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

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.

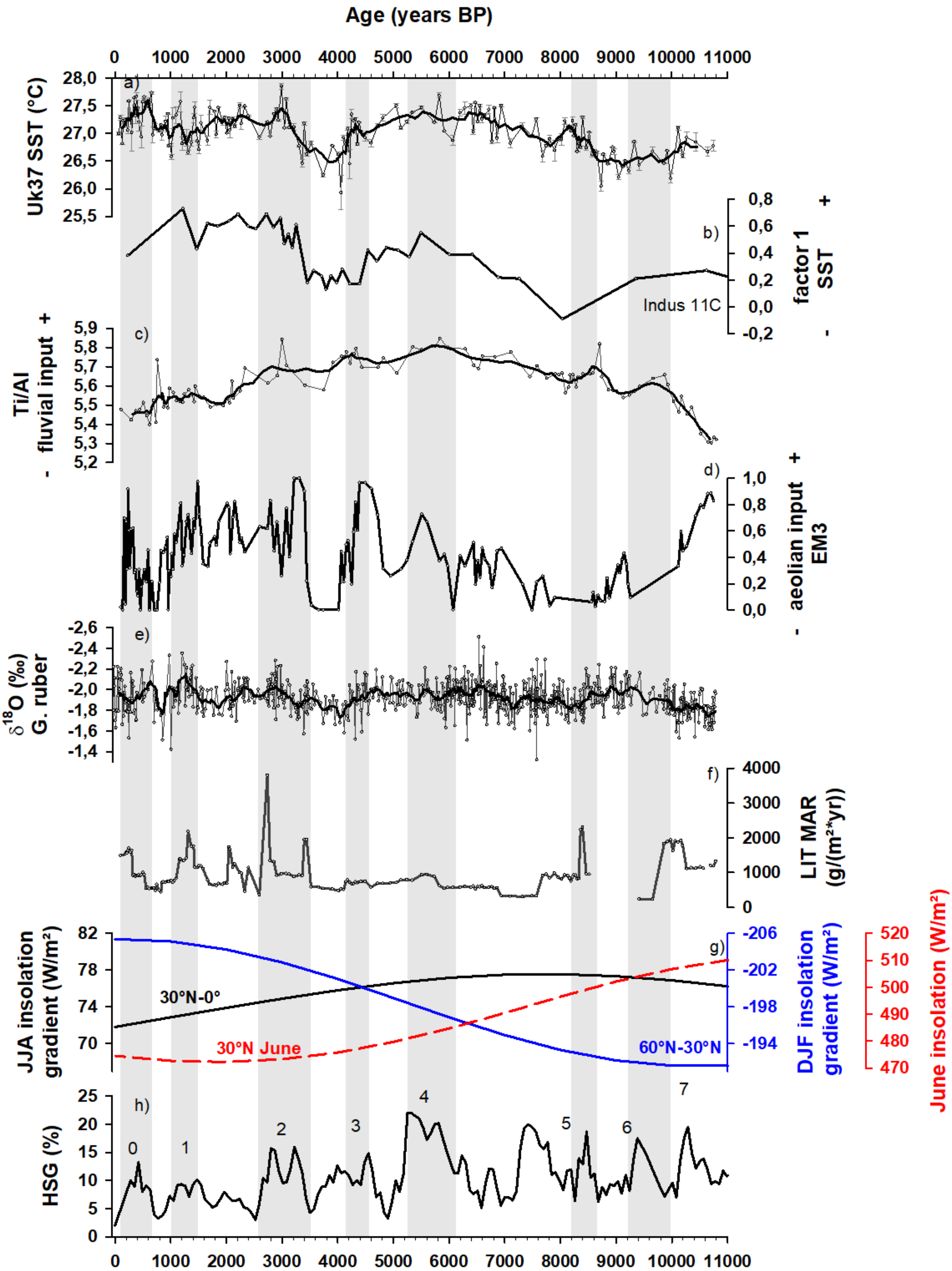
RC2-6: Because the data are not presented in a dedicated "result" chapter prior to the discussion, one has the impression that the authors build their interpretation by jumping from one proxy to another, highlighting the patterns that suit their hypothesis. Every now and then they even appeal to important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