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Interactive comment on "A dynamic global model for planktonic foraminifera" *by* I. Fraile et al.

I. Fraile et al.

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Our manuscript has received constructive interactive comments by F. Peeters. We are most grateful for the very helpful comments. Herewith, we would like to respond to the main points brought forward in the review.

Answers to interactive comments by F. Peeters:

Following the recommendation of the reviewer, we changed the title of the manuscript (Predicting the global distribution of planktonic foraminifera using a dynamic ecosystem model). Because the reviewers suggest to include additional information the shortening of the manuscript is not possible. We also think that merging results and discussion is generally not appropriate.
In order to compare spatial distribution patterns, we used the 'Root Mean Square

Error' between model output and core-top data. The use of statistic for the time series comparison turned out to be difficult. We focused on the season when maximum foraminiferal production occurs (Table 4 in the new manuscript)

Comment: ***'Page 4327, line 11. ?For the implementation, all vertical structure is ignored ?.?.I'm not sure if I understand this correctly, but if this means that the entire upper water column is similar to the mixed layer properties, then I believe this choice in your model set up is an oversimplification with great consequences for the model outcome...'***
"For the implementation, all vertical structure is ignored". This refers to the mixed-layer structure. We assumed a mixed layer biologically and chemically homogeneous, which is equivalent to assuming that the physical mixing rate is fast compared to the growth rates of the organisms. In case of foraminifera this assumption could be a oversimplification, but the five species of foraminifera simulated with our model live in the upper part of the water column for most of the life cycle . Results might be improved by adding a third dimension. Nevertheless, this is a project on its own since it will require also major changes of the ecosystem model.

Comment: ***'Page 4328, line 1. I'm not aware that P.F. feed also on ostracods and pteropods. Are you sure? I do not know all refs. that are given I have to admit.'***
Spindler et al. (1984), elaborated on feeding behavior of PF in laboratory experiments says: "The spinose species feed mainly on zooplankton organisms, such as copepods. Copepods are the main food source although other crustaceans (amphipods, ostracods and various crustacean larvae), tintinnids, radiolarians, polychaete and gastropod larvae, heteropods, pteropods, and tunicates are digested as well". In any case, our model only contains a general group of zooplankton, which includes the organisms mentioned.

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Comment: ***'Page 4328, line 14. Foraminfera of different size have different carbon concentration. How is this dealt with in the model?'***
As we state in the original manuscript (Page 4334, line14-21), first, we calculated the volume occupied by the cytoplasm for each species using mean sizes for each species (Peeters et al., 1999) and approximating the shape of foraminifera to a sphere. We assumed that all the space is occupied by the cytoplasm and that the carbon content of the cytoplasm is the same in all species (value of 0.089pgC/μm^3 from Michaels et al., (1995)).

Comment: ***'Page 4328, line 16. As diatoms are listed separately, what is small phytoplankton?'*** Small phytoplankton is the general group of phytoplankton with exception of diatoms and diazotrophs. We explained this concept in the manuscript (Page 4, section 2.1, line 11-13). More details can be found in Moore et al., (2002).

Comment: ***'Page 4329, line 5. Table 1 caption is incomplete. I.e. the parameters listed in the table are not all discussed in the caption. E.g. p2SP P2D, p2Z etc. Or did I miss something?'***
We changed the name p2 to $p\prime$ andincluded to the listed parameters in Table 1.

Comment: ***'Page 4329, line 19. ? To include this contradictory informaition??. I don not understand the part ?..when productivity is maximal??. What productivity do you mean: primary prod. Or secondary prod.? Note that the timing in the maximum of primary or secondary productivity is different, and that these maxima also differ from the time when inorganic nutrients differ at the sea surface.'***
G. ruber (white) has preference for oligotrophic waters (low productivity, both primary and secondary). This mathematically implies a negative relation with food availability. On the other hand it grows during the months in which food availability increases, requiring a positive relationship to food. To account for adaptation to low productivity

regions we limited the growth to oligotrophic waters using maximum nutrient and chlorophyll concentrations (productivity indicators) as a separate input variable to monthly nutrient and chlorophyll data. We applied a 'positive relation' with monthly food availability (the same as the other species); but we multiplied the 'Total Grazing' (Eq.2) by a hyperbolic tangent function. This function becomes 1 when maximum nutrient and chlorophyll concentrations are below a threshold values, and 0 when it is above the threshold value (clarified on Page 7, line 15-19 in the updated manuscript).

Comment: ***'Page 4329, line 26. What do you mean with growth? I guess not the growth but the reproduction rate varies as a function of food abundance or type! (...)'***
Growth rate refers to the growth rate of the population, and not the growth rate of a individual. The main factor increasing the size of a population is indeed reproduction.
'(...) In addition, specimens during a period of strong production are also smaller, which in turn may affect the amount of Carbon per specimen. (...)'
Our model is based on biomass, with units of mmolC (organic carbon); does not account number of individuals. Therefore, under the assumption that the carbon content of the cytoplasm is constant, the size of the individuals does not affect the biomass (there could be more small ind. or less and bigger ones).

