

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

Anonymous Referee #2

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The manuscript presented here by Dylmer, Giraudeau, Hanquiez and Husum is a description of the stock of two species of coccolithophores in the nordic seas: those stocks are described from two East-West transects, for the surface ocean and at two seasons. This type of information is very important for our present limited understanding of the evolution of the coccolithophore communities in the high latitudes. The northern oceans are evolving rapidly and data describing the impact of those changes on the planktonic communities are awaited. The manuscript is well written and the data are of good quality (they use a standard protocol to obtain them). The data shows an expected dominance of *E. huxleyi* over *C. pelagicus* in the Arctic waters. This was not the case in previous published work. This is an important result that deserve publica-

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tion in Biogeosciences, but not in its present form because of 1) the discussion is not yet matured, 2) the figures are not informative enough, 3) other important parameters could come out of this study.

Comments: 1) The dominance of *E. huxleyi* was unexpected because previous works have shown that *C. pelagicus* dominates the Arctic water mass in the western part of the Norwegian Sea. This unprecedented shift in the coccolithophore ecology in the Arctic is not developed enough. It should be illustrated by comparing the present data with previous works. The authors explain this by 1) a higher summer solar irradiance favouring *E. huxleyi* (in the fall the dominance of *E. huxleyi* is less important) 2) by a stratification due partly to ice melting in the most western arctic water masses, that also would favour *E. huxleyi*. These two explanations are not convincing because it is not really supported by the data. The data shows that 1) for example in sample 4 of the Northern transect in the summer the stock of *C. pelagicus* is quite high although the irradiance should be the same (no cloudiness data are given) than at the other collection location. This indicates that this species can thrive with that type of irradiance. 2) The two CTD that are located the closest to the sea-ice are CTD 3 and CTD 5. The latter shows no stratification and is associated with low stock of *C. pelagicus*, and the former shows stratified waters and higher abundance of *C. pelagicus*. The distribution of *C. pelagicus* in the Northern Transect does not appear to be clearly related to a stratification at a place of ice melting. There is a part of the discussion that is related to the temperature effect on the two species. I had a lot of difficulties to follow it. Looking at the data I do not see strong effect on temperature on the relative dominance of *E. huxleyi* which is found in higher abundance of *C. pelagicus* at temperature of 10° but also in Sample 22 of the southern transect in fall with a negative temperature.

2) The theoretical basis of those interpretations (irradiance and stratification) are not clear to me 1) why irradiance would help more *E. huxleyi* than *C. pelagicus* ? This appears to come from the Baumann et al. 2000 paper, but the reason of this are not express in this manuscript. 2) why irradiance would have change in the recent year (do

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they have data showing the difference in irradiance between Baumann et al., sampling and this sampling. 3) why irradiance effect would be mainly effective in the Northern Transect, and not the southern transect where *C. pelagicus* is more abundant. 4) the same questions are also valid for the stratification theory : what are the data that show the affinity of *E. huxleyi* for stratified water ? Why this is effective especially in the Northern Transect, and not in the southern transect ?

3) I do not stay in my comments 1) and 2) that irradiance and stratification are not responsible for the observed pattern of abundance but that the given explanations are not yet convincing. Explaining first, in detail the theoretical background with the state of the art, then showing the data with graph dedicated for that would certainly help the reader to accept this.

4) Some aspect of the data are not discussed: Why there is no coccoliths in Samples 7 to 12 in Autumn? In the table, the number of coccosphears counted should be counted to give an idea of the robustness of the method. *Algirospherea robusta* has been counted (the results are in the tables). The pattern of abundance of this species is not discussed in the text. Are there other species present in the assemblage (e.g. *G. muellerae*). In the data table, it can be noted that CTD samples have been counted: at which depth (it would be instructive if several depth has been studied to show the results) ?

5) The figure are difficult to read : -In the Figure 1, the currents are plotted but not the water masses, those that are indicated in Figure 3 and 4 at the bottom of the graphs. -the temperature scales in Figure 2 are too small to be seen. The water masses could be indicated in that figure. -Abundance data presented in Figure 3 and 4 could also be presented as maps using ocean data view for example: One for *E.huxleyi*, one for *C. pelagicus*. Plotting the relative abundance of each other could be instructive. Also interpretative plots would be welcome.

6)some additional data would be very interesting : -Are there several morphotypes of

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E. huxleyi present in those samples or just one ? Which one? SEM photography of them would be informative. -It is mentioned that the two phases of the life cycle of *C. pelagicus* are present. Why not showing the relative abundance of those?

7) In the conclusions, it is mentioned, that the "impact of the anthropogenically forced ocean acidification upon calcifying plankton in polar environments " might induce regional changes in the structure of the phytoplankton communities with major effects on the carbon cycle as well as the entire food web of the Nordic Seas". To discuss ocean acidification, and carbon cycle it is necessary to estimate the amount of calcite produced. A first and easy way to do this would be to multiply the species abundance per a calcite quota that could correspond of the coccolith weight found in Young & Ziveri (2000) for several species or in Cubillo et al. 2013 for *C. pelagicus*, multiplied by the number of coccoliths per coccosphere. The best would be to measure the coccosphere diameter, coccolith number/ coccosphere in each sample. Those data would be very informative.

8) Why the discussion is structured on the two transects. It should be mixed.

In the present state this manuscript is more a data report than a matured research contribution. However this data set is very interesting. By reshuffling and strengthening the discussion, and possibly by adding some other data, this manuscript should become a very interesting contribution to our understanding of the ecology of the changing Arctic Ocean. I recommend major revision.

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