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Interactive comment on "Decadal resolution record of Oman margin upwelling indicates persistent solar forcing of the Indian summer monsoon after the early Holocene summer insolation maximum" *by* Philipp M. Munz et al.

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We would like to thank the two anonymous referees and Sebastian Luening for their constructive comments, their time and effort which helped to improve the quality of the manuscript. The main issues, as expressed by both anonymous referees, is concerning the age model and significance of the data presented in our study. We are aware of these points and added a more critical discussion about the age model and significance of our data in the revised version of the manuscript. Below is a point-by-point statement of the reviewer comments and our responses supplemented with a revised

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version of our manuscript indicative of the changes made to the text.

Authors response to referee #1 1) There are two age reversals within the sediment core. It is okay, if you can fit a smooth spline model to it which results into continuous depositions rates. However, to my opinion, considering the sample resolution of about 19 years and the observation of the Gleissberg cycle this needs some more discussions.

> We thank the reviewer for this comment and added a more critical discussion of potential artefacts from the age model. The offset of the reversals is relatively low (<70 and <40 years) and we therefore hypothesize that either changing upwelling intensities or changes in intermediate water mass, as it was shown from the Peruvian upwelling (Fontugne et al., 2004), or bioturbation processes ages could be responsible for the slight offset of the radiocarbon dates (see p.8, l. 3-8). However, as the two age reversals are no clear outliers from the smooth spline fit and we did not find evidence for interruptions of the sedimentary succession (see response to comment below), we are confident that our age model represents a continuous deposition. Further, we are aware of the overall error of the reservoir correction factor of $\pm$31 years being close to the observed frequencies (75-95 years and 80-90 years) of the supposed Gleissberg cycle. However, we also found a 110-130 year cyclicity in the Mg/Ca-SST record, which is close to another prominent sunspot cycle of $\sim$132 years, probably a subharmonics of the Hale cycle (Attolini et al. 1990). We added this to the discussion on p. 11.

2) Many studies in the past recent years have demonstrated the impact of solar forcing on paleoceanographic and climatic records (e.g Moffa-Sanchez et al. 2014; Knudsen et al. 2011). Total solar irradiance (TSI) is controlled by different cycles such as the shorter Gleissberg (87 ys) and the longer de Vries (210 ys) cycle. The latter is not dominant in the present records. I wonder why the spectral analyses reveals the shorter Gleissberg and not also the de Vries cycle as this was clearly shown by other studies (e.g Steinhilber et al., 2012; Moffa-Sanchez et al. 2014 etc). This may give a hint that this is a statistical artefact as discussed by Turner et al. (2015), especially for cycles

ranging between 120-140 years.

> The apparent absence of a ∼200-year cyclicity is a common finding also known from other studies of Asian monsoon records. Studies using stable isotopes off Japan (Sagawa et al., 2014) and in the South China Sea (Wang et al., 1999), found cycles in their data matching cycles of the TSI record, except the 210-year De Vries cycle, although being very prominent in the TSI record. Duan et al. (2014) showed with a speleothem record from Dongge cave (China), that coherency with the Gleissberg cycle is persistent over the last 4 ka, whereas the De Vries cycle occurred only over a short 1 ka long period. We therefore hypothesize that the De Vries cycle is not detectable in our record, due to a transient link of the solar-monsoon relationship, potentially suppressed by changes in the ocean-atmosphere system in the ENSO domain (Berkelhammer et al., 2010). We added this to the discussion on p. 11, l. 5-14. However, our Mg/Ca-based SST record shows strongest coherency with the SSN record of Solanki et al. (2004) on both, the ∼88-year Gleissberg and the ∼190-230 year De Vries frequency, as shown in Fig. 7e. See p. 11, l. 18

3) The authors conclude that atmospheric forcing (solar forcing) is the origin for OMZ dynamics rather than intermediate water mass dynamics. Also modelling results reveal a response of intermediate water masses to solar forcing (Seidenglanz et al. 2012). However, to state something like this the authors should compare their record to other paleoceanographic records (if available at this resolution) and not only to stalagmite records.

> We thank the reviewer for the constructive suggestion. We added a comparison with other high-resolution records from the Arabian Sea (Staubwasser et al., 2003 and Deplazes et al., 2013) to the revised version of the manuscript (see Fig. 7 and p. 10, l. 18)

Other comments on the manuscript: Page 2 Line 18: There are studies revealing SST variations probably forced by changes in total solar radiation in the North Atlantic

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(Moffa-Sanchez et al. 2014).

> We thank the reviewer for pointing out this ambiguity. Of course we refer here to SST records from the Arabian Sea, which are, to our knowledge, not available at a decadal-scale resolution. We clarified this in the text.

