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 manuscript “Holocene climatic changes in the Westerly-Indian Monsoon realm and its anthropogenic impact” 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. Overall there may well be something interesting in this manuscript, but in the current state it is not clear what this finding/story actually is. These are just a few problems.

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

RC1-2: In my judgement, the biggest issue is the combination of results and discussion. In the absence of a dedicated results chapter the text rather selectively describes certain findings, whilst (largely) ignoring others.

Response: We agree with referee #1 and also referee #2 who have raised concerns about the combination of results and discussion sections. We have rewritten the “results & discussion” part and have divided it into two separate sections. We also agree with both referees that this highly increases the readability and flow of arguments of our manuscript.

RC1-3: As an example, the authors place emphasis on relationship of the early Holocene rises in the LitMAR record (ignoring at this stage that there is a gap in the record near the time period being discussed) and Bond event 5. Later in the manuscript, again, it is being emphasised that there is a relation between the final Bond events and variability in the LitMAR record. What about the Bond cycles between 7.5. and 3.5kaBP. There is no relation in my view between these cycles and the LitMAR record, which in this interval has no obvious signal. What is this mismatch driven by, the alleged climatological connection between the Bond events and sedimentation in the Arabian Sea, or is the LitMAR proxy not sufficiently sensitive?

Response: We thank the referee #1 for stressing this mismatch, which was not properly explained in our manuscript. We are aware that we cannot exclude enhanced LIT MAR within the gap period (9.4 to 8.5 ka BP). However, between the gap period and the enhanced LIT MAR around 8.3 to 8.2 ka BP two low data points suggest to us that this period of enhanced LIT MAR may a short event caused by high soil erosion around the 8.2 ka event. We have added this information to the manuscript. The referee #1 is correct that there is no apparent relationship between the Bond cycles in the time interval 7.5 to 3.5 ka BP and the LIT MAR. This is in our interpretation an additional support for our hypothesis that the period between 4.6 and 3.0 ka is a transition period from low to mid-high latitude influence. We argue that the Bond event signal and the climate variability in the North Atlantic, respectively, have a stronger teleconnection to the Makran region, when STWJ is located more to the south. Therefore, the Makran region is more influenced by the mid-high latitude climate during the past 3.5 ka than during the Mid-Holocene (ca. 7.5 to 3.5 ka BP). Thus, the ISM and the low latitude climate were dominant drivers in the Makran region and suppressed the influence of the Bond events during the Mid-Holocene. We stressed the teleconnection between the mid-high latitude climate and the Makran region in the previous version of the manuscript (lines 210 -213):

"*We suggest that the time period between 4.6 and 3 ka BP marks a transition from an ISM-dominated climate system towards one which is more influenced by the STWJ. This strengthened the teleconnection between the Makran coast and climate variability in the North Atlantic, which is most visible by the link between the LIT MAR record and the Bond events since the end of the time period and associated fall the Indus civilization (Figure 2).*"

and (lines 284 - 287)

"*Since the transformation of natural into cultivated landscapes favors soil erosion, it is likely that this early human land-use change intensified the impact of the NAO and Bond events on the sedimentation in the NE AS during the late Holocene. The latter in turn documents a clear shift of the climatic system that was associated with the collapse of and deep crises of Late Bronze Age societies in the Mediterranean, Middle East and East Asia.*"

However, since a clear discussion about the teleconnection of Bond events and LIT MAR during the Mid-Holocene period was lacking in the previous version of the manuscript, we have now included it in the discussion.

RC1-4: With regard to the wavelet power spectrum, I am not convinced of the usefulness in this case, the main problem being the lack of a clearly visible signal in the LitMAR record between 7.5-3.5 kaBP. How does that affect the overall analysis? Also, it would help to inform the reader of the main findings based on this analysis (in the main text) rather than just alluding to change in frequency.

