

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

Anonymous Referee #2

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This study presents $d_{18}O$ analyses on individual tests of the planktic foraminifer *G. truncatulinoides* from a sediment core retrieved from the South Atlantic (sensitive to the passage of Agulhas rings from South of Africa). The authors observe an increase in the variance of $d_{18}O$ measurements from individual samples during glacial termination II (TII), which they contend reflects an enhancement in the frequency of Agulhas rings reaching their site and a corresponding increase in Agulhas leakage (AL). This conclusion is in agreement with earlier studies based on planktic foraminiferal fauna found in two cores situated up-stream with respect to the pathway of AL.

The paper conveys an interesting idea and presents high quality data – the conclusions are certainly appealing. The paper is well written and concise. My initial thought matched that of Reviewer 1 (the question of whether increased variance during TII

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might be due to a blurring of the d18O signal across the termination). To avoid repetition and acknowledging the authors response to Reviewer 1, I will here add some additional thoughts that the authors might consider in a revised ms.

The authors describe the ‘abrupt’ decrease in *G. ruber* d18O across TII (Fig. 1 of their response to Reviewer 1) and will therefore appreciate how much faster this is than the deglacial decrease in (for example) ‘global’ deep water d18O across this transition. If their reconstructed increase in AL really occurred during TII then this implies that the decrease in *G. ruber* d18O occurred at the beginning of TII. Can the authors comment on what is driving this change in d18O, which precedes the global shift? Perhaps it reflects warming of the South Atlantic in an analogous way to warming during TII associated with HS1?

I wonder why the authors do not plot the mean of their individual analyses in Fig. 3 of the main text as well as the bulk analyses? It seems to me that many (more than half) of the individual d18O values for several depth intervals during TII plot lighter than the bulk values. Does this mean that the bulk values do not actually represent the spread of individual shells i.e. do the lighter d18O values derive from lighter (as in lower mass) shells? Did the authors weigh the individual shells prior to analysis? Furthermore, some of the lightest values for individual shells coincide with the peak in variance associated with TII. Apart from partly allaying the concern that bioturbation might be mixing down shells from MIS 5, plotting the average for the individual analyses might produce a curve that looks more like that for *G. ruber* – I assume the authors have tried this but somewhere it would be good to see how their bulk analyses compare with the computed mean for the individual measurements. Additionally, as pointed out by Reviewer 1, some of the samples for which individual shells have been measured appear bimodal in their distribution. If this reflects populations of shells that were or were not influenced by Agulhas rings, perhaps it might be worth attempting to identify these distinct groups and calculate an estimate for the evolution of each throughout the termination?

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The authors state that their observed increase in variance for individual G. truncatulinoides d18O analyses starts well before TII, “before maximum glacial conditions”. They then go on to draw an analogy between their results and two papers attempting to explain the mechanism of glacial termination (Cheng09 and Denton10). The authors should note that both of these papers emphasise the role of northern hemisphere forcing for glacial termination (which results in southern hemisphere warming via the bipolar seesaw). In neither case would we envisage the initiation of deglaciation within the southern hemisphere before maximum glacial conditions had been achieved. In fact the southern hemisphere warming associated with HS1 during TI really defines the initiation of the last deglaciation. The authors should consider this in their revision.

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