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Climate of the Past Discussions

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.

Anonymous Referee #1

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The manuscript by Munz and others " Decadal resolution record of Oman margin upwelling indicates persistent solar forcing of the Indian summer monsoon after the early Holocene summer insolation maximum" is based on sediment cores from the Oxygen Minimum Zone (OMZ) and upwelling cell from the Oman margin and covers about 3.000 years of climatic and paleoceanographic history during the Holocene. The manuscript is well written and easy to read. The reconstruction is based on several methods such as faunal assemblages, foraminiferal Mg/Ca ratios and bulk sediment geochemistry. Bases on these records the authors identify the presence of a solar activity cycle and therefore suggest atmospheric forcing controls OMZ dynamics. It is a

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Discussion paper

nice study and will make a step forward in understanding this complex region. However, the discussion lacks some thoughts about the age model and significance of the data as well as comparisons to other published datasets. Nevertheless, the study certainly fits into the scope of CP and should be considered for publication after minor/moderate revisions.

Major comment:

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. 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. 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.

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 (Moffa-Sanchez et al. 2014).

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

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

Page 4 Line 7-13: I am a bit worried about the error of the ΔR 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 ΔR.

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°C could the authors provide an error for the temperature reconstruction? 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.

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.

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

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 ΔCO32-! 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).

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?

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

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.

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.

Table 1.

The error should read $\pm$

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