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Climate of the Past Discussions

Interactive comment on "Terrigeneous material supply to the Peruvian central continental shelf (Pisco 14 S) during the last 1100 yr: paleoclimatic implications" *by* F. Brice no Zuluaga et al.

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We are very grateful for the quite thoughtful anonymous reviewer 2. Clearly, the reviewer spent much time and effort as put on the part of the reviewer and we appreciate his/her comments very much, which will support to greatly improve our manuscript.

General comments:

Remarks 1: The manuscript presented by Zuluaga et al. use grain size distribution of two laminated sediment cores collected off Peru to reconstruct terrigenous material supply to the Peruvian shelf over the last âĹij 1100 yr at high resolution. Although the manuscript falls within the scope of CP, to my knowledge, it does not add novel

information about the past climate of the area.

Response 1: We think that our paper provides a substantial contribution to scientific progress because we show new evidence for a mechanistic understanding of the large changes of terrigenous input (fluvial vs Aeolian transport) that occurred in the Eastern Tropical South Pacific during the last millennia. In fact, our data of grain-size distribution provides an accurate and specific proxy of fluvial input changes which confirms the previous findings by Rein et al. (2004), Sifeddine et al. (2008) and Gutierrez et al. (2009) using mineralogical proxies. Moreover, we present new evidence for the variability of wind intensity and then atmospheric circulation in a temporal scale. We also provide an accurate proxy of runoff linked to the ENSO-like variability. In the new version we will carefully revise the whole manuscript in order to separate the new contribution of this manuscript from the previously published works and highlight their importance.

Remarks 2: Additionally, the manuscript lacks description of the sediments (at least a short lithological summary for both cores; lamination throughout? in part?), collection sites and detailed composite chronology. Moreover, the interpretation of grain-size data seems to be oversimplified for a continental shelf area that is geologically not that simple. More information on the physical setting of coring sites and transport mechanisms of particles from the continent needs to be provided.

Response 2: We agree with the reviewer that we didn't include the full sedimentological characteristics of the cores used in this study. As it was discussed (with Referee 1) that these characteristics have been fully described in other papers such as Gutierrez et al., 2006; Sifeddine et al., 2008 and Salvatteci et al., 2014 for core B06. However, in the revised version we will add more information about the sedimentological characteristics as suggested by the reviewers. In the new version of the manuscript we will explain in more detail the rationale behind our method and explain in detail each aspect. Also, as it was discussed with the Referee 1, we can make a small overview of the most important oceanographic and sedimentary characteristics in finding a better understanding of the properties of the records.

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In the full review and interactive discussion, the referees and other interested members of the scientific community are asked to take into account all of the following aspects:

1. Does the paper address relevant scientific questions within the scope of CP? YES

2. Does the paper present novel concepts, ideas, tools, or data? NOT REALLY We agree with the reviewer that is better explain the scope of the paper and mention that in fact there are no works of the sedimentological dynamic input as proxy of the behavior of the atmospheric variability i.e. precipitation (runoff) and continental wind intensity in high resolution in the eastern of Peru. Besides, the proxy we used, disentangling the data showed the evolution of the climatology mechanism (ITCZ-SPSH) in response of the principal climate periods and its relationship with both wind intensity and fluvial input variability. Important mechanisms for understanding the atmospheric-ocean dynamic in this area are relevant for the Humboldt upwelling system dynamic and the relationship with the ENSO conditions. In the new version we will carefully revise the whole manuscript in order to underline the new contribution from the previously published works. For the first time the origin and the mechanism of transport of the terrigenous materials in the continental shelf are elucidated and deciphered as direct indicators of the behavior of the atmospheric variability i.e. precipitation (runoff) and continental wind intensity.

3. Are substantial conclusions reached? NOT REALLY

In the new version we will pay attention in guide better each one of the conclusions and its relevant importance. We think that our paper provides a substantial contribution to scientific progress of the paleoclimatology occurred in the Eastern Topical South Pacific during the last millennia.

