

Interactive comment on “Sensitivity of Red Sea circulation to sea level and insolation forcing during the last interglacial” by G. Trommer et al.

Anonymous Referee #2

Received and published: 7 June 2011

Review of manuscript “Sensitivity of Red Sea circulation to sea level and insolation forcing during the last interglacial” by G. Trommer et al. The manuscript presents new data from three sediment cores from the central Red Sea encompassing the transition between late MIS 6 and MIS5. Based on a combination of census data on planktic foraminifera and geochemical data as well as a statistical assessment of the results the authors aim to address the succession of changes in water circulation in the Red Sea surrounding Termination II and early stages of MIS5.

In general the manuscript is well written although some language polishing seems in order at times. The quality of the figures is good, albeit some labels are a bit small and they seem to vary in size between graphs and within graphs (e.g. figure 6). Whilst the overall thrust of the manuscript is interesting (and eventually may warrant publication)

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there are a few issues preventing me from recommending publication in the present form.

The main issue surrounds the reliability of the proxies used that seem to suffer from varying degrees of uncertainties/problems, hampering a straightforward interpretation of the results. To start with, the authors use the census data on planktic foraminifera to calculate the chlorophyll a concentrations at the core sites. Setting aside for a moment my principle reservations with regards to the validity of the approach, the authors seem to use their data in a rather inconsistent fashion. On page 1205 for example the authors start off by naming periods in both cores with analogue/non-analogue conditions with regards to the foram data. Interestingly these periods seem to be scattered throughout the cores, i.e. encompassing periods with substantially varying climate conditions. Based on the original work (Siccha 2009) the method should only be applied under conditions that “are fundamentally comparable to modern conditions”. In other words, applying the technique to other periods than MIS5.5 is problematic. Yet, the authors present results from Termination II and parts of MIS 5.4, with climate conditions definitely being very different from modern conditions. How valid are these data? The authors should discuss the subject in more detail.

Also the discussion of the Tex86 results is not very convincing. The general problem is that Crenarchaeota occur in a wide spectrum of habitats, making it very difficult to believe that they actually do contain information that can be specifically linked with sea surface temperature changes. In addition, in case of the Red Sea, the Crenarchaeota population seems to be a mix of endemic Red Sea species and those advected from the Arabian Sea. The discussion on this subject seems a bit short in particular with regards to varying advection rates through time to the Red Sea as a whole as well as the knock-on effect on SST records for individual sites. How do the estimated SST changes for example align with the existing stable oxygen isotope data etc.? The manuscript would benefit from a more substantiated discussion.

Finally, the BIT index data are difficult to understand. The authors correctly mention

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that rainfall rates near the Red Sea region increased with the beginning of MIS 5.5. A similar change occurred at the beginning of the Holocene. Given that the BIT index is supposed to reflect fluvially introduced terrestrial carbon, should this index not peak during MIS 5.5 rather than thereafter? What is the significance of this finding? Does the BIT index work in this environment? A better discussion is required.

In summary, this study presents interesting results that may eventually warrant publication. In the current form, however, the discussion of the results lacks maturity.

Minor issues:

Page 1200 line 7-8: mentioning of MIS6-MIS 5d is inconsistent with the subsequent mentioning of depth/ages)

Page 1203: first paragraph in chapter 3.2 seems out of place – reads more like a “methods” text bit.

Page 1206: the first sentence is misleading because it might be interpreted as to suggest that the trends in both types of proxy records are similar.

Interactive comment on Clim. Past Discuss., 7, 1195, 2011.

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