

Interactive comment on “Variability in terrigenous sediment supply offshore of the Rio de la Plata (Uruguay) recording the continental climatic history over the past 1200 years” by L. Perez et al.

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RESPONSE LETTER TO REVIEWER 3

General comments 1. The paper fails in analyzing and reconstructs the sedimentary processes involved. The clarity of the reasoning is therefore not straightforward. Actually, I think that the South American Monsoon and SACZ have a major influence rather than the ITCZ.

The manuscript has been re-discussed and re-interpreted in order to include the suggestions from all reviewers concerning the influence of SAMS and SACZ. Reviewers will be able to appreciate the changes in the updated version. All additions and

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changes have been highlighted.

2. The authors have an interesting high resolution record, why do they not try to explore for each period studied (MCA, LIA and CWP) variabilities (Interannual, decadal, multi-decadal and centennial) triggering changes in hydrological changes using statistical methods (spectral analysis, wavelet) such as shown by Novello et al., 2014, Apaestegui et al., 2011.

The authors agree with this comment. Our record is indeed an outstanding potential climatic archive. However, this study is the first approach to the hydrological changes on the inner Uruguayan continental shelf related to the climatic changes in the catchment area over the past 1,200 years. The intention then, is a first step towards evaluating the main trends and changes during this time period. Inter-annual variability might be beyond the capacity of this record, but decadal-scale variations could probably be resolved. Thus the following step after incorporating new data was added (e.g., pollen distribution, ^{210}Pb). We plan to perform statistics and statistical methods to all these data sets in cooperation with mathematicians.

3. Concerning the divergence between the diatoms and the major elements records during the CWP and the anthropogenic interpretations proposed by the authors, I think that this claim is exaggerated. Various points have to be clarified. Increase in Ti/Ca, Fe/ca ratios can also be due to a decrease in Ca content as a consequence of weaker productivity, and not only to an increase in sediment discharge or an increase of diatom frustules dissolution. Another point is that both markers (diatoms and major elements content) do not show the same variations. Despite the trend diatom assemblages mark also high variability than major element content (Ti, Fe, ca, etc) during the CWP For that reason I don't think that they are above the same environmental and climate pressure during this period (CWP).

Analyzing the contemporaneous record was not the goal of this manuscript. The antropogenic impact during the last century was discussed in Perez et al (in press) and nat-

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ural contemporary variations were discussed in Marrero et al. (submitted, accepted, resubmitted). We are willing to provide you with the pdf files upon request. For this reason the authors have removed the contemporaneous inference from both the abstract and the discussion.

4. Finally, why the authors do not explore the natural intensification of the South American Monsoon System during the last century. The authors explained the climate mechanisms that govern hydrological changes in this region but avoid to discuss the anthropogenic impact on fluvial and/or Aeolian transport. They also describe superficially the environmental significance of the ratios used in this study.

Same answer as item 3.

5. I suggest to the authors to justify the use of the each major element ratio because they can vary independently in relation to of regional geological and geomorphologic setting and also in local hydrodynamic regimes.

The authors have added two paragraphs to address this issue. See: “Paleo-environmental proxies” in “Materials and Methods section”.

Questions 1. Why do they use bivalves for dating instead of organic matter?

This question comes as a surprise because the dating of offshore organic matter has long been proven to provide ages relating more to their old (continental peat) sources/production areas than to the timing of the final deposition. In addition, organic matter is composed of humic acids and the durable lignin. Many studies clearly document that these two compounds result in rather different ages when measured separately. Thus, dating carbonates in offshore environments should always be the procedures to follow.

2. How can they assume that they are in life positions and which confidence do they have in their dating?

These bivalve shells were often found in double-valved preservation, i.e., entire and

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articulated material. This preservation type is only possible if the organic tissue of the bivalve is still intact when it is buried. This is what we classically call "in life-position". Furthermore, many studies have also shown that well-preserved single-shell valves can be used for dating purposes in such environments, without illustrating a reworking history. In addition, the succession of age points in our core is quite consistent. Thus, we have no reason for not trusting our age model.

