Responses to reviewer #1

The review comments on the manuscript entitled "Diatoms as paleoproductivity proxy in the NW Iberian coastal upwelling system (NE Atlantic)" by Zúñiga D, Santos C, Froján M, Salgueiro E, Rufino, MM, De la Granda F, Figueiras FG, Castro CG, Abrantes F for possible publication in Biogeosciences.

Major comment

The objective of this paper is "How diatoms species determine primary production signal?" (quotation from Abstract), "to evaluate the use of marine diatoms as a paleoproductivity proxy for NW Iberian coastal upwelling system" (from Introduction). I understood the importance of resting spores and Paralia sulcata in diatom flora from sediment trap and sediment samples for paleoceanographic studies around the study area. It is meaningful to try to find the relationship among living diatom flora (biocoenosis), sinking flora obtained by sediment trap, and fossil assemblage in sediment (thanatocoenosis). The research results on diatom assemblage shows strong influence of lateral material advection and taphonomic bias due to seasonal downwelling event. As far as I saw the figures in this manuscript, it looks like that there is interannual variation in downwelling period length and intensity of rive discharge which probably reflects climate condition. The deciphering relationship between these events and diatom remains in sediment is another topic for further application of diatom fossils as a paleoenvironmental proxy. On the conclusion as the answer to objectives of this paper, it is a little bit uncertain for me to understand which of valve contents (valve number/dry g sediment) or % in total diatom valves is better to estimate paleo primary production signal. I feel that there are several points to be considered and/or corrected before the publication. My specific and editorial comments are as follows. Because I'm not a native English speaker, I did not care for the English style in this manuscript. I hope some of these comments are helpful to revise this manuscript.

Response: They agree with the idea that using diatoms fluxes as a paleoenvironmental proxy to infer downwelling conditions interannual variability is an interesting topic. However, they considered the need to focus the attention of the reader in the goal of the manuscript, the use of diatoms to infer the productivity in the past. On the other hand and in agreement with reviewer #1, they decided to better justify which is the best paleoproductivity production signal along the Iberian margin. To do that the authors modified some specific parts of the text. In this specific region, even the best diatom production signal is given by the total flux (valves/g), this regional calibration revealed that the export signal in terms of "absolute abundances" is distorted by strong resuspension processes during autumn-winter downwelling periods. This indicates that mean annual fluxes cannot be used, but only the upwelling period fluxes. In addition, this study states that the dominant species exported out from surface layer during upwelling productive seasons reflected the most prominent diatoms in the water column. All this means that although it is not possible to quantitatively determine the water column/sediment record preservation, the sediment diatom assemblages can be used to infer highly productive periods in this coastal upwelling system. Otherwise, the identification of the water column environmental variables favourable for *Chaetoceros* and *Leptocylindrus* blooms (the two dominant species preserved in the sediment record through the formation of resting spores) provides relevant information for past environmental conditions.

Specific and editorial comments

2. Regional setting

p. 3, l. 11 "important source of terrestrial sediments to the inner shelf"

In addition to lithogenic material advection by river discharge, are there any significant influences of river discharge to nutrients for primary production and abundance of particulate organic matters in study area?

Response: In Zúñiga et al. (2016) the authors has exhaustively described water column and sediment trap biogeochemical data, showing among other things how intense river inputs during autumn-winter left their imprint as nitrate rich water lens at the sea surface. Even so, the authors consider that this additional input of nutrients during autumn-winter has no influence on primary production. Indeed, no relationship between nutrients associated to river inputs and Chl *a* concentration has been observed. On the contrary, Chl *a* contents were clearly related with the upwelling of cold and nutrients rich subsurface Eastern North Atlantic Central Water (ENACW) on the shelf during spring-summer periods.

The authors would also like to clarify that the term "terrestrial sediments" also include organic matter. This aspect has been also explained in detailed in Zúñiga et al. (2016) where the authors stated how "river discharges constitute an additional source of lithogenic and terrestrial organic matter to the sediment trap, resulting from the bulk of sediment washed out from the rivers onto the continental shelf". This information has not been included in the present manuscript to avoid duplication of already published information.

3.2. Water Column

p. 4, l. 9 "For diatoms counting … collected as 5m water depth" Why water samples for diatom analysis were taken from 5 m depth? I'm not sure the water samples from 5m depth can be treated as a representative of diatom biocoenosis in water column. For example, would authors show vertical distribution of chlorophyll-a concentration during the monthly ship-board observation?

