Reply to: *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 (R2)
Received and published: 13 October 2020

We would like to take this opportunity to thank R2 for taking time to review our manuscript and for providing such constructive feedback that will help improve the overall manuscript. Please see our responses to this feedback below.

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 ln(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.

We are very pleased to hear that R2 values the progress represented by our revised depth and age model at Site 1264, and recognises the value for future research in this region.

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

We are very happy that R2 agrees with the different tuning strategies we applied.
Unfortunately, it is not possible to increase the overlap between the 2018 and 2011 data, as this would involve rescanning material from all four measurement campaigns to generate a new 2021 calibration. We chose the overlap between the 2018 and 2011 data, as it was the same overlap scanned between the 2017 and 2011 and 2017 data. This provided the opportunity to compare data across three measurement campaigns, albeit over a short interval.
We agree with R2 that we should explain the %CaCO3 calibration process better and that we should propagate the error associated with it. We will include a brief explanation of why certain outliers were ignored, propagate the error for both the %CaCO3 and the MARs and discuss how this may influence our interpretations. However, the error is mostly relevant to the absolute carbonate content, rather than the cyclicity or the trends (e.g., identification of the biogenic bloom), which are both visible in the raw ln(Ca/Fe) timeseries. The calibration error and/or potential later changes to the calibration won't alter the underlying trends or cyclicities in the raw data. Nonetheless, we agree with R2 that the error should be mentioned and will revise the manuscript to reflect this.

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?

We appreciate that the data and wavelets presented in Figure 4 are small, especially as we present the entire record in one figure. We plan to revise this figure to be comparable to those in Figure 6, but then in the depth domain. We will also change the text here to convey our main message more clearly, which is that the carbonate data itself shows clear cyclicity between 115 and 35 rmcd, but that the amplitude of this variability is reduced compared to the deeper interval. We appreciate that this is clearer in the $CaCO_3$ data itself, rather than the wavelets, which show this less well. We will also consider whether we can include a magnified insert of this interval to show the cyclicity or provide supplementary figures where expanded depth intervals can be shown across multiple panels.
The observations about the potential origin of the depth cyclicity are based on the data and wavelets in the depth domain compared to the data and wavelets in the age domain using the initial bio-magnetostratigraphic age model (supplementary figure 8). Both the depth and initial age model are independent of the astronomical tuning, which is only applied to the data in Figure 6, so the cyclicity observed and discussed in 4.1.3. is likewise independent from 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.

In the case of the early-mid Miocene and late Miocene, sections 5.2 and 5.3 are intended to provide greater detail about the observed carbonate trends and cyclicity, and what may drive these.
We mention that 110 kyr-driven cycles in benthic $\delta^{18}O$ and carbonate have been linked to Antarctic ice sheet variability in the 30-17 Ma interval by Liebrand et al., 2016/2017 and infer that a similar relationship may exist in the 17-13, or even 17-8 Ma interval; however, we do not have benthic stable isotope data to confirm that for those intervals. We will expand the discussion to discuss potential process-links. However, these will remain speculative based on carbonate content alone, so we will make sure the discussion reflects that tone.
We mentioned that benthic $\delta^{13}C$ records discussed in Bell et al., 2014/2015 indicate that Site 1264 was heavily influenced by NADW for the 5-0 Ma interval, but it wasn't our intention to specifically link the younger (late Miocene-recent) variability to the presence of NADW at 1264. We will adapt the discussion to reflect this.

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).

Thank you, we will make sure these issues are resolved during revision.

Section 4.1, line 10: "four" intervals.

Thank you, we will correct this.

Fig. 2: relabel 4 axes for mag sus.

Thank you for spotting this error, we will correct this.

Fig. 3: (b) there are some lower spikes in %CaCO3 that look like outliers/cracks: : :???

These lower spikes likely originate from abnomolously Fe counts in the ln(Ca/Fe) ratio, and should have been removed as outliers. We will do so and discuss the treatment of outliers in the methods.

Fig. 3: (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)?

We will add units and revise the caption text to be clearer. This panel contains two datasets: the bulk MARs and the carbonate MARs. The colours are used to denote both the type of data (bulk vs carbonate MAR) and the site at which they originated (1264 vs 1265). The dark and light blue were used to respectively denote bulk and carbonate MAR data from Site 1264. The Site 1264 data accounts for most of what is shown, except for 4 short intervals in the deepest section, where 1265 was used (this correlation was explained in detail by Liebrand et al., 2016). The black and grey colour was used to show the short intervals of bulk (black) and carbonate (grey) MARs from Site 1265. But in both the dark vs light blue and black vs grey, it is hard to see both records, because the bulk and carbonate MARs are near identical as carbonate is the main sedimentary component. We nonetheless wanted to show both datasets, and highlight which data originated from which site. However, we appreciate this is not clear from the current caption and figure, so will alter one or both to improve the clarity.

Fig. 5: is it possible to show tie points?

We are reluctant to include these in the main figure, as there are 368 ties in total and this would completely overcrowd the main panel. We are also concerned that including them in each sub panel could overcrowd the figure. We hoped to show the correlation by placing the data used to correlate over the target curve. Also, as this figure is presented in age only, the ties would not directly show how the data in depth relates to the tuning target in age. However, we will consider including expanded versions of this figure in the supplementary information, showing the data on depth with the tie points to the data on age.

Fig. 6: (h) periods are same as in Fig. 4c, but now we are in the age domain so I would expect ky periods on the left-hand axes.

The periods given for the wavelets are for kyr in Fig 6 and m in Fig 4c. We will change the axes to make this clear.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-108, 2020.