Biogeosciences Discuss., 10, C6377–C6381, 2013
www.biogeosciences-discuss.net/10/C6377/2013/

Biogeosciences
Discussions
Open Access

Interactive comment on "The coccolithophores *Emiliania huxleyi* and *Coccolithus pelagicus*: extant populations from the Norwegian-Iceland Sea and Fram Strait" *by* C. V. Dylmer et al.

Anonymous Referee #1

Received and published: 9 December 2013

General Comments

The stated aim of Dylmer et al. is to improve our understanding of the distribution of two species of coccolithophore in the Northern North Atlantic, through examination of coccolithophorid distributions across two transects of the Arctic Ocean. The intention is good as this is a poorly studied region of interest. However, this paper does not further our knowledge beyond confirming previously observed patterns in the literature. Along with the lack of novel research there are a number of serious issues which require addressing.

1) Why was no *in situ* environmental data collected beyond only 5 CTDs? Satellite

measurements of sea surface temperature (SST) and sea ice extent alone are not sufficient to understand coccolithophore distributions; the importance of nutrients, irradiance, carbonate chemistry and grazing, for controlling coccolithophore distribution is mentioned in the introduction, but are not examined in the study. All of these can be collected on board for sampling, either from CTDs or underway supplies. The final statement that it is important to continue surveys of coccolithophores is good, however in order for surveys such as this one to be sufficiently useful, they require *in situ* environmental data for studying the potential multiple controlling factors on coccolithophore distribution.

2) The paper discusses maximum sea surface temperature (SST) boundaries for the different species, however SST was derived only from satellite. While it is mentioned that the satellite data can be validated against the few CTD profiles, why was this not discussed? Comparing species distribution to SST requires accurate measurements of SST; the error between satellite and *in situ* measurements needs to be determined.

Furthermore the authors need to justify their use of 9 km 32 day composite images, a product with significantly more data averaging than 4 km 3 or 8 day composites. How do they account for sub-mesoscale variations in SST within the satellite resolution? How much did SST vary over 32 days, particularly during the onset of Autumn (September/October) when a general trend in decreasing SST occurred? With all these potential errors, what level of confidence is there that the observed SST trends are real?

3) What is the justification for combining the haploid and diploid phases of *Coccolithus pelagicus*? They have very different ecologies (Parke 1960) and distributions and are considered separately in the referenced literature. By combining the two phases, it is not possible to compare to the literature, nor make any conclusions about the distribution of *Coccolithus pelagicus*. The two phases should be separated and the analysis of species distribution re-performed.

4) Statistics. There has been no attempt to statistically test which factors control the distribution of coccolithophores. A multivariate analysis is required to be able to state which factors influence the coccolithophore community.

5) The authors make a number of statements in the discussion that lack supporting evidence. How do they know that higher irradiance led to high "production" of *E. huxleyi* and that sea ice was responsible for the community shift? What about the role of nutrients, carbonate chemistry? Although it is possible to hypothesise the community-environment interactions, it is not possible to make definitive statements as has been done here. A correlation does not imply causation, particularly for claims not supported by either statistical analyses or, in some cases, data.

Their general lack of environmental data means that the authors are only able to construct hypotheses on SST and sea ice. It is not surprising that of these two, temperature is the dominant control. The authors need to acknowledge how constricted this view is and how limited their ability is to attempt to understand controlling factors when they only have data for a couple of potential factors.

6) How can *C. pelagicus* have a temperature boundary of $6\,^{\circ}\mathrm{C}$; Figure 4a appears to show *C. pelagicus* in waters warmer than $6\,^{\circ}\mathrm{C}$? Tarran et al (2001) report extremely high abundance ($10 \times 10^5\,\mathrm{L}^{-1}$) in waters warmer than $10\,^{\circ}\mathrm{C}$? Milliman et al (1980) also reported very high abundances in early summer over Rockall Bank, and Baumann et al. 2000 report *C. pelagicus* in temperatures up to $14\,^{\circ}\mathrm{C}$. Furthermore, it doesn't make sense to have a boundary of $6\,^{\circ}\mathrm{C}$ in one location, and $4\,^{\circ}\mathrm{C}$ in another. Instead it is probably due to combined effects of multiple environmental factors (temperature/nutrients/pH/Ω_{C}).

Specific Comments

At over 400 words, the abstract is excessive.

The paper lacks a cohesive structure. The majority of the results/discussion concen-

trates on a detailed analysis of the currents with little of this analysis then used for analysing and discussing the coccolithophore distribution.

pg 15080, ln 7-9: Using 16 references for one point is far too many.

pg 15081, ln 27-29: Coccolithophores are not just found in surface waters, with numerous species found at depth. The reference is discussing only the Northern North Atlantic, rather than the whole ocean as written here.

pg 15081, ln 29 (and mentions then on): This is not coccolithophore production, but abundance. Production is a rate measurement and cannot be determined from single time point measurements of abundance. The term "coccolithophore production" is also ambiguous, do the authors mean growth, photosynthesis or calcite production? None of these can be determined from their data.

pg 15083, ln 5-7 Other species of coccolithophore are found in very cold waters (*Wigwamma*, *Pappomonas*, *Papposphaera*). These will be missed using only light microscopy.

pg 15085, ln 2: What pressure was the water from the deckwash pump, did this affect the samples and was the water supply clean?

pg 15085, ln 5: How much of the sample was analysed with the light microscope? What is the detection limit?

pg 15086, ln 21-25: Sea Surface Temperature, as extracted from satellite, is a finished product provided on the OceanColor website, and does not need converting from raw values. The details provided are unnecessary.

References: There are multiple instances in the paper where the referenced article has been incorrectly cited or is a secondary citation. For example:

pg 15082, ln 24-27: Neither Baumann et al. 2000 or Tyrrell and Merico 2004 show that the *E. huxleyi* has a higher comparable growth rate.

pg 15083, ln 7: Baumann et al. 2000 do not show a temperature optimum of $8\,^{\circ}$C.

pg 15093, ln 17-19: Charalampopolou et al. 2011 do not debate ocean acidification (only showing that pH and mixed layer irradiance controlled species distribution).

Figure 1: "ESC" is in the figure but not explained in the caption.

References

Parke, M., and I. Adams. 1960. The motile (Crystallolithus hyalinus Gaarder & Markali) and non-motile phases in the life history of Coccolithus pelagicus (Wallich) Schiller. Journal of the Marine Biological Association of the United Kingdom 39: 263-274, doi:10.1017/S002531540001331X

Milliman, J. D. 1980. Coccolithophorid production and sedimentation, Rockall Bank. Deep Sea Research Part A. Oceanographic Research Papers 27: 959-963, doi:10.1016/0198-0149(80)90007-2

Tarran, G. A., M. V. Zubkov, M. A. Sleigh, P. H. Burkill, and M. Yallop. 2001. Microbial community structure and standing stocks in the NE Atlantic in June and July of 1996. Deep Sea Research Part II: Topical Studies in Oceanography 48: 963-985, doi:10.1016/S0967-0645(00)00104-1

Interactive comment on Biogeosciences Discuss., 10, 15077, 2013.