Response: We agree with referee #1 that we have to include the main findings of the wavelet power spectrum analysis to better understand the visible and hidden signals. For this analysis we used a Matlab script and 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. The advantage of the wavelet power spectrum is that it shows how the periodicities differ/change during the time. For instance, there are time periods that do not show a specific periodicity signal. In our LIT MAR record, this is the case for the periodicity of about 512 to 1024 years observed during the last 3.5 ka compared to the prevalent periodicity of about 2048 to 3000 years in the time period between 7.5 – 3.5 ka BP. The absence of these shorter periodicities during the Mid-Holocene is a further evidence that Bond events (cycle length roughly of about 1500 years) and, therefore, mid-high latitude climate have played a minor role in the Makran region during that time. We have added further description to the results and the discussion section to highlight the findings of the wavelet power spectrum analysis.

RC1-5: There are quite a few statements that lack clarity regarding the implied change in the monsoon system and therefore appear contradictory. As an example in lines 154/5 there is this statement “This warm period encompasses the Mid-Holocene climate optimum period and is characterized by low LIT MAR and increasing fluvial input (Figure 2)..” Would increasing fluvial input not entail higher lithogenic sedimentation rates? If so, how does this compare the overall low LitMAR record?

Response: We agree with referee #1 that some statements were not clearly explained. As referee #2 has the same issue regarding the contrasting behaviour of fluvial input (Ti/Al ratios) and LIT MAR (indicator for soil erosion), we have added further text to the discussion to explain our reasoning. The LIT MAR is decoupled from the Ti/Al records as it is an indicator for soil erosion and is not necessarily coupled to only fluvial input, but also to aeolian input. Arid conditions are necessary for enhanced soil erosion to occur, because vegetation prevents soil erosion. But also during arid phases there can be flood events triggered by precipitation events, that cause a strong fluvial sediment transport. This is, for instance, also evident in the Namaqualand mudbelt sediments offshore western South Africa (Herrmann et al. 2016). We posit, that the combination of pronounced soil erosion, flood events and strong winds during arid phases led to the high LIT MAR in the late Holocene part of our record.

RC1-6: Similar inconsistencies regarding the general state of the monsoon circulation can be found elsewhere in the manuscript. With regard to the LIG approach there is not sufficient justification provided why the chosen gradients are the most appropriate. There have been other approaches (on different time scales) such as by Reichart who has used a different gradient. There should be a better explanation as to the reasons for choosing the LIG’s.

Response: We agree with referee #1 that the different approaches and concepts regarding the latitudinal insolation gradient (LIG) are not properly explained in the manuscript and have now added further information. In general, there are two different concepts for the LIG. Reichart (1997) and Bosmans et al. (2015) argued that the boreal summer cross-equatorial insolation gradient plays an important role for the inter-hemispheric moisture transport as well as the extent of the Hadley cell and may drive glacial-interglacial variability. The other concept by Davis & Brewer (2009) highlights the importance of intra-hemispheric insolation gradients during summer (driven by obliquity) and winter (driven by precession). These authors stated that intra-hemispheric LIG influences the strength of the monsoon system and its most poleward position as well as the position of the Hadley cell. For our study, we were interested in the northernmost position of the ISM and the southernmost position of the STWJ, because the core location is influenced by both systems. A study from the West African Monsoon region found a strong link of intra-hemispheric summer LIG (60° – 30°N) and strength of the West African monsoon (Küchler et al. 2018). Ramisch et al. (2016) found that the ISM variability over the Tibetan Plateau may be sensitive to the summer LIG between 44°N and the Himalayan barrier at 30°N rather than cross-equatorial LIG. Therefore, we have chosen the summer LIG between the equator and 30°N south of the Himalayan barrier (Ramisch et al., 2016). The STWJ reaches its southernmost position during the winter in the study area and the position of the STWJ ranged from 35°N and ~54°N during the last 6 ka (Fallah et al., 2017). Therefore, we have chosen the winter LIG between 30°N and 60°N.

RC1-7: Overall, there may well be something interesting in this paper. Currently, however, it lacks maturity and requires a substantial rewrite. There, should be a better separation between results (all) and the interpretation. In addition, the discussion should be “closer” to the actual data. Large parts of the text read like and general discussion with a loose relation to the actual observations. More could be said.