Response: Vertical chloropyll *a* profiles were defined for the entire trap sampling period. This data has been previously published in Zúñiga et al., 2016 and for that reason the authors have decided not to present it here. Even so, a carefully data observation showed that seasonal Chl *a* variations were principally remarked at the topmost samples, with maximum values always registered at 5 m water depth (except July 2009). For this reason surface samples were selected as the most representative of diatom biocoenosis in the water column. In any case, the authors have included additional information in the methods section to clarify this important aspect.

p. 4, l.12 “microorganisms were counted and identified”
If resuspended dead specimens are included in the assemblage data from water samples for assemblage comparison with sediment trap and sediment samples, please mention it.

Response: The samples recovered at 5 m water depth did not include “dead” specimens, but only “live cells” preserved with Lugol´s iodine solution.

3.3. Sediment trap samples
p. 4, l.16 “from March 2009 to June 2012” As far as I see Figure 5, it looks like there were no deployment periods. If there is a blank of sampling period, please mention it in text or figure captions.

Response: Following reviewer #1´s suggestion the authors have included additional information in methods section.

p. 4, l.16 “The trap was deployed at 35 m”
I hope that the subsurface chlorophyll maximum was locating shallower than 35m throughout the sediment trap deployment period.

Response: With the exception of June 2011 the subsurface chlorophyll maximum was always very shallow, well above the 35 m water depth. For further information please see Zúñiga et al. (2016).

p. 4, l.19 “Only in exceptional cases… mooring tilts between 15-20°.”
When this event was observed?

Response: Mooring tilts of 15-20º were only observed when current velocities were higher than 25 cm s-1. Here, in this table (published in Zúñiga et al. (2016) it is possible to check how this situation only occurred sporadically.

	Days	Average cm s-1	Maximum speed cm s-1	Speed < 20 (%)	Tilt < 5º (%)	Tilt < 10º (%)
Upwelling	198	10.5 (6.8)	41.0	94	76	92
Poleward	183	13.3 (10.6)	62.7	85	57	82

Mixing	63	15.4 (10.6)	66.7	76	87	98

p. 4, l. 32-p. 5, l.3 "Diatom flux was calculated as …where the flux F is expressed as number of valves m-2 d-1, …" How did authors count resting spores in sediment trap samples? The unit of resting spore flux in Fig. 7e is "number of spores m-2 d-1". In the result section (p.6, l. 27), Table 3, and Fig. 5e, total diatom flux including resting spores is expressed as "# m-2 d-1" which is different from the diatom flux unit explained here. The resting spore of Chaetoceros species listed in this study and Leptocylindrus danicus is composed of two valves. If total diatom flux including resting spore is expressed as the sum of vegetative cell "valve numbers" and "spore numbers" (not spore valve numbers), I think that the contribution of resting spore to total diatom flux will be underestimated and relative abundance of vegetative cells will be overestimated. If this is just a problem in expressive style in this manuscript, please revise the unit of spore abundance to avoid confusion. If resting spore contribution to total diatoms flux was actually underestimated in the dataset, re-analysis of CCA, replotting figures, data correction in tables 2 and 3 will be required.

Response: *Chaetoceros* and *Leptocylindrus* spores are counted as valves (not spores). In order to clarify this aspect units expressed as # m^{-2} d^{-1} have been modified to valves m^{-2} d^{-1} in figures 5, 6 and 7. In addition, clarifications in both figure caption 5 and Table 3 have been made.

p. 5, l.9 "a percentage higher than 2% of total abundance were considered for further analysis." Which species were applied to diatom analysis? please note it in appendix A table for encountered diatom taxa (for example, using bold font of "X").

Response: Species used for sediment trap analysis were those species that appeared in more than one sample with a percentage higher than 2%. As suggested by reviewer #1 these species have been highlighted in bold in Appendix A.

p. 5, l.11 "42°10'N N,"
Correct the duplication of "N"
Response: Done

p. 5, l.19 "main groups of diatoms (freshwater diatoms, benthic diatoms, …)"
Is the category of benthic diatoms containing freshwater benthic and terrestrial species? If possible, would authors show which species are categorized into which group in Appendix A table, please?

