

Interactive comment on “A global compilation of coccolithophore calcification rates” by Chris J. Daniels et al.

Chris J. Daniels et al.

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RC1 (L.T. Bach) In this study, Daniels et al. compile all available coccolithophore calcification rates from the field in a common data base. They use published and unpublished data thereby enlarging our current data coverage substantially. The dataset is quality-controlled and invaluable for coccolithophore research, especially in light of the increasing number of studies using space-borne and in situ sensor data to estimate calcification rate. The accompanying text is well written. I only have minor comments/suggestions.

1.1 Authors: We thank Dr Bach for his positive comments and clear statement on the value of the dataset. We respond to his comments below, noting that many relate to

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our wish to keep the ESSD manuscript short and simple.

Line 38ff: In this context it may also be useful to mention that satellites are restricted to the upper euphotic zone and miss/underestimate cocco blooms that are stretched out over the water column.

1.2 Authors: We have revised the abstract accordingly: “Satellite-based estimates of areal CP are limited to surface waters and open-ocean areas, with current algorithms utilising the unique optical properties of the cosmopolitan bloom-forming species *Emiliana huxleyi*, whereas little understanding of deep-water dynamics or the optical properties of..”.

Line 54f: Why would CaCO₃ export be a positive feedback on atmospheric CO₂. Wouldn't the ballasting effect outweigh the Alkalinity drawdown?

1.3 Authors: While we agree with the reviewer that ballasting could potentially alter the magnitude, or even the sign, of the overall biogeochemical impact of coccolithophores on atmospheric CO₂. We also note that global ballasting of POC flux by CaCO₃ is hypothesised rather than a fully resolved mechanism (e.g. sticky organic matter may drag down minerals rather than vice versa), and there are also timescale issues to consider (i.e. seasonal versus geological), whereas the chemical effect of calcification is straightforward chemistry. In addition, over the longer term POC export is likely to happen eventually either way, whether coccolithophores (and CaCO₃) are present or not, given that phytoplankton blooms tend to continue until nutrients are exhausted. Hence, on a global scale the total amount of POM exported over a bloom season is more a function of nutrient supply than mineral availability.

We accept however that there are uncertainties about the overall biogeochemical impact on atmospheric CO₂, but have left the sentence unaltered since our sentence refers only to the impact of calcification plus export and not to the overall impact.

Line 155: Hasn't temperature also always been measured? Temperature would be

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helpful.

1.4 Authors: Temperature (and nutrient concentrations) would indeed be very helpful, however it was not always reported (or made available) for many of the datasets collated from the literature or unpublished sources. Furthermore, ensuring that we can provide quality controlled ancillary data is also challenging, whereas internally we could ensure that the rate data was adequately quality controlled.

Line 155: In Line 163 you write 6 – 24 hours. In line 216 you refer to Lam et al. who incubated 5 hours. I suggest being consistent on this in the text.

1.5 Authors: Corrected to 5 to 24 h now in section 2.1.2.

Line 167: The approximated abundances of foraminifera and pteropods would probably require a reference.

1.6 Authors: We have now revised and added references to this line as follows:

‘..and rare calcifying organisms, such as foraminifera (typically ≤ 0.5 L⁻¹; e.g. Schiebel & Movellan, 2012) or pteropods (typically ≤ 0.005 L⁻¹; Burrige et al., 2017)

References added:

Burrige, A.K., Goetze, E., Wall-Palmer, D., Le Double, S.L., Huisman, J., Peignenburg, K.T.C.A.: Diversity and abundance of pteropods and heteropods along a latitudinal gradients across the Atlantic Ocean, Prog. Oceanography, 158, 213-223, 2017.

Schiebel, R., and Movellan, A.: Global distributions of modern planktic foraminifera abundance and biomass – Gridded data product (NetCDF) – Contribution to MAREDAT World Ocean Atlas of Plankton Functional Types, PANGAEA, <https://doi.org/10.1594/PANGAEA.777386>, 2012.

Line 246ff: Readers who are not familiar with the “optical depth” assessment/rationale (like me) may appreciate a more thorough explanation of this concept. It is a bit of a pain to go through the Kirk reference only for this.

1.7 Authors: We have now added the following explanation (@ Ln 246): 'Optical depth represents the path length of light through a medium and is the natural logarithm of the ratio of surface irradiance to irradiance at a specific depth, being proportional to the amount of light attenuation in the water column. Consideration of optical depth rather than absolute depth accounts for geographical patterns in the light field, recognising light as an important driver of CP. For example, 1% of surface irradiance (optical depth of 4.6 as natural log of 0.01) may reach 30 m in temperate waters with high attenuation, whereas it may reach 90 m in subtropical waters with low attenuation: if incidental irradiance was the same at both sites then both depths would receive the same light intensity independent of the difference in depth.'

