First of all, we thank the two anonymous referees for their constructive comments which helped us to improve our manuscript. In the following we give a step-by-step response to the referee comments (given in black, our responses are given in blue).

RC1-1: Review of revised manuscript "Signals of Holocene climate transition amplified by anthropogenic land-use changes in the Westerly-Indian Monsoon realm" by Burdanowitz et al.. The main thrust of this manuscript is to use high-resolution proxy records from the NE-Arabian Sea reflecting various aspects of Holocene monsoonal changes in the region. The main new data series include Uk37 based SST estimates, alongside a number of proxy time series (Ti/Al, endmember modelled aeolian input and lithogenics mass accumulation rates) reflecting lithogenic input in the region with the latter being of central importance. Based on these data the authors conclude that the Arabian Sea region around 4.6-3kaBP became more sensitive to changes in the tropical westerly jet controlling climate in the region. In addition, early agricultural activities in the region, may be reflected in the data. Compared to the initial version of the manuscript, there are areas of improvement in the revised manuscript. There are, however, still a number of issues which prevent recommending publication at this stage. Below there is a list of comments related to specific sections of the manuscript (line referencing based on version with track changes being highlighted). More generally, there are still too many occasions where the aspects focussed on the discussion are not properly described in the results sections and there seems to be some randomness with regard which events to discuss, despite the undiscussed events being of the same size. The results section should be expanded (including all the purely descriptive elements from the discussion). Also, the entire ENSO angle is rather unconvincing. It may help, in a revised version to remove this link and focus more on the change in vegetation.

The list below is in random order or importance/gravity:

Response: We thank the anonymous referee #1 for his/her constructive comments on our manuscript.

RC1-2: Lines 40-42 Can any of these things be proven. WD disturbances on which time scale do they occur? Can this be resolved in the existing data. How exactly are, probably short term changes in the STWJ affecting the ISM? This should be better explained.

Response: We have extended this part including a recent tracking algorithm study using ERA interim data (Hunt et al. 2018). The authors found 6-7 WDs per month during the past 36 years, with precipitation rates of about 3 mm/day and 40 mm/day for 95 % and 5 %, respectively. They also found a higher frequency of the WDs with a more southern position of the STWJ. We therefore think, that a latitudinal change of the STWJ during the past should have had a strong impact on the frequency of WDs and the amount of winter precipitation over Pakistan and India. We have added following to introduction of the manuscript: "*A tracking algorithm of ERA-interim data found about 3000 WDs (6 – 7 per month) between 1979 and 2016 in the region of Pakistan and India. About five % of the WD had precipitation rates of about 40 mm/day and the rest had about 3 mm/day (Hunt et al., 2018). Further, the frequency of WDs in Pakistan and India during the winter months was higher when the position of the STWJ was shifted to the south (Hunt et al., 2018). Therefore, a latitudinal change in the position of the STWJ during the past should have had a strong impact on the frequency of WDs and the amount of winter precipitation over that region.*"

RC1-3: Lines 130-131: It is not clear how this is supposed to work. First, what is the average and best resolution of the record. Second, there needs to be a much better explanation why it is justified (if it is) to "invent" data in a part of the record that does not have any measured data?

Response: It is necessary that the time steps are uniformly distributed to perform the wavelet power spectrum analysis using the Matlab Wavelet script by Torrence & Compo (1998). For that reason, we have used the highest temporal resolution of about 15.6 years existing between to samples and interpolated the data of the LIT MAR record to this time step. However, we are aware that this will lead to a high uncertainty, especially in the core region between 9.4. and 8.5 ka BP, where no data for LIT MAR exists. We have added further explanation to the manuscript: "***As uniformly distributed time steps are necessary to perform*** *the analysis we interpolated our data set to achieve a temporal resolution of about 15.6 years. This is the minimum time step between two samples and also covers the gap between 9.4 and 8.5 ka BP.* ***We are aware that the uncertainty may be high, especially during the gap period where no data for LIT MAR exist.***"

RC1-4: Line 143: 93 grain size classes between 10 and 3 mm??? - are there no small size classes in the sediments as would need to be expected in this region of the world ocean?

Response: There is a missing unit. It should be "between 10 **nm** and 3 mm". We have corrected it in the manuscript.

