

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 #4

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Ausín and colleagues present the results of a study on the temporal occurrence and of coccolithophores based on water samples taken monthly over a one-year period at two stations off the NW Iberian coast. Higher abundances at the offshore station were attributed to warmer and nutrient-depleted waters favouring coccolithophores in comparison to diatoms, whereas unexpectedly high coccolithophore abundances were registered in subsurface waters at the onshore station. The study seems to have been designed well, but the authors seem to have interpreted their results not properly enough, and also problems with the presentation of the data make this contribution hard to appreciate.

C1

General comments:

The manuscript reads well, but I have a hard time fully grasping its message. Thus, the paper may contribute to the understanding of the coccolithophore ecology of the NW Iberian coast, but just after a couple of clarifications. In particular, I am not fully convinced that usage of both coccosphere and coccolith counts is appropriate to infer the made conclusions. To my knowledge coccolith numbers never equals coccosphere numbers in water samples for several reasons. Those are, (a) dramatically different numbers of coccoliths/sphere for different species, (b) the occurrence of multilayered coccospheres, e.g. in *E. huxleyi*, and (c) the preservation potential of the different coccolith types that result in selective preservation, (d) the occurrence of faecal pellet grazers, just to name a few. What is the meaning of single liths in the water column, how do they get there, what is the influence of zooplankton grazing on their occurrences, and how far can they be transported, etc.? Further usage of the coccolith data would at least need an intense discussion on this issue! Furthermore, what is the total number of coccospheres identified per sample that statistically was not significant to ensure a correct estimation of the abundance of even the five majority species and species groups? Most of the information given is limited to the five major species/groups, so that minority species would not be included anyway. My main suggestion is thus to exclude coccolith data from the interpretation and focus only on the reasonably acceptable coccosphere counts!

The authors state that the results highlight the role of coccolithophores as significant primary producers in the study area, which contrast the occurrence of diatoms. If at all, such a statement can only be made on coccosphere numbers, should include information on other phytoplankton, and would also have to consider a similar range of diatom data. However, for the latter “only” a surface transect is presented, which limits any comparison dramatically. I also would recommend using the term “bloom” in a different way, neither as representative of the standing crops of coccolithophores nor as a term for coccoliths! It should be a large, temporary colony of coccolithophores (=coc-

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cospheres) or of coccolithophore species. Thus, I would name only the occurrence in May 2012 at CALIBERA station as a bloom event. Apart from this terminology issues, I would like to see a discussion on the reasons both for Type A and Type B “blooms”, if the coccoliths will still be used. Their differences are mentioned but that’s it. And if the “deeper blooms” are based on wave-mediated resuspension of sediments, this for sure is something different. But if this is an assemblage composed of resuspended coccoliths, it should be composed of species accumulated in the sediment. But to me it is not clear, if this is the case. It simply is not specified here!

It is mentioned in the oceanographic setting that down-welling favourable conditions and a decrease in primary productivity occurred during autumn and winter (October to March– April). However, the peak occurrence of coccospheres is during March 2012, at the end of a slight cooling of the upper water column. Is this an unexpected “upwelling” event or are similar events known from this area at this time of the year? However, apart from the temperature signal, this event seems to have only minor impact on the other parameters. Nutrients just slightly increase from close to zero to just 0.3 μM HPO_4^{2-} . Is this due also for the other available nutrients described in the methods? It would be important to add other nutrients, since, e.g. off Bermuda increasing coccolithophore abundances coincide with the seasonal advection of nitrate-rich but phosphate-poor waters to the euphotic zone (Haidar and Thierstein 2001). Thus, taking phosphate as a representative of all nutrients could hamper the ecological interpretation of the species. Actually, the discussion in chapter 5.2 species by species is a bit boring and rather superficial to me (and is based on coccolith data, of course). It mainly confirms previous interpretations of the species and “just” defines assemblages that may be used as local proxy indicators. I would discuss the CCA much more (if not based on coccolith data)

I have also problems with the figures. Actually, I don’t like this colourful tiny way of presentation and would favour similar graphs limited to grey scale! And since the data are described station by station, I would plot the information for each station together.

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I would also recommend adding the oceanographic currents to figure 1. And Wind is often mentioned in the text, but no data is shown nor is there any further information given. At least some general or schematic information should be given in Figure 1 as well!

Specific comments:

Page 1, l. 30: “group” not in italics

Page 1, l. 34: Please specify which relevant information you mean.

Page 2, l. 20: Those upwelling systems are generally named as Eastern Boundary Upwelling Ecosystems (EBUEs; Fréon et al., 2009).

Page 3: Chapter 2, Oceanographic setting – I would merge this together with the chapter 4.1, which would allow pointing to “unexpected oceanographic events”.

Page 4, l. 25: Young et al. (2003) not in reference list.

Page 5: Chapter 4.1 is not always clear to me. Sometimes the statements are only for RAI A station, sometimes for both. Please clarify. The text description in the text and what I got from the figures seems to be not fully the same!

Page 5: Chapter 4.2 is not needed if the data is already published.

Page 8, l. 17: Data from Ferreira and Cachao come from an estuary and coccolith data is at the same range (up to 4.8×10^5 coccoliths/l) at the RAI A station!

Page 10, l. 5 ff.: Is there anything known on *E. huxleyi* morphotypes for this region. The discussion of the environmental affinities of *E. huxleyi* would need further information on morphotypes, which may have different adaptations. In other areas such as off SW-Africa, distinct differences in the occurrences of morphotypes have been observed (Henderiks et al. 2012, Mar.Ecol.Prog.Ser. 448).

Page 11, l. 5 ff.: I am a bit worried about these minor species. So far nothing has

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been presented on these and information is only given in the supplement. Again, this information seems to be based only on coccolith data, but seems also been limited in a statistical sense. Otherwise please introduce information on the species earlier.

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