RC2

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.

We thank the reviewer for their useful comments on our paper. We reply in detail to the concern on paleolatitude below.

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.

We provide both CCSF depths and hole-core-section-interval information for our study interval in the spirit of making our study fully reproducible to those who may wish to test our ideas. Additionally, we now include a reference to a splice table in the supplement, which was added at the request of Reviewer 1. We have deleted the sentence detailing 1 cm sampling for micropaleontology – we originally included this to explain that we took 1-cm whole round samples for foraminiferal work (rather that 2-cm quarter-round samples as is common) because of low sedimentation rates.

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?

The late Miocene increase in sedimentation rate identified by shipboard bio-magnetostratigraphy at ~9 Ma (between 100-130m CSF-A, Fig. F14 in the U1443 Site Chapter) does indeed correspond to the increase in sedimentation rate at ~8.6 Ma in our new age model (Fig. 4d). This is confirmed by revised nannofossil biostratigraphy performed as part of this study (Fig. S1).

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?

The paleolatitude of Site U1443 at 10 Ma was ~2°N (based on paleolatitude.org), and this was shown on Figure 1c in the original submission (yellow star on map). We have now calculated more precise paleo-positions to ensure that our interpretations remain valid.

Based on the G-Plates online portal which allows calculation of paleo-positions at 1 Ma resolution (http://portal.gplates.org/service/reconstruct_points), the position of Site U1443 changed from **1.71°N, 88.06°E** at 9 Ma to **3.27°N, 89.04°E** at 5 Ma (see Fig. R1 below).

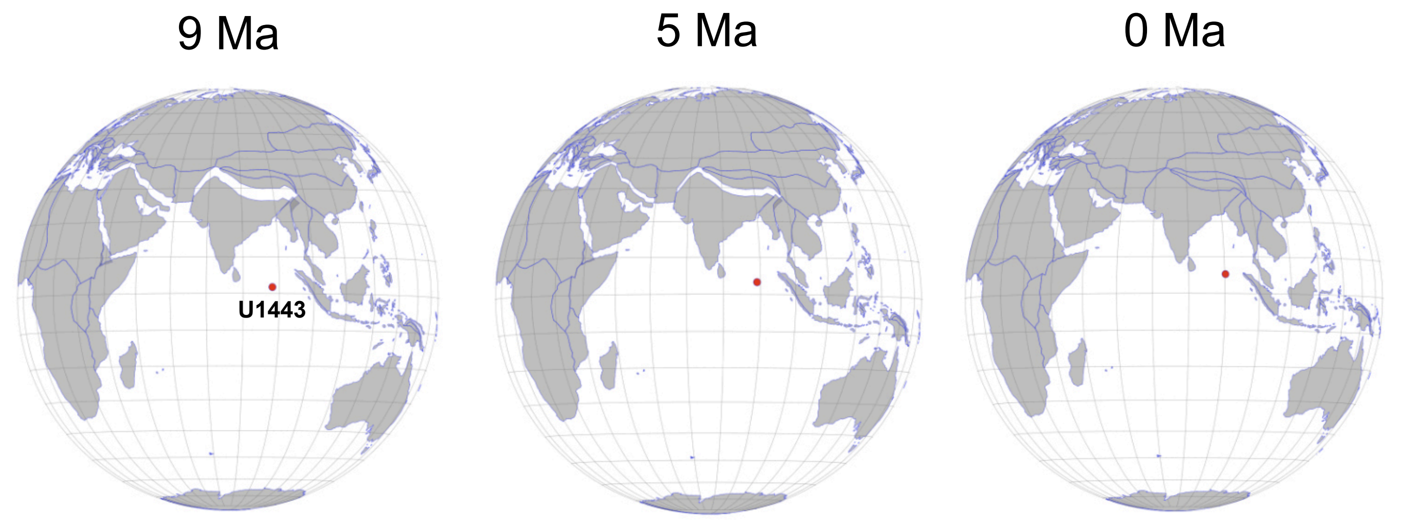


Fig. R1: Paleo-position of Site U1443 (red dot) on paleogeographic maps for 9 Ma, 5 Ma and 0 Ma (computed using G-Plates)

The Ninetyeast Ridge (NER) lies in a zone between the Indian, Australian and Capricorn Plates, which result from the break-up of the vast Indo-Australian Plate along diffuse boundaries during the Neogene (e.g., Krishna et al., 2012). The current deformation regime of the NER is complex and its potential role as a plate boundary is debated. The northern part of the NER is dominated by left-lateral transpressional deformation (c.f. Sager et al., 2013). At 10 Ma, we expect this deformation to have generated a maximum differential motion between sites located on both sides of the NER of ca. 1° latitude, considering the difference of northward motion between both sides of the NER (Pubellier et al., 2003). Northward movement of the northern Ninetyeast Ridge where Site U1443 is located has paralleled that of the Indian Plate (including the Indian subcontinent) over the late Neogene (Fig. R1), thus the paleo-position of Site U1443 relative to the southern tip of peninsular India has remained relatively constant. This implies that important monsoon surface ocean currents (such as the Southwest Monsoon Current on Fig. 1c) likely had a similar influence in late Miocene waters overlying Site U1443 as they do today.

