Biogeosciences Discuss., 4, S1780–S1786, 2007
www.biogeosciences-discuss.net/4/S1780/2007/

Biogeosciences Discussions

Interactive comment on "Calcite production by Coccolithophores in the South East Pacific Ocean: fromdesert to jungle" *by* L. Beaufort et al.

Anonymous Referee #3

Received and published: 2 November 2007

1. General comments

The authors presented results of coccolithophore production (both calcite and standing stocks) from 20 stations located mainly in the highly oligotrophic South Pacific Gyre (SPG), but also in the high productivity areas of the equatorial and the Peru-Chile coastal Upwelling (PCU) systems. The objectives of the paper are not clearly stated (see lines 15-20 p.3269) but I guess were two: 1) to monitor the coccolithophore production (both coccospheres and coccoliths) and their calcite in high and low fertility regions; 2) to present automation microscopy for taxonomical and calcite quantification (coccosphere and coccoliths and unknown suspended small calcite particles) in these samples. There are a lot of interesting data in this manuscript but unfortunately the outcome is confusing and the conclusions are not supported by the data. I would pre-

fer to see a clearer and more straightforward paper where no unjustified corrections of the data are done. One of the main interesting conclusions is that maximum coccolithophore production is occurring at the DCM. The title is catchy but misleading since the results don't really show these very large cell concentrations or calcite production.

2. Methods:

It is unfortunate that the only 4 stations sampled in the high productivity waters of the PCU don't really allow a quantitative estimation. To filter 4 liters of waters in 25 mm diameter membrane (the authors probably mean 25mm standard size and not 23mm) in such a region can be a major methodological problem (usually for high productivity regions no more than 2 liters of water on 47mm diameter filters). The large thickness of the material on the filters doesn't allow absolute quantification. The assumption that the absolute numbers are underestimated in a constant way is not necessarily correct since it depends on the type of particles filtered and the even distribution of them. In addition, I am not sure about the precision of the automatic method for the calcite particle analyzer with such samples since there is already an issue related to the difficulty of focusing calcite particles on a membrane (lines 25 -29 p.3273). This is not discussed.

I like the idea of automatic calcite particle quantification but I have a general comment. Since it is a new method (particularly for water samples), it is important to quantify errors and possible error sources.

The automated analysis (ANN) of coccolith and coccospheres (SYRACO) is limited to Emiliania huxleyi and Gephyrocapsa oceanica in the spherical coccospheres $<$10mm in diameter. It is fine to quantify only these two species but what it is unclear to me is how the system doesn't identify very similar spheric coccospheres as stated in lines 11-13 p.3273. In addition, the results from the reliability test briefly mentioned in lines 14-22 should be presented and discussed.

Since the results of automatic counting of individual coccoliths is not satisfactory (al-

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ways lower than human counts lines 23-24 p.3273) why have they been shown with no estimation errors or used at all? Regarding the E.huxleyi size correction (line 20-25 p.3274), it is stated that for small placolith-like E.huxleyi the entire distal shield is not detected in cross-polarized light. The measurements are then multiplied by a factor of 1.25. Why 1.25? Also if the authors expect different degree of calcification, probably this factor can drastically change.

In chapter 2.6 the 'complex'identified by SYRACO is described as EGC Emiliania, Gephyrocapsa and Crenalithus. Crenalithus has never been mentioned before as one of the automatically identifiable genera. What is the importance of Crenalithus in these samples? The authors need to provide convincing data that the EGC complex is mainly E.huxleyi. Only a paper in preparation is mentioned as a reference (Couapel et al., in prep). I was also puzzled by the quantification of the EGC with respect to the total coccolithophore community. If scanning electron microscope (SEM) is being used for quantifying the total assemblages (and these are new results) this should be explained in detailed in the Methods. In general in the method chapter what I missed the most is a robust statistical testing of the automatic methods used for the water samples and the corrections applied to the data set. It is also unclear when the automated microscopy and human counting results are used. Also the authors should state clearly the species that were detected both for coccospheres and coccoliths. If not all the species are counted, how are the total coccoliths /ml plotted (fig.6)?

3. Results

In chapter 3.2, based on the observations on an almost monospecific sample (ST18 at 30m) the authors conclude that automated system missed 60% of coccoliths because it was out of focus. What is surprising is that the authors apply then a correction factor of 2.5 to all samples to conclude that the EGC coccoliths represent 50% of all suspended calcite particles $<$46micron. To extrapolate the semi quantitative error of one sample to all the data set is incorrect. Different samples at different depths will have different particle distributions, different assemblages and different possible quantification errors.

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With the information presented here there are no robust data to conclude that EGC are the main carbonate producers in the Pacific.

In chapter 3.4 is stated '..implying that maxima of the coccolithophores parameters are found most often at the chlorophyll maximum'. I don't know what coccolithophore parameters are. If this means that the Chl maxima corresponds to high coccosphere and individual coccolith density and heavier coccospheres and coccoliths, I can't really see it. Fig 9 is quite unclear: The top plot doesn't show any scale. The bottom is misleading since it looks like that the Chl maximum is represented by a very wide layer (about 100m). The scale of Chlorophyl a concentration is not shown as well as the data used in the contour map. I also don't know exactly how the weight of coccospheres and coccoliths and coccoliths and coccospheres/ml are presented.

