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Discussions

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Interactive comment on "Spatial and temporal variability in coccolithophore abundance and distribution in the NW Iberian coastal upwelling system" *by* Blanca Ausín et al.

Anonymous Referee #3

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I think the study by Ausín and co-authors is interesting and eventually worth publishing, but there are several points where major improvement is needed. Also, the authors focus too much on the novelty of their particular site, which on an ocean scale is not so obviously novel at all, as there have plenty of studies in the same region when a larger scale is considered. Perhaps they need to point out why these sites are oceanographically different (not just geographically, but perhaps impacted by distinct processes), or on some other novel feature.

Other major concerns relate to the identification based only on polarized light microscopy, without more powerful scanning electron microscopy used to supplement

this technique. Their interpretation of some ecological associations could particularly be affected by this. In particular, they appear not able to distinguish well enough within the Noelaerhabdaceae (Emiliania, Gephyrocapsa, ...). They may well be able to distinguish large-vs-small Gephyrocapsa's, but probably not intermediate species (G. muellerae). In the case of Emiliania huxleyi, their approach ignores that great morphological, physiological, phylogeographical, and genomical differences. It is now well demonstrated that different morphotypes seem to have contrasting ecological associations, and it has been recently demonstrated that the genomes of offshore and coastal E. huxleyi may show major differences. These differences within the species or species-complex can especially complicate patterns in upwelling systems, where different types may be observed. I would much prefer that they include an SEM analysis at morphotype level of E. huxleyi and other Noelaerhabdaceae.

Another problem – which in fact is one of the most interesting aspects of the paper – is the uncoupling of coccolith and coccosphere patterns. In some near-bottom samples this appears to be due to resuspension of coccoliths from the sediment, but even excluding that, the coupling is not close. If they did a graph of free coccolith vs coccosphere abundances they would probably see this. That means that inferences about ecological associations of coccolithophores may be different if coccospheres or coccoliths are examined. Of course, they do mention that part of this might be due to the stage of a "bloom" that is observed (so free coccoliths may be sampled when the major bloom phase was missed). Would there be any way to combine these two data sets for a more complete picture? That would be interesting.

I think with a couple major improvements and refinements along the suggested lines (which hopefully won't present too much difficulty) this will end up being a very nice article.

Detailed comments, in order through the manuscript.

The English language use in the Abstract needs refinement and extensive editing. Mi-

nor comments: "For the first time . . ." This isn't necessary. "On the contrary, despite minimum abundances were generally found during downwelling periods, unexpectedly high coccolithophore abundances were registered in subsurface waters at the onshore station" . "on the contrary" is redundant to "despite", and not sure what is being contrasted to.

Introduction, lines 27-30, and later. It isn't clear that one would expect that studies in the southern Iberian coast at about latitude 39ËŽ-40ËŽ N would not be reflect patterns at 42ËŽ N. How different are oceanographic patterns and processes at in the Northwest and Southwest Iberian coasts? There is a bit too much emphasis trying to sell the study based on this particular site not having been studied much for coccolithophores before, but that doesn't sell it in and of itself (and I think the study is interesting enough without forcing this issue).

p. 4, line 8: "Upwelling index". So we don't have to go look up which Zúñiga used, please say Bakun upwelling index (well known index).

Comment on 3.2 Coccolithophore analyses: All identification seems to have been done only with polarizing microscopy, not electron microscopy. They reference identification guide of Young et al. 2003, but that guide is based almost exclusively on scanning electron microscopy. I am not clear to know to what degree polarizing microscopy is sufficient for species-level identification, and where the limits are, partially as I am not that experienced with polarized microscopy identification.

Section 4.1. The text description of oceanographic processes seems a little bit disconnected from what is shown in the graphs. For instance, in 2C I do see that from July to Oct 2011, upwelling favorable winds seemed to dominate, considering the Bakun upwelling index. By February 2012, there were large periods of upwelling-favorable winds, but I wouldn't say they continued through June of that year, as there were substantial periods of downwelling favorable winds in April-May and the end of June. It might be nice to overlay 8-day and 30-day running means for the indices in 2c. In

statistical analyses, the upwelling index averaged for several days prior to sampling should be used, as it takes several days for upwelling to develop when favorable winds blow (so winds have to blow for at least a few inertial periods/days). While I see clearly the correspondence between Fig. 2e and the statement "Finally, during February, winter mixing conditions were also detected with the water column being characterized by colder (< 13 âÅř C) and more saline (< 35.8) waters", February doesn't seem to be especially salty compared to the rest of the time series in Fig. 2f. In fact it is less salty than January. Saying the patterns in Chl-a were comparable between CALIBERIA and RAIA stations (lines 2-3) also seems not to correspond to Fig. 2h. The highest concentrations of Chl-a were found in July in RAIA (>2 mg m-3), were also high in September (>1 mg m-3), while values in Caliberia were low (<1 mg m-3) in July and Sept 2011. January surface (10 m) levels were comparatively low at RAIA but moderate in CALIBERIA. . .

