

Interactive comment on “Sedproxy: a forward model for sediment archived climate proxies” by Andrew M. Dolman and Thomas Laepple

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Dear Paolo Scussolini,

Thank you for taking the time to read and comment on our discussion paper. We appreciate that in using your study as an example we should have made more effort to justify our choice of parameter values and to check their realism. Our main aim was to illustrate the capability of the sedproxy to simulate IFA type studies and to explore potential alternative explanations for patterns in paleo data. We should have stressed more clearly that we see bioturbation as an alternative explanation rather than presenting it as the most plausible explanation, which of course will depend heavily on the parametrisation.

C1

Your suggestions will greatly improve the manuscript. Here we respond to your specific points in turn. We have included some relevant text from your comment in blue italicised text.

1. Seasonality of $d^{18}O$ at 400 m:

“First, Scussolini et al. (2013) analysed the planktic foraminifer Globorotalia truncatulinoides (sinistral coiling variety). This organism calcifies at depths beyond 400 or 600 m, according to the relevant literature and to Scussolini and Peeters (2013, Paleoceanography; doi: 10.1002/palo.20041; see also references therein), who compared values from core-top specimens to modern hydrography. At these depths, at the core site, there is hardly any seasonal variation in temperature and salinity. To assume 0.5 ‰ seasonal noise to mimic the $\delta^{18}O$ signal seems therefore inappropriate. I expect that this shouldn't change the position or magnitude of the peak in variability simulated by 'sedproxy', but it would be advisable to rectify the calculations to reflect this.”

We accept that there is very little seasonality in $d^{18}O_{sw}$ at these depths and we have removed seasonality from the calculation. However, we also see that we overlooked the much larger variation in $d^{18}O$ over the depth habitat of Globorotalia truncatulinoides. Consequently, we have modified this example to demonstrate how the habitat weights can be used with a depth resolved rather than seasonally resolved input climate. We refer to Figure 2 of Scussolini Peeters (2013) to approximate the $d^{18}O$ depth gradient and use a Gaussian distribution with mean of 520 m and standard deviation of 50 m for the habitat weights.

2. Correction for instrumental variance measured on standards:

“Second, Scussolini et al. (2013) report that they 'corrected the variance of foraminiferal $\delta^{18}O$ by subtracting that of external calcite standards measured in the

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same sequence', with the aspiration to clean their proxy from the spurious effect of measurement noise. It seems that Dolman and Laepple do not take this into account, as they 'assume a measurement noise of 0.1‰ del18O for the IFA and the bulk measurements'."

Unfortunately, our description of the method was too brief as we did in-fact subtract this variance from the IFA variance estimated for the simulation output (line 484 in the supplementary .Rmd file). The effect on our Fig. 8 is however small, as the variance due to measurement error amounts to only 0.01 ‰².

3. Speed of the climate transition:

"Third, Dolman and Laepple assumed 'a climate transition from 0.4 ‰ at 190 ka BP, to 2.6 ‰ at 90 ka BP'. The signal in core 64PE-174P13 goes from ca. 1.6 ‰ at 190 ka BP, to 1.3 ‰ at 90 ka BP (see fig. 2 in Scussolini et al. 2013). Where were the values of 0.4 and 2.6 ‰ taken from? In any case, this choice of such extended time frame is puzzling, as the sharp change in del18O occurs obviously across the glacial termination (ca. 140 to 125 ka BP)."

Regarding the assumed climate transition between MIS 5 and 6, unfortunately the quoted 0.4‰ at 190 ka BP was a typographical error, the actual value used was 1.4‰ (it was correct in the code in Supplement 01). The upper value of 2.6‰ was taken from fig. 2 in Scussolini et al. 2013 as the approximate mean value prior to the transition at around 140 ka BP.

Additionally, we could have described the logistic function more precisely. The end points of the function were set at 190 ka BP (1.4‰ and 90 ka BP (2.6‰ but most of the transition occurs during a much shorter window between about 130 and 135 ka BP. We will improve the description in the revised version.

4. Bioturbation depth:

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*"Further, assuming bioturbation reaching 10 cm from the top of the sediment will obviously produce a peak in variability in any record across a signal transition such as a glacial termination. While it is unrealistic to think that bioturbation is absent from core 64PE-174P13, Scussolini et al. (2013) advanced multiple lines of reasoning to exclude strong bioturbation in core 64PE-174P13, not least visible laminations in parts of the record (see also the author's response to referee 1, who raised specifically the point of bioturbation: <https://www.clim-past-discuss.net/9/C511/2013/cpd-9-C511-2013.pdf>). An additional argument against the role of bioturbation and in favor of an interpretation of the variability signal as proxy for Agulhas rings comes from Scussolini et al. (2015, Geology, doi: 10.1130/G36238.1). There, a tight coupling is shown between the Agulhas rings proxy with the ice-volume-corrected seawater del18O of *G. truncatulinoides*, a proxy for the high salinity anomalies that Agulhas rings seem to have introduced at the core location (see below a snapshot of the relevant figure in Scussolini et al. 2015, showing the two proxies). It is important to note that the two proxies are analytically independent of one another. It is not clear from the manuscript whether the authors have reasons to prefer the interpretation of the signal in terms of bioturbation."*

We accept this point. The plausibility of bioturbation as an explanation for the variance peak will depend strongly on the bioturbation depth, which is poorly constrained.

