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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.

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We thank the referees for having provided thorough feedbacks and for their suggested corrections. Both reviews agreed on the importance of illustrating the distribution of the two dominant coccolithophore species (*E. huxleyi* and *C. pelagicus*), in surface waters of the northern North Atlantic, an area which presently desperately lacks field observations on coccolithophore stocks despite its rapidly changing hydrological setting. Before addressing each of the comments, we find important to highlight our general objectives which, to a high extent, explain the methods and analytical strategy used in this present study.

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The choice to strictly investigate the distributional patterns of the coccolithophore species *E. huxleyi* and *C. pelagicus* essentially stems from the purpose of using this information in the interpretation of fossil records from marine sediment cores where both taxa are overwhelmingly dominant. From this research study, we, as paleoceanographer, aimed to get information on distributional patterns of key fossilizable species in view of large-scale surface water features, ie. water-masses and surface hydrological fronts (Arctic Front and Polar Front), in order to assess changes of the surface hydrology in geological records. In addition, the sampling took place during research cruises which aimed specifically at collecting sediment material; hence the collection of plankton samples had to be conducted according to ships opportunity, hence limited to en-route sampling using the ship seawater system; although rather “primitive”, this sampling stragey – and limitations in sampling for other biotic and abiotic characteristics of the surface waters – is a standard procedure which provided important information on coccolithophore distributional patterns in previous well-acclaimed studies (as Baumann et al., 2000, DSR II). Finally, the choice of limiting the taxonomical investigation to the two above mentionned species, was induced both by their overwhelmingly dominance in marine sediments (as coccoliths), as well as by the standard procedure of census counts using a light microscope with a X1000 magnification – as routinely used in marine micropaleotological investigations – which limits the identification of cocolithophore cells to the heterococcolith-bearing species - .

Would the editor allow for the submission of a revised version, please be aware that this new version will not include additional environmental parameters, nor information on the standing stocks of other species, other than the ones included in the preliminary version. We are therefore asking the editor for his decision for the opportunity of a re-submission, before starting to revise our manuscript. Part of his decision will obviously be based on our answers to the following reviewer comments :

Reviewer 1 : - Comment 1 “No is-situ environmental data beyond 5 CTDs and remote-sensing SST”. Part of the answer was given in the above introductory section. While

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acknowledging the importance of discussing coccolithophore standing stocks in view of a wide range of biotic and abiotic conditions, we'll make sure in the revised (1) to insist on logistical constraints which limited the investigations of these additional parameters, (2) to better explain that our purpose here is to describe the species abundances in the surface waters in view of the general oceanography – large scale distribution of the main water masses (Atlantic, Arctic and Polar water) and associated frontal domains - in order to grasp schematic patterns which will be later applied to geological records.

- Comment 2) on the reliability and efficiency of remote sensing data to grasp the surface water conditions at time of sampling – choice of remote-sensing time and resolution windows. Given the objectives of the study – comparing our phytoplankton distribution with large scale hydrological conditions -, and after a thorough look at available MODIS and AVHRR images, we believe that (1) satellite imagery provides the needed largescale view on water mass distribution, (2) that a 32 day-9km window is perfectly adapted to the space resolution of our sampling across both transects as well as to a reliable assesment of seasonal/monthly distribution of the water masses.

- Comments 2) and 6) on the use and reliability of maximum SST boundaries for the different species. We agree with reviewer #1 that the 32 day average remote sensing-derived SST profiles cannot obviously strictly trace the SST value at the location and exact time of the sampling. Hence our assesment of maximal observed boundaries are bounded by large errors. We therefore decided to discard these information in the future revised version. Hence, while acknowledging and agreeing with comment 6) by the same reviewer, we do not need to answer this comment. We'll however include the Tarran et al., Milliman et al., and Baumann et al. references on the *C. pelagicus* tempperature constraints in the revised version.

- Comment 3) on the combination of the haploid and diploid phases of *C. pelagicus*. We apologize for this erroneous statement on the used analytical procedure; we did not combine the motile-haploid (*Crystallolithus hyalinus*) and the related non motile diploid phases (*Coccolithus pelagicus*). All census counts refer to the non-motile

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heterococcolith-bearing phase (*C. pelagicus*). Not only is the motile, holococcolith-bearing *C. hyalinus* hardly taxonomically distinguishable under a light microscope (at x1000 magnification), but its coccolith structure (a combination of identical small calcite units) hampers its preservation in marine sediments; the decision to provide only counts of the diploid phase of *C. pelagicus* also refers to the overall perspective of this study (cf introductory section).

- Comment 4) on the lack of statistics. Given the only available parameter (SST), as well as its nature (remote-sensing-based) we do not consider that statistics will help further when discussing the general variability of the coccolith distribution in view of the distribution of the water masses.

- Comment 5) statements on coccolithohpore distributon not supportive enough due to to insufficient environmental data. We fully agree with reviewer 1 and will modify the text accordingly, making sure not modulate our statements according to the lack of evidences for certain biotic and abiotic conditions at the time of sampling.

- We additionally acknowledge the minor specific, and mainly technical comments which will be taken into account in the revised version.

Reviewer 2 : - Comment 1) This refers to several comments by reviewer #1 (comments 2, 5, 6), for which we are providing some answers as aobve. In addition, we'll make sure to improve part of the discussion on the effect of surface temperatures on the general distribution of both coccolithophore species.

- Comments 2) and 3) irradiance and stratification impacts on *E. huxleyi* and *C. pelagicus*. By referring to Baumann et al. (2000) we unfortunately discarded from our discussion all references included in Baumann et al. (such as Kleijne, 1990; Brand, 1994; Samtleben et al., 1995) which all provide some assumptions –though sometimes not strictly constrained by environmental datasets -. We'll make sure in the revised version, to include this references and to better discuss the proposed importance of water column stratification and irradiance on some of our observations.

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- Comment 4) various data not discussed in the text. This comments refer partly to technical aspects (total number of coccolithophore counted per samples, depth of CTD-sampled assemblages) as well missing information (short presentation of the distribution of *A. robusta*, absence of coccolithophores on a series of samples collected in fall 2007) which will be both taken care in the revised version. This however will not change the take-home messages of this study.

- Comment 5) improving the figures. We agree with reviewer #2 on the lack of information in figures 1 & 2 on the distribution of the water masses. In order to highlight these features, but with a view to limit additional colours/features in already dense figures, we propose to draw the interpreted locations of the Polar and Arctic fronts which indeed define the water masses boundaries (Atlantic vs. Arctic vs. Polar waters). We however consider the Ocean Data view (pie plots) is not well adapted to the geographical distribution of the samples along both transects. Bar plots such as the ones provided in figures 3 and 4, are better illustrating standing stocks when compared with changes in SST along the transects (and associated distribution of the water masses).

- Comment 6) Morphotypes. In the absence of SEM examination, one cannot define precisely the *E. huxleyi* morphotype encountered within our transects. However, our light observation indicate that *E. huxleyi* cells all belong to a single morphotype ca. 5-6 μm wide, with characteristics (central area) close to morphotype B (sensu Paasche 2001).

- Comment 7) ocean acidification and coccolith mass weight. We fully agree with reviewer #2 on the need to provide the amount of calcite produced by coccolithophores when addressing such phenomenon as ocean acidification (OA). We however underline that OA investigation and its effect on coccolithophore-induced carbon cycle is definitively not the aim of our study. Mentioning OA in the conclusion of our manuscript was merely intended to highlight a general view on impact of global climate changes in polar areas. We therefore do not consider necessary to provide additional data related to coccolith mass weight within our study transect.

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