Response: We thank the referee #1 for his/her constructive comments which help us to improve the manuscript. Based on these comments we have separated the "results & discussion" part into "results" and "discussion" chapters. In addition, we have added information about the various proxies employed and their links to mid-high latitude climate and Bond events, respectively. We have rewritten parts of the discussion to better support our findings. We have also decided add additional data relevant for this manuscript as supplementary material. This includes results of grain size analyses and end-member modelling analyses as well as data on the *n*-alkanes (CPI_{27-33}, ACL_{27-33} and *n*-alkane distribution). Further we changed the title of the manuscript to "Signals of Holocene climate transition amplified by anthropogenic land-use changes in the Westerly-Indian Monsoon realm"

RC2-1: The paper submitted to Climate of the past by Burdanowitz et al. (Holocene climatic changes in the Westerly-Indian Monsoon realm and its anthropogenic impact), aims at providing new insights into how the potential interactions between ITCZ dynamics and Indian Monsoon, in the one hand, and Sub-Tropical Westerly Jets, in the other hand, may have driven orbital and millennial climate changes over the NE Indian monsoon area during the Holocene. Although this issue is clearly an important one, the discussion does not successfully reach its objectives because the manuscript gives a feeling of confusion and ad-hoc argumentation in many places.

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

RC2-2: The discussion is based upon six main sets of data obtained in core SO90-63KA, among which only three are (apparently) published here for the first time, and are given a thorough description in the method chapter (Lithological Mass Accumulation Rates, Uk37'-SST, and the average chain length of the n-alkanes homologues 27-33). The other data having been published elsewhere, the readers are left with only minimal to no piece of information about how these proxies were obtained and/or are interpreted. The lack of information is detrimental to a clear understanding of the authors' arguments.

Response: We agree with the referee #2 that we need to include more information about the data published earlier. We have added further information about the extended EM3 record and the previously published Ti/Al record in the method section and discussion, respectively. As also mentioned in the response to RC2-4 below, we realized that the citation of Giosan et al. (2018) for the Indus 11C record is missing in the figure caption and have added it now in the revised version of the manuscript. We are sorry that this led to confusion.

RC2-3: For instance, Ti/Al is interpreted, here, as being positively associated to higher river contributions (fig. 2), which is opposite to what has been concluded for sediments from the tropical Atlantic (Govin et al., 2012). Why is that so? Clearly elemental ratios should be interpreted in the light of regional/local rock sources, transport mechanisms, etc.. The basis for the interpretation of Ti/Al in the Arabian Sea should be summarized somewhere in the method chapter.

Response: Our interpretation of the Ti/Al record is based on earlier analyses (Lückge et al. 2001) of elemental ratios of marine sediments nearby our core site as well as fluvial sediments along the Hingol River, which adds to the sedimentation at our core site. They found high Ti/Al ratios in the fluvially deposited sediments along the Hingol River and also found high Ti/Al ratios in thicker varves of marine sediments in the NE Arabian Sea than in thinner varves. Lückge et al. (2001) concluded that high Ti/Al ratios indicate increased transport energy and therefore more intense fluvial discharge than low Ti/Al ratios. As mentioned in the response to RC2-2, we have added further information about the Ti/Al record and its interpretation in the method section.

RC2-4: I've had the same kind of issues with basically all the proxies used in the manuscript. How EM3 record was obtained? What does it mean? What about the planktic DNA ? Etc.

Response: As for the Ti/Al record we have included further information about the EM3 record, which is based on grain size endmember modelling, in the method section (see also response to RC2-2). Concerning the Indus 11C SST record based on planktic DNA (Giosan et al., 2018) we realized that the citation "Giosan et al., (2018)" was missing in the caption of Figure 2 b) and have added it in the revised version of the manuscript.

RC2-5: I was also surprised that LIT-MAR is given such an importance, given the fact that the core was retrieved thirty years ago. It is very likely that wet weights obtained for calculating DBD have been largely modified by evaporation since the core retrieval. The authors themselves point out that some part of the core completely dried out. To which extent can this drying impact the DBD and does that have a significant effect on LIT-MAR estimates?

Response: The referee #2 has a valid point concerning about the impact of drying of core material on dry bulk density and LIT MAR. However, the core was well preserved except the core section between 547 – 597 cm. This section was dried-out and had shrunken by about 10%. During the core sampling we compared each core section with existing radiographies of this core and found that there was no apparent depth bias. Of course, there may be some evaporation effects for the whole core after thirty years and it is likely that there is a bias of the DBD. But as all core parts, except for 547 – 597 cm, were in the unchanged condition it is unlikely that differences in the LIT MAR are due to punctuated drying artifacts.