RC1-5: Line151-152: "High SSTs were registered between 8.1 and 5.2 ka BP during the Mid-Holocene." This is not correct. There is a short term cool period centred at ~7.8 Ka BP. Also, how is the averaging done. Is this just, as stated, a five point moving average (without any time control) or a time controlled filtering (it should be the latter).

Response: The referee #1 is correct that there is a short-term cool period about 7.8 ka BP visible with about 0.5°C cooler temperatures than before and after. However, the uncertainty of the Uk37 based SST record are about 1°C, which is why we have mainly concentrated on the time periods with a stronger and more rapidly emerging temperature contrast. We have used a simple five point moving average for both, SST and age.

RC1-6: Lines 217-220 The link between the 8.2ka event and the G. ruber isotope data is unclear. Also, the Ti/Al ratio change is small and not quite in phase (it seems). This sections would benefit from a rewording.

Response: We have rephrased this section and included the dates of changes of each proxy described. However, we are aware that there is no strong evidence of the 8.2 ka event in the core.

RC1-7: line 245 "indicated by..." The end of this sentence does not make sense.

Response: The referee #1 is correct. There is a left over. We have rephrased this part to "*which is indicated by salinity sensitive foraminifera*".

RC1-8: lines 248-250 The entire discussion surrounding SST/d18O-data is based on marginally significant data (i.e. most of the signal in either the Uk37 and the d18O ruber data is within the uncertainties for either method). There is some change in those records, but the discussion should also reflect that this is based on a small signal.

Response: We agree with referee #1 and have added the following sentences to the discussion "*Although considering both variations of the SST and δ18O records may be partly within the methods uncertainties, we are convinced that they show a climatic signal. They are independent of each other and, in addition, the EM3 record shows a rapid change during that time.*"

RC1-9: Line 253 It is unclear how a climate change in one direction leads to a two (opposite) changes in the early civilizations. Better to remove the link from the study or explain better (interpret jointly with other data in a section on anthropogenic change?).

Response: We follow the suggestion of referee #1 of removing the link to the study and deleted "*and coincides with the rise and fall of the Indus civilization (Wright et al., 2008).*"

RC1-10: Line 257 The statement is not correct. The LitMar data over the last 3ka are generally low with some superimposed spikes.

Response: We agree with referee #1 that the LIT MAR is not "high and variable" like the SSTs. To avoid confusion and misunderstanding we have rephrased the section to *""… high and variable SSTs, highly variable LIT MAR with strong peaks, and..."*.

RC1-11: Lines 278 and following: The entire argument surrounding cross-equatorial insolation gradient as a driver for short term change in the record is weak. As the authors noted with regard to summer insolation, smooth changes in the latter are very unlikely to be the main cause for short term change. The gradient records are equally smooth and it is therefore not clear how these help to explain short term change in core SO90-63KA.

Response: We totally agree with referee #1 that the smooth changes of the gradients alone are not the cause for the short-term changes in the record. However, as we noted in the manuscript there is a tipping period between 4.6 and 3.5 ka BP marking a transition period from an ISM dominated climate system towards one which is more influenced by STWJ. This is a cause of the different seasonal changing insolation gradients. Therefore, a stronger teleconnection to the North Atlantic climate and Bond events (short term) are found for the last 3.5 ka BP in our record. To add clarity, we have added "***This may be the cause of the different seasonal (summer and winter) LIG changes and*** *strengthened the teleconnection between the Makran coastal climate and climate variability in the North Atlantic, which is best illustrated by the link between the LIT MAR record and the Bond events during the last 3.5 ka (Figure 2).*" to the manuscript.

RC1-12: Lines 313-317 It is unclear which period is being referred to. In the first sentence a change between 4.2 and 3.5 ka is introduced. Thereafter, suddenly there is a reference to the period 7.5-3.5 kaBP -which is it?

Response: We have specified and rephrased this part to "*This* ***change of periodicities around 3.5 ka BP*** *indicates that the Makran region was more influenced by the mid-high latitude climate during the past 3.5 ka BP than during the Mid-Holocene*.".

RC1-13: Lines 338-354 This, again, is an unconvincing section. My understanding is that the authors try to discuss a non-finding, i.e. that there is not signal in the data that matches the ENSO signal. It would be better to remove the ENSO link entirely. First, in modern climate the link between ENSO and the ISM strength is complicated and not linear. Second, the change happens on times scales beyond the scope of the study and thirdly, the data of core SO90-63KA do not support a link. It therefore does not seem to make much sense to include a rather long discussion surrounding ENSO in the manuscript.

