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Climate of the Past Discussions

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

B. Metcalfe (Referee)

brett.metcalfe@lsce.ipsl.fr

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B. Metcalfe

Quantifying uncertainty within palaeoclimate archives is essential if we are to make better predictions of the past, in light of this Dolman and Laepple (2018) have produced a R package, sedproxy, that combines the seasonal weighting of Mix (1987), the mixing through bioturbation of Berger and Heath (1968) and the measurement noise within Laepple and Huybers (2013) that is handy and easy to use. The authors have provided examples on a couple of applications that showcase the potential of their code. These examples are logical for the most part, however why not continue this trend and provide an example of IFA based upon MD97-2141 by expanding Figure 4. Moving

the discussion of Scussolini et al's (2013) results til later, that way a new-user has an example with the same dataset from start to finish?

The problem with this paper though, is that whilst it is needed by the community the authors seem to be presenting code that is more a version 0.5 as outlined, throughout the text, by the authors themselves. Throughout the text the authors offer suggestions of 'easy' improvements that they could do to their own code, which is commendable. However, in a couple of instances they note that other code, by other groups, exists that does a similar job and in some parts this weakens the whole. For instance, sedproxy's "season and depth habitat in the recording (but not necessarily the climate) is assumed to be invariant over time" which contradicts Mix's temperature based weighting of the seasonal signal (Roche et al., 2017). The authors state that this will help to compare models with proxy data, but if the monthly weighting is static through time can't someone bypass sedproxy and compare model-March, or a seasonal weighted, output with G. ruber Mg/Ca directly? Likewise, is March really equivalent through time?

The model also uses the same units as the input series, "we do not explicitly model the encoding process for specific sensors. Other tools have been developed to do this. . ..and could be used to pre-process the input climate signal" with the authors suggesting that "a back-transformation can then be applied to the generated pseudo-proxy records, which itself might model uncertainty by varying the parameters of the calibration". My question, why not cut out the middle man in which they risk being supplanted by the code of others and add this into their code? In trace metal geochemistry the calibration(s) of Mg/Ca vs. Temperature is by far one source of error that is overlooked repeatedly. Likewise, the authors should consider who will be their end-user (e.g., whether some end-users may or may not be comfortable with or take the time with pre-processing the data using other code). Therefore, I think the paper could benefit greatly from expansion of the code in ways that the authors themselves list.

Specific comments

(Pg. 3 Line 11-12) “we do not explicity model the encoding process for specific sensors” maybe explicity state for clarity that sedproxy doesn’t model conversion between temperature and Mg/Ca or Uk37, i.e. calibrations are not used. As it is not clear, as demonstrated by Reviewer 1: “ The mathematical formulation of the transformation from Mg/Ca (and UK’37) to temperature is not clear in the text. Which calibration is being used? Can the user input one of their choice?” Perhaps making this clear earlier (on page 3) like you do later at pg 14 line 31 – pg 15 line 5 would benefit the readership.

(Pg. 4, Line 10) “We assume here that these effects are minimal” Dissolution is far from minimal, the lysocline is a marked boundary because it is when dissolution becomes apparent (because the rate of dissolution increases) but dissolution is still occurring above the lysocline. Berger suggested that only a small percentage of the flux reaches the seafloor / ends up preserved. If one were to consider it theoretically, productive months (rich in Corg) will likely lead to increased benthic activity and increased CaCO3 dissolution. The authors acknowledge that sedproxy doesn’t include a flux component (pg. 15 lines 6-16), if they do add in such a component, it is worth considering that some seafloor processes might also be seasonally driven (or driven by seasonal flux of food/organic matter that can be respired, to the seafloor).

(Pg. 4 Line 16) “Due to bioturbation these individuals will be a mixed sample that integrate the climate signal over an extended time period” I would disagree that this is solely a function of bioturbation, low sedimentation rate (e.g. 1 cm per kyr) means that individuals are from potentially any point within 1000 years irrespective of benthic seafloor processes. Perhaps mention here, that low SAR is already a ‘smoothed’-integrated record regardless of bioturbation.

(Pg. 7 Section 3.3) It would benefit the reader, and add clarity, if the authors better express this section so that sedproxy doesn’t become a black box. The independent error term for each proxy type, am I correct in assuming that this is the same as: (Laepple and Huybers 2013; Section 5. Application of the correction filter) “each record requires estimating the two adjustable parameters that define the background variability: the

spectral slope (beta) and the standard deviation associated with (eta). We perform an exhaustive search over the values of beta = (0, 0.1,...1.9, 2.0) and STD(eta) = (0, 0.05,...1.95, 2.0), searching for the pair of values that minimize the mean square deviation between the logarithm of the observed sepectra and logarithm of the model spectra.” later on in the same 2013 paper stating “and a 0.25 and 0.45 standard deviation of η is prescribed for Uk37 and Mg/Ca respectively”. I think, within the text of this paper, the authors need to justify the value of the standard deviation of their gaussian random variable, how it is constructed for each proxy, its limitation etc. As this will essentially create a model-specific result.

