Climate of the Past Discussions

Clim. Past Discuss., author comment AC1
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Reply on RC1

Marco Yseki et al.

Author comment on "Millennial variability of terrigenous transport to the central–southern Peruvian margin during the last deglaciation (18–13□kyr□BP)" by Marco Yseki et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-183-AC1, 2022

"The authors present two marine sediment records of the last deglaciation interval from the Peruvian margin. They use XRF data and granulometry analyses to distinguish aeolian from fluvial sources in the sediment. The variations in these sources indicate changes in coastal surface winds and in the strength of the South American monsoon. The granulometry brings here novel data in this region where aeolian transport can be significant. The study clarifies the processes driving the sediment flux to the Peruvian margin, and allows for a reassessment of previous record of terrigenous input. The methods are sound and the data support the conclusions.

The authors provide important new paleoclimatic observations for the Pacific South American coast in the key period of the abrupt deglacial warming.

The article is generally clearly written, but the quality of the english is variable throughout the manuscript. The text could be somewhat condensed to make the reading easier."

We thank the reviewer for the constructive comments. As suggested by the reviewer, an attempt will be made to condense the text and improve the English.

Detail comments of RC1:

"L97: at what depth are the anoxic conditions?"

The information will be added to the new manuscript.

"L110: precise "austral" winter and "austral" summer"

The text will be modified according to the reviewer's suggestion in the new manuscript.

L113: "large quantities" seems in contradiction with L115 "scarce flows of coastal rivers".

That true, the text will be modified according to the reviewer's suggestion in the new manuscript.

L119: "Aeolian clays and silts are transported offshore by trade winds" implies that the

fine fraction is not only related to fluvial transport but also partly to aeolian transport, which is at odds with the conclusions. A clarification is required.

Briceño-Zuluaga et al. (2016) report a grain-size mode of 3±1 µm in Pisco sediments during the last millennium. As the Reviewer mentions, the authors suggest that the origin of these particles may be of fluvial and/or aeolian origin. However, in our results, the finest end-member presents a median of ~ 10 µm in Callao and Pisco sediments. The presence of particles of this size in marine sediments in the southeast Pacific and different parts of the world are mainly associated with a fluvial origin. In fact, this is consistent with our results from surface sediments in Callao. The aeolian inputs represent a wide range of particle sizes, and during atmospheric transport, the coarser particles are deposited first. This results in the coarser particles dominating the deposition along their dispersion. A large fraction of the particles smaller than 10µm (desert aerosols) are transported across the continental shelf (Schultz 1980; Saukel et al., 2011). The fine fraction of eolian origin in the continental shelf sediments is therefore negligible and the fine fraction of the continental shelf is largely dominated by fluvial inputs.

The text will be modified in the new manuscript to avoid confusion.

L128: please indicate the number of samples and the range of depth in the main text.

The number of samples and the range of depth will be added to the new manuscript.

L130: 2017 coastal El Niño anomalies did not extend south of Lima. "El Niño" conditions may thus not be justified for Pisco.

More details on the El Niño 2017 will be added in the new manuscript. The flow of the Callao and Pisco River reached extremely high values during austral summer 2017 (Fig 1 Supplement). This increase is associated with the Coastal El Niño. Likewise, in the month of April 2017, wind surface anomalies were positive along the coastal band off Peru (Fig 2 Suplement). Chamorro et al (2018) found quasi-linear relation between surface wind and pressure gradient anomalies during El Niño 1998. The alongshore pressure gradient anomalies were caused by a greater increase in near-surface air temperature off the northern coast than off the southern coast, associated with the inhomogeneous SST warming similar to El Niño 2017. Therefore, we believe that the samples collected in April 2017 may reflect changes in fluvial and maybe aeolian transport associated with the El Niño.

L140: which calibration dataset does CALIB8.1 use? Base on the figure, I understand that the depth-age model is a sequence of linear models. This should be explained in the method section.

The 14C ages and calibrated ages are listed in Supplementary Table 2. CALIB 8.1 used the Marine 20 dataset

L149: "laminated packaged"? do you mean "lamination"?

Laminated sediments

L154: the relevant information is the 14C calibration dataset, not the software.

We used the 14C ages published in Rein et al (2005) and calibration was by

CALIB 8.1 with Marine 20 dataset. In the new manuscript, the 14C and calibrated ages will be added in supplementary information.

L174-178: this paragraph needs to be clarified, in part by improving the english.

The text will be modified according to the reviewer's suggestion in the new manuscript.

L181-182: What about wind blown terrestrial material? Doesn't it contain Ti as well? In Haug et al 2001, Ti was used indeed as a proxy for river discharge but aeolian transport was not an issue there.

Indeed, in rocks, Ti is hosted principally in Ti-bearing heavy minerals and therefore present in the silt sand fraction of sediments (Calvert & Pedersen, 2007). But in soils it appears in a variety of forms including primary and secondary minerals as well as organically bound and amorphous compounds. Ti, like Zr, is not very mobile, less than Al, Si, Fe, ... and for this reason, the products of pedogenesis are enriched in Ti. As the main of these products are clays, they are enriched in titanium compared to the initial rock (Taboada et al., 2006), it is also the case of iron oxy-hydroxides often adsorbed on clays. And it is for these reasons that clays are richer in Ti than silts and sands.

