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Interactive comment on "Modeling seasonal and vertical habitats of planktonic foraminifera on a global scale" *by* Kerstin Kretschmer et al.

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I have carefully read the manuscript 'Modeling seasonal and vertical habitats of planktonic foraminifera on a global scale' by Kretschmer and coauthors, which presents a model to predict global concentrations of five species of planktonic foraminifera and their depth habitat. This model could aid paleoclimatologists to correct for habitat depth when using shells of planktonic foraminifera to reconstruct ocean conditions. I need to remark that I have no experience using PLAFOM, or any practical experience with either the BEC model or CESM1.2(BGC) configuration. Therefore, my comments are rather general and an experienced user should review e.g. the use of model parameters and choice of configuration. I only have a couple of remarks that mainly focus on the usability and applicability of the model to reconstruct past depth habitats.

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General comments

In general the authors should avoid certain ‘model jargon’, if they want to convince the broad foraminiferal society to use and apply this model. It is sometimes difficult to follow which steps are taken and assumptions were made to test or simulate certain scenarios (e.g. page 6, lines 23-25).

Even though habitat tracking is very important when using shells of planktonic foraminifera to reconstruct ocean conditions, it is still (more?) crucial to pinpoint the actual calcification depth within the depth habitat, since this is where the calcite is formed. Even though the model can reasonably well predict (globally) the vertical distribution, this does not mean that at this specific depth the environmental signal was ‘logged’ into the shell. Please include somewhere a couple of sentences on the reconstructed depth habitat compared to the actual calcification depth. Could this be the next step for PLAFOM3.0?

Section 2.3.1. What about other ocean parameters that vary over geological timescales which might influence growth rates? Like [PO3−4] (Aldridge et al., 2012, BG) on SNW or the effect of carbonate chemistry on calcification rates? For instance Lombard et al., 2010 found lower growth rates of several species with lowered [CO32-] conditions and Davis et al., 2017 (Sci. Rep.) observed lower calcification rates with decreasing pH. Why are these parameters not taken into account in the model? Are these effect minor compared to temperature and food availability?

Section 2.5.2. and 2.5.3. The authors use the sediment trap/plankton tow samples to test the accuracy of the model in predicting seasonality & depth habitats. However, the amount of data used for this comparison is not covering the total range of oceanic settings, since big parts of the ocean are underrepresented. Is it possible to extend this database by adding other published sediment trap data? This way you can show your model can predict depth habitat in a wider range of ocean conditions, which will make it more robust for application in deep time. Just some quick suggestions: Mediterranean

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Sea: Mallo et al., 2017 BG; SW Atlantic: Venancio, et al., 2016 Marine Micropaleontology; Mozambique channel: Steinhardt et al., 2014 Marine Micropaleontology; Panama basin: Thunell et al., 1983 EPSL; Indian Ocean: Guptha et al., 1997 JFR.

Figure 2. Is it possible to add an 'offset map', in which you correlate e.g. the coretop data with the model data, to see where the model exactly over-/underestimates the data? This way you would be able to perform some (correlation) statistics, and this would clearly show the areas where the model didnot predict the correct distribution. I understand you are trying to capture the global signal (as stated several times in the manuscript), but paleooceanographers are more interested in specific areas when correction for e.g. depth habitat, and these are often also in more complicated oceanic settings (for example coastal/upwelling/river run off areas).

Page 11, line 27-31 and page 12, line 20-21. The authors state that part of the mismatch between the model and coretop data might stem from different genotypes having varying ecological preferences, and therefore their own unique model parameters. If so, does did not create a major bias for the whole model, especially when reconstructing depth habitats in deep time? For geological samples it is not possible to distinguish between genotypes, and therefore certain species might respond different in terms of depth habitat than the model will predict? Also, could it be that certain ecological preferences have changed over time? Can the authors predict how far in geological time you could still use this model to obtain reliable data on global distribution and depth habitat?

Minor comments

Page 2, line 18, 32; Page 6, line 16; page 11, line 23: Some problem with bracketing, e.g. double backeting etc.

Page 6, line 24: quasi-steady

Page 7, line 15: space missing between '(Figure 1a).' and 'We'

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Page 8, line 5 and page 11, line 17: Arctic Circle

Page 12, line 10-14. Can you explain the underestimation of the model in scenarios were assemblages are dominated by two species?

Page 12, line 21: change or remove ‘see’

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