Response: Main diatom ecological groups are shown in new version of Appendix A. The following ecological preferences were included: benthic, meroplanktonic, planktonic, coastal, open ocean, cosmopolitan, marine, marine to brackish, brackish to freshwater, brackish and freshwater.

3 4.1. Environmental conditions
p. 6, l.9-11 "Hydrographically, in a first phase … Later on, we differentiate the mixing period…"Would authors show the first phase and later mixing period in the result figures?

Response: The authors decided not to show hydrographic periods in the present manuscript since the objective of the paper is the comparison between upwelling and downwelling periods, as highlighted with the white and grey bars, respectively.

p. 6, l. 14 "(~14 cel mL-1)"cells?

Response: This is correct. It is referred to diatom cells. Modifications have been made in both figure 3 and 4.

p. 6, l. 14 "Small centric cells" What is the definition of small cell size? The aim of this paper is to evaluate the marine diatom availability as a paleoproductivity while there is no discussion on the relationship between diatom abundance in cell volume (or carbon content) of each species and chlorophyll concentration (or primary production) throughout the manuscript. I see the high numerical dominance of Chaetoceros and Leptocylindrus in cell number in this study, and I agree their significant contribution among diatom species to primary production. However, some documentation on diatom contribution to primary production may be required in view point of cell volume or particulate carbon content for each species in studied samples. Because BioSi may not be primal component in sinking particles (Fig. 5b) and no information on other component in sinking particles, readers will not be sure that diatom is the most important contributor to primary production in the study area.

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.
and Table 3

The authors really agree with reviewer #1 in the sense that most important aspects to be resolved when analysing diatoms´ export and accumulation in sediment records is the fact to estimate how much of the particulate organic carbon attributed to primary production is susceptible to be transported to deep waters through these tiny organisms. In this frame, even long-term sediment traps are extremely important to analyze seasonal diatom-derived organic carbon export rates to the deep, they do not provide us such a quantitative analysis since diatoms preserved in sediment trap samples are only represented by their valves, not by their cells. In this context and considering that water column diatoms are counted as "living cells" but sediment trap diatoms are numbered as "dead valves" the authors considered that evaluation of diatoms as a paleoproductivity proxy by analysing long-term sediment trap samples may only be carried out in terms of abundance.

The authors would also like to remark that they do not consider diatoms as the most important contributor to primary production in this margin. Indeed, there are some studies in the region that show how other phytoplankton groups are also important contributors to primary production. One good example is the manuscript from Espinoza-Gonzalez et al. (2012) entitled "Autotrophic and heterotrophic microbial plankton biomass in the NW Iberian upwelling: seasonal assessment of metabolic balance". Even so, the authors decide to evaluate the use of diatoms as a paleoproductivity proxy due to the capacity of their silica frustules to be preserved in the sediment record. They are frequently used in this margin to estimate productivity in the past but no attempt had yet been made to evaluate their abundance and assemblage composition in the photic zone and its transfer to the sediments up to this study.

The authors are aware that they have not estimated primary production and organic carbon export via diatoms, but comply with the objective by providing relevant information on how environmental conditions promote different diatom abundances and determine the blooming species as well as their preservation potential in the surface sediments.

p. 7, l. 10 "Diatom abundance in GeoB 11002-1 … higher than in the offshore GeoB 11003-2" What is main reason on the difference of diatom valve contents in surface sediment at two sites? On the cell contents and relative abundance of proxy species in sediment samples, which is better to use for paleoproduction proxy?

Response: Higher diatom abundances in the surface sediment at GeoB 11002-1 onshore station responded to primary production signal in the photic layer. Even not presented here the authors have evaluated Chl *a* contents through a transect perpendicular to the coast during almost the entire sampling period. They observed how seasonal Chl *a* at the surface productive layer is intensified close to the coast, in agreement with diatom´s abundance in surface sediment samples. This confirms the use of valves/g as a good indicator of diatom´s production in the photic zone. In that sense, since onshore surface sediment sample is closest to the RAIA station the authors consider it better to reflect the conditions at this site and will be the only one used in the new version of the manuscript.

p. 7, l. 11 “# valves gr-1” The unit should be revised as # valves g-1 based on International System of Units.

Response: Corrections have been made in the new version of the manuscript.