To illustrate the paleo-position and migration of the site more clearly, we now include these 9 and 5 Ma paleo-positions on all of the Figure 1 maps (yellow stars – although paleogeography in these maps is modern). This shows that, assuming modern current positions and oceanography for the Late Miocene, the seasonal contrast in mixed layer depth related to monsoon wind-driven mixing is similar at the paleo and modern positions for Site U1443.

In summary, we do not think that the late Miocene paleo-position of our site affects our interpretations related to monsoon dynamics and paleoproductivity, as the site remained north of the Equator and under the same influence of the Indian monsoon wind system (even assuming no southward shift of modern oceanographic currents, revised Fig. 1). We agree that a site migration on a similar scale in the central or eastern equatorial Pacific Ocean could have much more important consequences (e.g. migration out of the equatorial high-productivity band), but this is not the case for Indian Ocean Site U1443.We have added a sentence on paleo-position and northward migration in the "Site and Sampling" methods subsection.

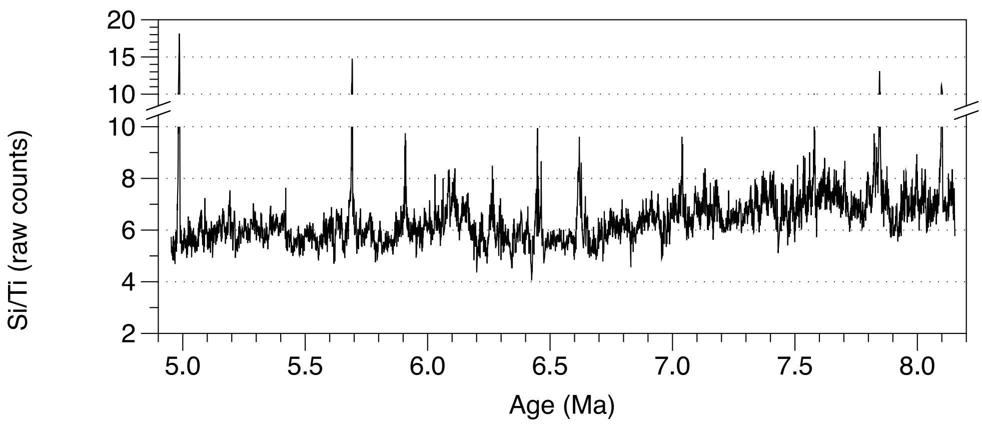
I was not clear why a section on primary productivity, winds, and sediment traps were included with the drill site information. I didn't see where this was used later in the paper. If this is actually used it should be a separate subsection with a topic sentence to explain why they are making these observations. 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.

We included a section on modern oceanography, winds, and productivity as rationale for the interpretation of our Site U1443 sedimentary paleoproductivity data as representative of summer monsoon wind strength. We think that the inclusion of this data (and description of the methods/datasets we used) is important for the paper, so that the reader understands the modern link between monsoon dynamics and export productivity in the region. We have moved this paragraph out of the "Site and Sampling" subsection of the Methods and into a separate subsection with a topic sentence, as suggested.

Although the drill site was located ~2° further south during the late Miocene study interval (see detailed response above), this does not make a large difference to monsoon-driven seasonal oceanographic changes (see revised Figure 1). We don't think it would be necessarily more relevant to extract modern data at the late Miocene paleo-locations, because the position of Site U1443 relative to peninsular India was similar. In addition, because we compare modern oceanographic data to sediment trap data (also from 5°N), we prefer to use a box around the modern site position.

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?

It is correct that Si was not independently calibrated. This was because the acid digestion protocol used included hydrofluoric acid resulting in the formation of SiF_4, which is a volatile compound. Thus, Si concentrations determined by ICP-MS are not considered to be accurate enough, so were not used for calibration.



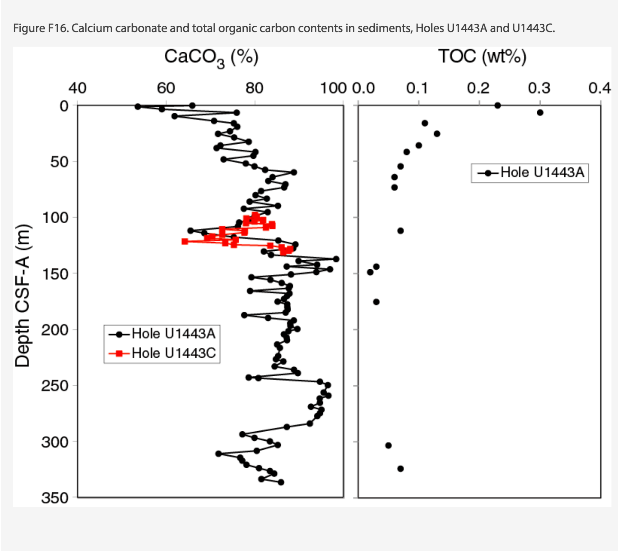
The amplitude of Si/Ti XRF intensity ratio is generally small as shown by the above figure. Si/Ti remains within a relatively narrow band centred around 7 below 95 m CCSF and around 6 above 95 m (~6.8 Ma), this switch appears driven by an increase in Ti. We also note that no siliceous microfossils were observed by shipboard biostratigraphers or sedimentologists over this interval. Considering the primary sediment component is carbonates (between ~60 and 90%, generally higher than 70%, Fig. 7b) the small Si/Ti changes most likely reflect changes in the detrital fraction.