Comment: ***'Page 4330, equation [3]. Although this is often used see also [7] (I know that Michael has a copy of this paper), I must admit that there is no strong evidence for this Gaussian temperature response. Or is there? Alternatively, why not just accept a temperature range in which a species can exist (like a simple block function?). Please discuss.'*** The "normal distribution" in respect to temperature is based on Žarić et al. (2005). Without a strong evidence to use another type of distribution, we think that a continuous function, such as a Gaussian relation, could be more realistic than a 'block function'. Furthermore, a 'block function' would

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make the model mathematically unstable. We still can adjust the standard deviation (temperature tolerance range), σ, making the Gaussian bell narrow or wide.

Comment: ***'Page 4334, line 13. I?m not sure if I understand this correctly: the relative abundance of species from sediment samples were recalculated using only the five species that are under consideration in this paper, in such a way that hundred percent thus is the sum of the relative abundance of G. bulloides, N. pachyderma (sin), N. pachyderma (dex.), G. ruber and G. sacculifer! Correct? If indeed, you may want to make this more clear'***
We modified the text so this is more clear for the reader (Page 11, line 28 - Page 12, line 1).

Comment: ***'Page 4335, last two paragraphs (on sediment traps). I do not understand why the trap shell flux data, for traps that operated longer than one year, for a given location were not monthly averaged?'***
There are two reason why the monthly averages are not shown: Firstly, the sampling periods do not coincide with calendar months. Interpolating these data introduces an extra error. Secondly, interannual variability is high. If the maximum shifts in time from one year to the other, calculation of monthly mean can even result in a lost of a peak in the averaged time-series (e.g. textitN. pachyderma (dex.) time-series record at Northwest Pacific). Therefore, we decided that the original time series data is the most adequate for comparison. On the other hand, most of the available time series of sediment-trap data have a sampling duration of one or few years, and have some gaps during this interval. The use of statistics makes sense in long time time series, but in our case we have tried to look and explain year by year. In the revised manuscript we include the distribution of time lag between maximum production month in the model and maximum production month in trap data.

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Comment: ***’ [...] Calculating the difference between model and observations, using a Chi-square test for the goodness of fit, for example, will also allow the authors to quantitatively express their findings, which is something I believe vital here to carry out. The results of such analyses will provide testable numbers on how well their model, given the present parameter values, performs.’*** We considered this recommendation very carefully. However, a meaningful statistical test is hampered by several factors:

1) For a Chi-squared test the spatial autocorrelation would lead to a reduction in the degrees of freedom. This reduction is not straightforward to assess given the sparse distribution of the data. A further problem arises because the test assumes counted data for which error follows a Poisson distribution. Unfortunately, we do not know whether or not this assumption holds true for the foraminifera counts.

2) As alternative we considered a Monte Carlo test. Again the problem is that error of the core-top data is not known. Since the model has no internal variability, it is also not possible to add meaningful random errors to the model output.

3) Syrjala (1996, Ecology, 77, 75-80) described a permutation test for assessing the difference between spatial data. We successfully implemented this test. However, during our analysis we found that the test is not robust and depends very much on the choice of the coordinate system in which the data are represented.
Since we trust the visual comparison more than some vague statistical test, we decided not to include any statistical test in the revised text.

Several issues raised by the reviewer on pages 4336 to 4342 have been adjusted accordingly.

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Comment: ***'Page 4343, line 9. It is with great surprise, given that one of the co-authors of this paper himself is involved in genetic studies of modern planktic foraminifea, that I here read: ??different genotypes of N. pachyderma (sin.) can be grouped as a single species?. I guess the authors have at least twice considered their wording here, as this is quite a firm statement (or a is this just a ?slip of the tongue??).*** The sentence was meant to refer to the model, and not taxonomy. The differences in ecological preferences of different genotypes of *N. pachyderma* (sin.) are small (Darling et al., 2006). Therefore, we conclude is that for this particular model (which predicts general distribution patterns) and this parametrization the different genotypes of *N. pachyderma* (sin.) can be grouped and treated as one ecological group in the context of our model.

Comment: ***'I'm not convinced, that the model also performs well when looking only within the temperature range were the species occurs. Such data, analysis of the present results, cannot be found in this paper (which is a weak point of the present ms. So, I just wonder, for the species discussed here, how much of the total spatial variability can be explained by only temperature, and thus by this very simple mathematical constrained to temperature alone?')***
Using statistics to quantify how much of the variation is explained by the temperature and how much by the food turns out to be difficult. The main reason is that food availability is also temperature dependent and statistical analysis assumes that the variables are independent. On the other hand, for the reason that it is a dynamic model the foraminiferal biomass is time dependent. Thus, for each combination of temperature and food we can find any value of the biomass. We tried do a qualitative analysis showing the results for *G. bulloides* in the experimental run with constant temperature. With a constant temperature of 12°C, the absolute biomass does change, but the seasonal pattern of *G. bulloides* remains very similar to the standard

run. This experiment suggests that within the temperature tolerance range food availability is the factor controlling the distribution.

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