Responses to reviewer #2

Zúniga and co-authors describe time series of diatoms fluxes and the species-specific composition of the assemblage in a high productive area off the northern NW Iberian coast and compare them to those preserved in nearby-by surface sediment samples. They studied the diatom community at 5 m (plankton samples), at 35 m (sediment trap) and at 75 m (slope bottom). Samples were intermittently collected between November 2008 and July 2012. Based in previous observations, they identify upwelling and downwelling intervals between early 2009 and mid 2012. The length of these intervals was variable. As collected with sediment traps, highest biogenic silica (BSi) and diatom fluxes correspond to downwelling intervals. This was due to strong southwesterly storms and wave-driven resuspension. The riverine input of terrestrial materials during fall and winter represents an additional source of particles and nutrients.

General comments:
- Several gaps, up to six months duration, interrupt the trap record. Neither further information nor detailed discussion on these issues are offered. Unfortunately, no continuous record for a full calendar year is presented. This makes the seasonal description and the seasonal comparison between years difficult.

- Results description is confusing and needs strong revision. I have listed below several sentences/statement, which need strong clarification.

- Discussion is driven by the idea of seasonal differences in the particle dynamics (upwelling / downwelling; see Figs 3-6), independent of the fact that for several of these intervals not enough data have been collected. Because of the lack of (continuous) observations, data tend to be over-interpreted.

- A native speaker should first revise the manuscript. Several language issues make the reading heavy. In particular, the English in the Discussion section is messy and tedious to read.

Response: The authors are really aware that the existence of gaps in the time series may mask some particular oceanographic features. However, the authors do not consider that the interruption of the trap record could lead to a misunderstanding of the seasonal patterns, since those repeatedly appear along the time series. They would like to emphasize that when they statistically analysed the time series in terms of seasonality, one of the main issues taken into account was to be confident of the existence of samples collected during each oceanographic season for every year. With the exception of year 2010, the authors found some general patterns in diatoms fluxes as being characteristics of each of the oceanographic seasons described by Zúñiga et al., (2016). Statistical analyses also corroborated our finding.

Results description has been revised and the authors hope it to be tighter now.

Following reviewer #2 suggestion, English has been revised. The authors hope that reviewer #2 will appreciate the improvements of the manuscript.

Specific comments:
Introduction: it is a smorgasbord of subjects and needs a more strong focus. We all know the critical role played by oceans in the global carbon cycle and the role of diatoms in fixation of CO2. However, how relevant is this for a manuscript dealing with a very regional signal under very particular oceanographic and atmospheric conditions?

Response: The authors agree with reviewer #2 that most people know the critical role played by the ocean in the global ocean carbon cycle. Though, in this first paragraph the authors not only asserted this idea but also highlight the importance of diatoms in a context of ocean carbon cycle as a key issue to introduce the manuscript. Furthermore, the authors find it important to justify the importance of these studies in regional coastal upwelling systems. This study(jointly with previous ones about diatom fluxes in coastal regions) is fundamental not only because these regions are the most productive regions in the global ocean but also because they are characterized by a strong seasonality both in terms of hydrodynamic and biological processes. Therefore, these areas are fundamental for paleoceanographic studies since they are likely to preserve relevant information on past productivity conditions and its causes.

In any case, first and second paragraphs of the introduction have been modified to clearly state the aim of the manuscript.

P. 2, l. 10-12: stating that "Nevertheless, primary production reconstructions suffer from the uncertainty about how diatoms respond to particular environmental conditions, and how particular diatom species transfer primary production signal via exported and buried particles" is misleading. The authors ignores a huge body of published in the past 20 years addressing the issues of diatom signal in the uppermost water column and is preservation in surface and downcore sediments. This needs revision.

Response: Paragraph has been rewritten. Please see comment immediately above.