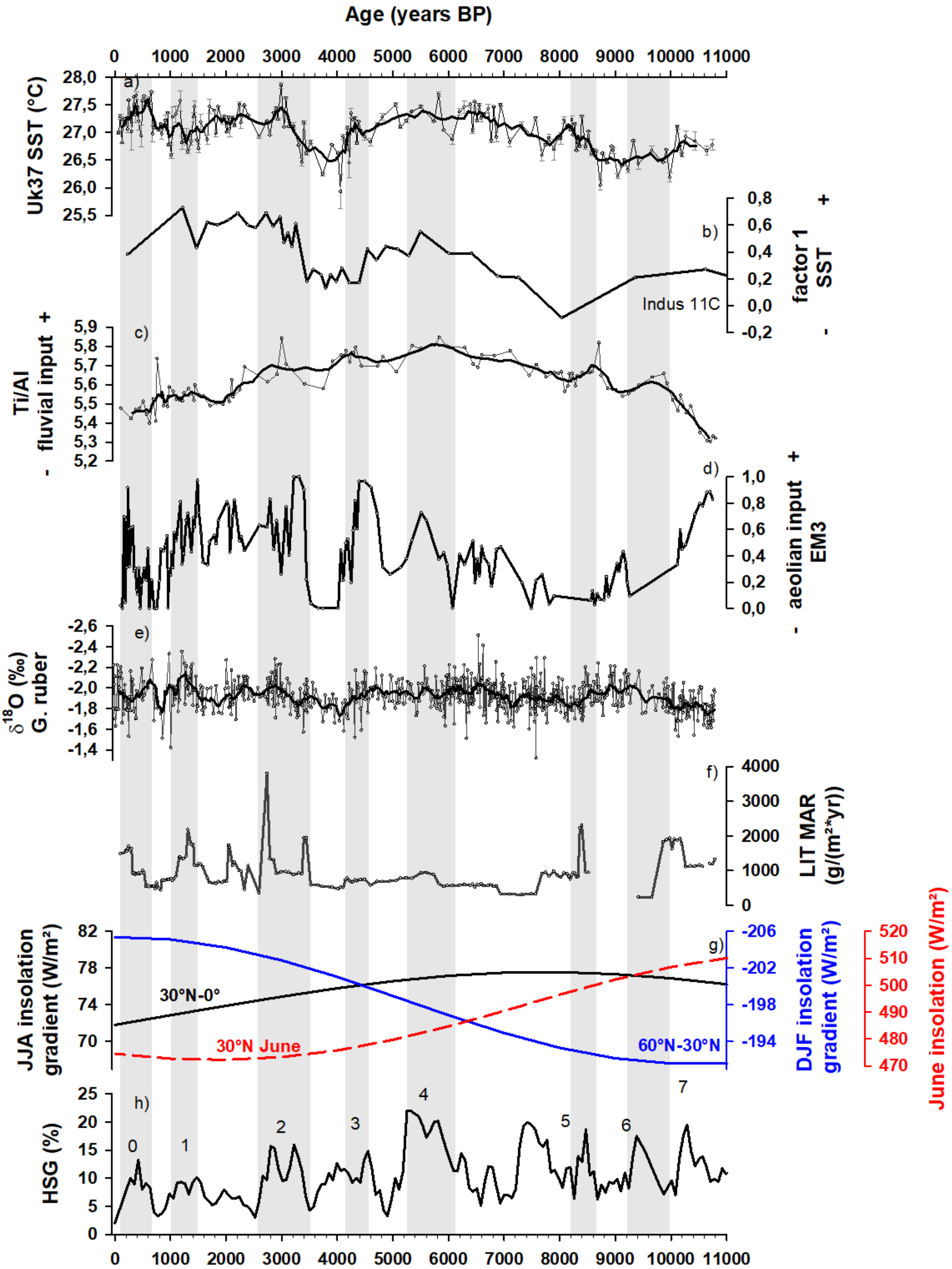
RC2-6: Because the data are not presented in a dedicated "result" chapter prior to the discussion, one has the impression that the authors build their interpretation by jumping from one proxy to another, highlighting the patterns that suit their hypothesis. Every now and then they even appeal to important data, not presented in the article.

