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

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Review of Fraile et al. : A dynamic model for planktic foraminifera.

In this manuscript the authors present their numerical model "PLAFOM" that can be used to explore the response of planktic foraminifera to ecological/physical boundary conditions. The model aims to quantify the seasonal flux cycle of planktic foraminifera. Based on previous literature the authors have assigned values to model parameters that characterize the ecology of some commonly used planktic foraminifer species in palaeo-oceanographic studies. The model results on the spatial and temporal distribution patterns for various species are compared to sea-floor distribution patterns and shell flux data from sediment trap time series. The authors conclude that the overall spatial distribution patterns of most of the species are "comparable" to core-top data, and that the modeled seasonal variation (in the shell fluxes?) "match well" with the

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sediment trap records in most of the locations for the species *N. pachyderma* (sin.), *N. pachyderma* (dex.) and *G. bulloides*, while the model results? for *G. ruber* and *G. sacculifer* in general show lower concentrations and less seasonal variability in all sites. Model output is characterized by lower variability and lacks small scale variations in several locations.

I find this a very interesting approach and believe, despite some major concerns inherent to the fact that this study is the first of its kind, this is a brave and important attempt that aims to bridge the large gap between oceanography, biology, palaeoclimatology/oceanography and (predictive)modeling. There are, however, a number of comments and recommendations I would like to put forward here to improve the present version and I believe should be included/discussed in the revised version. I therefore recommend publication of this ms. with revisions.

Major recommendations:

Shorten the Abstract.

Shorten the manuscript by at least 10 - 20%! (I suggest to merge results with discussion).

Use statistics to indicate how well observations and model predictions agree!

I suggest that the authors should stress more clearly that this is an (first) attempt. I personally consider these results more like an example/illustration of what the model "can do", rather than "the truth". To date, there are still many uncertainties on the ecology of foraminifera.

<u>Title</u>: a more "active title" is recommended that includes the purpose of the model (predicting the temporal and spatial distribution of P.F.) as well as the kind of model used (a non-linear dynamic growth model coupled to an ecosystem model).

<u>Abstract</u>: **Shorten the abstract and be more precise in your conclusions!** Start for example with: "We present a...model that can be used as a powerful tool to...".

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Recommendation: start with the last sentence of the abstract and cut out stuff discussed in the introduction e.g. the first sentence. Be much more precise in reporting the results: e.g. line 12.: " Overall, the spatial distribution patterns of most of the species are comparable to core-top data". What do you mean with "comparable"? It is possible to compare anything with anything! This is no scientific writing and cannot be accepted in a scientific publication! This also holds for the sentence starting in line 16. "Modeled seasonal variation match well with sediment trap records in most of the locations for...".What do you mean with "match well", and what is "most of the locations"? **I stronly recommend to use statistics to indicate how well observations and predictions agree! See my later comments.**

Page 4326, line 4. "This bias arises...flux and temperatue". This is only partially true. Also (seasonally) varying depth habitats contribute to this bias.

Page 4326, line 10. I don't like the word "proxy producers". It are the different organisms on which the proxies are measured that have different ecology and hence seasonal preferences.

Page 4326, line 20. "...used as proxies". Proxies for what?

Page 4326, line 21. "...with observations from surface sediments and...". Be much more precise here: what kind of "observations"? It is obvious to me, but this kind of imprecise wording makes the paper hard to read.

Page 4327, line 8. "...and from climatologies". Give refs.

Page 4327, line 11. "For the implementation, all vertical structure is ingnored ...". 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. Most planktic foraminifera use the base of the mixed layer and the upper part of the seasonal thermocline to find food here, as this level is often associated to the DCM. (see papers

of e.g. [1-4]). I guess ignoring all vertical structure is thus very far away from reality and will strongly affect foraminiferal ecology and thus densisties. I guess this should be discussed and raised at the end of the ms. listing potential improvements of the present model.

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.

Page 4328, line 14. Foraminfera of different size have different carbon concentration. How is this dealt with in the model?

Page 4328, line 16. As diatoms are listed separately, what is small phytoplankton?

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?

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. See e.g. [5]!

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! G. bulloides for example may increase its shell flux by at least a factor of 100-1000 with weeks! This can only be explained by an increase in the reproduction rate in response to food availabitly. E.g. [6]. In addition, specimens during a period of strong production are also smaller, which in turn may affect the amount of Carbon per specimen. I realize, however, that this kind of information may be not possible to take into account into the present model. This, however, may be listed again at the end of the paper under "further recommendations for improvements of the present model".

