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Climate of the Past Discussions

# *Interactive comment on* "Nutrient utilisation and weathering inputs in the Peruvian upwelling region since the Little Ice Age" *by* C. Ehlert et al.

**Anonymous Referee #2**

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I have reviewed Ehlert an co-authors' results on radiogenic sedimentary Nd and Sr isotopes, biogenic silica, total nitrogen and the isotopic composition of biogenic Si and bulk N between 1300 and 2000 AD from 2 new sedimentary archives off the Peruvian coast.

The paper is well written and it could provide the community with a better understanding of the relationship between global climate and local oceanographic changes happening during the past 700 years and the LIA potential impacts on the eastern South Pacific. I think that this would be very relevant for future predictions on how this critical part of the ocean for food supply will evolve as climate changes. The new data (radiogenic isotopes and silicon isotopes) definitely merits publication in Climate of the Past but I would suggest some major revisions to the way the authors have presented their



dataset.

My main concern is the following: since a great deal of the data presented here has been already published elsewhere, Gutiérrez et al 2006, 2009; Siffedine et al., 2008, and the true novelty comes from the Nd, Sr and Si isotopes, it would be more natural to focus the bulk of the publication on these variables and how they fit in a more global picture of changes associated with ocean circulation and climate change, specially at the critical period that they have highlighted following 1820-1870AD. Instead, it was very disappointing to see that in its present form, the paper presents a very long discussion on nutrient utilization, which is highly debatable. What can the authors say about the Nd signature of the 2 cores and changes associated with ENSO or SAM during this period?. In my opinion, the paper's major strength and novelty should be set in linking the radiogenic isotope results (and silica isotopes) to climate and leave the nutrient utilization discussion as it is or shorten it up.

My second concern with the paper in its present form deals with the M771-470's age control during the critical transition period highlighted and discussed thoroughly in the paper. The core's age model beyond 1850AD was constructed by adopting the sedimentation rates of (not so) nearby core B0405-13. In this respect, it is worth noting that sedimentation rates at both sites appear to be very different. Just take the case of the sed. rate of M771-470 from 10 to 25 cm being as low as half of B0405-13's sed. rate (0.9 vs 1.78 mm yr-1). Why should these 2 cores have a similar sed. rate in the past if recent sedimentation regimes are so different? I am not disregarding the age model proposed here, but I would like to see a much thorough justification of it.

Another minor comment would be to ask the authors to refrain from asserting certain climate connection as certain since this is all very speculative yet. For example, in the abstract the authors write "...These patterns *were caused* by permanent El Niño-like conditions characterized..." (emphasis added). This is too strong phrasing that implies complete certainty. I would advice the authors to try to keep a more speculative language when asserting hypothetical causalities that are going to be surely subject of

confirmation (or rejection) by future work.

In summary, I would recommend the editor to accept this manuscript for publication considering the comments that I just mentioned.

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Interactive comment on Clim. Past Discuss., 10, 3357, 2014.