Page 3 Line 10: What are the oxygen concentrations? Line 14: What are the salinities?

> We added oxygen (0.07-0.52 ml/l) and salinity (35.5-36.8 psu) values of the intermediate waters from the literature (Emery and Meincke, 1986; You, 1997) on p. 3, l. 19 and 21.

Page 4 Line 10: It is not appropriate to cite only the website you should refer here to the original study.

> We thank the reviewer for this comment and refer now directly to Southon et al. (2002) and von Rad et al. (1999).

Page 4 Line 7-13: I am a bit worried about the error of the deltaR as the authors claim to see the Gleissberg cycle of about 87 years, which is nearly the same compared to the overall error of the deltaR.

> Please see the response to first major comment.

Page 5 Line 24: I think the ECRM 752-1 should read 3.761 (Greaves et al. 2008). As the authors discuss Mg/Ca based SST variability of less than 2 degC could the authors provide an error for the temperature reconstruction?

> The ECRM value of 3.75 is correct, 3.761 was a value after removal of one result with a lower value, but without any indication that it was a wrong measurement (Table 4 in Greaves et al. 2008). We also added the error estimation using error propagation (Mohtadi et al., 2014) to the methods section, which was previously mentioned in caption of Fig. 6. The overall average of the propagating 1-sigma error is 0.89 degC.

Line 32: What do the correlations say between the individual elemental/Ca ratios against Mg/Ca? What about Al/Ca and Fe/Mg ratios? As the authors discuss later Mn/Al ratios from bulk analyses it would be nice to know the variability of the Al/Ca ratios.

> We added the correlations of other element/Ca vs. Mg/Ca values to page 6, l. 5. Al/Ca (r=0.06) and Fe/Ca (r=0.16) are not correlated to Mg/Ca, Mn/Ca has weak correlations (r=0.36).

Page 6 Line 2: A fragmentation index not only tells us something about dissolution, but also about changing bottom water current strength. If there are strong currents at 600m water depth these might transport lighter particles, which in turn would indicate less dissolution. I do not believe this study has to tackle severe dissolution problems, but I think a fragmentation index is not an appropriate proxy for that.

> We agree with the reviewer and assume that dissolution did not have any effect on the samples, which is also supported by the presence of pteropods. Undercurrents related to the upwelling process, however, are probably shallower (<200m). We therefore removed the fragmentation index from the text.

Page 7 Line 10: Instability or strong bottom currents? Similar as off the Peruvian margin (Erdem et al 2016)?

> We did not find any evidence for interruptions of the stratigraphical record in the form of slumps, turbidites, erosional surfaces or phosphorites, below the unconformity at 56 cm core depth. We therefore assume that the record we present here is homogeneous and undisturbed. The diatomaceous ooze above the unconformity at 56 cm core depth of SL163 showed an excursive increase of the sedimentary water content, relative to section below 56 cm, of more than 17%. We therefore assume this might have caused a decrease of gravitational stability of the sediment. However, we feel the assessment of a potential mechanism for the observed hiatus above the interval we are focusing on is beyond the scope of this study and will be presented elsewhere.

Line 5: How do the pteropods look like? I think a better indication is the in situ carbonate saturation. There is data available to calculate the in situ _CO32-! Interestingly, there is a sharp increase in alkalinity at 1000m water depth, can this be explained by the dissolution of the pteropods? (Jansen et al. 2002).

> The paper by Jansen et al. (2002) is dealing about the North Pacific. The authors state, that carbonate/aragonite saturation is much lower there compared to the Atlantic and Indian Ocean. Thus aragonite dissolution can occur in the water column, as the depth where the in-situ carbonate saturation becomes <1 is shallower. However, as pteropds are always present in our record, except for 11 samples where they were probably diluted due to high concentrations of foraminiferal shells, there was no aragonite dissolution in the water column, nor in the sediment. We therefore expect that foraminifera are not affected by dissolution, as they build calcite shells, the more dissolution resistant polymorph of calcium carbonate compared to aragonite.

Page 10 Line 4: Can this statement be proven by a comparison to the cosmogenic nuclides record of Steinhilber et al. (2012)? Moreover, modelling results for the North Atlantic suggest that the phase shift of TSI on SST is about 40 years (Seidenglanz et al. 2012). What would you expect for your location? Can the authors comment on that?

> The study by Seidenglanz et al. (2012) revealed a response of Arabian Sea surface temperatures to an idealized TSI forcing within less than 20 years for both idealized cycles, 90 and 200 years, which is probably not resolvable by our 19-year record.

Line 10: The authors should cite here earlier studies that made similar observations.