Response: The referee #1 is absolutely right that there is no link to ENSO as we have described it in our manuscript. We are also aware, that there is an ongoing debate for the present and also the past. Although there is no link between ENSO and our data, there are paleo-studies within the IM realm showing a link to ENSO (see lines 319 -320 "*Several studies have suggested ENSO as an important driver modulating the climate in the IM realm during the Holocene (Banerji et al., 2020; Munz et al., 2017; Prasad et al., 2014, 2020; Srivastava et al., 2017).*" We think, that it is not only important to describe possible links to climate drivers, such as ENSO or NAO, supported by data but also when there is no support by data.

RC1-14: As indicated above, the manuscript has improved but it is still not in a state supporting publication at this stage. A further revision is recommended.

Response: We thank the referee for his/her critical comments and have carefully and critically revised our manuscript based on the referee's comments.

RC2-1: The manuscript has been clearly improved, both in terms of its structure (with a "data results" section that was missing in the original version) and content (with a clarified and much improved discussion).

Yet, I still have some questions/commentaries and suggest that the manuscript could be accepted but with additional minor improvements.

Response: We thank the anonymous referee #2 for his/her constructive comments on our manuscript.

RC2-2: LIT MAR: Considering the key importance of the LIT MAR record in the discussion, this proxy deserves an in-depth presentation in the manuscript. Instead of just providing the final, computed LIT MAR record, I would like to see also a figure with the bulk data that have been used to construct this record, namely: (i) the dry bulk density record, (ii) the non-carbonate % and (iii) the sedimentation rate record derived from the numerous 14C dates (with the location of those dates along the core). This could be done in the main manuscript, or in the supplementary material.

Response: We agree with referee #2 and have added the lithogenic content, sedimentation rate and dry bulk density to the supplementary material (Figure S1, see answer to RC2-3). We also have included the LIT MAR record and all ^{14}C dates in Figure S1.

RC2-3: Not only should the bulk data for LIT MAR be provided to the readers, but they should also deserve an in-depth discussion too. It is striking that some of the main LIT MAR events described in the text (eg. LIT MAR peaks at ca 8.4 ka, ca 3.4 ka and ca 2.8 ka) are just defined by 1 or 2 data points only.. I would be interested to know what explains those extremely narrow and high amplitude changes (do they reflect peaks in sedimentation rate and a potential issue with dating? Do they reflect peaks in DBD or peaks in the lithogenic material %? Do they result from a combination of those three elements?). How confident can we be regarding those peculiar peaks? How sensitive is the Wavelet Analysis to the presence of such short, high amplitude peaks?

Response: We have followed the referee's suggestion and have provided further discussion regarding the LIT MAR record. The LIT MAR record is mainly controlled by the sedimentation rates and less by the dry bulk density and the lithogenic content itself (see Figure S1). This is the case for LIT MAR peaks around 8.4 ka, 3.4 ka and 2.8 ka BP. The LIT MAR peak around 1.4 ka BP coincides with slightly increased sedimentation rates and increased bulk density.

We have added following sentence to the results section (3.3): "*LIT MAR is mostly controlled by the sedimentation rate of SO90-63KA and less by the dry bulk density and the content of lithogenic material (Fig. S1), although the lithogenic material content slightly increases from the Mid- to Late Holocene.*" and to the discussion: " *However, this signal is not very pronounced in the SO90-63KA record and the peak around 8.4 ka BP in the LIT MAR record is mostly controlled by the sedimentation rate (Fig. S1).*" and: "*The highly variable LIT MAR record might be a combination of variable sedimentation rates and dry bulk density. For instance, the high LIT MAR around 3.4 and 2.8 ka BP coincide with high sedimentation rates but not a strong change in the dry bulk density or lithogenic material content (Fig. S1). However, the high LIT MAR around 1.4 ka BP coincides with slightly increased sedimentation rates and increased dry bulk density.*"