4. Are the scientific methods and assumptions valid and clearly outlined? NOT REALLY

The methodology used in this work (the Flow Particle Image Analyzer technology) has several advantages: First, each of the particles can be seen in each sample, which

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assures the analysis of mineral particles only, assessing the chemical attack efficiency and offering the possibility of elimination of the non-lithological particles. Images may be used to identify the origin of particles by their optical characteristics such as the case of black carbon (Flores-aqueveque et al., 2014). Moreover, if one ignores the images, this method provides grain-size information comparable to that obtained with a classic laser granulometer methodology. Finally, in the aim to indicate the different sources and/or deposition processes, several works use the deconvoluted gran size with success as Gomes et al., 1990; Prins and Weltje, 2012; Stuut et al., 2002, 2007; Weltje and Prins, 2003.

5. Are the results sufficient to support the interpretations and conclusions? It is not a self-sustaining paper

Previous sedimentological studies in the upwelling area as Böning and Brumsack, 2004; Gutiérrez et al., 2006, 2008, 2011; Reinhardt et al., 2002; Salvatteci et al., 2012, 2013; Sifeddine et al., 2008; Suess et al., 1987 and others, showed that these markers can be used to reconstruct paleoceanographic/paleoclimatological variability. This area presents laminated sediments material without bioturbation and low currents erosion action (Gutiérrez et al., 2006; Reinhardt et al., 2002; Suess et al., 1987) which represents an area suitable for paleoenvironmental studies. Based on these studies, we applied a new grain-size proxy used as a marker of terrigeneous input as used by Flores-Aqueveque (2015). The grain-size distribution is an accurate proxy to reconstruct aeolian transport as used in (Mulitza et al., 2010; Prins and Weltje, 2012; Stuut et al., 2002, 2007; Sun et al., 2002; Weltje and Prins, 2003).We will include more information about the study area, the cores, and the composite record to have a self-sustaining paper. We agree with the reviewer in the fact of including sedimentological characteristics section of the cores used in this study although this has been done in other papers (i.e. Gutierrez et al., 2006; Sifeddine et al., 2008 and Salvatteci et al., 2014).

6. Is the description of experiments and calculations sufficiently complete and precise

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to allow their reproduction by fellow scientists (traceability of results)? NOT COMPLETELY; chronology not given; full range of grain-size not given.

These two cores have independent chronologies based on several 210Pb and 241Am data points, several 14C ages, and the identification of sedimentological structures by X-ray images. Although this data was published in other works, we will show the chronological models in the supplementary material. On the other hand, we must apologize for forgetting to mention that particles coarser than 200 μm were never found in any samples. In fact particles >130 μm were very rare to find. As consequence our data, represent the full range of the grain size.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES

8. Does the title clearly reflect the contents of the paper? YES

9. Does the abstract provide a concise and complete summary? YES

10. Is the overall presentation well structured and clear? YES

11. Is the language fluent and precise? NEEDS SOME WORK

Thank you for the suggestion; we will work in this item as suggested.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? SEE BELOW, SPECIFIC COMMENTS

14. Are the number and quality of references appropriate? YES, although there seems to be too many references, and some are not relevant.

15. Is the amount and quality of supplementary material appropriate? NO. The suppl. Material should include the age model of both cores B040506 and G10-GC-01, and

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especially details on how the composite record was build. This is a critical point.

We agree with this suggestion and as appointed before we will include this information in the supplementary material looking for a better understanding of the manuscript. On the other hand, more details about the composite record will be added to the revised version of the manuscript. Specific comments:

Although the authors present new data (i.e. grain-size) for the Pisco shelf area, I have 3 main concerns that need to be addressed before this manuscript can be considered for publication:

1) Physical setting of the collection sites needs to be given as well as a summarized sediment description.

The manuscript lacks presentation of the sampling sites with respect to processes (other than eolian input) that may affect the transport of particles from the continent to the ocean (e.g., strong or weak bottom currents?, erosional processes, slumps/earthquakes, etc.). Moreover, the Salvatteci et al. (2014 in CP) paper in its supplementary information reveals 2 slumps in core G-10, some clearly laminated sections and several banded intervals. X-radiographs of nine cores are shown in this publication (including G-10 and B-06), all of them showing intervals with slumps.