> We agree with the reviewer and cited previous studies using speleothem (Neff et al, 2001; Burns et al., 2002; Fleitmann et al., 2003; Dykoski et al., 2005; Wang et al., 2005; Duan et al. 2014) and marine records (von Rad et al., 1999; Wang et al., 1999; Gupta et al., 2005).

Page 11: Line 3: Mn has been introduced, but what about Al? How does the Mn/Al downcore record look like? What is the variability of Al? Can the authors please clarify.

> We used the enrichment factor relative to the detrital background ("average shale" as reported by Wedepohl, 1971, 1991) to decipher a possible secondary alteration of the geochemical composition. The enrichment factor is calculated as the element/Al ratio of the sample divided by the element/Al ratio of average shale. Hence, element/Al ratio and enrichment factor are covarying. We added this clarification to the methods section.

Figures and tables Figure 9: This does not really look convincing to me. Maybe the authors should consider to show also a simple evolutionary spectra revealing the intensities of the present cyclicity through time.

> We added a wavelet power spectrum of both SST time series to demonstrate the evolution of the dominant signals through time.

Table 1. The error should read $\pm$

> We added the correct sign to the column header.

Authors response to referee #2 The authors themselves mention that a section of the top of the core is missing (section 4.1) alluding to slope instability as a possible cause. If so, is it possible that the sections with age reversals represent other periods of sediment instability (turbidites)? This should be discussed in more detail.

> We thank the reviewer for this comment and discuss possible disturbances of the stratigraphical record in more detail. Please see the answer to referee #1 comments.

The authors state that the overall time resolution across the entire section presented in this manuscript is around 20 years. Whilst undoubtedly true for the section younger than 7.5 KaBP, it is not true for the section covering the time period between 8.5 and 7.5 KaBP. The sample resolution in this section seems much closer to 50 years. This should be clarified.

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> We agree with the reviewer that the sample spacing is larger in the lower part of the record. The lowermost 15 cm of the core (six samples) span 287 years of deposition, in fact close to a spacing of 50 years. We added this for clarification to the text (p. 7, l. 22). However, the overall average sample spacing is 19.9 years for the Mg/Ca-based SST time series, 15.3 years in the upper half and 28.9 years in the lower half of the record. The assemblage-based SST record has a slightly higher average resolution of 18.4 years, as there were no samples removed due to potential contamination.

Statements related to highlighting differences in the spectral analyses results for different time periods should be double checked as well (see for example page 11 lines 16-18). To me, the fact that higher frequencies have not been recorded between 8.5 and 7.5 KaBP is likely a result of the insufficient time resolution in this section.

> We included a continuous wavelet transform of both SST time series, which shows significant periodicities of the Mg/Ca-SST record on the ∼80- to ∼130-year bandwidth within the lowermost part of the record (see Fig. 10).

There is a little bit of confusion related to a statement made on figure 8 (page 10 lines 4-6). Based on the text a rather strict relation between solar insolation and SST change has been found, i.e. solar insolation leading SST change by roughly 200 years. I may read figure 8 incorrectly, but does this figure not partly show the opposite relation. Between 7.7 and 6.7 KaBP for example my read of the figure implies that amplitude variation in SST (certainly for the assemblage based data) leads the respective change in solar insolation. Other sections of the record also do not show the alleged relation. This part of the manuscript should be revised. This should also have ramifications with regard to the relation of solar forcing and the response in the climate system.

> We agree with the reviewer that the statements made about Fig. 8 are misleading and that this section has to be revised. Our statements are mistakenly based on another version of the figure, which compares filter outputs of both SST time series with filter outputs of the reconstructed total solar irradiance based on cosmogenic nuclides

(Steinhilber et al., 2012). The version of Fig. 8 in the manuscript, shows solar energy based solely on orbital solutions. Our findings from spectral analysis and the following interpretation regarding solar forcing of the monsoon system is, however, focusing on the solar cycle and sunspot activity, which are expressed in the TSI record. We corrected and clarified this in the revised manuscript (p. 11, l. 26-33) and changed Fig. 8 accordingly (now numbered Fig. 9). Although there is a slight offset in some parts of the record, the new figure shows that the amplitude modulation of both SST time series filtered on the 88-year frequency shows a maximum centered at ∼7.4 ka B.P., which is co-varying with a modulation maximum of the 88-year band-pass filtered time series of TSI. On the 132-year bandwidth, however, the Mg/Ca-based SST time series and band-pass filtered TSI are offset by ∼400 years. Modelling results indicate a response of surface temperatures to solar forcing within less than 50 years (Seidenglanz et al., 2012). We therefore assume no direct forcing of Arabian Sea upwelling intensity from the ∼132-year solar cycle.

