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Climate of the Past Discussions

Interactive comment on "Dynamics of primary productivity in the northeastern Bay of Bengal over the last 26 000 years" *by* Xinquan Zhou et al.

Anonymous Referee #2

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The manuscript by Zhou and co-authors is interesting, adding new insights in the monsoon-PP connections in a region which is relatively understudied compared to other parts of the Indian Ocean. The combination of proxy and model outputs helps to understand better the relationship between salinity and stratification in the water column, beyond the reasonable assumptions anyone can made about the link between salinity and productivity. I think the overall quality of the manuscript is good, and deserves publication in Climate of the Past after some moderate revisions.

The structure of the manuscript needs some improvement. While the first part (Introduction, Material and Methods) are very well written (although lacking some details about age-model), the discussion relative to the model outputs is not so clear. I find the discussion about model output section very difficult to follow, needs to be simplified

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Discussion paper

in order to improve the understanding and to be better integrated with the proxy data, not to be discussed separately. The figure 2 is not very useful, it repeats data that are shown later in other figures several times. For example, showing the d18Osw and the GISP2 ice-core d18O is not really relevant, as we see the proxy data already tuned to the ice-core data. I assume that Marzin et al., (2013) contains a plot showing this, so these two curves are not needed here. An important point regarding the age-model is that if, despite the large number of radiocarbon ages, the proxy data is tuned to the GISP ice-core d18O, later comparisons between proxy and ice-core data are not very well sustained (circularity). The authors should keep this in mind when discussing about it at L. 205-207.

I find particularly intriguing the change in the salinity-PP relationship before and after LGM (L. 213-222).. The authors suggest that the higher PP during low salinity between 26-19ka are due to higher wind mixing. Are there independent proxy evidence of this coupling? For example, loess deposits that could record changes in wind intensity which could support their view? And why the wind-forcing gets weaker after the LGM?

Finally, in the section Data availability the authors indicate that "Data to this paper can be required. Please contact the X. Zhou or S. Duchamp-Alphonse.". Copernicus journals (including Climate of the Past) have a very clear policy regarding data curation (https://www.climate-of-the-past.net/about/data_policy.html), which "requests depositing data that correspond to journal articles in reliable (public) data repositories, assigning digital object identifiers, and properly citing data sets as individual contributions". Clearly the current statement about data availability does not meet this criteria, and all data and code should be archive somewhere or included as supplementary material.

Some minor corrections: L. 104. Abbreviate Arabian Sea L. 177. Strange symbol between longitude and latitude. Fig. 1f, why choosing SON instead of JJA as the other panels?

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