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Biogeosciences Discussions

Interactive comment on "Simulating the growth and distribution of planktic foraminifer using an ecophysiological multi-species model" *by* F. Lombard et al.

F. Lombard et al.

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In addition to the comments I give here, I have made many notes on grammar and syntax within my copy of the manuscript, which might be better communicated to the authors in personal.

Response: We corrected the manuscript accordingly to the reviewer's paper copy.

After having read the manuscript of Lombard et al., I would suggest changing the title of the paper to ' Modelling planktic foraminifer growth and distribution using an eco-physiological multi-species approach', which would suit the content of the manuscript better than the current title.

Response: The title has been changed accordingly.

The model presented by Lombard et al. is largely based on the earlier paper of Lombard et al. (2009), on modeling temperature dependent growth rates of planktic foraminifers. Accepting the ideas of Lombard et al. (2009), those results should be presented as brief as possible. In general, the present manuscript of Lombard et al. is very long, and would benefit from shortening. I would suggest combing the eight panels presented in figure 1, and including the model results (lines) only (possibly in colour), and in one panel.

Response: This figure was presented in the present state in order to have an idea of the model fit compared to the data extracted from Lombard et al. (2009). It is important to note that the "model" fit here is not a result from Lombard et al. (2009) but of the present model (combining nutrition, photosynthesis and respiration). However, those results were presented very briefly in only six lines. We judged this intercomparison was important to be presented at least in the review-discussion process but if needed this figure can of course be condensed in only one for the final manuscript. However having 8 species on the same figure (model and data together) risk to produce a rather confusing figure.

I do not agree to the modeling result of growth rates of N. dutertrei, though, which are not supported by data (dots in figure 1). Maximum growth rates around 25 deg C coincide with a gap in data, and maximum growth rates would be around 17 deg C from the data presented here. When re-evaluating temperature dependent growth rates of N. dutertrei, Lombard et al. may want to do the same for the other species presented here.

Response: we agree with the reviewer comment about the fact that there is an uncertainty here due to the gap of data between 20-28°C. However we did not agree with the fact that we should arbitrary fix the maximum to 17°C: since growth rate constantly increase in the 7-20°C region and the drop of growth rate was only recorded in

28-33°C, the maximum could be located everywhere in the 20-28°C region. In bibliography, N. dutertrei is considered to be in optimal condition within 23.2+- 3°C (Bé, 1977) and 13-33°C (Pujol, 1980). This means that our 25°C optimum seems coherent with observation (but maybe a little bit too high) whereas 17°C seems too low. The results shown here is the result of the model best fit compared to the data (statistical fitting, not arbitrary).

I do not entirely agree to the statement that there is 'about no information on the foraminifer population biology (i.e. fecundity, reproduction, mortality)' (p.23, lines 13-14; see also p.10, lines 7-9) [in general, Lombard et al. use the word population in a very broad sense, and might want to reconsider the use of the word for each specific case]. Information on reproduction cycles and mortality rates of planktic foraminfers can be deduced from, for example, Schiebel et al. (1997; and references therein). Information on all modern species is presented by, for example, Schiebel and Hemleben (2005). If not in the present model, the information on reproduction and mortality could be utilized in a future model approach of Lombard et al. I would be happy to supply more (published and unpublished) data to support the modeling approaches of Lombard et al. In combination with the growth rate, the mortality rate would then facilitate better estimation of foraminifer abundance (see p.12, lines 19-20).

Response: we corrected and completed the sentence in order to be clearer. Of course some information exists, but those are punctual and did not take in considerations some important processes such as the environment effect (i.e. T°C, food concentration or other such as salinity, turbulence, predators etc...) on fecundity, reproduction and mortality. In the present state, we could not include those processes based on so few results where so much interaction with the environment are unknown.

Does the initial carbon weight of 0.73 μg used by Lombard et al. (p.7, line 8) refer to cytoplasm carbon or calcite carbon or both? I would arrive at about 1 μg for the shell calcite C only.

Response: we completed the sentence. This corresponds to organic weight (i.e. cytoplasm). Our model currently do not consider calcite.

The paper of Bé and Tolderlund (1971) provides a good first approach to model planktic foraminifer distribution, but on a relatively limited size spectrum (>200 _m). More modern data bases might include smaller specimens, and would add information on small sized specimens (e.g., T. quinqueloba). Please take this as a suggestion on future work. In turn, p.17, line 22 to p.18, line 1 (Secondly: : :) could be cut from the manuscript, since it makes no sense to comment on work on different species, which has not been done.

Response: we chose to separate data sets in different categories corresponding to different levels of model construction (calibration or validation), and taking care to keep independent data sets for this and do not mix sample type between those two steps (not validate on the same data type that was used for calibration). More recent data sets such as multinet samplings were used for calibration, because we believe they were more confident and less biased than Bé and Tolderlund data. Using other data types should be of course done in future studies, but for validation we chose datasets that got the advantage to be homogeneous and with a large geographic scale (ie. Bé & tolderlund and MARGO). p.17, line 22 to p.18, line 1 (Secondly: : :) were significantly reduced.

The discussion of different morpho- (and geno-) types of Neogloboquadrina (p.24, line 23 to p.25, line 6) is insufficient and unnecessary in the context given here, and should be cut from the manuscript.

Response: This discussion is needed as it may explain some differences between the model and data, as well as it is a subject of taxonomic consistency between the different datasets used in our study.

The same is true for the discussion on carbonate ion concentration (p.25, lines 11-14).

Response: These different parts are, from our point of view, necessary to explain some discrepancies between model and data. They were rephrased in order to be shorter and clearer. Some were removed.

Figures 9 and 10 do present the final outcome of the modeling approach, and are hence of central importance for the manuscript. I would hence suggest providing more detailed figure captions. I guess that Lombard et al. want to say 'Estimated LOCATION of maximum growth rate: : :', which would explain the maps presented here.

Response: This have been modified in order to be clearer

Please take into consideration all of the papers so far published on the modeling of planktic foraminifer population dynamics, including: Fraile, I., Mulitza, S., Schulz, M., 2009, Marine Micropaleontology 72 (1-2), pp. 1-9 Fraile, I., Schulz, M., Mulitza, S., Merkel, U., Prange, M., Paul, A., 2009, Paleoceanography 24 (2), art. no. PA2216

Response: Those were now considered in the discussion part

References Pujol, 1980 Pujol, C., (Ed.), 1980. Les foraminifères planctoniques de l'Atlantique Nord au Quaternaire. Ecologie-Stratigraphie-Environnement. Mémoires de l'Institut de Géologie du Bassin d'Aquitaine, 10, Univ. Bordeaux I, France, pp. 254.

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