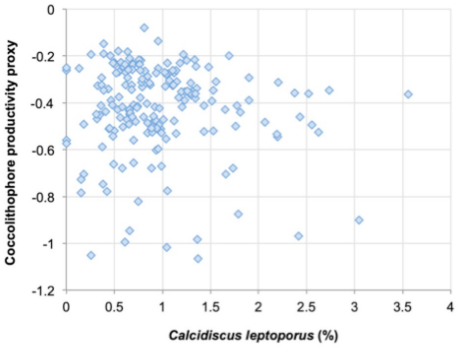

Figure 1

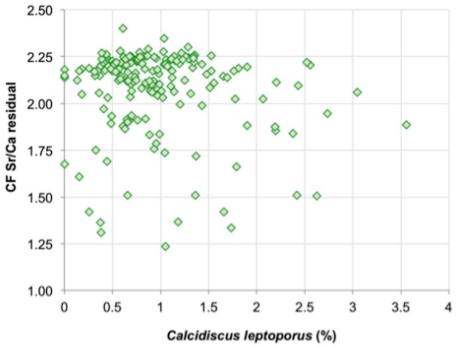

Figure 2

Fig. 1. Figures 1 and 2 showing cross-plots of Calcidiscus leptoporus with coccolithophore productivity proxy (Fig. 1) and the coccolith fraction (CF) Sr/Ca ratio (mmol/mol).

Pearson's Correlation coef.	R	p-value	*H0 (5%)*
CF Sr/Ca ratio (mmol/mol) vs *C. leptoporus* (%)	**-0.14**	0.07	*accepted*
Coccolithophore productivity proxy vs *C. leptoporus* (%)	**-0.09**	0.21	*accepted*

Fig. 2. Table 1 shows the Pearson's correlation analysis of Calcidiscus leptoporus abundance and both CF Sr/Ca ratios (mmol/mol) and coccolithophore productivity reconstruction