Biogeosciences Discuss., 11, C3679–C3682, 2014
www.biogeosciences-discuss.net/11/C3679/2014/

Biogeosciences
Discussions
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Interactive comment on "Projected impacts of climate change and ocean acidification on the global biogeography of planktonic foraminifera" *by* T. Roy et al.

Anonymous Referee #1

Received and published: 23 July 2014

I have read with great interest the manuscript of Tilla Roy and co-authors on 'Projected impacts of climate change and ocean acidification on the global biogeography of planktonic foraminifera'. Modelling the various effects of environmental change on the production of foraminifers from empirical data help to better understand past, present, and future scenarios of ocean and climate change. In turn, considering the limited base of data used here (only 8 out of some 50 morphotypes, and possibly many more genotypes), modelling approaches as the one presented here are mere sensitivity studies, and any further interpretation should be done with care. Unfortunately, the manuscript is based on some facts, which are not correct, and the final outcome of some lines of thought is incoherent and misleading. Nonetheless, I would assume that the manuscript will make a nice asset to the current understanding of planktic foraminifers and ocean acidification when getting the basic assumptions right, and when carefully interpreting and discussing some of the modelling results.

Starting from the abstract, it is very unlikely that planktic foraminifer calcite production will significantly affect marine carbon cycle (Buitenhuis et al., 2013, Earth Syst. Sci. Data, 5, 227–239). In addition, production of the calcareous planktic foraminifer tests has close to zero effect on the short-term marine CO2 budget by releasing the same amount of CO2 to the ambient seawater as fixed by test calcite.

Highlighted for the first time in the abstract, it is stated that geographical shifts [in planktic foraminifer species distribution] are driven by other factors (i.e. temperature (T) only) than vertical shifts (i.e., 'multiple drivers', again T and phytoplankton [possibly as prey]). To me, it is not clear why vertical and horizontal effects should result from different causes. In addition, it is almost impossible to disentangle the effects of temperature and phytoplankton distribution, the distribution of the latter being largely driven by temperature (plus nutrient concentration [also related to T at the global scale], and light). Further, planktic foraminifer species occur over a very broad range of temperature, and are not good indicators of absolute T and changes in T, as indicated by figure 1 (from Lombard et al., 2009, Marine Micropaleontology; and not correctly reproduced here; plus wrong reference – Limnology and Oceanography), and correctly stated later in the manuscript. The limits of planktic foraminifer 'optimum temperature' (Lombard et al. 2009, fig. 2) are identical with the limits of global ocean SST (just above 30°C in the Indian Ocean, and WPWP), and hydrographic fronts (polar fronts, $\sim$3-4°C). Ecological changes across hydrographic fronts include more than changes in T only, and effects on planktic foraminifers are –again- not easy to disentangle.

To my impression, growth rate curves produced by Lombard et al. (2009 and 2011) from laboratory data, can not directly be applied to naturals systems, and would result in over-interpretation. For example, specimens dwelling in warm, stratified, and oligotrophic waters of the subtropical gyres would possibly not reproduce at a monthly cycle due to lack of food, and might continue to grow. Assuming a monthly reproduction cycle, growth rates would be assumed to high in this case.

In addition to the more general comments above, I have listed some more specific remarks in the following.

1) What, to the author's idea, is the difference between 'environmental' and 'ecological' change (p. 10085, line 15). In the same paragraph, references for all of the seven points would strengthen the given statements. 2) P. 10086, lines 15-19: Planktic foraminifers are not dominant plankton in the oceans as can be seen from the final version of the paper of Buitenhuis et al. (2013), and planktic foraminifers are minor contributors to ballasting (please have a second look at the reference given (De La Rocha & Passow, 2007). 3) P. 10087, lines 7-10: Bijma et al. (1990) show limits in T and S, and Bijma et al (1992) show that other factors than T are also important to explain distribution pattern. In general, many references are not correctly used. Another example: Beer et al. (2010) show that carbonate ion concentration exerts different effects on the test production of different species, and not 'generally reduced' calcification as stated by the authors in lines 377-378. 4) P. 10087, lines 15-18: The fact that symbiont bearing species are dominant in oligotrophic gyres does not say that those species are most frequent in oligotrophic gyres. On the contrary, also symbiont bearing species are more frequent at higher than at lower levels of prey. 5) P. 10087, line 26: 'microfossil sediments' do possibly not exist. Please change to 'microfossil rich sediments'. 6) P. 10088, line 22: '. . . historical emissions. . .' of what? Please specify. 7) P. 10092, line 16: Riebesell et al. (2000) discuss coccolithophorids, and I can not see any connection to the present manuscript. 8) P. 10092, line 26-28: Please have a look at Schiebel and Movellan (2012), and the information on size-related biomass of planktic foraminifers. 9) P. 10093, 13-14: Since the data of Bé and Tolderlund (1971) include a rather large size (>200 μm) only, abundances are largely underestimated and not overestimated. 10) P. 10093, lines 15-18: What happens at tropical, temperate, and subpolar waters, which you have (possibly) not included in your model? 11) P. 10095,

line 5-12: I agree that CPR data are interesting. CPR data are resolved for different water depth, nor integrating all of the surface water layers. In addition, CPR data only include the >200 μm size fraction, and hence miss most of the planktic foraminifer fauna. 12) P. 10095, line 20 and following: Please specify what you mean by climate change, i.e. the absolute change in T, CO2, etc. 13) P. 10096, lines 9-18: Looking at three species only does not really give a good idea of what will really happen, and you might want to choose your wording more carefully. 14) P. 10097, line 13: 'too warm' 15) Chapter 3.4: You may want to consider the paper of Feely et al. (2004) 16) P. 10100, line 10: I like the idea of 'wild foraminifera'. However, 'in nature' might be the better wording. 17) De Villiers 2004, and Barker and Elderfield (2002) discuss data from sediment samples, and which include other effects (e.g., early diagenesis), on foraminifer tests than in the water column. Same in p. 10103, line 12-15: In sediments, a range of entirely different processes are to be considered, which can possibly not be discussed in the manuscript. 18) P. 10100, line 26: Phosphate concentration does possibly not affect heterotrophic planktic foraminifers. 19) P. 10101, line 23: The morpho-species G. siphonifera includes at least two species (Huber et al. 1997, Darling et al. 1997, De Vargas et al. 2002) 20) From p. 10101, line 27 to p. 10102, line 12: Can you present any proof for your statements? 21) P. 10103, lines 20-21: How might symbiont bearing species react, which can possibly not decide to move to the deeper water column?

Interactive comment on Biogeosciences Discuss., 11, 10083, 2014.