3. The authors should discuss the ^{14}C reservoir effect, which is important for the calibration of ^{14}C ages.

This point is correct but provocative. Anybody who works with offshore radiocarbon dating is fully aware that the reservoir effect is an important issue. At the same time, this effect is difficult to determine on local scale. The shallower the water is, the better mixed it is, so the exchange with the atmosphere is more direct. Thus, the conservative value of 405 years is probably an overestimate. However owing to lack of local data the general practice has been to apply this conservative value in an acceptable way. A better solution cannot be found and we trust that the reviewer is aware of this.

4. For the CWP period, ^{210}Pb analyses of the top of the marine core are really needed if they pretend to infer changes during this recent period.

Same answer as item 3.

5. Which confidence do the authors have in the XRF analysis?

Since we do not use the XRF ratio data for a quantitative estimate but rather for documenting general trends and variability, we do, indeed, follow the common practice. As long as we clearly indicate that these ratios are proxies, and not used with the intention of coming up with a full quantitative approach, we do not see why the quality of the data or the variations interpreted from the signals should be questioned. Besides, the XRF scanners at MARUM are among the most modern and sensitive equipment available globally. Therefore, if the data show variability, this is a natural environment-related

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signal.

6. It will be necessary that the authors add a figure of bulk results of the Ti, Ca, Fe independently. This will help to follow the variation of each element and will consolidate their interpretation based on the ratios as marker of increase in sediment discharge, or of the dissolution of calcium carbonate, etc.

This is an interesting comment. There are several technical articles clearly indicating that single-element plots may have an artificial component. This component results from the fact that the XRF scanner counts relative intensities, and in the case where one element dominates all other elements would be relatively suppressed. Thus, simple ratios or the logarithmic ratio between two elements are more reliable than single-element projections. With this in mind, there would be, from our perspective, no additional value by plotting Ti, Ca, and Fe individually. Nevertheless, if the editor wishes we would be willing to add these curves. Furthermore, the data will be stored in the world data bank, PANGAEA, which allows everybody in the community to play with the data.

7. The authors should estimate the total fluxes of inorganic sediment discharge by comparing the modern sediment discharge and the intensity of the hydrological processes and the SASM activities.

Since the mid-shelf mudbelt extends over more than 1000 km to the north, and is neither mapped nor sampled sufficiently, an offshore number in terms of material storage is simply unknown. Thus, a somewhat reliable budget calculation is, in our view, not a possibility at this stage. The reviewer is right in that such an estimate would be a great input, although it is not feasible and unrealistic with the existing data.

8. The authors make a simplified lithological description of the core. Slumps, turbidity and discontinuity in sedimentation are not discussed in the manuscript.

The muddy lithology of this sediment core is as monotonous and simple as the description shows. The comment by the reviewer focuses on different mechanisms of

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deposition. Coauthor Hanebuth is very familiar with the sedimentary expression of these processes. a) Slumps are common features on continental slopes since they require a certain seafloor gradient. We don't see any indication of laterally moved material in the paleo-valley, neither in the sediment cores nor in the 3.5 kHz acoustic lines. Even the valley walls seem to be very stable. b) The term "turbidity" is usually used for high matter concentration in the water column. Can we assume that the reviewers means "turbidite beds" in this context? If so, such event deposits are common on continental slopes, and in exceptional cases they appear on river delta fronts. But why should they be expected to be present on the inner shelf? In any case, their lithological expression is easily recognized - even for muddy turbidites - and there is no indication in the sediment core. c) From a sedimentological point of view, the recognition of short-lasting hiatuses is indeed not easy to detect, and they are probably often undetected in all fine-grained sediment cores from the shallows and deep ocean. In the case of the Uruguayan mudbelt, we would guess that the sedimentation rate is seasonally controlled and therefore might fluctuate. But with a temporal resolution of a bit less than 1 cm per year, a longer interruption in sedimentation is not expected.

Interactive comment on *Clim. Past Discuss.*, 11, 1343, 2015.

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