Line 275: I totally agree with removing zero values. However, also some very low measurements which are slightly above zero may still be below the detection limit and thus unreliable. I wonder if it wouldn't be helpful to give an approximate detection limit of the method just to show how trustworthy such very low values would be.

1.8 Authors: The MDT method includes a formalin-killed blank whereby each triplicate light incubated rate measurement has a blank value subtracted, with the blank accounting for any abiotic isotope uptake. Hence, zero values represent CP rates where the live values are equal (or less) than the abiotic values and any value above zero has a positive measurable calcification rate. As the methodology of the measurements has no standard in terms of radioactive spike, volume incubated or incubation length, the relationship between live values and formalin-blank values can be very variable between studies. Hence, it is not possible to approximate a standard detection limit as it will vary between studies depending on aspects of their methodology; it is possible to lower the detection limit (and increase sensitivity) to some extent through increasing the magnitude of the radioactive spike, though we also note that in some cases this can also lead to elevated blank values.

Line 349ff: This is an interesting aspect. Can you assure that the incubations were made at light levels too low for photosynthesis (and not only the sampling depths)?

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Otherwise the hypothesis would not hold.

1.9 Authors: The incubation irradiances are not contained in the database. However, Poulton et al. (2017) estimate that irradiance levels below the subtropical DCM (at 1% of surface irradiance) are less than 20 $\mu\text{mol photons m}^{-2} \text{s}^{-1}$ (equivalent to $<1.2 \text{ mol quanta m}^{-2} \text{d}^{-1}$, assuming a 16 h day), an irradiance level often seen to light-limit cultures and North Atlantic phytoplankton (e.g. Siegel et al., 2002). We have now slightly reworded this sentence: 'CP at depths below the light levels for photosynthetic growth may...'

Siegel, D.A., Doney, S.C., Yoder, J.A.: The North Atlantic spring phytoplankton bloom and Sverdrup's critical depth hypothesis. *Science*, 296, 730-733, 2002.

Line 359: "global CP" or "global CP by coccolithophores"? I guess it is the latter but this should be clarified here.

1.10 Authors: Indeed, now corrected to 'global CP by coccolithophores'.

Line 375: I find this not particularly convincing because per cell calcification rates may vary massively in between species. So I wonder if it is useful normalize to cell abundance without accounting for the different sizes of the various species. There seem to be too many degrees of freedom to come to any particularly useful conclusions. You basically say this yourself in the subsequent paragraphs.

1.11 Authors: Indeed, the influence of different cell calcite content on cell-normalised calcification can be significant, and this is in effect what we are suggesting – normalising calcification rates to cell numbers give you an idea of the community-wide calcification rate per cell numbers; in a *E. huxleyi* community, cell-normalised calcification rate can infer physiological changes and controlling factors, in a diverse community it infers the compositional influence on calcification rates. A large fraction of the CP measurements have been collected from waters that can be considered *E. huxleyi* dominated, and so in these cases there is the possibility to examine cell-CP in light of variability in

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environmental conditions (see e.g. Poulton et al., 2010, 2013; Charalampopoulou et al., 2016).

Ideally it would be good to normalise measurements of CP to community calcite (from cells, not detrital) and examine differences in growth rates (see Poulton et al., 2010); however, this has several difficulties for mixed communities in terms of weighted means and potential variability in growth rates and relative abundances (see Daniels et al., 2016). In light of these (potentially current) difficulties we have normalised to cell abundance as a first order measure of (a) whether the CP rates are physiologically realistic, and (b) a (admittedly very rough) index of the relative CP rates between different communities.

Our central tenant in terms of normalising to cell numbers is to provide a first-order check on whether the CP rates are physiologically possible, and encourage future studies to look in more depth into the coccolithophore community responsible for these rates.

Line 796: 2% (10 to 28)? Do you mean 20%?

1.12 Authors: We are not sure where this error crept in but this sentence should read: PPMDT measurements was 14% (2 to 66%) and 19% (1 to 72%) for CPMDT, across a range of PPMDT from 1.3 to 5.0 mmol C m⁻³ d⁻¹.

Appendix: Maybe this is just a problem for me but I found the acronym “diff” (for difference) a bit unlucky because I confused it with diffusion all the time.

1.13 Authors: We have now changed to FF for the difference (diFF).

Figure 4: I guess the x-axis is on a logarithmic scale? If so, it would be good to label it as such.

1.14 Authors: Figure 4 and Figure 5B have now been changed to reflect this.

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