

Interactive comment on “The haplo-diplontic life cycle expands niche space of coccolithophores” by Joost de Vries et al.

Anonymous Referee #3

Received and published: 18 August 2020

General comments This is an interesting paper, timely, and relevant to the field of physiological ecology of phytoplankton. It deserves to be published but needs some minor revision. The paper was a bit sloppy in spots, with a number of typos. The paper should be checked over carefully prior to final submission.

There are some terms that really need to be clarified in the revision to avoid confusion and to sharpen their points. First, when discussing nitrogenous nutrients they refer to “fixed nitrogen” as the sum of nitrate and nitrite (line 90-91). This reviewer has no idea why they are using the adjective “fixed” for the sum of these molecules (and they do not include ammonium or urea in that sum, for example). Typically, the fixation of nitrogen by phytoplankton is describing the uptake and assimilation of N₂ gas into organic nitrogen fractions, which is not what they are describing. I would advocate that

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they globally scrub the term “fixed nitrogen” and replace it with something like dissolved inorganic nitrogen (DIN, here defined as nitrate + nitrite only). Second, in their equation about niche expansion (line 150) they refer to terms describing the “intersection of hypervolumes” and the “union of hypervolumes”. If there is a union of hypervolumes, then they also intersect, right? The authors must very carefully define the difference between these. As long as there are ambiguities in the definition of those terms, then the entire niche expansion argument won’t have much relevance.

Finally, they talk about a 7% contribution of holococcolithophore abundance to the total coccolithophore abundance as being significant (abstract and line 331). It may be statistically significant, but it seems to this reviewer to be a little overblown. I would suggest that holococcolithophores more appropriately would be considered a minor constituent of the total coccolithophore assemblage. For holo/heterococcolith paired species, the holococcolithophore abundance represents ~18% of the paired species abundance, only about a fifth, definitely still a minor fraction, at best. This doesn’t detract from the results. It is still a fascinating observation and the question that arises to this reviewer is why is that fraction so small? This paper requires some revision but it provides new insights to a very real problem in coccolithophore ecology. It deserves to be published and will be cited well. The authors simply need to clean it up a bit.

Specific comments Line 4 after “diploid life cycle phase” are they referring to coccolithophores only or other organisms. Please clarify.

Line 13 “ballast” not “ballasts”

Line 13-15 They are describing the biological carbon pump, not the carbonate pump (aka alkalinity pump). The linkage of calcite production to the biological carbon pump is a strong one via ballasting of organic carbon to the sea floor. This is not the alkalinity pump however. Klass and Archer (2002) were looking at the impact of ballasting of sinking POC and the effect on the rain ratio.

Line 16 Globally, about one quarter of all marine sediments are calcium carbonate.

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Citing the 30-90% value presents a skewed view of the importance of calcite sediments on Earth.

Line 18- Given that this sentence is going back to the biological carbon pump, you might move it up in the paragraph where you are first mentioning the biological carbon pump.

Line 30 add an "s" . . ."A few organism"

Line 91 Reference to "fixed nitrogen" and all subsequent uses of that term in the paper. . . see general comments above.

Lines 151 and 152- Must describe how the "union" and "intersection" of hypervolumes are being distinguished. See general comments above.

Line 162- Again, they are describing the function in R to calculate the "intersection" of hypervolumes when the reader may not be clear about the difference between this calculation and that of the union of the hypervolumes! This is a really important distinction.

Line 185 As they state, on a regional basis, holococcolithophores generally contributed <6% of total coccolithophore abundance. This seems pretty minor to be honest!

Line 190- change to . . ."where a HOLP-index" . . . not "an Holp-index"

Line 193- add comma, "In the global data set, heterococcolithophore. . ."

Line 218- change to . . ."high nutrient concentrations, cold water temperatures at depth or other factors not addressed in this study".

Line 253 They show significant positive correlations with silicate. This is a very interesting observation. The Discussion section should have a few sentences explaining how this could be!

Line 273- add "s" to heterococcolithophore to make it plural.

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Line 285 add comma, "overlap metrics, respectively"

Line 331 "Our meta-analysis shows that holococcolithophores are important contributors to coccolithophore abundance and ecology contributing ~7.3% to total coccolithophore abundance" This observation doesn't match the data. 7.3% is a small number. Call it like it is!

Line 336- re-word this so that it agrees with the minor contribution. . . "past and future oceans could have other biogeochemical effects. A shift towards" . . .

Line 363- Remove "?"

Line 366- improper hyphenation of wrap-around word "coccolithophore"

Line 370- I disagree with this statement. Calcification measurements are including the calcite production of holo- and heterococcoliths. However, the standing stock of calcite is being underestimated by not including the holococcolithophore abundance. Also, leave off the last words of the sentence, "or activity".

Line 380- Sentence "Overall observations in the haploid stage of *E. huxleyi* are. . .". There is some classic literature that the authors should cite from the mid 1990's: Campbell, L., et al. (1994). "Immunochemical characterization for eukaryotic ultraplankton from the Atlantic and Pacific oceans." *Journal of Plankton Research* 16(1): 35-51. They used immunochemical antisera to identify haploid stages of *E. huxleyi*. Line 385; Again, there were a number of classic SEM studies from the Pacific Ocean. One by Reid (1980). Reid, F. (1980). "Coccolithophorids of the North Pacific Central Gyre with notes on their vertical and seasonal distribution." *Micropaleontology* 26: 151-176. The SEM plates in the paper are meticulous and it might be worth a look before you discount all Pacific SEM observations. See also previous work of Honjo and Okada from the Pacific.

Line 404- There is contrary evidence you should cite to be balance, though: Rivero-Calle, S., et al. (2015). "Multidecadal increase in North Atlantic coccolithophores and

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the potential role of rising CO₂." Science 350(6267): 1533-1537.

Line 407- eliminate the "s" from "compositions"

Line 414- Rework, "Our analysis shows that holococcolithophores constitute about one fifth of total paired coccolithophore abundance..."

Figures:

Figures 3 and 4- The font on all the axes is way to small to be readable. These must be increased in size.

Figure 4- Legend is reversed for red and blue colors. . .Heterococcus are plotted in red (not blue) and holococcus are plotted in blue (not red).

Figures 5 and 6- No units are provide in this figure or the legend for the color bars!

Fig. 6 change "fixed nitrogen" to DIN (see also Fig. 7)

Table 2 is excellent and a great reference. Should you state the names for the holo forms of *R. clavigera* and *R. xiphos* since you have left them blank?

Tables 6 and 7- You never discuss the significant relationships with Silicate (not "Si" as you say in the table!) This really deserves some discussion.

Tables 8 and 9 The legends are very minimalistic. Please move your definition of NE1 and NE2 to the legend from the footnotes. This needs to be more obvious to the reader. Also, maybe specify in the table legend what the Jaccard and Sorensen columns refer to (and units?) or refer the reader to the text.

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