

Interactive  
Comment

## ***Interactive comment on* “Calcite production by Coccolithophores in the South East Pacific Ocean: from desert to jungle” by L. Beaufort et al.**

**L. Beaufort et al.**

Received and published: 27 March 2008

Dear Editor

We acknowledge the work of the reviewers. We follow all the reviewers' comments when this was possible. Referee#1 was generally positive. The relation between the chemistry and the calcification of coccolith has been changed and extended by the use of in situ measurements of the carbonate chemistry. In order to do that, we included Catherine Goyet who performed the chemistry in BIOSOPE, as a coauthor of the paper. Reviewer #3 raised several important criticisms in particular related to the methodology used in that manuscript. Consequently he felt that the conclusions are not supported by the data (that basically he did not trust). In this new version we spend a lot of efforts to convince the reader that the methodology is adequate. For that we compared the results of the automated techniques with “classical” human counts. We used some of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



these counts when the automatic method apparently failed. We also gave statistics and we revised some sections that could have been confusing. Also we provide counts on the specific composition of the Isochrysidales and coccolithophores, to show the reader we know what has been measured. The data presented are hopefully “robust”, and the interpretations trustable.

Following are the specific answer to referees’ comments:

Answer to referee #1:

*“The data is quite interesting, although it would make for an even more interesting paper if coccolithophores species count were also included for comparison”.*

This has been done. The percentage of different species of Isochrysidales in the entire coccolithophores assemblages has been estimated with classical methods. These data are included in that manuscript now.

*“The paper needs copy-editing to improve grammar.”*

The manuscript has been corrected and edited by Pr. Aubry from Rutgers University.

*“By analyzing of a station where *E. huxleyi* was overwhelming dominant they realized that the coccolith Analyser VI package catches only 40”% of the coccolith.*

We understood that this was a problem of density of particles laid onto the membranes in some station. We used human counts when estimating the abundance of coccoliths and coccospheres at those stations instead of automated counts.

*“The estimations of coccolith diameters are also subject to error.”*

This is not exactly an error, but a small constant bias of 20% that is understood and calibrated. It is due to the size and the light intensity of the small coccoliths. We do not use the size of the coccolith very much in that paper since we recognize it is not a diagnostic parameter. The estimation of the coccolith weight is not dependent on the estimation of their size; therefore the discussion on the weight that is at the heart of the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



paper, is not altered by this constant bias on the size measurements. We are convinced however that those data are robust and should be presented here. We withdrew the correction part.

*“The manuscript speculates that the degree of calcification (...) may correlate positively with carbonate ion concentration and calcite saturation state.” The referee further proposed that we should use GLODAP to prove that.*

Instead of using GLODAP, we use the in-situ measurements of carbon chemistry done by C. Goyet. We included her as co-author of the manuscript. Including those data, we do not “speculate” anymore and we are able to show several correlation plots that prove that there is the degree of calcification is related to carbon chemistry of the ocean. We thank the referee for having insisted in that direction because the result that 94% of the weight variability of the Isochrysidales is related to few environmental parameter is a very strong result.

*“The manuscript appears to have been produced in haste.”*

(Unfortunately not really!)

The spelling errors were corrected and the manuscript edited.

*“It would have been interesting to have compared calculated PIC from coccolith weight against PIC measured using a chemical technique.”*

We agree and plan to do that soon. This cannot be included into that work.

Most of the other small changes asked have been done.

Referee 3

*“The objectives are not clearly stated but I guess were two”*

We change the objectives section to make them clearer. They are more than two.

*“The title is catchy but misleading”*

**BGD**

4, S2807–S2817, 2008

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



We changed by withdrawing “from jungle to desert”.

The Chile upwelling problem : *“It is unfortunate that the only 4 stations sampled in the high productivity waters of the PCU don’t really allow quantitative estimation.”*

We agree that the quantification of the stock is tricky here. Accordingly with the referee, we withdrawn the quantification of the stock of cells, liths and calcite in that part of the transect.

*“I like the idea of automatic calcite particle quantification but (...) it is important to quantify errors and possible error sources.”*

A paragraph on error source has been added and we give now a quantification of the error.

*“The automated analysis (ANN) of coccolith and coccospheres (SYRACO) is limited to Emiliana 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”.*

We precised the way we trained the ANN. We trust that this ANN is recognizing only Isochrysidales, because other order have distinct coccoliths, because we checked every coccosphere recognized, and because we now compared ANN results with coccosphere counted by human on light microscopy. We corrected the manuscript consequently.

*“In addition, the results from the reliability test briefly mentioned in lines 14-22 should be presented and discussed.”*

Done.

*“Since the results of automatic counting of individual coccoliths is not satisfactory why have they been shown with no estimation errors or used at all?”*

**BGD**

4, S2807–S2817, 2008

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



We try to show that the automatic counting of individual coccolith is satisfactory by comparing with human counts. The problem we had was that the coccolith and coccosphere density at 4 stations was extremely high, and a problem of aggregation occurred. When we had submitted the manuscript we did not have done human counts for all the samples yet, and we did not know exactly that this problem was limited to those stations. Now we have used human count for those stations and ANN counts that we trust more for the rest of the station. As we mention now in the text, the difference between the two sets of data (human and ANN) is not significant and does not change at all the results and the discussion.

“Why 1.25 ?”

We explained that in the text. We withdrew that correction. This is minor in the data (constant factor of 1.25 is only 20% and does not affect the variance. Also we do not use the length in the discussion.

*“In chapter 2.6 the ‘complex’ identified by SYRACO is described as EGC Emilia-nia, Gephyrocapsa and Crenalithus. Crenalithus has never been mentioned before as one of the automatically identifiable genera.”*

Done now. We also changed the name EGC to Isochrysidales.

