

Interactive comment on “Climate, cryosphere and carbon cycle controls on Southeast Atlantic orbital-scale carbonate deposition since the Oligocene (30–0 Ma)” by Anna Joy Drury et al.

Anonymous Referee #2

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The authors present a revised composite depth scale and orbitally-tuned age model for Walvis Ridge ODP Site 1264 (S. Atlantic), based on XRF core-scanning In(Ca/Fe) data which has been calibrated to shipboard %CaCO₃. This is a substantial undertaking and will be extremely useful for further studies. The methods are described in detail and sufficiently illustrated, hence the new splice and age model appear to be robust. Overall the paper is well-written and organised, and I'm happy to see it published close to its present form.

Key methodological points that the authors address are, first, correcting the XRF results from over several years, and from different instruments/settings (Supplementary fig.

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1 – it would be better if there was a bit more overlap of 2018 data with 2011 data, but access to rescanning some sections may not be possible at present? If not, I'm happy with the correction as it is), and second, calibrating the scanning Ca data to shipboard %CaCO₃. The latter correlation shows some scatter, but outliers are ignored (I presume based on visual identification?) and the inferred %CaCO₃ is within 2% at 1SD. This error isn't taken into account when discussing the %CaCO₃ time-series nor when calculating CaCO₃ MARs, and I'm not sure how much difference it will make to the conclusions because carbonate content is so high (~92-97%). But given that the discussion, especially about the Biogenic Bloom, hinges on accuracy of the CaCO₃ MARs, which in turn hinge on accuracy of %CaCO₃ according to the authors, then some mention of error on the inferred %CaCO₃ & its MARs is perhaps warranted. Finally, I agree with the 3 different tuning strategies for the different intervals – this appears to be justified.

Regarding the wavelet analysis, I'm not entirely convinced I can see the cyclicity that the authors see, particularly the comment in 4.1.3, line 11. This may in part be due to the small reproduction of the wavelet figures, but I also think there's some ambivalence here. A related issue is how much can be inferred from the wavelet analyses without bias, given the orbital tuning methodology?

Section 5.1. The authors link orbital cycles at site 1264 to Antarctic ice-sheet variability and NADW, but there's no explanation of how this process-link is made. Maybe elaborate or be more speculative.

Minor edits. One of the authors with excellent English should proof-read the manuscript for grammar/spellings as there are several incorrect verb formats. I started noting them in section 3.2 (line 12: occur should read occurs or occurred, etc). Section 4.1, line 10: “four” intervals. Fig. 2: relabel 4 axes for mag sus. Fig. 3: (b) there are some lower spikes in %CaCO₃ that look like outliers/cracks...??? (c) units? Also, I can't see the black & grey lines. . .is it because they exactly underlie the blue lines (seems a bit odd if they are identical)? Fig. 5: is it possible to show tie points? Fig. 6: (h) periods are

C2

same as in Fig. 4c, but now we are in the age domain so I would expect ky periods on the left-hand axes.

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