

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 1

a. My major concern with the manuscript is that the XRF ratios as proxies for continental versus marine input or of eolian versus fluvial input are not explained or critically evaluated and justified.

The authors have re-defined and explained concepts regarding the XRF ratios as suggested by Reviewer 1. In this sense, the authors have added two paragraphs to address this issue. Refer to the section, Paleo-environmental proxies within Materials and Methods (lines 195 to 217).

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b. For example Ti/Ca as a proxy for continental influx assumes Ca flux doesn't vary much – is this the case?

Assuming Ti is related to the terrigenous fraction and Ca to the marine fraction, they should behave in a reciprocal way. Thus, the Ca content should automatically vary with the amount of terrigenous material. We think that Ca flux would not have changed drastically over such a short time scale.

c. Fe/K is sometimes used as a proxy for continental weathering – why is it discussed in the Introduction as an indicator of eolian versus fluvial flux?

The authors agree with this comment, so we have deleted this assumption from text and added the following information: "On the other hand, the Fe/K and Ti/Al ratio was used in South America to reflect the degree of chemical weathering in areas without significant eolian input (Govin et al., 2012), such as in the case of the RdIP (Mahowald et al., 2006)."

d. I suggest that the authors expand the Introduction to explain the potential causes of variation in the XRF data in more detail (see detailed comments below), taking care to describe the mechanisms that affect the ratios from source to sink.

Same answer as item 1.

e. Also, as suggested in the comments below, some of the climate interpretations are not rigorously justified or explained.

The manuscript has been re-discussed and re-interpreted in order to include the suggestions from all reviewers concerning the climatic interpretation with special regard to the influence of SAMS and SACZ on the study area.

Finally, there are a few grammatical errors and awkward phrases;

1. The manuscript would benefit from editing by a native English speaker.

The manuscript has been corrected by a native English speaker Vivienne Pettman.

2. Page 1349, lines 5-10: Some more information should be given here about the rationale for using these ratios in this particular setting. These ratios are widely used in paleoceanographic studies, none-the-less the factors that affect concentrations of the individual elements may vary geographically dependent on regional geology, regional hydrogeomorphic setting, and local biogeochemical processes. So an additional paragraph to explain the basis for the individual ratios and that justifies the choice of these ratios in this particular basin is warranted.

Same answer as item 1.

3. Page 1350, lines 0-3: It's unclear what the relevance of this sentence is. An additional sentence should be added to specify that this approach was used also in this study to evaluate freshwater influx.

An additional sentence has been added specifying that the same approach as Romero et al. was used in the current study. Please refer to lines 236-237.

4. Page 1350, lines 21-25: You say the core was analyzed at high resolution from AD 1483-1534 but then you talk about peaks at AD 1373, 1407, 1470, etc. – I assume the 1483 value is a mistake? And why did you chose this interval for high-resolution analyses? This should be included in the methods section. Alternatively this section could be eliminated – you don't really discuss these higher resolution data in the text.

As suggested by the reviewers, this section has been effectively eliminated from the MS.

5. Page 1352, line 4: Given that the age-depth relationship is relatively straight, I don't see the evidence for "significant changes in sedimentation rate". To me this suggests the changes are relatively small.

The authors agree that the sedimentation rate changes were rather small, and assumed a constant sedimentation rate. We deleted this sentence.

6. Page 1352, lines 13-14: You say the elemental ratios indicate a mixed terrestrial-

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marine signal –what is it that suggests this? The information on the next page about the sources of Fe, Ti, and Ca should be included here to introduce the causes of variation.

The authors agree with the reviewer and have added the information on Fe, Ti and Ca. Refer to lines 284-289.

7. Page 1353, line 2-4: The wording here suggests that both the flux of terrestrial material decreased and the flux of marine material increased – it seems likely that it is only one or the other that is changing the ratio. Also, how do you know that diagenesis is not affecting the ratio?.

Here the authors argue that a reduced RdIP water/terrigenous sediment discharge during MCA may have led to a major and more constant sedimentation of marine particulated carbon, as recorded by Perez et al. (in press), by looking at the $\delta^{13}\text{C}$ and C/N ratios in the Lamb et al.(2006) model.

8. Page 1353, lines 4-11: In the introduction you indicated that Fe/K and Ti/Al were indicators of fluvial versus eolian inputs, yet all 4 of the ratios show the same general trends (low values prior to 1300 and higher values afterward), which suggests they are all indicators of the same thing (continental flux in the rivers). You end up saying this on the next page – but the discussion would flow more logically if a summary of the potential influences on the elements was described in the Introduction.

The authors have re-defined and explained the concepts regarding the XRF ratios as suggested by the Reviewer. In this case, the authors have added two paragraphs to address this issue. Please refer to the section, Paleo-environmental proxies within Materials and Methods (lines 195 to 217).

9. Page 1354, line 19: I think it is more appropriate to say that some studies have suggested or inferred that the LIA was characterized by stronger El Nino events – it is certainly not a fact. Also, given that strengthened SASM, changes in the westerlies, and changes in ENSO dynamics affect precipitation variation in this region – it needs

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to be clearly stated that any one of these things may have caused the changes in your sediment record – the text (and abstract) presently seems to suggest that all of these things changed.

The text has been modified as suggested by this reviewer. Refer to the section: Abstract, Discussion and Conclusion.

10. Page 1354, line 28: What is the basis for concluding that this variation is forced by strong El Nino conditions? This is not at all clear based on the information given.

The authors removed this part along with Figure 4 following the recommendation made by the Reviewers.

11. Page 1355, line 4: Again, make clear what evidence you are using to indicate higher river discharge.

The authors removed this part of the text.

12. Page 1355, line 6-15: These are not the only potential explanations – perhaps the diatoms have dissolved; perhaps increased nutrient concentrations have favored algal groups other than diatoms. I'm sure there are many others. I suggest that this paragraph (and the last sentence of the Conclusions) be a bit more circumspect about the causes of the observed patterns in the post 1850 sediments.

The dilemma of disappearing diatoms (Quaternary Science Reviews 28(1–2), 120–136, 2009) is usually observed under high carbonate content conditions (Ryves et al. (2006), "Physical and chemical predictors of diatom dissolution in freshwater and saline lake sediments in North America and West Greenland" (2006). Papers in the Earth and Atmospheric Sciences. Paper <http://digitalcommons.unl.edu/geosciencefacpub/5>). This is the case of, for example, South Africa or Mozambique (personal observation): Regarding the east coast of South America, where carbonate content is rather low (see reference Govin et al., 2012 within manuscript), diatoms are very well preserved in sediments.

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13. Figure 4: This is not really discussed in the text, other than to say that one section of the core was analyzed in higher resolution and shows peaks. The extra figure needs to be justified by more discussion of the data; otherwise I suggest deleting it.

Figure 4 has been removed.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/11/C1011/2015/cpd-11-C1011-2015-supplement.pdf>

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