P. 2, l. 31: is RAIA an acronym? In that case, what does it mean?

Response: RAIA is not an acronym; it is the local name for the northern border between Spain and Portugal. The term 'RAIA' is also the name of a EU FEDER funded project (INTERREG 2009/2011; 0313/RAIA/E). Our mooring line deployments were partially funded by this project as mentioned in the acknowledgments.

P. 3, 10: “During these hydrodynamic periods, this site ...“, Which site? Revise. Same line: what is the yearly discharge of rivers Minho and Douro? Data are presented in Fig 2d, though there are hardly comments on this.

Response: The sentence has been modified in the new version of the manuscript.

P. 4, l. 16: the RAIA was not “monitored”, but waters overlying the station?

Response: The sentence has been modified.

Below in l. 17, was the trap deployed or within the photic zone and the mixed layer?

Response: The sediment trap was deployed at the base of the photic zone, located on average at 35 m water depth. This information was obtained making use of a Secchi disk.

P. 5, l. 17: statistics were performed by using which diatom species data, relative or absolute abundance?

Response: The authors agree with reviewer #2 that additional information is required. The sentence has been modified in the new version of the manuscript.

P. 5, l. 22-23: “resulted from water column data interpolation by considering sediment trap sample recovering intervals“, difficult to understand, needs rephasing.

Response: The sentence has been modified.

P. 6, l. 6-11: this paragraph is copied from Zuniga et al., Continental Shelf Res. 123, p. 92.

Response: The authors do not agree with reviewer #2. Hydrographic features are the same than those described by Zúñiga et al. (2016) because both studies were carried out over the same samples but the paragraph was not copied.

In Zúñiga et al. (2016) the paragraph is as follow:

Prevailing south-westerly winds (negative –Qx values) were registered from October to April-May causing strong downwelling conditions. (Fig. 3(a)). Such conditions were accompanied by strong SW storms responsible for significantly high wave heights (HS) (up to 9.1 m)(Fig. 3(c)). Wave action lead to resuspension of bottom sediments as reflected by the significant increase in deep water column turbidity under stormy conditions(Fig. 4(e)). At the same time, intense Minho and Douro river discharges (4400 m3 s_1), left their imprint as low salinity (35.6) and relatively high nitrate content water lens at sea surface (Fig. 4b, e).

In the present manuscript the paragraph is:

From October to April-May, the NW Iberian margin was generally characterized by the prevalence of low irradiance levels and south-westerly winds as shown by the negative -Qx values (Fig. 2a and 2b). This downwelling season was also accompanied of strong SW storms promoting wave heights frequently higher than 4 m and intense Minho and Douro River discharges (Fig. 2c and 2d).

P. 6, l. 11: I do not understand why "higher nutrients levels" occur when temperatures went down. Higher nutrient content might be due to stronger mixing or stronger eolian input/riverine discharge or a change in prevailing water masses, and might temporally match the occurrence of low(er) temperature. This needs revision.

Response: The sentence has been modified.

P. 6, l. 14: What are these "small centric cells"? it is too vague and needs more accurate description.

Response: Small centric cells are referred to centric diatoms that for their size were not possible to be identified to the species level. In order to clarify this important aspect, additional information has been included in the methods section, figure caption 4 and table 3.

P. 6, l. 15: "From May to October", this is true for 2010, though it seems to be March to Oct in 2009, while March to Sept in 2011 and Jan until July in 2012. Revise.

Response: The sentence has been modified in order to be coherent with first paragraph of section 4.1.

P. 6, l. 16: to me the use of "upwelling relaxation" and "promotion of upwelling of cold and nutrient rich ENACW" seems contradictory. Relaxation implies slackening of upwelling

intensity (see e.g. Fraga et al., 1988, Continental Shelf Research 27, 349-361), hence if upwelling tends to weaken, why should influence the dynamics of temperature?

Response: From March-April to October, northerly winds are responsible for the upwelling of cold and nutrient rich waters on the NW Iberian continental shelf. In this oceanographic context, it is frequent that upwelling favourable winds persist during spring months. However, during summer months, a series of upwelling-relaxation cycles are more characteristic of this study area. This means that during summer, we observe periods of intense ENACW upwelling and periods of winds relaxation that promote water column stratification. In any case this sentence has been modified to avoid confusion.

