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## Comment on cp-2021-77

David De Vleeschouwer (Referee)

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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-RC1>, 2021

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### Peer review for Climate of the Past.

"Secular and orbital-scale variability of equatorial Indian Ocean summer monsoon winds during the late Miocene" by Bolton et al. (CEREGE, Aix-en-Provence, France)

The manuscript by Bolton et al. presents new proxy records and an astronomically-tuned age-depth model from a recently-drilled IODP deep-ocean sediment core (U1443). Proxy records span the late Miocene (9 – 5 Ma) and include downcore benthic isotope records ( $d^{13}C$  and  $d^{18}O$ ) and XRF-derived productivity-related and detrital-related elemental data. All proxy records are of sufficient resolution to resolve precession cycles, i.e. the shortest astronomical frequency. Based on their results, the authors present three important conclusions:

First, the authors observe a 3-fold increase in  $\text{CaCO}_3$  mass accumulation rates at 8.66 Ma, but no change in their export productivity proxy  $\log(\text{Ba}/\text{Fe})$ . They interpret this pattern as the result of a contemporaneous increase in coccolith productivity and improved preservation. This interpretation supports a weathering alkalinity and nutrient change as the driver for the expression of the so-called "biogenic bloom" in this region. Second, the authors infer that monsoonal dynamics throughout the studied interval are dominated by eccentricity-modulated precession on orbital timescale. Third, the authors do not find an intensification of the South Asian monsoon over the late Miocene, as has been proposed by some previous works.

The Site U1443 proxy records in themselves are precious and already deserve publication in their own right. The three conclusions that accompany them are an important step toward a mechanistic and regionally-differentiated understanding of late Miocene monsoon dynamics on orbital and geologic time scales. I thus recommend this paper for publication in Climate of the Past after minor revisions. Indeed, I would like the authors to consider

my three major comments that could potentially make their paper even stronger.

### Major comments

[1] Throughout the paper, the authors filter precession with a Tanner-Hilbert filter with a bandwidth between 40 and 46 cycles/Myr (22 – 25 kyr periodicities). This bandpass is too narrow to encompass all relevant precession components (see Table 1).

**Table 1.** Frequency decomposition of the precession of the Earth's axis, using g frequencies from Table 3 in Laskar et al. (2004) and the precession frequency of the Earth  $p = 50.475838 \text{ arcsec yr}^{-1}$ . (p+g3) and (p+g4) are in red because they are important components of the precession frequency decomposition, yet they are not included in the used bandpass filter.

	"/year	cycles/Myr	kyr	Planet
p+g1	56.065838	43.26067747	23.1156805	Mercury
p+g2	57.927838	44.69740586	22.372663	Venus
p+g3	67.843838	52.34864043	19.1026929	Earth-Moon
p+g4	68.391838	52.77147994	18.9496296	Mars
p+g5	54.73329	42.23247685	23.6784597	Jupiter

p+g6	78.720838	60.74138735	16.4632394	Saturn
p+g7	53.563789	41.3300841	24.1954504	Uranus
p+g8	51.148859	39.46671219	25.3378086	Neptune
p+g9	50.125898	38.67739043	25.8548984	Pluto

The inclusion of the (p+g3) and (p+g4) terms in a precession-centred bandpass filter is important for the correct amplitude demodulation. This is because the four most important terms that compose short eccentricity involve (p+g3) or (p+g4).

**Table 2.** Frequency decomposition of the four most important terms in the short eccentricity evolution of the Earth's orbit. These four frequencies all involve either (p+g3) or (p+g4). When these terms are not included in a precession-centred bandpass filter, the short eccentricity terms cannot be extracted from the filter's amplitude demodulation.

	"/year	cycles/Myr	kyr
(p+g3) - (p+g2)	9.916	7.651234568	130.697862
(p+g4) - (p+g2)	10.464	8.074074074	123.853211

(p+g3) - (p+g5) 13.110548 10.11616358 98.8517032

(p+g4) - (p+g5) 13.658548 10.53900309 94.885635

The consequences of too-narrow precession filtering clearly appear in Figure 10. The amplitude modulation signals only exhibit low-frequency variations at the rhythm of the 405-kyr eccentricity cycle. The 405-kyr appears in the authors' amplitude demodulation because it is created by (p+g2)-(p+g5) and both terms are included in the 22 – 25 kyr precession filter. The 100-kyr terms however do not appear because they require the inclusion of the (p+g3) and (p+g4) terms into the precession filter. I would thus strongly recommend the authors to widen their precession filtering settings. This will markedly improve the results since it can already be recognized by eye that there are ~100-kyr amplitude modulation cycles embedded in the  $Ba_{xs}$  and  $\log(Ba/Fe)$  time series (as well as in the SITIG forcing of course).

[2] I find the obliquity peaks in the detrital proxies (Ti, Fe and Al) in Figure 8a-c intriguing. They do have about the same spectral power than the precession peaks. The authors briefly discuss the possibility that this result might indicate a decoupling between monsoon winds (driving productivity on precession timescales) and monsoon precipitation (terrigenous variability on obliquity timescales) [lines 617 – 621]. I would encourage the authors to explore this observation a little deeper. Does wavelet analysis show that obliquity primarily appears when eccentricity is low? Are there any modelling studies that corroborate this idea?

[3] The introduction nicely displays how there are two productivity peaks per year in the Bay of Bengal. This annual course creates the potential for half-precession cycles in the Barium-related productivity proxies. Indeed, one might expect productivity to be fuelled both during a precession minimum (stronger summer winds) and during a precession maximum (stronger winter winds). This potential is not discussed in the paper, yet the temporal resolution of the Ba proxies (<1 kyr) does allow the authors to report on the presence or absence of such cycles.

## Minor Comments

**Throughout:** A lot of acronyms are used. To my taste, a little too much. Please consider whether you could spell out some of them. For example: BOB, NER, SMC, MLD, NPP, ...

**Lines 60-65:** The use of X *versus* Y does not work well in all cases. I would recommend to spell out the contrast you would like the reader to consider.

**Line 91:** Also check out Ding et al. (2021), Climate Dynamics 56

**Line 136:** In ... In ... Delete repeated wording

**Line 177:** The geographic coordinates could be a little more precise.

**Line 185:** It is not exactly clear to me which splice has been used. There are two U1443 splices online on the IODP LIMS database, but both are already more than 5 years old. The authors should make the affine and splice tables available in the supplements, or on Pangaea, or cite a reference where the splice is available.

**Line 204:** avoid subjective qualifiers like “small”

**Line 236:** Replace “high-resolution” by “~1 meter resolution”

**Line 345:** What exactly is meant by “spectral analyses ... on filtered records”. Why would one do bandpass filtering prior to spectral analysis in this case?

**Line 371:** The y-axes of the phase graphs are not very helpful, and even a bit misleading. Please cut them off at -180° and +180°. Of course, confidence intervals can go beyond this range, but it should be clear that -180° = +180° = anti-phased behaviour.

**Lines 444 – 456:** I miss a statement here about the step-wise character of the MAR series. It should be acknowledged that these steps in MAR are related to age-model-induced stepped sedimentation rate changes.

**Line 544:** Section 5.3

**Line 1259:** Section 3.1