Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
https://doi.org/10.5194/cp-2021-144-RC1, 2021

Comment on cp-2021-144

Michael E. Weber (Referee)

Referee comment on "An Intertropical Convergence Zone shift controlled the terrestrial material supply on the Ninetyeast Ridge" by Xudong Xu et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-144-RC1, 2021

Xudong Xu and colleagues use clay mineralogical assemblages (mainly smectite) to demonstrate millennial-scale shifts in the ITCZ as documented in a core site from the Ninetyeast Ridge in the northeastern Indian Ocean. According to their interpretation, smectite maxima during the Last Glacial Maximum (LGM) point to regional shifts in the ITCZ. They further argue that glacial-to-interglacial times scales, smectite did not reach the core site during glacials because islands were exposed (due to low sea level) and because chemical weathering (producing smectite in warm times) was diminished.

The topic of the paper appears suitable for CP. I consider both the overall scientific significance and quality to be good to fair and hence recommend minor to major revisions. A better understanding of latitudinal ITCZ migrations, both on orbital and millennial time scales is indeed desirable, and existing knowledge is either sparse or conflicting. The authors have worked extensively in the general area and topic before and provide sufficient evidence of a sound understanding on regional paleoceanographic change associated with shifts in the ITCZ. To base their whole story around a single, short core (171106) and mainly one property (smectite) with the additional, occasional provenance data (Sr-Nd) is a little thin in my eyes. The chronology is sound and includes age modeling and uncertainty evaluation. The same appears to be the case for the clay mineralogy and provenance tracers.

The whole discussion on smectite suffers, in my opinion, from the fact that the record is too short (MIS 3-1) and does not cover a full glacial-to-interglacial cycle. One cannot see how the response was, for instance, during MIS 6 and 5, which would be critical to know here. Any discussion of orbital variability is hence hampered. This is specifically true for any statement implicating changes in sea-level and and their effect on changing provenance and clay mineralogy (availability or lack of accommodation space on the shelves). The core is apparently only 162 cm long - why? Wasn’t there a longer alternative to conduct such a study?

The same principal problem surrounds the discussion of the smectite peak around the LGM (21-18 ka). If there were a record for termination 2 (MIS 6/5 transition) one could see if there are common rules established during glacial maxima that are either regional or not. MIS 6 and termination 2 have comparable orbital configurations relative to the LGM and termination 1, hence possible shifts of the ITCZ – which are invoked in the discussion – should have been quite similar, at least from a global view, from which regional deviations could then be derived or discussed if present.

There is the complete lack of mentioning turbidity currents, which provide the vast majority of sediments from the river mouth of the Swatch of no Ground (SONG) to the BoB and neighboring regions. Even though the short core presented here seems to show a rather steady deposition, plumes from turbidity currents will reach the site and affect the clay mineralogy. In this context, it is also odd that none of the IDOP-Expedition 354 studies, which are in close proximity to the core cite, have been cited ore discussed.

I also miss the discussion on the variability of the Oxygen Minima Zona (OMZ) when the authors invoke the connection to the Northern Hemisphere. What would be the consequences for the area of the core site? For the Arabian Sea, Schulz et al., (1998, Nature) clearly linked the millennial-scale coupling to Greenland to shifts in extent of the OMZ, pointing to changing water mass composition and oxygenation.

Another lack of discussion surrounds the length of the core, which is only 162 cm. The sample resolution of 1 cm implies a 300-yr resolution, however, bioturbation should mix the sediment over several cms and smear according ages. What are the author's assessment of this effect and how would it affect their conclusions?

More specific comments:

Fig. 3
Sr/Nd resolution is too low to determine temporal variations. The only real change happened at 14-15 ka, probably as a result to changes surrounding Meltwater Pulse 1A, which are indicative of a major re-organization of the global thermohaline circulation. However, the data shows that the glacial-to-interglacial and millennial-scale sources likely did not change. Why is that?

Also, the authors mark H1-H4 and discuss the relation to smectite variability. The data does not show that in my opinion. Even if slight shifts are employed to account for potential mismatches in the age model, there is no consistent relationship, i.e. the various clay minerals occur either at the highs, lows or transitions of H events.

Line 147-149
This is important and needs clarification and elaboration. How far above the surrounding is

the core site? As mentioned above the authors completely ignore the possibility of turbidite deposition. Even if only the fine grained upper sediment clouds of distal turbidites (those naturally contain a high percentage of suspended fine material, i.e., clay minerals) reach the sediment site, it would have a large effect on the clay mineralogy of site 171106. After all, the sites in not far from the channels on the eastern side of the BoB.

In this context, Fig. 1 is missing a depth legend for both the marine and terrestrial elevations. Figures 1 and 5 could potentially be combined into a single figure.