P. 6, l. 18: a more through description of diatom values is needed here. Stating that "During the highly productive upwelling seasons, diatom abundances achieved maximum levels (up to 7629 cel mL-1)" only partially resemble results. The highest maximum is a unique event for the entire trap recorded period (most of values of total diatom flux are below 10*10(6) m-2 d-1).

Response: The text on line 18 only refers to water column diatom abundances, not to the total diatom flux record.

P. 6, l. 24: there is own BSi value higher than 10% (Fig. 5b), revise.

Response: BioSi % is indeed higher than 10% by some decimal points, but values were all rounded to the unit.

P. 6, l. 25: I do not quite understand how the percentage of silicoflagellates was calculated.

Response: Percentage of silicoflagellates was calculated in relation to the total of siliceous microorganisms. This information is included in figure caption 5.

P. 6, l. 27 and p. 7, l. 1: This statement "Maximum total diatom fluxes (up to up to 22.6 106 # m-2 d-1) were registered under downwelling conditions when benthic and freshwater diatoms fluxes became relevant" is wrong. Highest diatom fluxes (in the traps) do not temporally match highest relative contribution of benthic and freshwater diatoms. A gap of months is seen (compare Fig 5e and Fig 6b and d).

Response: The authors are comparing fluxes therefore you should compare Fig 5e to Figs 6a and c. Total diatoms and spores showed a major flux peak at about one month delay from the beginning of the period and a second smaller peak flux towards the end of the downwelling

period. Both freshwater and benthic diatoms show the same two peaks with similar fluxes. Anyway, what the sentence labelled as wrong by the reviewer want to highlight is that "all three groups show maximum fluxes during downwelling periods". Furthermore, even in terms of relative percent contribution to the diatom assemblage, the higher contribution of both the freshwater and the benthic groups occurs during downwelling periods.

P. 7, l. 3 and 4: what do the authors intend to say here? This sentence is difficult to understand. Rephrase.

Response: Sentence has been modified in the new version of the manuscript.

P. 7, l. 7: how these "mean seasonal values" were calculated? A table with the sampling intervals should be presented.

Response: Sampling intervals are those from the figures. The authors considered not necessary to show sampling intervals in this manuscript since they were presented in detail in Zúñiga et al. (2016) and they do not provide additional information to the manuscript. Mean seasonal values have been calculated considering all samples recovered during upwelling periods and all samples recovered during downwelling periods.

P. 8, l. 2: "The siliceous microorganism fluxes, mostly represented by diatoms", what about radiolarians? Low abundance? No present at all? Same line: it is not correct to state that "diatoms were strongly linked to biogenic silica fluxes and presented abrupt changes along the entire time series". In addition to the wrong grammar, it is usually interpreted the other way round: BSi fluxes are delivered by siliceous primary and secondary producers.

Response: Radiolarians are not abundant in shallow coastal waters and they were not observed in the trap samples nor in the surface sediment samples.

P. 8, l. l. 13: at which depth/s occur benthic diatoms?

Response: Benthic diatoms as photosynthetic organism can occur in the sediments down to the depth of the photic layer, as such it varies from region to region.

P. 8, l. 17: where does the information on "lower salinity water lens at the sea surface" come from? Own observations? Fig 3a shows temperature data, no salinity measurements though.

Response: Salinity data is not presented in this figure and referred to Zúñiga et al. (2016). The sentence has been modified in the new version of the manuscript.

P. 8, l. 24-30: this part of the Discussion section is convoluted and difficult to follow. It needs strong rephrasing. If, Paralia contributed to the 2010 downwelling interval and to the latest part of the 2011 downwelling intervals. Other than that, the lack of continuous records makes –at the least- this kind of generalizations. Although it is common in the two surface sediment studied, it relative contribution to the diatom preserved assemblage is always lower than that in the traps. Therefore, I do not understand why the authors state that "(Paralia sulcata) gets enriched in the sediments" (l. 27). Their interpretation is just erroneous.

