

Interactive comment on “Diatoms as paleoproductivity proxy in the NW Iberian coastal upwelling system (NE Atlantic)” by Diana Zúñiga et al.

Anonymous Referee #2

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Zúñiga 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

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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/statements, 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.

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?

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.

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

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.

P. 4, l. 16: the RAIA was not “monitored”, but waters overlying the station? Below in l. 17, was the trap deployed or within the photic zone and the mixed layer?

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

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

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

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.

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

P. 16, 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.

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

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temperature?

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).

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

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

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).

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

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

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.

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

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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.

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, its 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.

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

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

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?

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

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

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?

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

P. 9, l. 23: “mostly associated to spring-summer periods (Fig. 8), revealing the onset of

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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?

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

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.

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

Technical corrections

P. 6, l. 2, bracket missing.

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