Clim. Past Discuss., 7, C1106–C1109, 2011
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Climate of the Past Discussions

Interactive comment on "High-latitude obliquity forcing drives the agulhas leakage" *by* T. Caley et al.

Anonymous Referee #1

Received and published: 19 July 2011

I am reviewing the manuscript "High-latitude obliquity forcing drives the Agulhas leakage" by T. Caley, J.-H. Kim, B. Malaize, J. Giraudeau and co-authors. The authors present 800 kyr-long records of planktic foraminiferal d18O and Mg/Ca, and complementary records of Uk37 and Tex86 that are used to derive ambient SST. The records are derived from core MD96-2048 off SE Africa at 26S. The authors use the SST and inferred SSS pattern to suggest that Agulhas leakage in the past was associated with an obliquity-paced high-latitude forcing mechanism; one of the mechanisms they envision is the migration of the southern hemisphere subtropical convergence (STC) which, in their view, modulated the inter-ocean water transports from the Indian Ocean to the Atlantic.

Agulhas leakage increasingly is recognized as a potentially powerful driver to modulate

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the strength of the Atlantic MOC. Much of this mechanism is inferred from numerical modelling. The study by Caley et al. hence is very timely in that it aims to demonstrate the variability of the Agulhas Current in the past and the associated variability of the Agulhas leakage. The data the authors present are of good quality and the manuscript for most parts is well written.

I have two main concerns regarding the concepts the authors put forward and several technical issues that I feel need to be resolved in order to support (or reject) some of the claims the authors make. My first conceptual concern is that I cannot see that the data profiles from core MD96-2048 can serve as robust indicators of Agulhas leakage variations that occur at the southern tip of Africa, some 10 degrees of latitude to the south of the core position. The leakage is a complex system that is driven by the Agulhas' rotary momentum (i.e., vorticity), wind stress and latitudinal fluctuations of the STC in the south. Obviously, these components are interconnected while, I find it difficult to follow a concept by which hydrographic variability (SST, SSS) is used to infer the rather complex dynamical (=physical) circulation in the Indian-Atlantic gateway. My second concern is directly related to the first: I am fairly unconvinced that the SST stack is as robust a mean SST indicator as the authors seem to think it is. The SST stack is built from three SST records that are derived from very different methodologies (and signal carriers): planktic foraminifera, algae, and bacteria, every single one with their own ecological niche, hence recording different kinds of ambient temperature (summer, spring, Fall; surface, sub-surface, etc). As a matter of fact, looking at the individual SST records as displayed in Figure S2A it is quite obvious that they share similar features while they are also quite different from each other in fine-structure (perhaps as a result of different temporal resolution?), in amplitude, and in timing of SST shifts. Hence I do not agree with the reasoning that stacking them into one single record achieves a higher degree of robustness.

This then leads to my technical concerns.

A) SST stack. The approach of sampling the Tex86 and Uk37 SST records at the

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time step of the Mg/Ca record because this “is the SST dataset with the highest time resolution” is statistically wrong; this procedure oversamples the biomarker SST series hence potentially aliasing frequencies that do not exist in the raw records. To apply EOF/PCI to check if the stack “represents the common temporal variation of the three individual SST records” does not rectify this problem.

B) Spectral analysis, orbital periodicity, phasing The same sampling issue comes into play again for the spectral analysis: “For spectral analysis all records were sampled at 0.5 kyr intervals.” As is mentioned in the Materials and Methods section, mean sedimentation rates along core MD96-2048 are 2 cm/kyr; the core was sampled every 2-5 cm for isotope and Mg/Ca analysis, yielding time steps between 1-2.5 kyr for d18O and Mg/Ca; the core was sampled every 5–10 cm for alkenone and GDGT, yielding time steps between 2.5-5kyr. Sampling the records at 0.5 kyr intervals for spectral analysis hence oversamples the records by a factor of 2-10. Again, this is illegitimate as it can cause frequency aliasing that then potentially perturb the spectra. I strongly recommend running the spectra on the Mg/Ca record (or SST record derived from it) alone, this is the highest resolved record and directly coincides with the sampling rate of the d18O record. Running spectra for Mg/Ca shall reveal if Mg/Ca-derived SST picks up a precession component that seems absent from the oversampled SST stack.

The observation that “...SST stack and d18Osw records are nearly in phase with changes in high-latitude annual mean insolation” may well be true while the amplitude modulation is off, as is visible in Fig. 2E. Therefore, an immediate mechanistic linking with obliquity modulation of northern hemisphere insolation cannot be claimed without a further discussion of the factors that may have shifted the amplitude away from that of the input signal.

In conclusion I shall say that the data are good, give and take a reconsideration of combining the three SST records into one single record, and the hypothesis that obliquity pacing of the Agulhas leakage may have set the pace at which the Atlantic MOC shifted in the past seems worth to be followed up upon. The conclusion “We argue

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that the important transfer of heat and salt via the AC, which affected the resumption of the AMOC and the initiation of interglacial conditions (Weijer et al., 2002; Knorr and Lohmann, 2003; Biastock et al., 2008) ... " is not new, while worthwhile mentioning again (the work of Peeters et al. 2004 must be used as reference here!). However, I do not feel that the data that are presented support or even confirm such hypothesis because 1) in my view the core location at 26S is not the best position to monitor what the leakage did in the past and 2) construction of the SST stack and the data pre-processing for time series analysis are flawed in that they oversample the original records.

As for the final recommendation I am drawn between "reject" and "major revisions". In the end I opt for the "reject" on grounds that the data processing both for the SST stack and the time series analysis is inadequate to the extent that the spectra may come out quite differently if the sampling rate is corrected, and the spectra are run for the highest-resolving of the records only i.e., Mg/Ca. This may change the main conclusions of the paper and give it a different direction, which in my view warrants a "reject".

Interactive comment on Clim. Past Discuss., 7, 2193, 2011.