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Interactive comment on "Reconstructing past variations in environmental conditions and paleoproductivity over the last ~8000 years off Central Chile (30° S)" *by* Práxedes Muñoz et al.

Anonymous Referee #1

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The paper presents a multiproxy analysis of two short coastal sediments cores collected off Coquimbo, Chile, with the aim to document paleoclimate and paleoceanographic variability during the Holocene. The data presented is original and valuable to understand the millennial dynamics of the South East Pacific coastal upwelling. Authors analyzed a broad range of geochemical and microfossil indicators which should lead to a robust interpretation. However, substantial work is still needed on the manuscript before being published. I have a few methodological concerns with the chronology and with the way metal concentrations are used, that need to be addressed. The text also requires a lot of work. Except for method sections, the text in general lacks clarity, partly because of inappropriate word choices, and partly because of a lack of

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focus. The introduction needs to be rewritten since it does not present the context, the research motivation, or the objectives of the work. A proper paleoclimate discussion is missing. Almost no comparison with published results was made and none of the relevant literature on the regional paleoceanography or paleo ENSO is cited.

This study deserves to be published but the manuscript requires substantial revision. So far the article is essentially focused on sediment chemistry but lacks depth in the paleoceanographic interpretation and discussion which is the objective. I recommend a more active contribution of co-authors in writing the introduction, discussion and conclusions.

Detailed comments:

- The presentation of results in the abstract is unclear

- The introduction is a lengthy, disorganized list of unfocused information about upwellings in general and sediment proxies. It needs to be entirely rewritten to present the context, the motivation of the research, the scientific questions, the objectives and the scientific strategies chosen to achieve them.

- L132-139: this paragraph on pigments seems unnecessary

- L145: the words “relevance” and “relevant” are repeatedly used in an inappropriate way throughout the manuscript.

- L167-172: unprecise

- L176-178: the fact that two sediments cores were analyzed and their location should be mentioned in the introduction

- Trace metal concentrations:

The normalization of Me concentrations using Al does not seem justified to me. The analytic technique used here (ICPMS analyses of dissolved samples) yields quantitative and absolute concentration values thanks to the standards used. As far as I know,

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uncertainties related to machine variability and matrix effects are not an issue with this technique as it would be with laser ablation technique. In addition, Al does not have a conservative behavior as mentioned: figure 10 shows on the contrary a substantial increase of Al concentration through the Holocene. Normalizing systematically with this element may actually produce biased interpretations.

I recommend to use the accumulation rate from the age model and absolute Me concentration to calculate metal fluxes to the sediment.

Since Al has mainly a continental origin, ratios with Al is informative for elements whose flux is related to productivity to discuss relative contribution of marine vs terrestrial contributions in the sediment.

Finally, the usefulness of the enrichment factors is not obvious. Figure 9 is barely discussed. In addition, I wonder if wetland sediments are really representative of crustal metal concentrations since they also contain organic matter.

- Geochronology

L248: Calpal2007_HULU calibration curve is an odd choice for radiocarbon calibration. It is also inconsistent with L255 in which Marine13 is mentioned (which is the correct calibration curve to use). There is a couple of issues with the regional radiocarbon reservoir age used for calibration. First, the method to calculate it is not correct. 14C reservoir age should be calculated in the 14C age scale, not in the calendar scale as it was done here. dR is the difference between the marine sample 14C age and the 14C age that corresponds to the absolute age (here obtained from the 210Pb model) using the Marine13 curve. See Southon et al. (1995) for details on the technique. The dR value obtained here is larger than any dR values obtained previously on the Chilean coast Authors should read and use Ortlieb et al., 2011; Carré et al., 2016; and Merino-Campos et al., 2018. The latter reference presents 37 prebomb dR values all along the Chilean coast measured with a reliable technique. Using a value from this publication would be more reliable. The first 2 references show changes in dR values through the

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Holocene that should also be discussed. Finally, instead of BC/AD, ages should all be presented in the BP scale as it is usual in paleoceanography for Holocene studies.

- Discussion:

L505-L514: unclear

L521-L536: the discussion about d13C values is unclear, in part because there seem to be a confusion between Total organic carbon(TOC) in the water column and suspended particulate organic Matter (SPM). Is it possible that the difference between d13C values in the water and in the sediment are due to the difference between TOC and SPM? A preferential degradation of 13C enriched particles is mentioned (L528-529): could you support this with a reference?

L563-L568: the discussion about K is not very convincing. A reference about the detritic origin of K is needed. Ca could also have a detritic origin so close to the shoreline. Al, Fe are also clear terrestrial input indicators. Why not discuss them together?

L602-L606: references needed

Section 5.3 should be shortened. It is somewhat redundant with other discussion sections and the result section.

- Climatic interpretations

This section lacks in-depth discussion. The results here should be compared to published results to understand how they contribute, support or contradict existing hypothesis about millenial oceanographic variability in Chile.

L720-L723: “past changes are analogue with the present meridional displacement of the ITCZ and the SPCH”. This should not be taken as a fact. It is only a hypothesis used as an interpretation model.

L744-L747: this part is unclear and sounds contradictory (a poleward shift of SWW

should not promote humid conditions in central Chile). In addition, this is a model result. Why not compare with existing paleoenvironmental and paleoceanographic data?

There is a series of sediment cores that document past oceanographic conditions in the Peru-Chile upwelling system during the Holocene. This includes Lamy et al (1999, 2001, 2002, 2010), Kim et al. (2002), Hebbeln et al. (2002), Rein et al. (2005), Salvatecci et al. (2014, 2016). On a regional scale, the data presented here confirm a La Niña-like situation in the early to mid-Holocene, which is in agreement with previous datasets including Koutavas et al. (2002), Fontugne et al. (2004), Conroy et al. (2008); Carré et al. (2012), and model experiments such as Brown et al. (2008); Braconnot et al. (2012), Luan et al. (2015). This list is clearly not exhaustive.

The influence of ENSO variability needs obviously to be discussed. It is here briefly mentioned in the text, appears in the key words, but there is no discussion. Data on past ENSO activity do exist (Koutavas et al., 2006; Cobb et al., 2013; Carré et al., 2014) and they need to be included in the discussion if the role of ENSO in the presented data is to be evaluated.

Figure 2: what about st14? Font on Y scale too small Figure 3: SPM is not the same as TOC Figure 5: it is not clear which curve is grain size and which is susceptibility Figure 6: Al and Fe are both related to terrestrial input. What information ndoes Fe/Al provide? Figure 9: This figure is not commented in the text. EF calculation does not seem useful.

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