

Interactive comment on “Autoregressive Statistical Modeling of a Peru Margin Multi-Proxy Holocene Record Shows Correlation Not Causation, Flickering Regimes and Persistence” by Seonmin Ahn et al.

Anonymous Referee #1

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The article by Ahn et al. use statistical approaches applied to an exceptionally well resolved Holocene record of hydrological proxies from a marine core collected along the peruvian margin.

As I am a paleoceanographer, not a statistician, so I judge the proxy- and sedimentology-related processes with only a limited scope on the statistical methods.

First, I could observe the efforts made by the authors to explain as much as possible the terms and concepts used in their analysis, which are helpful to the marine geol-

C1

ogist reading the article to figure out what is meant. But this effort is far from being enough, though, if the authors' aim is to advertise the benefits that such analysis could provide to geologists who are interested in the method to their own records. In the text, the authors stack statistical concepts, description of numbers located in complex matrix, one on top of each other, and the geologist gets confused. I then urge the authors to wrap-up the article in a more comprehensive way. It is very frustrating, after reading a sentence such as “Thus, a key conclusion from the statistical models is that the lagged autocorrelations are significantly better predictors of proxy variability than the different proxy-to-proxy cross-correlation either at lagged times or as induced by correlated noise (Fig. 5).”, to actually have a look at Figure 5 that is otherwise quite confusing. As it stands, the article is not written appropriately to be informative to the general audience of the journal “Climate of the Past”.

This being said, the claims put forward in the article are interesting, and intriguing, but again the statistical analysis is far too disconnected from the sedimentological and proxy-related processes to be of interest for geologists. More discussion about what you're really dealing with is here warranted.

For example, it is notorious that alkenone-based SST estimates along the peruvian margin are warmer than the mean-annual SST if global core-top calibrations are applied (see e.g. Prahl, 2010, GCA; Kienast, 2012, Paleooceanography), probably because coccolithophorids live over time periods when upwelling ceases (e.g. the summer-stratified season of during El Niños). It implies that, in the global view that intensified upwelling is expected to increase productivity and decreases SST, the upwelling intensity / SST relationship you suggest might not intuitively be correlated as the authors claim, since alkenone-based SST might not be a good predictor of upwelling-related SST decreases. In the same vein, alkenone concentration might not be a good predictor of upwelling-induced productivity, since they might be synthesized during more stratified periods. I wonder why, in the end, you don't use biogenic opal instead of alkenone concentration to infer upwelling-induced productivity. The Chazen paper

C2

shows there is quite a different signal in bio. silica compared to alkenone concentrations.

Also, you may want to comment more on the long-term evolution of your statistical outputs. If for some reason there is a long-term factor that strengthens productivity from the mid to the late Holocene, it is expected that oxygen consumption drove the appearance of laminations in the late Holocene, which artificially enhances variability (through decreasing bioturbation). Where exactly laminations occur? How bioturbation can act as a filter which would heavily lead your AR-HMM model to spuriously outperform the decadal predictability? It is easy to imagine that your memory effect could almost exclusively be driven by sediment mixing, and more discussion on this artifact must be discussed. In your figure 4, I visually tend to see the black dots trending from increasing densities from the "calm" to the "noisy state", which could be the signature of an increase in the occurrence of laminations through time.

To conclude, I felt the authors completely forgot the object they're looking at: an amazingly well resolved dataset that is amazingly complex to interpret because of the uncertainties associated with the proxies employed. The authors claim that their statistical, once applied blindly to a dematerialized set of numbers, "shows correlation not causation, flickering regimes and persistence" (see the title). I suggest the authors to pause and think more about what is actually measured, and how the signal goes through sedimentary processes prior to fossilization in the geological record.

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