

Interactive comment on “Migrating subtropical front and Agulhas Return Current affect the southwestern Indian Ocean during the late Quaternary” by D. K. Naik et al.

Anonymous Referee #1

Received and published: 9 December 2013

In their paper “Migrating subtropical front and Agulhas Return Current affect the southwestern Indian Ocean during the late Quaternary”, Naik and co-authors present isotopic, trace element and abundance count data on two planktonic foraminifera species to infer past condition in the Southwest Indian ocean. The dataset and part of the interpretations drawn are interesting and sound. There is, however, some overlooked issues and unsupported interpretations. Considering the location and depth (4022m) of the core, AABW probably bath the core site and dissolution have to be seriously considered. The authors currently argue for a small effect of dissolution based on *G. bulloides* shells weight. This is a good approach but there is different reason why shell weight may not well represent dissolution [initial shell weight controlled by various pa-

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rameters, optimal growth rate (de Villiers, 2004), or seawater carbonate ion (Barker and Elderfield, 2002)] and this need to be seriously assessed in the manuscript. The other main critics is that some interpretations are either too far reached or even non-supported by the data. An example of this is lines 21 (p5534)-4(p5535) but I will try to list them in the detailed comments. Finally, since 2006 *Neogloboquadrina pachyderma* Dextral was renamed *N. incompta* (Darling et al., 2006) and the name must be changed in the entire manuscript.

Detailed comments - Line 4 p 5522: replace ‘has’ by ‘have’

- Line 9 p 5522: Ca is not a metallic element, the notation trace metal ratio is not strictly speaking correct and I would recommend using trace element ratio. Trace metal/Ca ratio is sometime used.

- Line 6 p 5523: replace ‘early’ by ‘earlier’.

- Line 19 p 5524: The link between high productivity and upwelling is not only true for the Indian ocean.

- Paragraph 2 the study area: this paragraph is not well organized and therefore a bit confusing. A description of the currents from Nord to South (following the flow) for example would be easier. The details on the northern part of the region (ITF, Bay of Bengal, SEC) are not necessary here.

- Lines 9-14 p 5526: Use only one decimal. Where do these number come from? World Ocean Atlas? If so, then add references.

- Lines 16-18 p 5526 suggestion for modification: “The top 1.2m section of a gravity core (SK 200/17, hereafter referred to as SWIOC) collected from 39.03_S latitude and 44.97 E longitude, at a water depth of 4022m was sampled every 1 cm.”

- Line 26 p 5526: replace ‘dried sample was’ by ‘dried samples were’

- Line 3 p 5527: replace ‘The plus 63 μm fraction was then transferred in to small

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beakers for drying. The dried > 63 μm fraction was weighed and stored in plastic vials. The dried > 63 μm fraction was dry sieved using a 150 μm sieve. The > 150 μm fraction was used for picking planktic foraminifera by 'The fraction larger than 63 μm was then transferred in to small beakers for drying, then weighed and stored in plastic vials. The samples were dry sieved using a 150 μm sieve and used for picking planktic foraminifera'

- Line 7 p 5527: what is 'coning and quartering? Splitting?
- Line 6 p 5528: Mashiotta gives 1.1°C as error on this calibration, where does the 0.8 °C come from?
- Line 13 p 5528 add 'of' between d18O and *G. bulloides*.
- Line 20-21 p 5528: The writing is not very elegant, please rephrase.
- Pages 5529 and 5530: This long description of the figure is very boring to read. Please synthesize, extract the main information and make a lot shorter.
- Lines 3-23 p 5531: I found this whole paragraph very confusing and hard to understand. Neither the Be and Hutson nor the Fraile references show the numbers cited here. Dissolution should be discussed here as it is likely to produce the difference in preservation between the 2 species. See Berger 1970 for the difference resistance to dissolution between the 2 species.
- Line 28 p 5531: 0.1‰ is within the error and therefore not significant.
- Lines 1-3 p 5533: The effect of dissolution should be carefully examined and the effect on foraminifera abundances and geochemistry critically assessed.
- Line 7 p 5533: The high abundance in *G. bulloides* – and therefore high productivity – is observed AT THE END of the cold period. This shortcut of high productivity during old period is found throughout the manuscript and should be corrected. The description of the curves and interpretations must be precise.

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- Lines 19 p 5533 to line 4 p 5534: This paragraph is a repetition of lines 10-29 p 5524, shorten one of the two paragraphs to avoid repetition.
- Lines 4 p 5534: change "A difference in relative abundance of *N. pachyderma* Dextral and *G. bulloides* is" for "The lag between *N. incompta* and *G. bulloides* peak in abundance is"
- Lines 7 to 9 p 5534 : This is a very far reached statement, it needs to be justified or deleted.
- Lines 12-15 p 5534: Typical shortcut of this paper, The observation describe here is correct for MIS 4 but a lot less for MIS 2.
- 16-17 p 5534: The peak in abundance at the pycnocline is observed for vertical profile but not necessarily in absolute abundance. This cannot be simply used as it is currently presented. The explanation of the seasonality effect presented a few lines after is much more plausible.
- Line 22 p 5534 to line 8 p 5535 appear largely unsupported. The authors should carefully examine their signals, in MIS 3 *incompta* abundance stays high but the Mg/Ca in *G. bulloides* decrease. A lot of processes (thermocline, dust, ice-rafted debris) are cited without clear logic. The authors should definitively look into the seasonality of the 2 species.
- Lines 9-12 p 5535: If warm and salty water invaded the core site during MIS 4, why is there no change in Mg/Ca or d18O?????
- Lines 10-12 p 5536: show a temperature scale on figure 4.
- Lines 17-18 p 5536: Seasonality may also explain the difference between radiolarian and foram-based temperature.
- Line 24 p 5536: cite for example Duplessy et al., 1991.
- Line 24 p 5537 to line 9 p 5538: a figure comparing the data with the results of

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Martinez-Mendez is necessary to evaluate this paragraph.

- Line 18-19 p 5538: 'poor preservation of the test', Yes! This should be discussed and foraminifera abundance and fraction >63um can be used.

- Figure 1: The location of core SK200/17 is marked by the RED square, right?

References cited.

de Villiers, S., 2004. Optimum growth conditions as opposed to calcite saturation as a control on the calcification rate and shell-weight of marine foraminifera. *Marine Biology* 144, 45-49.

Barker, S., and H. Elderfield (2002), Foraminiferal Calcification Response to Glacial-Interglacial Changes in Atmospheric CO₂, *Science*, 297, 833-836.

Darling, K. F., M. Kucera, D. Kroon, and C. M. Wade (2006), A resolution for the coiling direction paradox in *neogloboquadrina pachyderma*, *Paleoceanography*, 21, PA2011, doi: 10.1029/2005PA001189.

Berger, W. H. (1970), Planktonic Foraminifera: Selective solution and the lysocline, *Marine Geology*, 8(2), 111-138, doi: 10.1016/0025-3227(70)90001-0.

Duplessy, J.-C., L. Labeyrie, A. Juillet-Leclerc, F. Maitre, J. Duprat, and M. Sarnthein (1991), Surface salinity reconstruction of the north Atlantic Ocean during the last glacial maximum, *Oceanologica Acta*, 14(4), 311-324.

Interactive comment on *Clim. Past Discuss.*, 9, 5521, 2013.