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.

Page 4333, line 15. I now see mixed layer depth is included in the model. I'm not sure on my previous remark on mixed layer depth (see above).

Page 4333, line 28. You mean the mixed layer is "thin", not shallow.

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.

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? This would have allowed the authors to re-calculate monthly statistics on the fluxes and, most important, express the degree of deviation between model-output and observations! The model does not show any inter-annual variation anyway, so I don't get the point why this was not done? The monthly mean flux value can be plotted directly as a line in the existing figures, which will then also make clear which years are very different from this mean, and then can easily be compared to the modeled values. 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 quan-titatively 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. In addition, the results for different species and different locations can then be objectively compared to one anther, tested and fi-nally tabulated. This will provide the authors with robust arguments to base upon their conclusions. Another good argument to do this is that only in this way it will be possible

to compare directly and quantitatively *future* model results (new improvements of the model, other parameter values or a comparison to new trap series etc. etc.) to the results obtained in the present study.

GENERAL REMARK ON "RESULTS SECTION". Presently the Results section contains quite some background information, while from what there is written now, it is **not** clear at all how well the model and the observations agree! **This section thus strong revisions!** Besides a more quantitative discussion, consider shortening of this section by transferring information on ecology, spatial distribution optima as reflected in the core-top abundances, to section 2 (be selective). In my opinion, at the end in this chapter it is all about reporting model results and observations and reporting the most important issues related to agreement/disagreement for the various species.

Another alternative suggestion I would like to make is to merge results and discussion, as there seems quite some repetition in both sections.

Page 4336, Paragraph on *N. pachyderma (dex.)*. It is not clear from the text how well the model results for *N. pachyderma* (dex.) agree with the observations? Only where the model predicts high abundances. This should be corrected.

Page 4337, line 21. What is "upper temperature band"? Give temperatures instead.

Page 4337, line 24. "The relative abundance is lower compared to other four species in the model". This sentence is not clear at all. The relative abundance from where is lower? What is the relevance of this remark at all?

Page 4337, line 26. "...very low concentrations...". What are very low concentrations? Give numbers...

Page 4337, line 28. Again the same remark "...very high concentrations...".

Page 4337, "...in the upwelling of the Arabian Sea". You mean, "... in the sediments below upwelling areas in the Arabian Sea". This is not true, there's

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Page 4338, it is not necessary to mention the sediment trap areas, this information is already in the figure captions.

Page 4338, line 27. Again like earlier remarks: avoid phrases like ". . . the predicted concentrations match fairly well the trap record". This is scientific writing!

Page 4340, at various points: Avoid discussion here!

Page 4341, section 3.3. If you insist on making references in this section, in this context I recommend to mention the paper [8] here as well!

Page 4342, line 11. Instead of ". . . we can expect. . . ", I recommend". . . it can be expected. . . ".

Page 4342, line 15. Sedimentary assemblages??? Shouldn't this be "..fossil faunal assemblages.." or ". . . assemblages found in sea-floor sediments. . . " or something? Check this!

Page 4342, line 21. This is incorrect! There are other P.F. species that live in polar water masses!! It is a bit bold to say that it growth(s?) in sea-ice! *N. pachyderma* may survive in sea-ice!

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"?).

Page 4343, line 10-11. "in agreement. . . "? Although futher explanation is given in the next sentences, try to avoid such wording as it is imprecise.

Page 4344, line 13-15. I suggest, that this is because the turnover-rate of *G. bulloides* is higher than that of *N. pachyderma* (dex.). This is, as can be understood (it so far has not been quantified), not included in the model and may be suggested here as a

reason for the observations.

Page 4344, line 19-20. "There are a few. . . . . . model". This sentence is not clear to me at all. It is necessary to rephrase. I can't give suggestions here as I really don't know what you intend to say.

Page 4345, line 27-28. Although I leave this up to the editor(ial office) writing style of latitude & longitude should be in degrees/minutes/(decimal) seconds notation. Check this notation (In my impression such notation is only used in plotting and calculations)!

Peg 4348, line 18. I suggest to change "at monsoon seasons" into "during the monsoon seasons". Note here that highest production of zooplankton including planktic foraminifera is actually observed *after* the maximum in the monsoon wind-strength, i.e. when waters rich in nutrients warm-up. The productivity increase during the SW monsoon is caused by a different mechanism (upwelling) that during NE monsoon (deepening of mixed layer and erosion of the nutricline) [3].

