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Interactive Comment

Interactive comment on "Paleo Agulhas rings enter the subtropical gyre during the penultimate deglaciation" *by* P. Scussolini and E. van Sebille

Anonymous Referee #1

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The paper "Paleo Agulhas rings enter the subtropical gyre during the penultimate deglaciation" by Scussolini and van Sebille implicitely attempts using individual foraminiferal analysis (IFA) to capture snapshots of the frequency and/or amplitude of Agulhas rings in the South Atlantic subtropical gyre during the penultimate deglaciation. The working hypothesis is that repeating d18O analysis of thermocline-dwelling planktonic foraminifera - supposed to provide a mixed signal of temperature and salinity (d18Osw) - might eventually lead to capture/record some of those rings leaking through the cape of Good Hope and traveling through the South Atlantic, ultimately impacting the variance of IFA. It is a fairly well written story, of which the conclusions sound plausible. I however feel the data are misinterpreted, and that the conclusions reached are unsupported by the data presented for many reasons which I develop below.

First, on a pure statistical point of view, I doubt that measuring 18 to 30 specimen can help commenting on the frequency and amplitude of Agulhas rings. This issue was recently raised by Quinn et al. (2012, AGU fall meeting) who performed a cautious statistical analysis of IFA - focused on extracting ENSO, though, but certainly applicable to that study as well - and questionned the representativeness of IFA variance as a tool for documenting past oceanological variability using IFA samples based on many more than 30 replicates.

Second, the core-top d18O values of G. trunca from the southeastern Atlantic Ocean in the vicinity of the coring site displays a fairly large array of values, probably because the water depth at which the forams live is far from being constant/established. Indeed, the paper by LeGrande et al. (2004, G-cubed) indicates that G. trunca size fraction largely determines the d18O, ranging from approx. 1 to 0 permil for the 250 um size fraction to approx. 1 to -1 permil for the 300 um size fraction. Given that the d18O measurements were performed on the 250-300 size fraction, then there remains the possibility that most of the variability seen in the variance is simply due to small changes in the proportion of those two size fractions which, even though the changes in size were small, could have had large consequences on the overall variance.

This being said, there remains the trend observed in the variance during the termination that might, at first sight, suggest that none of the two above-mentioned limitations would be at play. The key to explain the variance recorded during the termination is, however, likely embedded in bioturbation. The mean sedimentation rate between 138 and 125 ka - i.e. from the last sample from MIS6 dated at 138 ka to the first sample of the MIS5 dated at 125 ka according to the bulk d18O analysis - is about 1.5 cm/ka. Clearly, such a low sedimentation rate along with a 4-cm resolution sampling for the IFA prevents any interpretation of the IFA in any process other than bioturbation, unlike the authors claim. The citation of Broecker was indeed challenged by Stott and Tang, 1996, Paleoceanography, who demonstrate that there is a considerable overlap between core top and LGM samples from the tropical Atlantic because of bioturbation.

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The sediment thickness involving bioturbation processes studied in Stott and Tang involves a sediment thickness of around 30 cm of mud, which is even more than the 22 cm sediment depth between MIS6 and MIS5 in the present study, indicating that most - if not all - the d18O "outliers" interpreted as reflecting the influence of the Aghulas rings can be explained by reworking of forams through bioturbation.

In fact, such signature is clearly seen in the data presented in Figure 3. A rough estimate of the MIS6-MIS5 transition recorded in the bulk d18O values suggest values ranging from a maximum of 2.6 permil for the MIS6 to a minimum of 1.2 permil for the MIS5. Such range of 1.4 permil is smaller than the full range of the benthic stack of Lisiecki that suggests a d18O shift of 1.6 permil, where the influence of temperature is expected to be even smaller than for planktonic forams. Such a mismatch is probably the signature of the smoothing effect of bioturbation, since individual values of d18O seem to be clustered around values lighter and heavier than the bulk values for the MIS5 and MIS6, respectively. Accordingly, during the penultimate deglaciation, increases in the variance are mostly due to negatively anomalous d18O values during the early part of the termination and positively anomalous d18O values during the late part of the termination, leading to the reduced magnitude in the deglacial d18O trend recorded in the bulk d18O. It is simply due to the fact that bioturbation incorporates foraminifera from the MIS6 into the MIS5 sedimentary interval and vice-versa. If, as the model suggests, the anticyclonic eddies are/were by far the most prominent types of eddies - i.e. to be captured as positive d18O anomalies-, then not only the full MIS6-MIS5 transition recorded in the bulk d18O would be larger in magnitude, but also the d18O would be mainly skewed toward positive values over the full deglaciation, which does not seem to be the case. Performing a skewness test such as it has been done in Khider et al., 2011, Paleoceanography, would have been helpful in such case, but the amount of IFA performed in this study is probably not sufficient enough to reach the basic statistical requirement to diagnose whether or not the IFA pass such test. In the end, the increase in the variance recorded at the midpoint of the glacial termination, which is recorded in samples situated within a sedimentary thickness of no more than

10 cm, might well represent perfectly homogenized samples merging both forams that lived during the MIS6 ad MIS5 (Trauth et al., 1997, Paleoceanography).

I acknowledge my review is quite pessimistic, and feel sincerely sorry to not be more supportive. Perhaps such dataset might be used to emphasize the need to perform IFA instead of bulk d18O analysis to better constrain the full range of deglacial d18O, as bulk d18O has been widely used to constrain e.g. the entangled magnitudes of temperature and sea level shifts in low sedimentation rates environments. Following on this, flagging outliers - perhaps with the use of combined d18O and 13C as in Stott and Tang - to check which foram likely represents an overlapping of MIS6 and MIS5 intervals can help providing a diagnostic on how much bulk d18O shifts underestimate the full spectrum of seawater + temperature changes. I hope the authors will find a way to extract valuable information from the dataset presented here, but limitations associated with the reduced dataset and the sedimentation patterns impacting their coring site clearly impede, in my opinion, any interpretation in terms of changes in Agulhas ring influence on the IFA d18O.

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