First of all, we want to thank Reviewer #3 for their in-depth review that has helped to improve our manuscript. We have modified it whenever possible, while discussion on point by point and explanation of main changes can be found here.

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). We did distinguish *E. huxleyi* from *G. aperta* and *G. ericsonii*, even the coccospheres! *G. aperta* and *G. ericsonii* were lumped together and merged within the group "small *Gephyrocapsa*", following a general approach based upon the assumption that they are influenced by the same ecological conditions. And we do distinguish the intermediate species *G. muellerae*!, which was also shown in the Supplementary pictures. All these species are mentioned in the Appendix, and shown in independent figures.
Counts and classification of coccolithophores in our lab are always performed by polarized light microscope. Although the use of SEM can facilitate coccolithophore classification, our access to those resources is (as for many other groups) costly and therefore very limited, and in any case the use of polarized light microscopy is a well-established approach used by many other research groups (e.g. Ferreira and Cachão, 2005; Bai et al., 2014; Sun et al., 2014; Balestra et al., 2017; Bonomo et al., 2017).

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. All this is discussed below in detail in three related comments.

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

Minor comments: "For the first time . . ." This isn't necessary. Deleted.

"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. Corrected.

Introduction, lines 27-30, and later. It isn't clear that one would expect that studies in the southern Iberian coast at about latitude 39-40N would not be reflect patterns at 42N. 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).

There are several features that make the NW Iberian Margin interesting for the study of coccolithophore distribution and different from the SW Iberian Margin. 1.- Its proximity to the Minho River mouth. 2.-It is next to the Rias Baixas, a region that is the target zone of dozens of studies itself due to its related specific processes. 3.-Its distance to the Strait of Gibraltar, from which the different branches of Mediterranean Outflowing Water outflow, which strongly impacts column water dynamics in the SW part, not so strongly in the NW part. 4.-Its distance to the Gulf of Cadiz, source of reworked coccoliths in the SW part (Ferreira et al., 2008). Nevertheless, we believe the vast majority of the readers interested in this paper will be aware of such oceanographic differences, but in any case, we have rephrased it according to the Reviewer comment since certainly that was not the point we wanted to make anyways.

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). Corrected.

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.

Polarized light is widely used in many oceanographic, also for this region (e.g. Cachão et al., 2000; Ferreira and Cachão, 2005; Balestra et al., 2017), and also in most paleontological studies based on coccolithophores and nannofossils. Coccolith identification by polarized light is very easy and straight-forward, not that much for coccosphere identification, which can be time consuming and less straight-forward. We acknowledge that the use of Scanning Electronic Microscope is more precise for coccosphere classification. Unfortunately, we do not have access to those resources to undertake such task and make the most of this material. It has to be kept in mind that most of the interpretation deals with general temporal coccolithophore abundance patterns, and for that only the total coccosphere abundance is needed. Inferences on coccolithophore ecology are only discussed in the last section 5.2., for which counts of coccolithophores at the species level are used in this new version.

Regarding confidence levels, and following 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 (even the lower one), and other studies based on a maximum count of 100 coccospheres have been proven to provide valuable and useful information (e.g. Bai et al., 2014; Sun et al., 2014).

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.

We refer the reader to a previous work for a better visualization (since our figures are limited by the width of the station figures) and identification of oceanographic processes associated to the shown oceanic conditions occurring in the area during the sampling days (Zúñiga et al., 2016).

We have produced a 3- and a 4-point moving average smoothing spline for the Bakun Upwelling Index. Below is shown the 4-point moving average in green (higher than 4 the resolution is very poor and some important events are lost). But its overlap with our 2c figure does not favor an easier visualization of the processes, not event taking this figure individually and making it bigger.
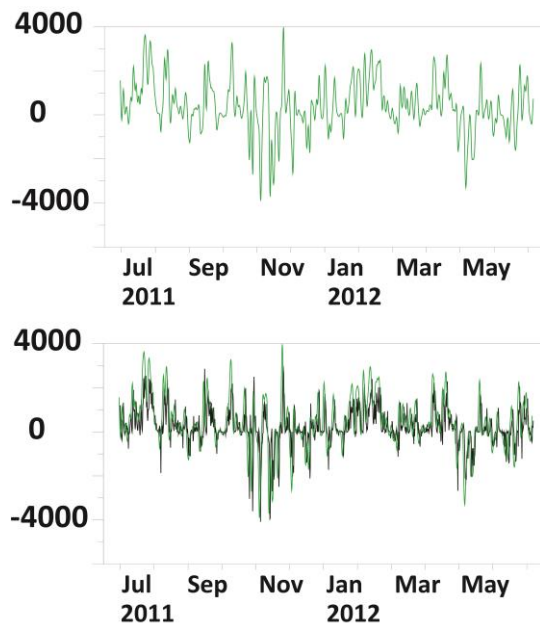
In statistical analyses, the upwelling index averaged for several days prior to sampling should be used, as it takes several days for upwelling to develop when favorable winds blow (so winds have to blow for at least a few inertial periods/days). We did so in an exploratory analyses. We calculated the mean values of each of the studied variables for 3 days (two days before the sampling day), and also for 5 days (4 days before the sampling day). First, we incorporated these three data sets to the variable selection, which suggested the use of averaged data from the sampling day. Second, and to make sure that possible collinear effects among the three different temporal resolutions of a certain variable were not guiding variable selection, we also performed exploratory CCAs with each set of temporal resolution. CCA for both 3-day and 5-day data set explained less variance in the coccolith data set. Considering this, we decided to use the 1-day data set (sampling day).

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 â ¸Aˇr 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. Corrected.

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. . . Certainly… we corrected this.

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. Dicussed above. 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. These "deeper blooms" are indeed dominated by coccoliths (actually there are not coccospheres at that depth at those times), but this confusion most likely derive from the broad and imprecise use we made of the term "bloom". We have corrected that and also clarified in the text when we mean coccospheres and when we mean coccoliths.

"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. Rephrased.

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. The term "coccolithophore productivity" has been replaced in the text by coccolithophore abundance, a more correct term for what we mean. We have clarified through the text when we are referring to coccoliths and when to coccospheres. Finally, we have deleted the orders of magnitude to avoid confusion.

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. Indeed, but we mean it for the regional (the Iberian Margin), not global context.

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 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. Detailed morphometric analyses were not undertaken for two main reasons: First, and unfortunately, we do not have access to (nor the resources to access) a SEM; second, a morphometric study is other investigation itself formulated to respond other questions and that should be conducted differently. Although we do not doubt of the usefulness of genome analyses, we do not see how these can be 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?). *E. huxleyi* is the dominant species (based on its relative abundance), something common in many other studies. But its temporal variability should not be assessed by looking at its relative abundance. The later just informs on the assemblage composition, but tells nothing about its temporal variability. *E. huxleyi* temporal variability must be assessed by looking at its absolute abundance, where indeed a distinct seasonal signal is observed, and therefore a clear link with the environmental conditions can be established (i.e. preference for upwelling regime conditions). A detailed study on the different *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.

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.

We agree such statement is too categorical and that we cannot establish such correlation. We have therefore rephrased to hypothesize that according to our results, higher coccolithophore abundances also occur along with periods characterized by high Chl-a concentrations, something that might indicate that this phytoplankton group can be contributing to some extent to higher Chl-a concentrations. We believe this is an important point to make, since coccolithophores are generally

an overlooked group when exploring the contribution of different phytoplanktonic groups to Chl-a values.

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