

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

S. Žarić et al.

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Response to interactive comment by J. Bijma (Editor)

- "Would it be possible to incorporate some of the issues raised in the discussion (e.g. using PP instead of PEX) already in the final version?"

Using primary production instead of export production is not possible, because there are no primary production data available for the time of sediment-trap deployment and in the temporal resolution of the trap samples. The available data by Antoine et al. (1996) cannot be used, because they are long-term monthly means calculated from satellite data, so information on interannual variability would be lost (see also answer to R. Schiebel's comment on page 865, last paragraph, below).

Response to interactive comment by R. Schiebel (Referee)

- "... the present paper tries to explain planktic foraminiferal flux by correlating with Sea

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Surface Temperature and Mixed Layer Depth."

In our model, planktic foraminiferal fluxes are correlated not only to sea-surface temperature and mixed-layer depth but also to (organic carbon) export production. These are the three environmental parameters used for the calibration and the model forcing as stated on page 852, lines 6-9.

- "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 reason for high similarities of the modeled abundance (at 1000 m) and the core top abundance: Planktic foraminiferal test flux $>150 \mu\text{m}$ does not change a lot below 1000 m depth (Schiebel, 2002)."

There seems to be a misunderstanding. Only organic carbon fluxes were normalized to 1000 m water depth (PEX) as stated on page 853, lines 2-5. Foraminiferal shell flux data were not changed at all, but were used as measured, independent of the trap depth.

- "Due to the fact that the presented model is aimed at the same target as the (re-ally 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."

The work of Schiebel (2002) was primarily based on multinet samples, and the goal was to assess the global amount of total foraminiferal calcite (mg CaCO_3 per m^2 and day) settling through the water column. Hence, it was rather "a first-order estimate of the global planktic foraminiferal calcite budget" (Schiebel, 2002) as compared to an empirical model of global foraminiferal species fluxes (shells per m^2 and day) presented here. In contrast to Schiebel's (2002) work, our study has a clearly different focus being based on hydrographical data and sediment traps that directly measure the downward foraminiferal flux instead of calculating it from standing stocks. Moreover, our study aims at a completely different target: predicting seasonal variations

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in species shell fluxes from variations in hydrography and productivity in order to improve calibrations of foraminifera-based proxies in paleoceanography. Being forced by environmental parameters (sea-surface temperature, mixed-layer depth and export production), our empirical model is the first to calculate monthly foraminiferal downward fluxes (shell fluxes) on species level and on a global scale thus allowing for a comparison of modeled species distributions to those observed on the seafloor. We think that a comparative discussion of these different approaches would rather tend to confuse readers than to elucidate an important aspect. Hence, we did not follow the reviewer's recommendation.

- "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."

Shortcomings of our method are discussed primarily in chapter 4.2.

- "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'."

Productivity is an important variable in the model. Export production calculated from the trap samples is used to calibrate the foraminiferal flux model, and a global set of satellite-derived primary production data were used to calculate export production for the forcing of the global model experiment. Within the model, export production is one of three environmental variables, on the basis of which foraminiferal species fluxes are calculated. Hence, we consider the original title as appropriate.

- "Page 850, lines 27-28: It 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."

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The entire food web is based on primary productivity, at least the relevant trophic levels. Even if the total planktic foraminiferal calcite flux is not correlated to primary production (Schiebel, 2002, Fig. 13), for specific foraminiferal species there still might be good correlations between shell flux and PP. For species exhibiting a low correlation, PP would only be an unimportant predictor due to the model setup.

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

Agreed. We attached 12 more figures to the appendix to show modeled relative species distributions in relation to coretop distributions of the hitherto not presented species.

- "Page 855, lines 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)."

Agreed. In the manuscript, the date has been changed accordingly and the respective citation has been added.

- "Page 857, lines 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."

Species fluxes and export production are not the same! One should clearly differentiate between export production (PEX) being the export flux of organic carbon (normalized to 1000 m water depth, see page 853, lines 1-7) and the flux of certain foraminiferal species giving the number of (more or less empty) shells per square meter and day that sink to the seafloor. For example, the export production includes organic tissue

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from primary producers as well. Both fluxes are of course not completely independent from one another (just like many other environmental variables), but using PEX as an environmental parameter in our model is certainly not a circular argument, either. If that was really the case, then we would expect the statistical results to show higher standardized regression coefficients in the PEX column (Table 3).

