We want to thank Reviewer #4 two for their comments and suggestions, and we have changed our manuscript accordingly whenever possible. Discussion point by point and explanation of main changes can be found here.

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 gracers, 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 been transported, etc.? Further usage of the coccolith data would at least need an intense discussion on this issue!

Indeed, and as already discussed for the other reviewers, we use and interpret the coccosphere data set in this new version in order to avoid these issues and to correctly address coccolithophore ecology.

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 been included anyway. My main suggestion is thus to exclude coccolith data from the interpretation and focus only on the reasonably acceptable coccosphere counts!

According to Fatela and Taborda (2002; Fig. 2), the detection of a minor species (here considered to be < 2% of the total assemblage) is performed at a confidence level of 100% when counting 500 specimens (i.e. coccolith data set); and it is of 90%-100% when counting 100-300 specimens (i.e. coccosphere data set). The latter is in any case a reasonable confidence limit too. This information relative to confidence levels of both coccosphere and coccolith counts has been added to the new version.
We agree with Reviewer #4 and the other reviewers that ecological inferences have to be based on the coccosphere data set. Therefore, we base interpretations about coccolithophore species ecology (section 5.2. of the manuscript) on the coccosphere data set in this new version.

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 been made on coccosphere numbers, should include information on other phytoplankton, and would also have to consider a similar rang of diatom data. Corrected

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 (=coccospheres) or of coccolithophore species. Thus, I would name only the occurrence in May 2012 at CALIBERA station as a bloom event. Corrected.

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 specfied here! We believe we did explain this in the text. This is the reason why we treat both data sets (coccospheres and coccoliths) separately, so we can assess processes that differentially affect coccoliths that are already deposited on the underlying sediments and coccospheres (which rarely make it to the sea floor). This "deep blooms" as we referred to them in the previous version (we have eliminated that term from the employed terminology) were and still are explained and discussed in this version.

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?

In the oceanographic setting we explain the seasonal oceanography of the study area in general terms. We refer the reviewer to the paper by Zuñiga et al. (2016), where a monthly time series of the conditions of the water column can be found for RAIA station from 2008 to 2012. As described in their work for this long time-series: ¨…*from January to March-April, the water column was thermally homogeneized (_14 °C) and characterized by high nitrate levels (4–6 mmol kg_1) due to the winter mixing and river runoff. Subsequently, the transitional phase from winter to the upwelling season was marked by the spring bloom episode, as registered in* ***March 2008, April 2011 and March 2012***". Therefore March is as a transitional month between both the winter and the upwelling regime, and similar events like the one identified in our Manuscript are likely to occur during this periods. "*During these spring transition periods, the development of sea surface thermal stratification due to incipient solar heating, jointly with the up-welled of nutrient rich subsurface waters, caused by the establishment of northerly winds, trigger the increase of Chl a levels in the water column*".

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 HPO42-. Is this due also for the other available nutrients described in the methods? Indeed, as shown in the new figure. It would 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.

We did not plot nitrate for two main reasons:
1.-All nutrients were considered in the initial variable selection prior to CCA, as stated in the text, but variable selection only pointed at HPO_4^{-2} *as being significant to explain coccolith variability, and rejected* NO_3^-, (and also $Si(OH)_4$, although this was expected)*. Therefore, we preferred not to show data that was not going to be included in the discussion.*
2.-Nitrate shows the same temporal and spatial variability as phosphate, although different absolute values.
In any case, Nitrate has been presented now for those readers that might wonder about its distribution and concentration. As coccolithophores utilize phosphate and nitrate, but not silicic acid, do not show the latter.

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).