We have re-run these simulations using a range of bioturbation depths and using the depth-resolved input climate and habitat weights mention above in place of seasonality. While the peak in variance remains clear down to bioturbation depths as low as 3 cm, the absolute value and width of the variance peak are a little lower than that seen in Fig. 2 of Scussolini et al. 2013 (see Fig.1). At the same time, for bioturbation depths of 3 and 5 cm, the apparent speed of the climate transition is consistent with the sharpness of transition (approximately 8 ka) seen in the bulk record for *G. truncatulinoides* in Fig. 2. of Scussolini et al. 2013 (see Fig.2). However, for 10 cm of bioturbation the transition is too spread out.

We cannot of course exclude enhanced Agulhas leakage as the source of increased

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IFA variance across the MIS 5-6 transition, and as noted there is other evidence for increased leakage such as the tight coupling between the Agulhas rings proxy and the $d^{18}O$ of *G. truncatulinoides*. However, given that bioturbation depths as low as 3 cm still produce a quite visible variance peak we think that bioturbation is at least a plausible mechanism behind some of the change in variance over the MIS 5-6 transition. We will modify the manuscript to improve the description of the simulation, to describe the use of depth rather than seasonal weighting, and to make clear that we see bioturbation as a possible alternative mechanism but that this depends heavily on the parametrisation.

Once again, we thank you for your comments,

Regards,

Andrew Dolman.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-13>, 2018.

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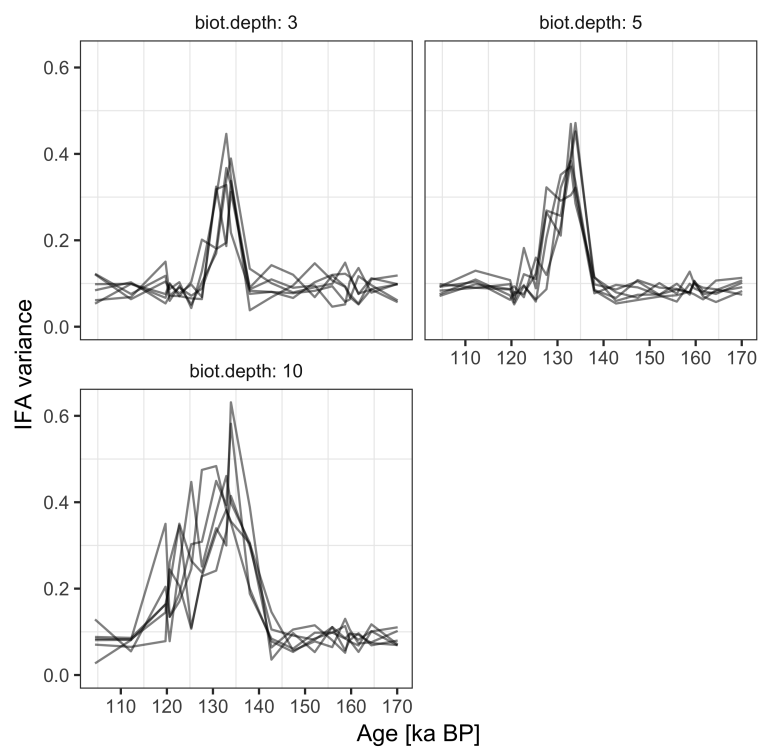


Fig. 1. IFA variance for different bioturbation depths.

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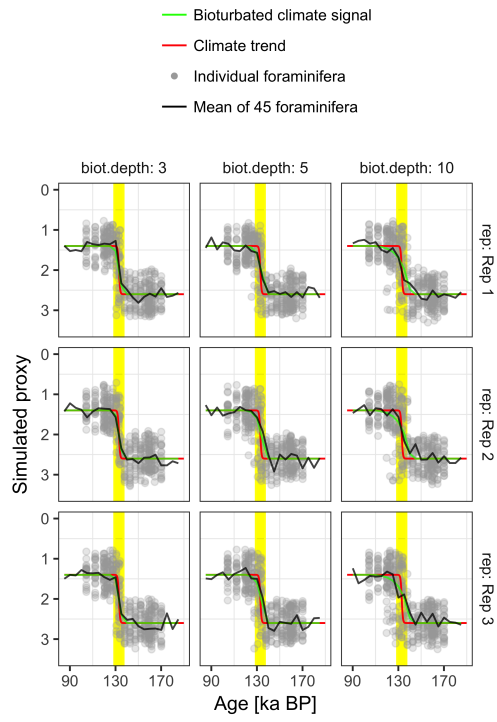


Fig. 2. Simulated bulk and IFA proxies for different bioturbation depths.