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Geoscientific Model Development Discussions

Interactive comment on "FAME (v1.0): a simple module to simulate the effect of planktonic foraminifer species-specific habitat on their oxygen isotopic content" *by* Didier M. Roche et al.

Anonymous Referee #1

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The manuscript by D. Roche et al, proposes a new module to simulate the effect of foram species-specific habitat (namely depth and season) on their isotopic content. This short and well-structured manuscript builds on the FORAMCLIM model to draw conclusions on the specific living depth of foraminifera. I greatly appreciated the fact that the code is open source, which will be of help not only for modellers but also for the paleo and modern foram community.

The manuscript posits that the growth and habitat of planktonic foraminifera can be simply described using a set of parameters, derived from culture experiments, which are mostly dependent of the temperature. This is based on the Lombard et al, 2009

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Discussion paper

FORAMCLIM model. This model is compared to the Late Holocene MARGO data-set, in order to have a first order idea of the distribution of planktonic foraminifera, and through the coupling with the d18O module computation from salinity-regressions, the range of d18O one could expect for a significant cooling (ΔT=4°C).

This is a stimulating contribution which from a micropaleontologist point of view raises a few questions, and two main ones.

My first concern in the manuscript is the model-data comparison: there are some visual comparisons by overlying the percentage of foraminifera species to the presence at some time of the year, with an ad-hoc threshold at 10%. As the focus is based on the oxygen isotopes, I wonder why the authors use the species distribution for testing this model which has been already validated by Lombard on plankton nets and surface sediments (just using the surface temperature). The issue here is to find an independent data-set to validate their isotopic model. I wonder if some stable isotope sediment trap data could not be a better benchmark to validate the model, rather than the comparison with surface sediments. There are some time series in the South China Sea (Lin et al, 2011); in the Gulf of Mexico (Thiraulamai et al, 2015), and the works lead by R. Thunell among others.

A second concern is the propagation of errors throughout the model which is not properly dealt. As the model picks a best fit for the response of the growth of foraminifera to temperature, and that in the code (and in the original paper), the uncertainties are given, it would be useful to propagate the errors of the response (growth) of foraminifera to temperature. It would be extremely useful for the community, as it would give the reader a sense of the sensitivity of the models. This point is also detailed below.

Technical comments :

[l. 18 page 1]: Expand the connection between the stratified nets and the isotopic derived values of Emiliani more in details. In Jones, there is no reference whatsoever to any isotopic analyses. You are making the connexion, but this was not put forward by Jones.

[page 2]: “It is this tempting to make one additional step”: I do not understand this statement : It is a fact that oxygen isotopes have been implemented in models, but yet, this is not the topic of the paper as the d18O values of seawater are computed from empirical basin correlations between d18Osw and salinity, not from water isotopes enabled models. I would recommend to move this sentence in the perspectives, as it is misleading here and one quick reader might think that isotope models were used.

[equation 1, page 3]: I would add here a reference to the original work (Kooijman 2000) which formalized this equation as referred in Lombard et al (2009).

[line 5, page 4]: The authors do use a Tl of 280 for G. bulloides instead of 281.1. This shows that the model is extremely sensitive to a minor change in Tl : could you please elaborate a bit on the reason on this 1.1°K shift ? Did you perform some sensitivity analyses to reach this temperature ? This is appealing because the overall inferred isotopic equilibrium depth calculated for this species is off the charts (see point below).

[Figure 1 page 4]: Fig 1 - I would add the original data as in this figure we lose the range of amplitude observed in cultures

[Figure 1 page 4]: Add reference to Lombard et al., 2009 in the figure caption

[page 7] Calculation of the best-fitting maximum depth: âĂć What is the rationale for assuming that the Late Holocene equilibrium isotopic value value would be the maximum depth in the model ? Do you imply that the isotopic signature of foraminifera is biased toward the maximum calcification depth ? âĂć The range of the depths calculated by the model are very deep compared to observed living depth. The most extreme case is G. bulloides : if one uses the last textbook written by R. Schiebel & Hemleben (Modern planktonic foraminifera, 2017) “Ecology: Globigerina bulloides mainly dwells above the thermocline within the upper 60 m of the water column, and is a non-symbiotic species”. The ecology of this species is extremely problematic, and

likely due to a combination of multiple cryptic species (eg Morard et al), I would tend to think that the cultures did not catch the overall variability in the dataset. âĂć I do not understand how does G. ruber has a living range reaching $+\infty$. It would be extremely useful to have a figure putting into context the ranges (by comparing with Rebotim et al, for example event though this is a single figure).

[lines 18-24 - page 7]: I do agree that those two effects (gametogenic calcite and dissolution) can somehow impact the signature of d18Oc in G. sacculifer. Yet, as G. sacculifer is bearing symbionts, it does have to live in the euphotic zone, which is not the case in the model. I suggest that the authors make a more solid case by removing the deep Pacific sites that they supposed to be influenced by the dissolution to check whether the origin of this deep signature is indeed mostly gametogenic.

[lines 28-30 - page 7]: As the error scheme does not include the error linked to the calibration of the FORAMCLIM model. It would be extremely interesting to have an idea of the sensitivity of the FAME model to the max/min range observed in the data set.

[line 31 - page 7]: I disagree with the statement that G. sacculifer and G. bulloides can be called "deeper dwellers". The output of the model does rank them as deeper dwellers, but out at sea, they do live mostly in surface to subsurface layers of the ocean (see for example Schiebel and Hemleben, 2017).

[Table1 - page 8]: The range is definitely too deep for G. bulloides (ibid.)

[Figure 3 - page 10]: I do not really understand what the figure does show : a percentage is highly depending of other species percentages – see my main comment #2. What is the rationale for the cutoff at 10% ? I do not see a physical nor biological rationale for this cutoff. I am wondering if the spatial coverage in the Indo Pacific Ocean is good enough to be included in the analysis as most core tops come the Atlantic Ocean.

[Figure 4 - page 11]: Consider changing the color scheme- rainbow does not give the best rendition.

[page 12]: Add a latitudinal/depth plot, it would be more easy to read.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-251/gmd-2017-251-RC1-supplement.pdf

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