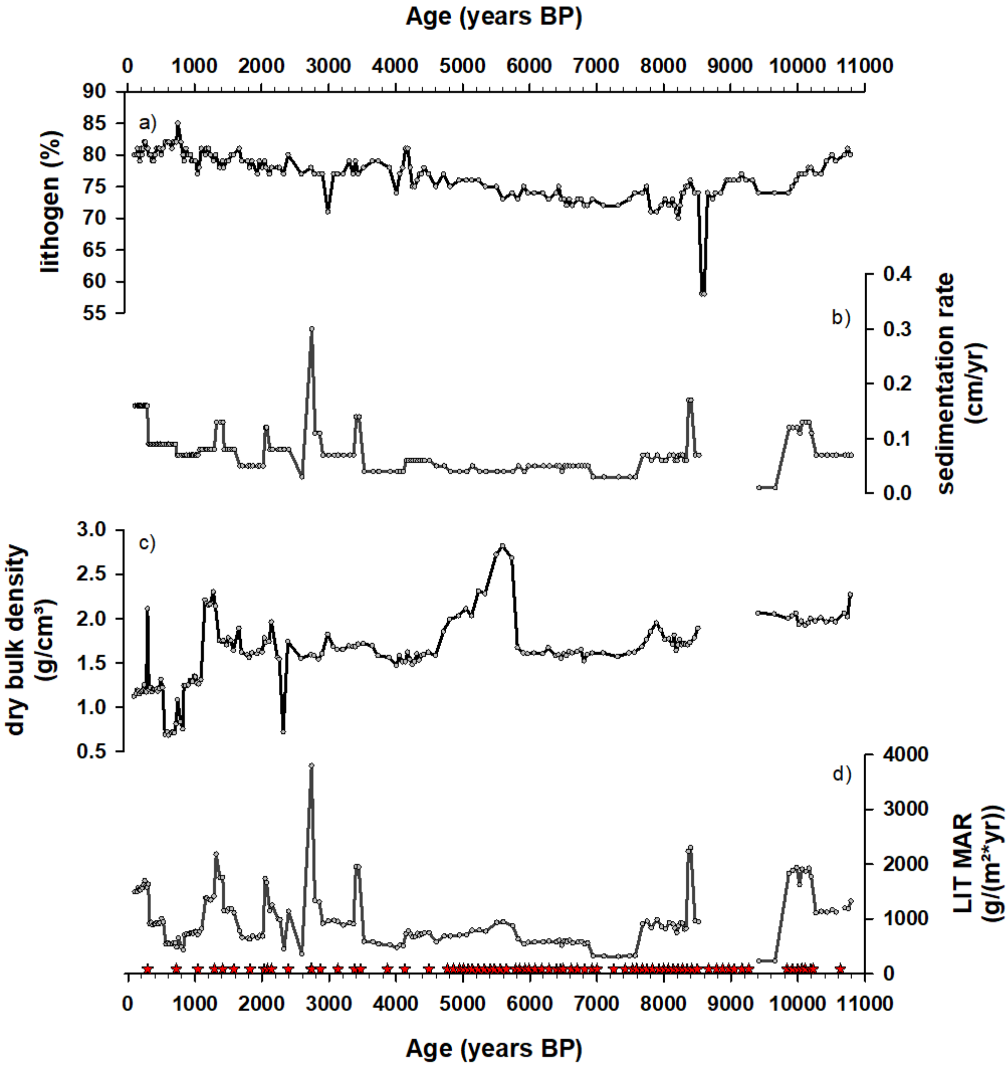


Figure S1: a) Content of lithogenic material, b) sedimentation rates, c) dry bulk density and lithogenic mass accumulation rates (LIT MAR) of SO90-63KA. Red stars indicate ^{14}C dates of planktonic foraminifers (Staubwasser et al., 2002, 2003).