Citing Salvatteci et al. (2014 in Marine Geology vol 357): "... two possible mechanisms can explain the presence of the homogeneous sediments: slumps triggered by earthquakes and sediment instabilities, and/or sediment transported by strong bottom currents". ... "Another mechanism that can be responsible for the re-deposition of sediment from upslope in some portions of the cores could be related to changes in the intensity of the Poleward Undercurrent which is stronger during El Niño events". ... "All the cores evaluated in the present work show discontinuities and the addition of previously deposited material". With so many factors at play, isn't the interpretation of grain size in this manuscript somewhat oversimplified? By the way, I could not find the reference to the frequently cited Salvatteci et al. (2014) in the reference list.

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We agree with the referee in this point and in the new version of the manuscript we will write a section describing the sedimentological and oceanographic features that allow the hemipelagic sedimentation and the formation of the laminated sediments in the Pisco continental shelf. These features have been previously addressed deeply by Gutierrez et al., (2006); Reinhardt et al., (2002); Saukel et al., (2011); Scheidegger and Krissek, (1982); Smith, (1983); Suess et al., (1987) and we can make a description in the new version of the paper. Regarding the comment about the difficulties to link two sediment cores, the slump in different sediment cores in fact exist. Differences between records do not necessarily represent losses as not direct evidence of discontinuities was found, and maybe it is product of differences in the sedimentation rate as was appointed by (Salvatteci et al., 2014). Nevertheless we can reconstruct historical record using different cores of the same area (Pisco mud lens) as was the case here. The paper by Salvatteci et al., Mar Geo, shows that it is difficult to establish the link between cores but it has been done for several cores off Pisco including B6 and G10 (see figure SM1 in Salvatteci et al CPD supplementary material). The distribution and deposition of particles is consistent in both sediments cores. In this work we did not consider that slumps in the record for analysis and for the temporal windows showed here processes as bottom currents erosions appear to be negligible. Indeed, the sediment section chosen here to complete the record (S5 in the core G10) has no hiatuses and cover the MCA period. We will mark in the figures and include additional material about how the composite record was assembled. Finally we will include the correct reference for Salvatteci et al. (2014) in the final version.

2) Chronology. Detailed chronology for both cores used in this manuscript needs to be included as well as an explanation on how the composite record has been built. Given the issues raised in (1), this is critical! Please add X-radiographs of the cores and the composite, a table/fig. with the Pb210 and C-14 data, and the overlap/match between both cores.

We agree with the reviewer that it is critical to explain how the composite record was

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assembled. We will do this in detail in the revised version. These two cores have independent chronologies based on several 210Pb and 241Am data points, several 14C ages, and the identification of sedimentological structures by X-ray images. In the new version we will include a table or figure with the chronological information.

3) Grain-size Analysis. a) Authors should state the advantages/disadvantages of using the chosen method (Flow Particle Image Analyzer) over other techniques. This goes in hand with the question:

a) Is the >200 microns fraction not important off Peru, on a setting such as the continental shelf? If so, please tell us why.

We apologize for this misunderstanding, we forgot to mention in the submitted version that particles coarser than 200 μm were never found in any samples after sieving. That means that the sediment samples do not contain such coarse particles, and that the grain-size distributions displayed in this study well represent the whole samples. On the other hand the Flow Particle Image Analyzer technology has several advantages: First each of the particles can be seen in each sample, which assures the analysis of mineral particles only, assessing the chemical attack efficiency and offering the possibility of eliminate the non-lithological particles. Images may be used to identify the origin of particles by their optical characteristics such as the case of black carbon (Flores-aqueveque et al., 2014). Finally, if the images are to be ignored, this method provides size information comparable to that obtained with a classic laser granulometer methodology.

b) Removal of opal. Have the authors checked that all opal was really removed? What is the opal content in Pisco sediments? (I believe these are sediments loaded with diatoms). Over the years it has become more and more evident that the removal of all opal from a sample is not an easy task. Please add a sentence or two about this issue in the methods section, making sure that the methodology employed has removed all opal from each sample.

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Using the Flow Particle Image Analyzer technology allows us to capture images of all the particles passing through the cell measurement. We observed that opal had been fully removed from all the analyzed samples, which confirm the efficiency of the pretreatments we used.

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