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.

We thank the reviewer for this comment, we agree that despite inter-basin offsets the $\delta^{13}C$ trends appear synchronous. We hope our new Indian ocean records will stimulate new research on the late Miocene $\delta^{13}C$ shift.

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.

We have moved the description of linear correlation coefficients and % $CaCO_3$ calculation to Section 3.5 of the Methods. Section 4.2 now describes only the long-term trends in calibrated and uncalibrated XRF data. The shipboard carbonate data was performed on Hole A and a small section of Hole C (shown below). Only four samples measured for $CaCO_3$% are in core sections that were scanned for XRF (following the splice between 72.75- 113.56m CCSF), so we could not use shipboard data for calibration. We think that our chosen method for $CaCO_3$ calculation is robust, given the excellent agreement with %$CaCO_3$ calculated in the subsequent interval of the core by Lübbers et al., (2019), based on calibration of XRF-derived counts of (Ca/∑(Ca, Al, Si, K, Ti, Mn, Fe, S)) to discrete $CaCO_3$ measurements (Fig. 7b).



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.
Thanks for raising this important question. $[Ba]_{xs}$ represents on average 83% of total [Ba]. We have added this information to the results.

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.
We have corrected this.

Line 215: What is CEREGE? Only the acronym is given in the address as well.
CEREGE is the name of our laboratory, Centre Européen de Recherche et d'Enseignement des Géosciences de l'Environnement. Because it is in French, we generally use only the acronym in affiliation listings. We have added the full name to the paper text.

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?
Biogenic Ba constitutes on average 83% of total Ba. We confirm that we think there is very little biogenic opal in the interval, and that clay content is represented by the noncarbonate fraction (100-$CaCO_3$%). Variations in the carbonate vs non-carbonate fractions are shown in Fig. 7b/c. Over the timescale of our study, there is a small long-term increase in the clay (non-$CaCO_3$) content (also visible in the "Terrigenous MAR" record in Fig. 7, consistent with a longer-term trend of increasing mineral flux in this region of the NER from the Miocene to the Pleistocene). This is discussed in Section 5.1.

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.
We agree that the good correspondence of our isotope stratigraphy with other, independently tuned, records provides important validation of our age model. We do think our age model for the 8.7-8.1 Ma interval is more robust than that of Lübbers et al., 2019, simply because a longer continuous isotope record was available at the time of age model construction. For example, the youngest tie-point in the age model of Lübbers et al (2019) is at 8.5 Ma and ages for the interval 8.5 to 8.1 Ma are just extrapolated using the sedimentation rate between 8.7 and 8.5 Ma.

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?
We agree that the MAR records are primarily driven by the age-depth tie points that we impose and sedimentation changes were likely less abrupt in reality, however we are confident that our age control points are robust (Fig. 4, 5,

and S1). We focus our interpretation on long-term MAR trends rather that step changes in our record. We have added the following sentence to the results: “The stepwise nature of MAR records results from age model-imposed stepped changes in sedimentation rate.”

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 CaCO3% 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?

Based on our records, non-$CaCO_3$ MARs (i.e., bulk MAR – $CaCO_3$ MAR, ~ clays) show a step increase of ~50% at this time, coincident with the increase in $CaCO_3$ MAR. The reviewer is right that fine-grained mineral dust, most likely from the deserts to the west bordering the Arabian Sea, could have contributed to the U1443 clay fraction. However, we consider it unlikely that wind-blown dust was a major constituent of clay at Site U1443. A recent study on detrital clay geochemistry in Site U1443 sediments (Bretschneider et al., 2021) discusses clay provenance in detail, and concludes that detrital material in Site U1443 late Miocene sediments was primarily supplied by the large river systems. One observation in support of this is the much higher clay contents at Site U1443 compared to Indian Ocean sites further from riverine detrital sources. To the best of our knowledge, there are no records documenting aeolian dust flux to the equatorial Indian Ocean spanning the Miocene. Because we cannot rule out the hypothesis that dust played a part in our record, we now raise this possibility in our discussion, and present our scavenging hypothesis as speculative. An increase in wind intensity from 8.6 Ma that could have increased aeolian dust delivery thus increasing total phytoplankton productivity and organic carbon export is not supported by our Ba records, which show no change in total export productivity over the increase in $CaCO_3$ and clay MAR.

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 CaCO3 interval caused by higher opal deposition.
Thanks, we have added reference to this study.

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