5. Discussion
p. 8, l. 9-10 “diatom contribution … achieved maximum percentage”
maximum percentage to what? To total biogenic opal flux or particulate organic carbon? Comment: In general, coastal sediment sample except for varve sediment will be treated as an accumulation of settling particles for several-decadal years. In the case of studied sediments, diatom assemblage should mainly reflect the downwelling season because much of settling diatoms were supplied in downwelling season rather than upwelling season.

Response: Following previous reviewer #1´s comments the whole paragraph has been modified.

5.1. Sediment trap diatom assemblage as a tracer of allocthonous sources in sinking material
Comment: Are there any relationships between lithogenic particle fluxes and occurrence patterns of freshwater and benthic diatom taxa if they are treated as a kind of proxies for sediment resuspension and lateral material advection? Can authors mention on time-series fluctuation of lithogenic particle flux in this paper?

Response: The authors agree that a comparison of the diatom assemblage and the lithogenic particle flux would help to justify it as a tracer of allocthonous sources. Thus, part of the text has been modified in the new version of the manuscript.

5.2. Seasonal succession of diatom species during upwelling seasons: the imprint over the fossil diatom assemblage Comment: As the answer to the objectives of this paper, it is a little bit uncertain for me which of valve contents and % of resting spores is better to use for the paleo productivity proxy? More additional and detail explanation may be required as my impression.

Response: After reading with attention the whole manuscript the authors agree with reviewer #1 that more explanation is required in relation with which diatom proxy needs to be used in order to infer the imprint of the water column diatom assemblage over the fossil diatom record. As explained in detail above, modifications in both abstract and discussion section have been made in the new version of the manuscript in order to clarify this fundamental aspect.

References

The style should be corrected to the format of Biogeosciences.

Response: Done

Table 1. "Main relationships" How did authors define the main relationships? What is representing the data value in bold with darker gray background?

Response: After carefully reading the paper the authors have decided to eliminate both the grey background and data values in bold. They consider the use of this format does not provide of relevant information for data discussion.

Table 2. "Pearson correlations" It is unclear whether this Pearson correlation coefficient was calculated on diatom valve flux or relative abundance of each taxon in total diatom flux. "Main relationships" How did authors define the main relationships? $p<0.05$?

Response: New information related with Pearson correlation coefficients has been included in the new version of the manuscript.

With regards main relationships and p-values the authors would like to state that the main suspicious relationships on a preliminary analysis correspond to variables with absolute correlation coefficient higher than 50%. Still, the selection procedure on this type of analysis is not mathematical, but the result of an interactive procedure to reach a set of variables with highest correlations among them and most representative of all sampled variables. The ultimate instance to test if this set is proper for further analysis is the variance inflation factor, as it is explained in the text.

Table 3.
What is the difference of 0 and blank in diatom flux data?

Response: Corrected.

Fig. 4(a)
There is small arrow symbol located near the maximum flux of Small centric species. What does this arrow mean?

Response: The arrow with the corresponding number was used to show small cells abundance in July 2010 because it was out of scale. Additional information was added to the figure caption.

Fig. 4(e, f)
Are these plots including the data on resting spore of Leptocylindrus and Chaetoceros?

Response: For the water column samples, *Leptocylindrus* and *Chaetoceros* spp. data do not include resting spores since they are difficult to observe. *Leptocylindrus* and *Chaetoceros* spp. are only referred to live cells as shown in figure caption 4.

Fig. 5. The downwelling period in 2010-2011 is differ from Fig. 2. Please check and fix it.

Response: All figures have been exhaustively checked.

Fig. 5(c) The flux unit is a little bit uncertain. For example, "individuals m-2 d-1" may be better (in this case, diatom flux must be treated as cell number rather than valve number).

Response: Modified. Check comment immediately above.

Fig. 6
The downwelling period in 2010-2011 is differ from Fig. 2. Please check and fix it.

Response: Done

Fig. 6(a) "Benthic" Is this mean that benthic form of both marine-brackish water and freshwater species are included in this data set?

Response: No. In this particular case, fresh water benthic species were included in the fresh water group and the marine benthic were considered as benthic. As explained above, additional ecological information was included in new Appendix A.

Fig. 7(a, b, e)
On the flux spike over the upper limit of vertical axis, I cannot find which species made the high flux.

Response: The figure has been modified in the new version of the manuscript by adding coloured circles to the numbers.