**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "Global prediction of planktic foraminiferal fluxes from hydrographic and productivity data" *by* S. Žarić et al.

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The paper by Zaric and co-authors presents a new way of modelling the planktic foraminiferal fluxes from hydrography data and productivity data. The paper is in general very clearly organized and easy to read and follow. This paper deserves to be published. I have only minor concerns, listed below:

- page 853, line 15: what is the reason to choose a constant density difference of 0.125 sigma units? Is this really constant all over the globe? And how does this work hypothesis on MLD calculation impact on the foraminiferal fluxes reconstructed with the model? In particular, will another definition for the calculation of the MLD have an impact on the results shown in table 3?

- figure 1: it seems that the model calibration and the global model experiments are not forced by the same input values for SST. SST for the model calibration is coming

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from IGOSS and SST for the global model experiment is coming from WOA 2001, why these two data bases are not the same?

- page 854, line 9: the ACE algorithm appears a little bit like a black box in this section. Is this possible to add a technical annex to give some clues about this method, and show look-up tables to have an example?

- page 855, lines 14-15: how the cutoffs mentionned in these lines are defined, on which bases?

- page 858, lines 2-6 and figure 2: the modelled annual foraminiferal flux is not discussed in comparison with observed fluxes, why?

- page 858, comments on figures 3 and 4: the authors are not commenting at all about the Indian Ocean (and Pacific), where the model results are relatively far from the data. This is true for almost all the species. Please add a few word on the differences seen in these oceans.

- page 861, lines 10-14: this is linked to my previous comment about the relative differences between data and model in the Indian and Pacific Ocean: the dissolution hypothesis is perhaps true for Pacific but will not be for Indian Ocean. Is this possible to test the dissolution hypothesis in the Indian Ocean by looking at the species fluxes and see if the dissolution-sensitive species are particularly abundant (using G. ruber for instance)?

- table 3: are beta1, beta 2 and beta3 supposed to be the a1, a2 and a3 coefficients of equation 1? please define the shaded areas (values above 0.390?)

- figure 2: the density of the calibration data set is really low in the southern hemisphere: is there a potential bias of the method linked to the geographical distribution of the calibration data set?

- figure 7: please use the same scaling

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- figures 11-12 and 13: in figures 11-12, the catchment interval is estimated 2 weeks and in figure 13: 3 weeks. Why? On figure 11, why the three runs of the model (one each year) do not give exactly the same results? I was expecting to see the same figure repeated each 365 days, and this is not the case as the shape but also the location of the calculated peaks are not exactly the same (particularly for G. menardii).

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