Furthermore, how is cleaning of foraminiferal tests parameterized? Would using the Mg/Ca variability in culture studies be a better source of inter-individual variability parameterization (i.e. one question to ask is, is inter-individual variability constant over values of temperature cultured or does it vary)?

(Pg. 10, Line 1&2) “the input climate signal smoothed to centennial resolution” why have the authors smoothed the input variable? Does this not contradict the point of the model? Furthermore, how was it smoothed, which method? It is only mentioned here and table 1 (where “block average” smoothing is identified) that there is mention of smoothing in the record, this should be stated within the main text.

Figure 3 – would it not be better in panel one (input climate) to show the annual minimum or maximum (as a shading)? Your model has a seasonal weighting component therefore the ‘full range’ should be included, at present the figure at a glance (without reading the caption) appears to show a narrow temperature window. It also makes it difficult to envision the seasonal weighting. Furthermore, might it be prudent to show the measured proxy values of temperature in more than one panel (other than panel 6)? At least plot the forward model and proxy result together in panel 5. Additonally, what is the error on the reconstructed temperature from Mg/Ca?

(Pg. 11 Section: Influence of the number of foraminifera per sample) Is figure 4 only a

single run of each n = 1 and n = 30 for G. ruber? If so, would it not be better to produce a figure similar to Figure 5 with replicates. It would/might show that replicates of n = 1 have a larger spread than replicates of n = 30. . . or not.

(Pg. 13 Section 7) Globorotalia truncatulinoides is a deep dwelling planktonic foraminifera ($\sim$500 m), the rationale behind Scussolini et al.'s species selection was that deeper dwellers would exhibit perturbations within the water mass through the movement of Aghulus leakage rings. Therefore, what is the rationale for adding a seasonal component (Pg. 13 Line 14) in waters >500 m that have little seasonality? (see figure 2 in Scussolini and Peeters 2013)

Also and this is just a point of note regarding Figure 7's mean of 45 foraminifera: larger planktonic varieties (such G. truncatulinoides) are generally heavy, most modern mass spectrometers have an upper or lower end in weight, the standard number of foraminifera that constitute 'bulk samples' of heavy foraminifera is 3-5 specimens (i.e. Cleroux et al., 2013 used 10-25 specimens to make four aliquots, x2 for trace metal and x2 for stable isotope geochemistry). Scussolini and Peeters 2013 took a small portion of a large number of shells thus negating this weight limit: "Between 35 and 55 shells for each species were crushed, and a portion of approximately 150âĂL'μg of homogenized calcite fragments was used for stable isotope analysis. This approach was adopted to maximize the number of shells involved and therefore the analyses' representativeness of the foraminiferal population". In the past measurements came from samples with more specimens, that is not the case today, so perhaps a mean with fewer specimens would be more fitting?

(Pg. 14 Line 23) "this enables more quantitative comparisons to be made between climate models and proxy data than would classical direct comparison" whilst sedproxy is for the most part better (theoretically) than a simple comparison of proxy data with Mean annual temperature provided by models, does the fact that neither season or depth vary add its own source of error?

(Pg. 15 Line's 32-35) The funnel effect, at least in sediment traps, in which foraminifera deposited may in fact be 'expatriates' does certainly suggest that foraminifera may not have a signal that is directly related to that above the core site. Personally, however if you combine the depth integrated growth (e.g. Wilke et al. 2006 and references therein) with the suggestion in culture of precipitation of calcite on preceding chambers then for the most part the signal preserved within a shell will be overprinted by the final chamber's signal, or a depth weighted function (Roche et al., 2017). Therefore, a model would need only to take into account the distance covered following mortality (settling speed $\sim$1-2 days from surface to sediment)

Technical

Pg. 1, Line 21: Remove 'marine', replace with planktonic or pelagic Pg. 2, Line 13: Would Mix 1987 and/or Mulitza et al., 1997 not be more appropriate references for 'the influence of seasonal recording' Pg. 3, Line 21: remove duplicate 'thus' Pg. 5 line 3: change 'or' to 'including', as vital effects (the potential metabolic effects) are not exclusively inter-individual variation (given the individual life histories of foraminifera found within the sediment and or plankton tow samples Pg. 13 Line 9 'choose parameter values resembling this study' but then state further 'these choices are partly arbitrary' Pg. 15 line's 25-27 The scenario envisioned is performed by Lougheed et al. (2017)

References Scussolini, P. and Peeters, F. J. C.: A record of the 460 thousand years of upper ocean stratification from the central Walvis Ridge, South Atlantic, Paleoceanography, 28, 426–439, doi: 10.1002/palo.20041, 2013 Lougheed, B., Metcalfe, B., Ninnemann, U., and Wacker, L., Moving beyond the age-depth model paradigm in deep sea palaeoclimate archives: dual radiocarbon and stable isotope analysis on single foraminifera, Clim. Past Discussions. 2017 Wilke, Bickert, Peeters, The influence of seawater carbonate ion concentration [CO32-] on the stable carbon isotope composition of the planktic foraminifera species Globorotalia inflata, Marine Micropalaeontology, 58, 243-258, 2006