Response: Enriched in the sediments relatively to their water column abundance. Their presence in the traps only occurs when resuspension of bottom sediments occurred. *Paralia* is well known as a resistant planktonic species which abundance has been found to increase in the sediments comparatively to their occurrence in the water column in trap studies off NW Africa, California, Central Chile and others. Anyway, the sentence has been rewritten in order to clarify.

P. 9, l. 5: which kind of highest abundances, relative or absolute?

Response: Absolute

P. 9, l. 7: writing "upwelling productive seasons" suggest that "upwelling non-productive season" occurred.

Response: The authors do not agree with reviewer #2. In this context, the term "upwelling productive season" is only used to provide the reader with regards upwelling periods, specifying that upwelling seasons are typically highly productive. Indeed, this term is frequently used in works published in the study area.

P. 9, l. 8-9: I do not understand where this statement comes from: : : it has been introduced in p. 6, l. 20, without referring to own observations or quoting previous work. Are the listed species typical of the coastal upwelling system along the Galician coast? Or are these species typical of any coastal upwelling system?

Response: This statement refers to our own observations. Indeed, the authors make reference to Table 3 where the data is presented, for easier comparison. The authors would also like to

underline that even most species are common to coastal upwelling systems, the listed species are known to be typical of the NW Iberian peninsula coastal upwelling system.

P. 9, l. 12: reference/s for ballast effect is needed here.

Response: Done

P. 9, l. 14: "Highly resistant" to what? I suppose to dissolution:

Response: Sentence has been modified in order to be more precise.

P. 9, l. 15-16: it is true that spores of Chaetoceros had high relative abundance during upwelling intervals. However, they do also contribute to the diatom assemblage during downwelling 2010. How can the authors explain this?

Response: By winter resuspension of bottom sediments containing what is delivered to them during the summer

P. 9, l. 23: how deep is the photic zone at the trap site? See below, same page, l. 26.

Response: The sediment trap was deployed at the base of the photic zone, located on average at 35 m water depth. This information was corroborated making use of a Secchi disk during monthly cruises.

P. 9, l. 23: "mostly associated to spring-summer periods (Fig. 8), revealing the onset of the upwelling seasons", more accurateness is needed here. How can the authors associate spring-summer periods (six months?) with the onset of the upwelling season?

Response: From an oceanographic point of view, different phases during the upwelling season may be observed in the NW Iberian margin. There is a first phase that mainly occurred between late spring and early summer, characterized by the persistence of upwelling favourable winds, and a second phase, associated to late summer-early autumn when a series of upwelling relaxation cycles occurred. During this second phase, when winds relaxation are sustained in time favour water column stratification. Sentence has been modified in the new version of the manuscript to avoid confusion.

P. 9, l. 27-29: how can “highly productive upwelling periods (as shown by Chl a levels)” correspond to “relaxation of wind promoting water column stratification”? This is fully contradictory!

Response: Please see response above.

Comments on Figures
The dots representing data are too large. It is almost distinguishing sample resolution and actual values (e.g., Fig. 7b, 2012). Revise.

Response: The authors agree with reviewer 2. However, even they agree with the fact that some of the data remain hidden due to values for the different species are close to zero, after several test they decided not to modify the figure. They consider reduction of data size difficult graphics reading. Indeed, even they are aware of this detail in Figure 7b they consider data used for the discussion section are clearly shown.

Figure 2b-d: these data were originally published in Zuniga et al., 2016, CSR.

Response: Even these data has been previously published in Zúñiga et al. (2016), the authors have decided to show them because they are completely necessary to discern between downwelling and upwelling main oceanographic features.

Technical corrections
P. 6, l. 2, bracket missing.

Response: Done