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

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General comments:

The manuscript of Zaric et al. does approach the subject of open marine calcite fluxes, studied on behalf of planktic foraminiferal test, from a different angle as previous papers, and seems to be an important step on the way to understand transformation of sinking matter between the upper ocean and the sea floor. I agree with the authors that a future version of their model should be coupled to an ecosystem model to account for the importance of population dynamics in foraminiferal test flux modes. To my concern, the manuscript is well suited to be published in BG, it does reveal a novel concept, and the methods are clearly described. As also stated in the manuscript, the assumptions should be seen more as step towards a larger goal, and can not be regarded as stand alone. In general, it is fun reading the properly structured and well written manuscript.

A former empirical model of Schiebel (2002, GBC, 16(4), 1065) did suffer from the non-linear coupling of primary productivity and planktic foraminiferal export flux (Schiebel's Figure 13), and the present paper tries to explain planktic foraminiferal flux by correlating with Sea Surface Temperature and Mixed Layer Depth. An improvement of Zaric and coauthors' new model in comparison to that of Schiebel (2002) is a higher horizontal resolution of 1*1 degrees. A problem of the Zaric et al. manuscript is the deep calibration depth of 1000 m, where the planktic foraminiferal assemblage has already undergone massive alteration in the twilight zone (100-1000 m depth). This might be the reasons for high similarities of the modeled abundance (at 1000 m) and the core top abundance: Planktic foraminiferal test flux $>$150 μm does not change a lot below 1000 m depth (Schiebel, 2002).

Due to the fact that the presented model is aimed as the same target the (really first) empirical model on planktic foraminiferal test flux of Schiebel (2002), I would be delighted to see some differential discussion of both approaches in the present manuscript. Although I appreciate the honest naming of shortcomings of their own method, I would suggest not only to name these points but also to discuss them at least briefly.

Specific comments:

Title: It is not clear to me how crucial productivity data are for the model. Productivity is obviously included in PEX, but no actual productivity data are used?! I would therefore suggest changing the title to 'Global prediction of planktic foraminiferal fluxes from SST and MLD'.

Page 850, line 27-28: I is shown by Schiebel (2002; Fig. 13) that primary production is not correlated to planktic foraminiferal test / calcite flux. Planktic foraminifera are mostly heterotrophic (Hemleben et al., 1989) and are therefore not coupled directly to primary production.

Page 852, line 6-7: Zaric et al. do not present global flux for 18 species at species

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level, but relative species distribution for six species. Fluxes are presented only at local scale. Tables and maps on the modeled distribution of all 18 species might be helpful to understand how the model of Zaric et al. works. The additional material could be presented as an appendix or made accessible as web page.

Page 855, line 12-14: More recently, it was shown that G. ruber pink got extinct in the Indo-Pacific during MIS 5.5. at 128 kyrs BP (see e.g. Ivanova et al., 2003, PPP 197, 61 pp).

Page 857, line 17-19: The authors explain fluxes of some species by changes in PEX (export production in 1000 m water depth). The export production in 1000 m water depth, in turn, is literally the flux. Hence, flux is explained by flux. This circular argument is a recurring feature of the manuscript, and should be carefully considered by the authors.

Page 859, line 2-5: It should be discussed later on why the distribution of G. bulloides is not properly represented by the model results.

Page 861, line 20: Please give a reference for the monospecific state of N. pachyderma sin. in polar waters.

Page 863, line 1: Schiebel (2002) presented area-wide investigations. Detailed data are available from http://www.pangaea.de/home/rschiebel/

Page 864, paragraph 4.2: Schiebel and Hemleben (2000, Deep-Sea Research II 47, 1809-1852) presented a study on interannual test flux change.

Page 865, last paragraph: The export production at 1000 m depth is no environmental parameter at all, and, if used would be no input parameter for planktic foraminiferal test flux but its result, already. Circular argument! Antoine et al. (1996, GBC 10(1), 57-69) [and the according web page] offer productivity data on high resolution, which could be used instead of PEX. However, primary productivity is no measure of 'true food availability for planktic foraminifera' (see Schiebel, 2002, Figure 13).

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Page 866, line 5: N. pachyderma does not per se live in the deep chlorophyll maximum (e.g., distinguish between sin and dex, and even then this statement is not valid) [the same is true for N. dutertrei, which might point towards the fact that this morphospecies might consist of different genotypes].

Page 866, line 18: Foraminifereal life cycles and in particular reproduction does possibly not affect monthly fluxes because almost all species reproduce once per month, every fortnight, or once per year (Schiebel and Hemleben, 2005, Paläontologische Zeitschrift 2005, 79/1, 135-1485).

Table 3: What is the table supposed to show? Shallow dwelling planktic foraminifera are of course distributed according to temperature and its related parameters. (1) G. bulloides is abundant in upwelling areas of different characteristics (including different SST) and temperate regions, and does possibly represent a group of different genotypes. Therefore, it is not surprising that G. bulloides does not fit to the general SST scheme. (2) G. glutinata is rater living close to the nitracline and does not strongly react to SST (despite that, I'm curious why not also other species, for example G. menardii, do show highest values in the PEX column). (3) G. truncatulinoides is deep dwelling and could not be attributed to both SST and MLD. - In addition, and not surprising, correlation coefficients are very low throughout. To my concern, Table 3 does not give any useful information.

Figure 11: Why do the modeled fluxes change between the three years (days 1-1100), if the model is not supposed to account for interannual changes? Could the authors also explain why the real and the modeled flux are negatively correlated, for example, in G. menardii.

Technical comment:

Page 863, line 26: change 'high fluxes' to 'high flux rates'

Interactive comment on Biogeosciences Discussions, 2, 849, 2005.