Response: We agree with both referees who objected to combined results and discussion sections. We have rewritten the "results & discussion" part and have divided it into two separate sections. We also agree with both referees that this highly increases the readability of our manuscript and makes it much easier to follow our arguments.

RC2-7: This is the case for G. ruber δ18O, which they cite to strengthen their argument on past changes in precipitation and river runoff. If the G. ruber δ18O record has already been published and can bring interesting pieces of information about salinity (precipitation, runoff) and temperature changes, it should be shown in the present manuscript and thoroughly compared with Uk37' SST and Ti/Al records . . . Not used to highlight just a specific feature observed in the Ti/Al record.

Response: We agree with referee #2 and have added the G. ruber δ18O record of Staubwasser et al. (2002, 2003) to figure 2. We decided against including the G. ruber δ18O record of Giesche et al. (2019) in figure 2, because Giesche et al. (2019) reported δ18O of G. ruber from a core interval corresponding to the time period of 3.0 to 5.4 ka BP of a different size fraction (400-500 µm) than Staubwasser et al. (2003, 315-400 µm). Although there is an offset by 0.23 ‰ between these two data sets, the 210-year smoothed trends of both records are in good agreement. Due to the short time period analysed by Giesche et al. (2019) this record was not included in the figure, bus has now been cited.

New figure 2:



RC2-8: The lack of a thorough discussion about the data also results in some key features of the records not given the proper attention. What about, for instance, the long-term change in the Ti/Al record, which amplitude contrasts with the small amplitude of the millennial-scale variations? Why is the LIT-MAR record showing a rather opposite mode of variability (ie. lack of long-term mode of variation, short episodes of higher MAR)? Why are the Ti/Al and LIT-MAR so evidently decoupled from one another?

Response: We agree with the reviewer that the discussion of some results lacked proper depth. The long-term trend of the Ti/Al record was already described by Burdanowitz et al. (2019), but we have added a more detailed description to this manuscript. The strong increase of Ti/Al ratios between about 11 ka to 9 ka BP may be due to a combination of the strengthening of the ISM and rising sea-level causing a more proximal core position and an altered fluvial input. During the Early and Mid-Holocene the Ti/Al record was mainly affected by Indian Summer Monsoon precipitation. From the Mid- to Late Holocene the influence of westerly induced precipitation on the Ti/Al gradually increased compared to the influence of the Indian Summer Monsoon. This combined Indian Summer Monsoon and westerly influence resulted in increased precipitation and therefore increased fluvial input, peaking during the Mid-Holocene. Conversely, a general aridification trend led to generally decreased fluvial input during the late Holocene. A spectral analysis of the Ti/Al record has shown a cycle of about 1425 years, similar to the Bond events, that is superimposed on this long-term trend (Burdanowitz et al., 2019). The LIT MAR is decoupled from the Ti/Al records as it is an indicator for soil erosion and is not necessarily coupled to only fluvial input, but also to aeolian input. Furthermore, more arid conditions promote soil erosion, because vegetation that prevents soil erosion is on the retreat. However, flood events resulting in a strong fluvial sediment transport also occur during arid phases. This is, for instance, also found in the Namaqualand mudbelt sediments offshore western South Africa (Herrmann et al. 2016). We assume that the combination of stronger soil erosion, flood events and also strong winds led to the higher LIT MAR during the arid phases of the Late Holocene in our record (see reply to RC1-5).

RC2-9: All the above questions about proxy interpretation and comparison should be addressed in the manuscript.
Thus, the manuscript needs a thorough rewriting with (i)added pieces of information about the proxies signification and interpretation; (ii) and a dedicated “results” chapter in which records are presented thoroughly before being referred to in the discussion. This should serve as a basis for a more organized and clearly argued discussion.

Response: We thank the referee #2 for his/her comments that helped to significantly improve our manuscript. As mentioned in the previous responses, we have added previously missing information on rationale for proxies to the manuscript and also as supplementary material, and have now divided the “results & discussion” chapter into two chapters.