The authors spend quite a bit of text on the technical details related to SST analysis. It would be useful to know what the error bar is with regard to both SST estimates (I might have overlooked a statement on this). My best guess is that it is in the ballpark of 1-2 degC. Using such an uncertainty, quite a bit of the SST variability would not represent statistically significant change. With regard to the Mg/Ca based SST estimates the main thrust of this paper might change to work assessing specific short term events with significant changes in temperature (e.g. around 8.2 KaBP).

> Error bars for both SST time series are given in Fig. 6a/b. Error assessment for the Mg/Ca-based SST is based on error propagation (Mohtadi et al., 2014). It yields an average propagating 1-sigma error of 0.89 degC. We clarified this and added error estimation methods to the text (p. 6, l. 9 and p. 9, l. 5; previously only found in the caption of Fig. 6). Error estimates for the assemblage-based SST reconstructions are given in Table 3 (RMSEP=0.92-0.95). We expanded the discussion towards the point whether the most noticeable features of the Mg/Ca-SST record centered at ∼5.9, ∼7.4

and ∼8.2 ka B.P. could be related to North Atlantic ice rafting events, i.e., Bond events 4 and 5 (Bond et al., 2001), which were shown to affect Arabian Sea upwelling intensity (Gupta et al., 2003). See discussion on p. 10, l. 24-31.

Please note that the statement on page 9 line 8 is misleading. There is a maximum change in temperatures of roughly 6 degC, the majority of the change is, however, much smaller.

> We clarified this in the text and state that the range of the Mg/Ca-SST is 6.16 degC and the mean absolute deviation is 0.72 degC (p. 10, l. 7).

Minor issues: Title is too long: (suggestion) Solar forced decadal-scale upwelling in the Arabian Sea during the early Holocene.

> We agree with the reviewer and shortened the title to "Decadal resolution record of Oman upwelling indicates solar forcing of the Indian summer monsoon (9-6 ka)"

Abstract: The first four lines are misleading. The abstract should introduce solar driven climate change and not the socio-political implications thereof.

> We moved this to the Introduction (p. 1, l. 17)

Introduction: There are some detailed statements related to monsoon circulation. in the Arabian Sea (page 2 lines 2-8) that would be better placed at the start of chapter 2. In the introduction more generic wording could be used.

> These statements introducing the general development of the monsoonal winds are leading to the establishment of the oxygen minimum zone. We agree with the reviewer that some of these statements are specific and could be placed in Chap. 2, but still think the upwelling process has to be introduced prior to introducing OMZ.

Importance of AAIW in the Arabian Sea (page 3 lines 11-18). Whilst Boening and Bard indeed suggest that there is a strong influence of AAIW in the Arabian Sea, most of the available work seems to suggest that AAIW in the modern ocean does not reach the

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Arabian Sea (at least not with near pristine properties).

> We thank the reviewer for pointing this out and clarified it in the text (p. 3, l. 17).

Figure 1: labeling partly too small.

> Labeling of Fig. 1, which was larger in a previous version, was increased. Thank you.

Figure 6: Labeling on the right side should have the same orientation. > We thank the reviewer for indicating this and changed the orientation of the labeling.

Authors response to short comment #1 It would be good if the authors can add a paragraph on the longer-term Holocene context of their time series. The described natural variability and cyclicity forms part of socalled millennial-scale cycles which have been first described in more detail by Bond et al. 2001 in their pionier paper in Science. Meanwhile, similar Holocene milennial-scale cycles have been reported from many other places around the world, including the Arabian Sea. See a literature overview in our recent paper on this subject, pages 289-299: https://www.researchgate.net/publication/308928345_The_Sun%27s_Role_in_Climate Interestingly, most of these studies link the millennial-scale cycles to solar activity changes. It would be good to see a comparison of your 2500 year long time series with the millennial-scale cycles of Bond et al. 2001 which form an important reference for Holocene climate evolution. In this context, you might also consider a comparison with Menzel et al. 2014 who documented millennial-scale climate cycles with repeated dry/wet shifts from a central Indian lake. http://www.sciencedirect.com/science/article/pii/S0031018214003009

> We thank Sebastian Luening for his suggestion and added a comparison with the North Atlantic drift ice record to the manuscript. In fact, the most noticeable changes of the Mg/Ca-SST record of G. bulloides occur at ∼5.9, ∼7.4 and ∼8.2 ka B.P., which are partly in concordance with the North Atlantic Bond events 4 (5.9 ka) and 5 (8.2 ka),

as well a less pronounced event at 7.4 ka. However, to significantly assess millennial-scale cycles in our record, the time span covered of ∼2.5 k years is likely too short (see discussion p. 10, l. 24-31).

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Please also note the supplement to this comment:
http://www.clim-past-discuss.net/cp-2016-107/cp-2016-107-AC1-supplement.pdf

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