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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 #1

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"Calcite production by coccolithophores in the South East Pacific Ocean: from desert to jungle" by L. Beaufort et al.

Beaufort and colleagues have previously developed a technique (SYRACO) for identifying coccoliths in sediment samples and identifying them to species/genus level. The technique makes use of the fact that calcite (the mineral form of calcium carbonate used by coccolithophores) polarises light. By interposing a polarising filter in front of the microscope, only polarised light can be seen and calcite particles (appearing bright) can be distinguished from background and particles of other materials (appearing dark). The development of SYRACO was followed by development of a new technique to indirectly assess the CaCO3 content (weight) of coccoliths. This technique rests on the observation that the intensity of polarised light summed across a coc-

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colith is closely related to the mass of CaCO3 in the coccolith. Additional software packages are also presented. In this paper Beaufort et al have taken these new techniques to sea and measured coccolith weights; and other properties on a transect from the mesotrophic Marquesas Islands across the ultra-oligotrophic SE Pacific to the eutrophic Peru-Chile Upwelling System.

Their main conclusions are:

1. coccoliths and coccospheres of the EGC complex (Isochrysidales, mostly E. huxleyi) correlate positively with eutrophic status, i.e. are smaller on average in the oligtrophic waters, larger and heavier in more fertile waters.

2. this trend may correlate positively with carbonate ion and saturation state.

3. the opposite tends to be true of the coccolithophore community as a whole, with smaller liths and cells predominating near to Chile where coccolithophores are most abundant. This is probably down to blooms of E. huxleyi in the more productive waters, i.e. a shift in the make-up of the community rather than a shift in size of each coccolithophore species.

4. small placoliths contribute on average 50% of particles and 20% of weight (PIC).

Overall evaluation:

This technique is potentially very useful because of the large amounts of time required to count coccoliths by eye under an SEM. Automated techniques such as described in this paper represent a promising approach towards more rapid and more objective data generation. The results presented here are of interest in evaluating trends in coccolithophores between very productive and very unproductive regions (ocean deserts and jungles) although more caveats need to be included given the limitations of the techniques (which the authors fully acknowledge). The data is quite interesting, although it would make for an even more interesting paper if coccolithophore species counts were also included for comparison - the editor may want to consider recommending combi-

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nation of this paper with Couapel & Beafourt in prep (footnote 2). The paper needs copy-editing to improve grammar.

Main comments:

1. The authors are commendably open and honest about the limitations of their approach, such as incorrect categorisations of tilted or overlapping or broken coccoliths, or coccoliths in parts of the field that are out of focus. By analysis of a station where E. huxleyi was overwhelmingly dominant they realised that the Coccolith Analyser VI package catches only about 40

2. The estimations of coccolith diameters are also subject to error: "it appears that for small placoliths like E. huxleyi the entire distal shield is not detected in cross-polarised light." (pg 3274), although there is an attempt to correct for this by applying a blanket 1.25 correction factor to all size estimations of small placoliths. There needs to be more discussion of how robust the measured spatial trend (Fig 7) in small placolith size (mostly E. huxleyi coccoliths) is likely to be, if only the inner parts of the coccoliths are visible. Again perhaps the relevant conclusion (number 1 above) should be downplayed or removed.

3. The manuscript speculates that degree of calcification (heaviness and size of small placoliths) may correlate positively with carbonate ion concentration and calcite saturation state. Given that two of the lines used in GLODAP (Key et al., 2004, http://cdiac.ornl.gov/oceans/GLODAP/images/survey_map1.jpg) are E-W lines in the SE Pacific (one along about 15S, another along about 30S) it is in principle be possible to test this against data, rather than speculate. The discussion of this hypothesis (section 4.5) and the conclusion (number 2, stated in abstract) need to be revised in any case because the chemistry in fact operates in the opposite direction to that stated: carbonate ion concentration declines with depth rather than the opposite, and for that reason carbonate ion concentration tends to be lower in recently upwelled waters. Therefore it seems more likely that degree of calcification will prove to be negatively rather

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than positively correlated with carbonate ion and saturation state (variations in calcium ion are not significant).

4. The manuscript appears to have been produced in haste. There are not many spelling errors but a large number of grammatical errors, understandably if English is not the author's first language. Nevertheless, grammar could be improved by use of a grammar checker such as in Microsoft Word; for instance "in average" should be changed to "on average" in several places, "consequences on" to "consequences for", "the same ... than" to "the same ... as", etc.

5. It would have been interesting to have compared calculated PIC from coccolith weights against PIC measured using a chemical technique.

6. Since some of the software packages are presented for the first time, it would have seemed to make more sense to present the results alongside comparable results derived from manual counting (SEM). Previous validation/calibration exercises by Beaufort's group have focussed on measuring particles in sediment traps and on measuring calcite powder, rather than on water-column samples.

Specific Comments:

1. It is somewhat confusing that there are so many different software packages (SYRACO, Coccolith Analyser VI, Calcite Analyser VI, Particle Analyser VI; is there also a Coccosphere Analyser VI?) all of which are first mentioned at different points in the MS. It would be helpful to list them all in a table near the beginning of the MS, together with a description of what each one tries to calculate.

2. Change Chili to Chile throughout.

3. It is incorrect to talk of a "high depletion of Ca++ ion in the euphotic zone" (e.g. pg 3280). Calcium ions are always present in superabundance (10.6 mMol) and even the most intense biological removal makes only a small dent in that very large concentration.

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4. The Conclusions section is particularly error-strewn: The first sentence tells us that coccolithophores in the S Pac Gyre are abundant and calcify. This latter point will hardly be surprising to most readers since they calcify everywhere as far as we know. Was the intent to say that they calcify *strongly* in the Gyre? "*over* a 300 m water column." "coccolith*ophore* community". Remove "e" from Carbone. Unmatched parenthesis following E. huxleyi. "(equal *to* the rate of photosynthesis), which induces " "90W - 30*W*" "This could result from", "cocco*lithophore* calcification." Change "At the reverse" to "Conversely". "Deep Chlorophyll *M*aximum"

5. Change "average" to "total" (pg 3272, line 20)

6. In section 4.1 there should be a comparison against abundances from direct counts!

7. The large numbers of E. huxleyi (and of coccolithophores in general) at depths $>$100m is of considerable interest given the proposed high-light niche for E. huxleyi (e.g. Nanninga & Tyrrell, 1996) and the observed restriction of calcification to shallower waters than silicification (Poulton et al, 2006). More should be made of this interesting observation, and it should be stated whether or not the observation is supported by direct counts in deep waters. Is it only detached coccoliths or also (living?) coccospheres?

8. Fig 3: it should be stated whether or not A and B are of the same field or of different filter papers. It would be most interesting if the same field is shown once under polarised light, once under SEM.

9. Fig. 4: (i) and (j) need to be referred to in the caption (shouldn't panel labels come before their descriptions?). Grey shading usually indicates the seafloor, white shading would be better for 'no data'). Attenuation coefficient is denoted by "c" rather than "cp"

10. Fig 6: draw on the one-to-one line. The text should discuss why there is no convergence to the one-to-one line at high concentrations of coccoliths (when it would be expected that nearly all particles are coccoliths).

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11. Fig 10: swap left and right in caption.

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