*“What is the importance of Crenalithus in these samples?”*

See new figure 4 done to answer to that question.

*“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).”*

Hope we do now.

*“ 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 quan-*

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

*tifying the total assemblages (and these are new results) this should be explained in detailed in the Methods.”*

We used both SEM and light microscopy. Most of the count have been done with the latter. This is mention in the text now.

*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.*

Already answered above. We have now given a more precise methodology with statistic tests. Also we published at least 3 methodological papers only devoted to that method and use this method in several papers (e.g. in Science in 2001). There is no basic difference between living coccoliths and resent fossil ones. It is a bit frustrating to have to justify at lengths a methodology we use routinely and published before. If we do that, the referees will certainly ask to publish two papers, one on the methodology, one on the science. We can already tell that the former will be boring and plagiary. The only new methods used here are on automatic morphometry and PIC estimates. But apparently the referee is skeptical not on those but on the ANN.

*It is also unclear when the automated microscopy and human counting results are used.*

On the previous manuscript only automated microscopy was used. Now we used human density counts of coccoliths and coccospheres in 4 stations for the reasons explained above. We hope we are clearer on that problem now.

*Also the authors should state clearly the species that were detected both for coccospheres and coccoliths. If not all thespecies are counted, how are the total coccoliths /ml plotted (fig.6)?”*

In the previous fig 6 all the coccoliths of the several species compsing the order of Isochrysidales were used as it was mention. We do the same now.

**BGD**

4, S2807–S2817, 2008

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



*"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."*

We do not think it is because it is out of focus (in the previous version we mention that possibility among other) but how we said earlier we now understood the problem was density.

*"What 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."*

We agree and we do not use that factor any more.

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

The data presented in the new version are hopefully more robust. For example we added Figure 7 that clearly shows that the best fit between coccolith and calcite particle is for the Isochrysidales and essentially *E. huxleyi*.

*"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 Chlorophyll 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."*

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We change the wording in the text and the figure. The upper panel of the previous figure has been withdrawn, we show the chlorophyll scale. We tried to explain better that the dots show represent the depth where the maxima are found. However if the “*Chl maximum is represented by a very wide layer*” it is only due to the data and we do not know how to change that.

*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 Raimbault 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).*

We do not want to say that the coccolith are produced at 300m but that if we consider the extent of depth where coccolithophores are produced, it is necessary to integrate the stock of coccosphere to the entire water column to evaluate the stock. This integrated stock is not that low. We added some words to explain better our point of view.

*It is stated that ‘One of the important findings 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.*

Our data show that in the area where most of the coccoliths are produced (between 100 and 90° W) is dominated (up to 100%) by *E. huxleyi*. Also and as discussed above,

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)



the best fit between particles and coccolith is for *E. huxleyi* (because of the previous relation). We are confident in our data. I hope this new version of the manuscript with new data and new figures is more convincing. We want to keep this important piece of discussion, because, in the small Nannoplankton specialist community, a paradigm is currently been proposed that *E. huxleyi* is a minor calcite producer because of its small size. The data presented here show the opposite: the cloud of high only scatter of dense particle detected by optic measurement in BIOSOPE transect corresponds to where *E. huxleyi* is, and where the other coccolithophores species are not that abundant. The backscattering is also relatively high in that area. The ballasting hypothesis is also important in that context since it could explain why the alkalinity is rather low in that same exact zone.

We transformed the discussion to make those points clearer.

*"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."*

We added a statement saying that more data are needed.

*"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."*

We change most of that discussion because we added the in situ chemistry. Therefore we hope it is less speculative.

*"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*

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

between 5 and 90 m.”

We withdrew the discussion on *F. profunda* and on stations GYR2 and STB11.

*“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 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.”*

The sampling has been made with the following procedure, 2 above the DCM, two in the DCM and two below. The DCM is the most important ecotone in the global ocean and we had to sample it in a regular manner. We had only 6 samples per station. In the case of an even distribution in the transect (e.g. 0, 50, 100, 150, 200, 250 m) we would have underestimated the vertical variability in the upwelling. In that way the distribution is evenly distributed. The fact in the gyre they are less sample at the surface is normal since the DCM is very deep. But change of the community is very small in the shallow photic zone in this gyre. The first major change in the community appears, as expected, close to the DCM. Therefore in the vertical space it is true that the shallow depth are underrepresented. But that does not mean that the structure of the community is underrepresented. Therefore we do not agree at all that the sampling could be the cause of the distribution. In any case we change the wording for the satellite bloom detection problem. Instead of using the entire data set we use only the stations in the place where high concentration of coccolith is found. It is only place where satellite could had detected something.

**BGD**

4, S2807–S2817, 2008

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



*“Conclusions and final remarks The conclusions need to be reconsidered and rewritten.*

We rewrite most of the conclusions.

*“ There is no basis for the extrapolation of the standing stock of Isochrysidales for a 300m water column production.”*

Why not? In any case we withdrawn that part of the conclusion.

*“ I am not convinced about the quantification of their carbonate estimations (see methods).”*

In the method, the referee appears to not trust the PIC data collected in the upwelling area. Those samples have been withdrawn from the PIC analysis.

*“The conclusions on the ballasting and Ca++ depletion are confusing.”*

Why ?

*“The paper doesn’t have any data on the carbonate system or on nutrient distribution (why the data presented in Raimbault et al. in Biogeoscience Discussion are not used?) so it seems that there are a lot of speculations not supported by the data.”*

We are presenting now in-situ carbonate measurements.

*“The coccolithophore results should be reconsidered as well as the limit of the sampling (both methodological and sample distribution).”*

We hope we successfully answer those concerns.

The other minor comments/problems found by the referee have been included/corrected.

---

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