4. Discussion

The short discussion on abundance distribution and the presence of coccolithophores at 300m water depth in the South Pacific Gyre stations is quite unclear. Figure 4 shows the results for the studied transect; at 300 meter water depth there are only 2 samples. Also please check the paper by Raimbaul et al. in Biogeoscience Discussion for the plot of Chl a concentration. The issue related to the quantification of PCU samples (large amount of material on a small membrane) is a very significant limit of the data set that needs to be taken into consideration before any major conclusion can be drawn (see methods).

It is stated that 'One of the important finding of the present study is a strong relation between the numbers of coccoliths of E.huxleyi and the number of suspended calcite particles (and therefore, the PIC)'. I don't think that this paper presents sufficiently robust data to say this. First of all no quantification of this species is presented here (only the EGC group). In addition and even more important is the correction of the EGC quantification applied to all the samples. The short discussion on the ballasting due to E.huxleyi can be removed.

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I am sure that in general there is a relationship between coccolith and coccosphere sizes. However, the proposed use of the 1.9 factor from the coccolith length to estimate the coccosphere diameter for EGC species in paleoceanographic studies is not really supported by enough statistical analyses.

The discussion on the link between shape (size and weight) of coccoliths and coccospheres and the carbonate chemistry and productivity of the water is quite speculative. I can't really follow the discussion.

First is stated that: 'In BIOSOPE, the highest number of coccospheres was found between 80 and 100W and it is also in the same samples where the least calcified Isochrysidales were found. The number of coccospheres in the PCU may have been underestimated, but not in the Marquise area. There is no relation between the number of coccospheres and their weight of CaCO3'. What does it mean?

Also it is written that 'This hypothesis (the carbonate ion concentration) could explain why the heaviest coccospheres are observed in the eutrophic and mesotrophic areas of the BIOSOPE experiment. At the reverse, the least calcified Isochrysidales are found at the subtropical front in the highest coccosphere abundance zone of the BIOSOPE experiment'. I am confused about the use of eutrophic areas. Eutrophic areas are characterized by high nutrients and high productivity. It is unfortunate that there are no carbonate chemistry data to support any of these hypotheses.

The chapter 4.5 on deep production shows interesting results on Florisphaera profunda but they do not really fit to the aims of the article. Also the discussion on the pigments without any introduction is out of place. The comparison in figure 10 of the stations GYR2 and STB11 is not very convincing since in GYR2 there are no samples between 5 and 90 m.

The chapter 4.6 on the deep production of alkenones and implications for paleotemperature reconstructions open different issues. It is absolutely true of the importance of knowing alkenone production depth for paleoreconstruction but my worry with

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the data set presented here is that the sampling could be one of the main causes of this distribution. In the article it is stated that 'From the 115 samples analysed in BIOSOPE, 62% of the coccoliths were found 15 at depth below 30 m, and therefore undetectable by satellite'. The authors didn't discuss the vertical sample distribution. It looks like from figure 4 that only 20-25 samples were collected above 30 m and only at 5m water depth. Also no samples between 5 and 80-90m were collected between 120W and 110W.

5. Conclusions and final remarks

The conclusions need to be reconsidered and rewritten. There is no basis for the extrapolation of the standing stock of Isochrysidales for a 300m water column production. I am not convinced about the quantification of their carbonate estimations (see methods). The conclusions on the ballasting and Ca++ depletion are confusing. The paper doesn't have any data on the carbonate system or on nutrient distribution (why the data presented in Raimbaul et al. in Biogeoscience Discussion are not used?) so it seems that there are a lot of speculations not supported by the data. The coccolithophore results should be reconsidered as well as the limit of the sampling (both methodological and sample distribution).

6. Specific comments

Repetition at Line 13 p.3269 and Line 8 p.3270 'The South Pacific Gyre (SPG) is the most oligotrophic region..'

Today's Ocean at Line12 p.3269. Why in capital letters?

Line 20 p.3269 'opposite natural trophic environments' is confusing.

Lines 20-21 p.3270 I have never seen the use of DCM as Depth of Chlorophyll Maximum but for Deep Chlorophyll Maximum.

Line 22 p.3270 'At most stations, water samples were taken at 6 water depths: at the surface (actually 5 m), between the surface and the DCM, at the DCM and two samples

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below the DCM'. This means 5 samples.

For consistency either use coccolithophores or coccolithophorid. The use of the first one is preferred.

De Vargas et al., in press and Goyet et al., 2007 are not in the references.

The particle density is shown usually as number / ml but in the discussion it is number / liter. Also check line 8 p.3279 cells m-2?

Line 21 3280. a parenthesis is open but never closed. The sentence doesn’t make sense.

Fig.4 needs to be improved both for the interpolation of the data and for the labeling (especially e and j) Chl a in mg /liter. I guess the dark profile are no data (?) it is confusing. Also in the conclusion there is a lot of said about coccolithophore production until 300 m water depth but only two samples at those depths are presented. I compared the data with the one presented by Raimbaul et al. in Biogeoscience Discussion and it would be good to find agreement within the same Special Issue about how to plot the data.

Comments on Fig. 9 are above.

The English needs to be corrected. There are several sentences that are not understandable. There are several spelling and typo mistakes.

Interactive comment on Biogeosciences Discuss., 4, 3267, 2007.