Page 4349, section 4.3. There seems quite some repetition here, again, with results chapter. Why not merge results and discussion into one section?

Summary and conclusions.

It appears to me that much of the spatial distribution variance, and may be this holds for the temporal distribution as well, is largely the result of the modeled temperature tolerance and sensitivity of the species. It is for example obvious to me that the model output for the spatial *N. pachyderma* (sin.) patterns must show the highest abundance of this species in the high(est) latitudes, simply because of the modeled temperature preference of this species that is set to temperatures lower than 8 degrees C. Since such temperatures only occur in the high latitude polar regions, model output and observations must agree well. This however, does not indicate, or at least 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? It may be that another (much simpler) model, only taking temperature into account, may explain a similar amount of variance as the PLAFOM model? Although I do agree with these (and other) authors that food availability is the second, if for some species not the first, most important factor controlling the spatial and temporal distribution patterns, this is not so obvious from the experiments and data shown in this paper, as this is in the present article, only based on one experiment.

The authors have chosen to introduce, use, illustrate, validate and discuss their model, results in only one article. It therefore is quite a "big paper" that, in my impression, would benefit from a stronger focus. To me it appears that also authors themselves fight a bit with this problem of "wanting to do too much" in one manuscript). Although this approach may have some advantages (all is done is one paper), I do not fully understand their choice to discuss so much in one manuscript (therby loosing a bit of the focus). In my opinion, the authors would have made a wiser descision to first introduce their model, in a separate paper, giving themselves a bit of room for discussing all (model) details including a robust sensitivity test of the model and discussion of these sensitivity experiment results (that is now lacking here), and then produce a second (and or third paper) paper that deals with the the choice of ecological parameterizations, the use, application and validation of the model.

If the authors and editor do want to stick to this "one-article includes all approach", it is recommended that the authors shorten the paper including its abstract and, most important, carry out statistical analysis of the model results in relation to the observations. Robust statisitcal analysis of the results will

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ultimately form the basis for the conclusions that can be drawn here and is necessary in order to permit objective comparison of future model results to the present study.

I hope my comments and suggestions will help the authors to improve this manuscript, of which I think should be published after major revisions.

Frank Peeters, Amsterdam, 20 January 2008.

1 J.M. Watkins and A.C. Mix, Testing the effects of tropical temperature, productivity, and mixed-layer depth on foraminiferal transfer functions, Paleoceanography 13(196-105), 1998.

2 J.M. Watkins, A.C. Mix and J. Wilson, Living planktic foraminifera: tracers of circulation and productivity regimes in the central equatorial Pacific, Dee-Sea Research II 43(4-6), 1257-1282, 1996.

3 F.J.C. Peeters and G.-J.A. Brummer, The seasonal distribution of living planktic foraminfera in the NW Arabian Sea, in: The tectonic and climatic evolution of the Arabian Sea region, P. Clift, D. Kroon, C. Gaedicke and J. Craig, eds. 195, pp. 463-497, The Geological Society London, London, 2002.

4 R.G. Fairbanks and P.H. Wiebe, Foraminifera and chlorophyll maximum: vertical distribution, seasonal succession, and paleoceanographic significance, Science 209, 1524-1526, 1980.

5 J.P. McCreary, K.E. Kohler, R.R. Hood and D.B. Olson, A four-component ecosystem model of biological activity in the Arabian Sea, Progres in Oceanography 37, 193-240, 1996.

6 S.M.-H. Conan and G.-J.A. Brummer, Fluxes of planktic foraminifera in response to monsoonal upwelling on the Somalia Basin margin, Deep Sea Research II 47, 2207-2227, 2000.

7 J.B. van den Berg, N. Davydova, B. van de Fliert, F.J.C. Peeters, B. Planqué, H. van der Ploeg and G.M. Terra, Reconstruction of sea surface temperatures from the oxygen isotope composition of fossil planktic foraminifera, in: Proceedings of the forty-second European study group with industry, G.M. Hek, ed. CWI Syllabi 51, pp. 91-120, Shaker Publishing, Amsterdam, 2002.

8 G.M. Ganssen and D. Kroon, The isotopic signature of planktonic foraminifera from NE Atlantic surface sediments: implications for the reconstruction of past oceanographic conditions, Journal of the Geological Society, London 157, 693-699, 2000.

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