We use the coccosphere species abundance data in these new version to address such issues and have tried to discuss species in more detail.
However, we believe the CCA has to be used to make more apparent relationships that both are and are not that obvious for the human eye from the simple visualization of the species distribution, but its interpretation must be made based on the background we already have on the species. This is, just because a CCA locates a species close variable *X*, that does not mean there is a causal relationship between them. Therefore, such interpretations have to be made with caution without trespassing the boundaries of the statistical possibilities. In our case, CCA seems to indicate coccolithophore-environmental relationships that have been found by other authors already, and does not reveal any striking/new feature, so we discuss this accordingly. Nevertheless, the new CCA performed using the coccosphere data set relates *Syracosphaera* spp. species with higher salinity and temperature, something that might be indicating its preference for the subtropical ENACW carried by the ICP in autumn, (and not necessarily for higher temperature and salinity *per se*.) This is now discussed in the new version. For other species, the same conclusions as in the previous version can be made since their location in relation to the environments on the bi-plot is still the same.

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. We would also like any journal to provide a page per figure, but this is never the case. There is a lot of data to present, and their size can be changed just by using the pdf zoom tools. Regarding colours, we used the default colour scale provided by ODV, with which most readers are already familiarized, and which helps to visualize the data. Figures can be also visualized in grey scale simply by printing them in black and white (and this works for both physical printing and virtual pdf printing). We are afraid the opposite (greyscale to colours) is not possible.

We have kept Fig. 3 as it was (comparing total abundances and % between stations) so it illustrates the discussion, in which differences and similarities between stations are discussed, but the rest of the figures have been altered so species distributions are plotted by stations as the Review suggests.

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! There is still some controversy about the Portugal Coastal Current (PCC) and its existence, for which co-authors of this Manuscript do not have a single and agreed opinion. This is the reason why it is not mentioned either in the oceanographic settings. Actually, it is not necessary for the discussion. Winds are explained in the text, but we don't think it is necessary to plot their arrows in the figure; besides they change in seasonal terms so this would lead to two separated (and unnecessary figures). Winds are mentioned because the upwelling occurring is this region is wind-driven upwelling. So whenever we mention the wind it is another way to refer to the upwelling (and its forcing mechanism).

Specific comments: Page 1, l. 30: "group" not in italics. Corrected.

Page 1, l. 34: Please specify which relevant information you mean. It is specified in the next 5 lines.

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

Page 3: Chapter 2, Oceanographic setting – I would merge this together with the chapter 4.1, which would allow pointing to "unexpected oceanographic events". We think the reader will appreciate an introduction to the study area, this is why this section goes right after the introduction.

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

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

Page 5: Chapter 4.2 is not needed if the data is already published. Only RAIA data is already published, not CALIBERIA.

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 x105 coccoliths/l) at the RAIA station! Added.

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).

E. huxleyi morphotypes would certainly provide some information on their relationship with the environmental variables. Nevertheless, such specific research question is out of the scope of this paper and such morphological analyses are certainly not essential or a requisite to provide the key information we need to answer our research questions (i.e. How are coccolithophore and coccolith abundance patters in the NW Iberian Margin? What can we say about their inner-shelf and outer-shelf temporal variability in relation to seasonality and/or diverse oceanographic processes?).

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

We presented both their relative and their absolute abundance in the Supplementary files so everybody can see these results, and they are presented on page 7. L21-23. We did not comment too much on these because they were not included in the CCA (relative abundance was not > 2% in at least 2 samples). Simply describing their abundance was, as mentioned by the Reviewer, a bit boring, so we decided to give this information to the reader in the form of figures in case anybody was further interested. As aforementioned, the ecological interpretations at the species level in this new version are based on coccosphere data set and not the coccolith data set, but information on minor species is still included in the Supplementary material.

Fatela, F., Taborda, R.: Confidence limits of species proportions in microfossil assemblages, Marine Micropaleontology, 45, 169-174, 2002.
Zúñiga, D., Villacieros-Robineau, N., Salgueiro, E., Alonso-Pérez, F., Rosón, G., Abrantes, F., Castro, C.G.: Particle fluxes in the NW Iberian coastal upwelling system: Hydrodynamical and biological control, Continental Shelf Research, 123, 89-98, 2016.