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Comment on cp-2021-77

Mitch Lyle (Referee)

Referee comment on "Secular and orbital-scale variability of equatorial Indian Ocean summer monsoon winds during the late Miocene" by Clara T. Bolton et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-77-RC2>, 2021

Review of

Secular and orbital-scale variability of equatorial Indian Ocean summer monsoon winds during the late Miocene

Bolton, Gray, Kuhnt, Holbourn, Lübbers, Grant, Tachikawa, Marino, Rohling, Sarr, Andersen

For Climate of the Past

By Mitch Lyle

Rating:

Good data set, manuscript needs work

Bolton et al use an XRF-derived, orbitally tuned Ba record from southern Bay of Bengal site U1443 in order to study changes in productivity and summer monsoon in the 9-5 Ma time period. They used XRF scanner barium to track productivity through time. They suggest that precessional variations were evidence of summer monsoon wind strength in the equatorial Indian ocean and that South Asian monsoon winds were established prior to 9 Ma, with no apparent intensification over the late Miocene.

They have produced a data set that is worth publication. My main concern that needs to be addressed is that during the period that they study, the site moved northward perhaps by 200 km (2° of latitude). They did not address how that movement may have affected the records they discuss and that needs to be considered. For the most part the data they have seems to agree that the late Miocene between 8 and 5 Ma are part of a global high productivity interval, and they don't observe evidence for intensification of the South Asian Summer Monsoon toward the present.

The data are from Site U1443 from IODP Expedition 353 located near ODP Site 758 on the Ninety East Ridge. They have a good discussion of modern oceanography and its relationship both to winds and productivity. The study adds an important data set in a region that needs more records.

Their description of the sediment column is in the Materials and Methods section needs more work. The depths of the late Miocene interval are not needed, and core-section-interval designations just clutter up the writing here, especially since specific sections from different holes are not discussed later. Why are they describing sampling for micropaleontology? I also didn't see CCSF depths for the interval they discuss. In addition, figures in the Proceedings chapter on Site U1443, there seems to be a speed up of sedimentation immediately older than their interval. At what age did that happen? In figure 4 there is significantly lower sedimentation rates at the beginning of the 9-5 Ma interval—could these be the end of the lower sedimentation rate interval?

Given that the Site report gives the paleoposition of Site U1443 as 5°S at the Oligocene-Miocene boundary, what were its paleopositions during the 9-5 Ma time interval? A quick estimate shows that the site would have been between 1.5 and 3.2°N. Could this affect their interpretation?

I was not clear why a section on primary productivity, winds, and sediment traps were included with the drill site information. I didn't see where this was used later in the paper. If this is actually used it should be a separate subsection with a topic sentence to explain why they are making these observations. The drillsite was significantly further south when the 9-5 Ma sediments were laid down, so observations at the modern position may not be relevant.

The description of the age model, XRF scans and stable isotope methods are clear. It doesn't appear that Si was independently calibrated. Is this true? How much did a ratio of

Si/Ti in raw XRF counts vary down the interval, as evidence that biogenic Si deposition was negligible?

The comparison of stable isotopes to other Miocene data is clear and shows the relatively low variability of stable oxygen isotopes in this interval. One of the interesting graphs is the comparison of the stable carbon isotopes. There is a clear offset between records from different basins, but a common shape to the curve signifying a strong global signal. It is likely that stable carbon isotopes may provide a decent chronostratigraphy.

I was puzzled why section 4.2 on XRF calibration is in the results. It clearly belongs in methods. They calibrate with a relatively small set of samples, but it seems adequate. Also, I don't understand why they didn't use the shipboard carbonates data to help calibrate the Ca record, since they were having trouble with the sediment digestion data. The Ca in clays doesn't vary a lot, so most variation in Ca is because of CaCO₃. If they want to see a way to calculate CaCO₃ from bulk sediment chemistry, check out Dymond et al (1976; DSDP Leg 34 Initial Reports, 575-588). The spikes in Rb and K are at the same depth in both records, so probably do represent felsic ash layers. This also shows from the raw Si data. Incidentally, how much of the total Ba was represented by the excess Ba? The productivity interpretations are more robust if the excess Ba is a large proportion of the Ba signal.

I trust the spectral analysis and am heartened that the Ba-xs has a cleaner orbital signal than Ba/Fe. They would have the same signal only if Fe was constantly deposited. Otherwise there is a composite signal of both elements.

Specific comments:

Line 116. Position of Site U1443 has been rounded off too much. It is OK to round to the nearest minute, not nearest degree. Actual position is 90°22'E, 5°23'N. This is important to track how the site position changed by plate tectonic motion over their time frame.

Line 215: What is CEREGE? Only the acronym is given in the address as well.

Line 320-325: One could better judge the relative amounts of detrital Ba and bio-Ba if there were more information on percentage of clays in the interval. It would appear from descriptions that there is very little biogenic opal in the interval. If that is true, the clay content is represented by the noncarbonate fraction (100-CaCO₃%). How did that vary over the interval?

Line 365: The authors should state at the beginning that they believe their newer age

model is better, for the reasons they list. When I first read this, it wasn't clear what they were claiming. Incidentally, a spectral test is not very sensitive to minor age errors. I place more credence on comparison with other tuned isotope records.

Line 445: the MAR record is driven strongly by the age picks and only secondarily by sediment composition. Square wave profiles like seen for bulk sediment can be caused either by errors in the ages of the intervals, or by major changes in sedimentation higher than the resolution of the age model. Which do they think is the cause?

Line 500: I am having difficulty with this attempt to reconcile an increase in carbonate accumulation rate first with a decrease in dissolution but then also with an increase in productivity. The argument about scavenging is completely ad hoc. Usually there is more than enough production to remove clays from surface waters, so higher production does not lead to higher clay deposition. Furthermore, the $\text{CaCO}_3\%$ and Ca/Terr records are consistent with an increase driven primarily by reduced dissolution. If there is higher clay deposition post 8.5 Ma, how can one disprove the alternative hypothesis, that of higher aeolian dust flux that may have triggered some higher production through iron fertilization or indirectly because winds were stronger and carried more dust?

Line 658, 659:

The observation of a higher productivity regime around 11 Ma has also been observed in the eastern Pacific, in what Lyle and Baldauf (2015) referred to as the "early carbonate crash" The period between 10 and 11 Ma has the highest biogenic silica deposition of the entire record at Site U1338. This deposition interval is distinct from the late Miocene Biogenic Bloom. It is a low CaCO_3 interval caused by higher opal deposition.