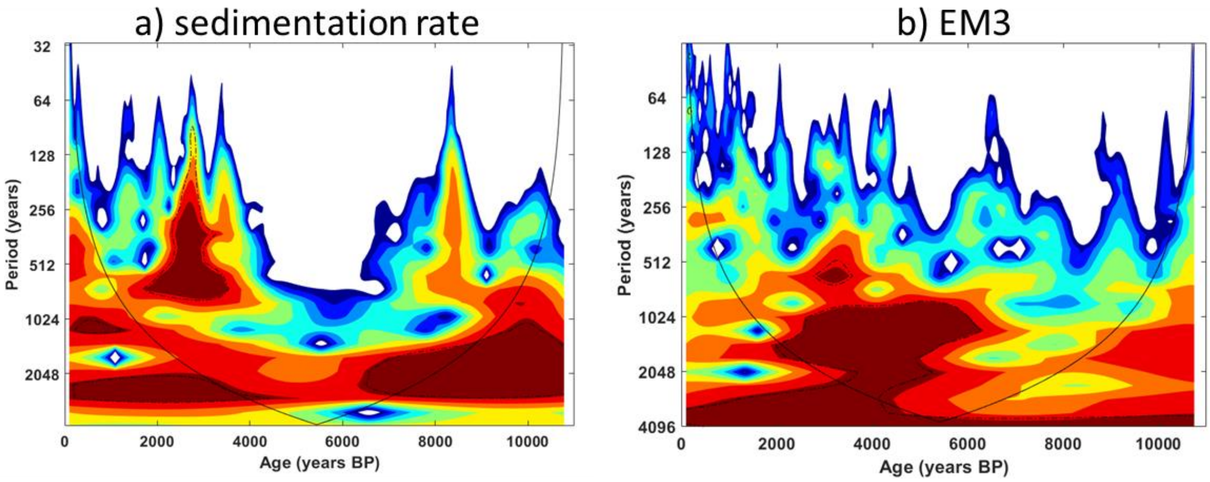
RC2-4: Line 143: the authors state that the last 3 ka show more variable SST that before. Couldn't this be - at least partially - attributed to the fact that the temporal resolution is also higher over the top 3 ka than in some older parts of the record ? I counted, for instance, about 20 data points in the SST record over the 4.5-6.5 ka interval, but ~ 40 points over the 0-2 ka time interval, thus twice as much. Change in sampling resolution should have an effect on our capacity to resolve rapid SST variations. The d18O record, which displays a more constant sampling resolution along the core than the SST record, suggests a relatively similar/invariant type of periodicity across the entire Holocene. Was this record run through Wavelet Analysis? How this compares with the LIT MAR spectral evolution?

Response: We agree with referee #2 that more variable SSTs over the last 3 ka BP than before could be due to the higher temporal resolution in that part of the core. For that reason we have deleted this statement. We have also performed the wavelet analysis for the SST record, but did not see a significant change in the signal.

RC2-5: Wavelet analysis: I wonder why wavelet analyses were not performed/shown for the other proxies as well. Is LIT MAR the only proxy that reveals a shift in periodicity across the Holocene? Shouldn't we expect to see also a change in periodicity in the EM3 record, for instance? Is it seen when performing wavelet analysis?

Response: We performed the wavelet analysis also for other proxies but have focused on the LIT MAR as it shows the clearest signal. For instance, the sedimentation rate show periodicities of about 512 years between ca. 3.5 ka and 2 ka BP (see figure panel a) below) as LIT MAR. However, in contrast to LIT MAR (see figure 3 in manuscript), the periodicities of 1024 to 2048 years are not significant (on a 95% significant level) until 3.5 ka BP but only ca. 6.2 ka BP for the sedimentation rate. The EM3 (see figure panel b) below) show significant (on a 95% significant level) periodicities of about 1024 to 2028 between about 6 and 2 ka BP.

We did not show all wavelet analyses, as we want to focus on the most, in our opinion, important one and do not want to "overload" the manuscript to keep focus on our main statement.



Wavelet power spectrum for the a) sedimentation rate and b) EM3 of SO90-63KA using a Matlab Wavelet script (Torrence and Compo, 1998). The black line indicates the cone of influence, the dashed black line the 95% significance level.

RC2-6: Figure 2: Is the “factor 1 – SST” (figure 2b; Giosan et al.) used and discussed in the text ?? I couldn’t find a call to this figure 2b in the text.

Response: We have cited Giosan et al. 2018 several times in the earlier version of the manuscript, but did not explicit mentioned that record. However, we agree with referee #2 that there should be a reference to Figure 2b in the discussed text as we show the record in that figure. Therefore, we have added “*Our Holocene alkenone based SST record shows a similar pattern as the nearby planktic foraminifera DNA based SST/winter monsoon record of core Indus 11C near the Indus River mouth (Giosan et al., 2018) (Figure 2b).*” to the manuscript.