Therefore, the Ti/Al ratio represents, in clays, the Ti richness of the parent rock and the intensity of weathering. So for a given basin and a relatively short period of time the Ti/Al ratio in clays varies little. In this case, Ti/Al variations in a sediment core can be due to an additional contribution of Ti-bearing non-aluminous particles (silts and sands) detected by higher Ti/Al values. This ratio are sometimes used as a proxy for wind input (Grousset et al., 1989) - but the Zr/Al ratio seems to be an even better proxy for these inputs (Calvert & Pedersen 2007).

The case is very different for the ratio Ti/Zr. Zirconium occurs in sediments almost exclusively as the mineral zircon ($ZrSiO_4$), which is mechanically resistant to weathering (Calvert & Pedersen, 2007). For that reason it occurs in the sand and silt fractions of sediment. So, when we do the Ti/Zr ratio, we compare Ti, which is present in all sizes of sediment, but especially in clays, to Zr, which is only present in the silt and sand fractions. This is why this ratio is an index of granulometric variations.

As the Ti/Zr curve of 106kl core nicely follows the finer sediment fraction in our core we believe that the main cause of Ti/Zr variability in this region is the supply of clays by fluvial transport. We will modify the discussion in this sense.

L185: "Zr has been widely used as a proxy for mean depositional grain-size"

The text will be modified according to the reviewer's suggestion in the new manuscript.

Figure 2: add the sample depth to each graph.

The sample depth will be added to each graph as suggested by the reviewer.

L238: "we interpret...based on our interpretation". So, what is the interpretation based on?

The paragraph will be restructured as suggested by the reviewer. There was an

error in writing the paragraph, we wanted to say: "... based on our results of grain-size analysis in surface sediments and the literature".

L238-246: This paragraph should be shorter. The absence of a fine fraction peak in april 2017 in station E5 should be discussed.

The paragraph will be modified as suggested by the reviewer and the discussion mentioned will be added.

L258: "when alongshore wind stress was anomalously enhanced at the mature phase of the Coastal El Niño". This contradicts earlier statement (introduction) about the increase of rainfall during El Niño. More details about the 2017 El Niño event are needed.

As explained above, an increase in surface winds and precipitation may occur simultaneously during El Niño 2017. More details on the El Niño 2017 will be added in the new manuscript

L262-263: I don't understand clearly what the authors mean.

In those lines we wanted to say that a change in the abundance of coarse particles is observed in the Pisco sediments collected during April 2017. But due to the small number of samples this variation is not statistically significant. The text will be modified in this sense.

L266: "desert" instead of "dessert"

The text will be modified according to the reviewer's suggestion in the new manuscript.

L276-282: keeping EM3 as a wind proxy and excluding EM2 and EM4 only on the base of the slight increase of EM3 in one station in April 2017, seems weak. EM4 increased in Pisco in April 2017. The argument needs to be strengthened. The whole section in general is somewhat long and confusing.

We agree that the section in general is a bit long and confusing. The section will be restructured in the new manuscript. However, to exclude EM2 and EM4 as proxies for eolian inputs in Callao we did not only rely on the probable increase of EM3 during April 2017 in Pisco. We will better explain this choice based on Callao sediment interpretation and also on bibliography.

L292: I did not understand from the previous section that EM2+EM3+EM4 would be considered as wind proxy in Pisco. This needs to be more clearly built and stated in section 4.1.

The section 4.1 will be modified according to the reviewer's suggestion in the new manuscript.

L296-297: a similar result would be obtained if Ti was both in fluvial and aeolian material, and Zr only in aeolian.

That is true but the good relation between Ti and fine fraction (see above) let us think that there is more Ti in the fine fraction. Zr is only present as Zirconium which are hard grains very resistant to weathering and for that reason include in the sand fraction. We observed a 50% increase of Ti in the April 2017 surface sample rich in fine fraction collected off Callao. We will add these data to the paper.

L320: "seasonal": which season? "poleward": North or South?

Sorry, it was not clear. Mean ITCZ has shifted southward. The sentence will be rewritten.

L333-334: "SST proxies..." please correct and clarify the sentence.

The text will be modified according to the reviewer's suggestion in the new manuscript.

Figure 6 and 7: when possible, indicating a modern value for reference would be useful.

You are right, we will do so.

Supplementary Figure 1: legend for triangles is missing

The legend for triangles will be added in the new manuscript.

supplementary figure 2: please add legend for the symbols. What are the stars? Dates from Salvatecci? Are they included in the calculation of the new age model? Indicate sedimentation hiatus.

As the reviewer says, the stars are the ages of Salvatteci et al. (2019). These ages were taken into account in the new age model and are listed in the table in the supplementary information. In the new manuscript the corresponding legends will be added.

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-183/cp-2021-183-AC1-supplement.pdf