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Climate of the Past Discussions

Interactive comment on "Strengthening of the Somali upwelling during the Holocene and its impact on southwest monsoon rainfall" *by* D. Balaji et al.

Anonymous Referee #2

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The paper by Balaji et al. presents an interesting data set of biogenic opal measurements from a core from the Somali upwelling and discusses the data with respect to upwelling intensity off Somalia in relation with precipitation over the Western Ghats on peninsular India.

The authors compare their biogenic opal record, as a productivity/upwelling record, with salinity records from the eastern Arabian Sea which probably reflect precipitation in the Western Ghats. The authors also use the rainfall record from the Qunf cave (Fleitmann et al., 2007) on the Arabian Peninsula. Recent observational data and the modelling data presented by Izuma et al. (2008) indicate that upwelling off Oman

and Somalia and moisture transport to India are anti-correlated affecting rainfall in the Western Ghats and thus runoff to the eastern Arabian Sea. Sea surface salinities could be recorded in (residual) delta18O records of calcifying plankton of published core records SK 19 and AAS9/21 from the eastern Arabian Sea. The authors also compare their biogenic opal record with SST reconstructions from the Somali upwelling (NIOP 905 and 929).

The age model and other productivity proxies of the same core have been published by Tiwari et al. (2010). The new interpretation with respect to indicators of precipitation needs more discussion of available literature on monsoon climate.

I have three general remarks and suggestions. If addressed adequately this would lead to complete rewriting of the paper and also require some new interpretation of the data.

- The comparison of the productivity record with the SST records is misleading. As the authors state, high productivity could be expected during periods of strong upwelling, i.e. low temperatures. The SST records, however, are dominated by the strong glacial-interglacial temperature increase. So during this phase it looks like SST and productivities are positively correlated. The authors discuss this (chapter 4.2.1.) and thus start the comparison with a phase when it does not work; so the Figure still does not help much with the data interpretation. A comparison of other productivity records, concentrating on the Somali and Oman upwelling areas could be more illustrative. The SST records of the Holocene (after the strong glacial-interglacial) may be plotted with reversed scale in order to better illustrate whether and when there is an anti-correlation. In addition: there is some discussion in Huguet et al. (2006) about the TEX86 temperatures; it may not represent annual average temperatures but has a SW monsoon bias. This needs to be addressed and could actually support the authors.

- The comparison of the biogenic opal fluxes form the Somali upwelling and the delta18O (precipitation) records is a new idea (not published by Tiwari et al.) but is

too vague to be the main part of the paper. The anti-correlation of Western Ghats precipitation with western Arabian Sea upwelling was modelled for the present Arabian Sea and the authors cite only one paper (Izuma, see above). As the authors also discuss, differences in evaporation and also surface water inflow from the Bay of Bengal have impacted the salinity off the west coast of India during the past so that much of the changes are related to several different processes (see Vijit et al., 2016; Mahesh and Banakar, 2014). Furthermore, even the present relationship between precipitation on the Indian Subcontinent and upwelling/productivity in the Arabian Sea is not very clear (see Levine and Turner, 2012) so this topic needs at least some further discussion.

- In the paper by Tiwari et al. (2010), which the authors cite, more data on core SS4018 are available such as carbonate contents and stable isotopes of carbon and nitrogen. These data can be utilized to better understand the processes in the Somali upwelling and would help to better understand the Holocene productivity changes. Tiwari et al. come to similar conclusions, e.g. that productivity does not decline during the late Holocene despite the decreasing insolation, based on a multiproxy study. The authors have now additional evidence that this is the case and can prove what Tiwari et al. suggested: the decline of carbonate could be due to the replacement of carbonaceous by siliceous primary producers. The carbonate/opal ratio could show this and strengthen the authors' point. The published data need to be included and elaborated on.

Throughout the text there are many questions arising which need clarification and more detailed discussion:

The authors use the term "glacial" and "deglaciation" without giving references for these phases. They should also give the correct time for the beginning of the Holocene. I think that the use of LGP is rather uncommon but LGM is more common and can be referenced (Clark et al, 2009).

Lines 55-59: it does not become clear why biogenic silica appears after carbonate, clarify in more detail.

Line 68: Is the age model used here different from the one used by Tiwari et al. for the same core, if yes, why? Is the same rate of sedimentation used for the whole core, despite available C14 ages? Why?

Lines 130-134: difficult to understand, explain in more detail. Why should variations be three times greater?

Lines 145-148: very short and therefore difficult to understand, explain in more detail (see also general comment on the comparison of SST and productivity records above). When do you expect a correlation, when an anti-correlation, why? This cannot be explained in two sentences.

Line 181: I find the use of the deglacial period (DP; 15-11 ka BP) rather problematic as it covers the Pleistocene/Holocene boundary.

Line 185: what does "entrainment of the SW monsoon" mean?

Lines 197-204: these lines again show that the comparison of moisture and upwelling does not work (see above). So when does it work and is it at all useful to show it for the whole period?

Lines 221: very short and not convincing. How does the record from the Qunf Cave come into the picture? How is rainfall related with the monsoon on the Arabian Peninsula. Is chronology such a big problem that the correlation does not work?

References suggested Clark, P. U., Dyke, A. S., Shakun, J. D., Carlson, A. E., Clark, J., Wohlfarth, B., Mitrovica, J. X., Hostetler, S. W., and McCabe, A. M.: The Last Glacial Maximum, Science, 325, 710-714, 2009. Levine, R. C. and Turner, A. G.: Dependence of Indian monsoon rainfall on moisture fluxes across the Arabian Sea and the impact of coupled model sea surface temperature biases, Clim. Dyn., 38, 2167-2190, 2012. Mahesh, B. S. and Banakar, V. K.: Change in the intensity of low-salinity water inflow from the Bay of Bengal into the Eastern Arabian Sea from the Last Glacial Maximum to the Holocene: Implications for monsoon variations, Paleogeogr. Paleoclimatol. Paleoecol.,

397, 31-37, 2014. Vijith, V., Vinayachandran, P. N., Thushara, V., Amol, P., Shankar, D., and Anil, A. C.: Consequences of inhibition of mixed-layer deepening by the West India Coastal Current for winter phytoplankton bloom in the northeastern Arabian Sea, J. Geophys. Res.-Oceans, 121, 6583-6603, 2016.

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