The uncoupling between abundances of free coccoliths and coccospheres makes me question the strategy of basing coccolithophore diversity patterns on free coccoliths, rather than on coccospheres.

How do the authors define "bloom"? This needs to be more clear.

For the "deeper blooms" that the authors attribute to wave-mediated resuspension of sediments, I would have thought they would mention at least one other evidence supporting this conclusion. If those blooms are from resuspension of sedimented coccolithophores, wouldn't they be dominated by free coccoliths and not complete coccospheres? On p. 6 lines 24-25 they say "The number of coccospheres drastically drops below 50 m water depth at both stations, suggesting that their disaggregation takes places right after the cells die". Indeed, the deep coccolith max at RAIA station during Nov.-Jan. is not matched by a deep max of coccospheres.

"our results show that freshwater lenses advected to RAIA station have negligible influence on coccolithophore productivity" The authors did not observe that in one year of

study, but that doesn't mean influence is always negligible

p. 9, lines 19-20 "At the offshore site, coccolithophore productivity was seasonally modulated, increasing five orders of magnitude during the summer/upwelling regime and decreasing drastically during the winter/donwelling periods". I have two problems with this: First, what is "coccolithophore productivity"? Productivity is often most strictly used to refer to a rate (e.g., primary productivity in g C m-3 day-1 or g C m-2 day-1), though less precisely the word is sometimes used to refer to patterns of biomass or organism abundance. Here I am not sure if they are referring to coccosphere or coccolith abundance, which follow somewhat distinct patterns, and they certainly aren't talking about a rate. They show one example why it is often better to use the more strict sense: They have an increase in coccoliths during winter at the onshore station that they attribute to re-suspension, not to production, so coccolith abundance does not necessarily reflect coccolithophore productivity on smaller temporal and spatial scales. Second, I don't see where they show "increasing five orders of magnitude". Their highest reported abundance of coccospheres is 3x105 cells ml-1. They have not defined their detection limit. I doubt they could easily detect 3x101 cells ml-1 (there would be 6-15 total coccospheres per filter, on average, and they would have to count the whole filter to be able to detect those, not just a "random piece"). It seems more possible they could have documented a "five orders of magnitude" change in the numbers of coccoliths, but again we need to know what their detection limit and minimum abundance seen was.

p. 9, lines 22-23: "This affinity of coccolithophores for summer stratified conditions during the upwelling season was already observed by Silva" Wasn't this already observed much earlier? Seems that more generally it was a pattern already recognized by the review of Margalef in 1978.

p. 10, lines 5-16. I have some difficulty with the discussion of the environmental affinities of E. huxleyi. This species seems to be everywhere outside the poles, mostly representing 50%-100% of coccolithophore communities (though occasionally lower

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percentages). Morphological, phylogenetic, genomic, and physiological studies now seem to suggest there may be quite different ecotypes with different sets of adaptations. Work by Young and Beaufort and later others (e.g. Cubillos et al. 2007; Cook et al. 2011, Hendriks et al. 2012; Poulton et al.; Smith et al. 2012) have identified different morphotypes (A, B, B/C, O, etc.) which seem to display different oceanographic distributions. Hagino et al. and Bendif et al. have shown that different haplotypes seem to be to be associated with different water temperatures. Read et al. (2013) showed that there could be major genomic differences, and von Dassow et al. (2015) showed that much of the genome content differences related to coastal/productive vs offshore/low latitude origins. So it does not surprise me at all that studies could find contrasting ecological associations of E. huxleyi, when all of these morphological, phenotypic, physiological, and genomic variants are grouped together. I think there is much more information in the non-E. huxleyi species, like G. oceanica, small Gephyrocapsa, and Florisphaera. This caveat should be properly discussed. Further, the study would really benefit from incorporating electron microscopy analyses, to be able to distinguish the different morphotypes of E. huxleyi, as they have been observed to show very different ecological patterns.

p. 11, lines 30-31: "Our results highlight the role of coccolithophores as significant primary producers in the study area, being strongly correlated with higher values of Chl a" I am not sure they have justified to go from a correlation between coccolithophore abundance and Chl-a concentrations to considering that coccolithophores are major primary producers in this area. Most of the coccolithophores all the time were either E. huxleyi (typically 5 μm diameter cells) or small Gephyrocapsa (even smaller!). Abundances of 1x105 cells L-1 do not mean very high biomass when talking about 3-5 μm diameter cells. I would be more convinced if they had used an estimation of cell carbon (based perhaps on volumes from Young & Ziveri 2000 and C:volume estimates from other studies) and a reasonable carbon:chl-a ratio, to show what range of phytoplankton biomass they might represent. Also, the correlation is never explicitly analyzed, as far as I can tell.

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