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

A possible explanation for the partial mismatch of relative abundances of *G. bulloides* between model and coretops is given in the discussion chapter 4.1.2 (page 862, lines 3-9).

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

We have rephrased this sentence in the new version of the manuscript. The Brown Foraminiferal Database indeed does not contain any monospecific seafloor samples. However, there are sediment trap investigations from the Weddell Sea and the Greenland Sea (Donner and Wefer, 1994; Jensen, 1998) that showed 100% *N. pachyderma (sin.)* during parts of or even the complete sampling period in the analyzed size fraction.

- "Page 863, line 1: Schiebel (2002) presented area-wide investigations."

As explained above, the work of Schiebel (2002) is primarily based on multinet samples, and CaCO₃ fluxes are calculated from standing stocks of planktic foraminifera. As our model does not calculate calcite fluxes, for a model validation we would need species fluxes (shells per m² and day) measured from sediment traps. To our knowledge, such investigations are not available on a global scale as stated on page 863, lines 2-3.

- "Page 864, paragraph 4.2: Schiebel and Hemleben (2000) presented a study on

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interannual test flux change."

Since our investigation concentrates on vertical fluxes of planktic foraminifera, we based our model on sediment-trap data instead of plankton-tow data (as used by Schiebel and Hemleben, 2000). Three examples of studies on interannual flux changes measured by sediment traps are given at the end of the paragraph (page 864, lines 24-25). However, the existence of such studies is not the focal point of this paragraph. The problem is rather that the actual hydrography is not monitored at the same time. Only SSTs are available for the specific times of trap deployment, so that information on interannual variabilities of the environmental input parameters is lost in the calibration and the model experiment.

- "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) 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)."

Regarding the first point, please refer to the answer to the comment on page 857, lines 17-19 (above). The export production at 1000 m water depth is one of three environmental input parameters in our model. Pitfalls as well as the reason for still using this parameter (the only productivity-related parameter in the temporal resolution of our samples) are discussed on page 865. The productivity data offered by Antoine et al. (1996) do not meet our requirements regarding the temporal resolution. They are long-term monthly mean values calculated from satellite data as opposed to organic carbon fluxes measured with the same trap cups as the foraminiferal fluxes (same sampling duration, interannual variabilities). Monitoring primary productivity during trap deployment or the ability to reliably calculate it from satellite data would certainly better serve our needs. Yet, currently these methods cannot be applied to previous sediment-trap experiments, so for the time being we have to use what is available. Regarding

the "true food availability", the respective sentences have been rephrased. However, for certain species there might in fact be good correlations between species fluxes and primary production (see answer to comment on page 850, lines 27-28, above).

- "Page 866, line 5: *N. pachyderma* does not per se live in the deep chlorophyll maximum ..."

Agreed. However, *N. pachyderma* and *N. dutertrei* are often associated with the deep chlorophyll maximum, at least for part of their life cycles. The sentence has been rephrased accordingly.

- "Page 866, line 18: Foraminiferal 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)."

This is in the context of the model calibration as already mentioned in the original manuscript (page 866, lines 15-24). The samples in the calibration data set are often covering time spans shorter than a month. Since many foraminiferal species reproduce on a monthly basis, these life cycles introduce a certain amount of scatter/noise in the data set that cannot be explained by changes in hydrography or productivity (see also Zaric et al., 2005).

- "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 rather 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

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not give any useful information."

Table 3 is not supposed to show surprising results. It shows statistical results that mathematically confirm some of our knowledge on foraminiferal species that is based on observations. Nevertheless, this table is important, because the statistics are the basis of our foraminiferal flux model. Moreover, it shows the quality of our multiple linear regression approach by giving the adjusted r^2 -values for each species as well (explained variance).

- "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*."

This is explained in the methods section 2.5 and has been added as a comment to the figure caption for clarification. Since actual monthly sea-surface temperatures are available from the time of trap deployment (IGOSS database), these data were used to force the model for the comparison to sediment trap data in order to account at least in part for interannual differences in environmental parameters. Hence, modeled fluxes are not exactly the same for the three consecutive years. The global model experiment, on the other hand, was forced with long-term mean temperatures from the World Ocean Atlas 2001, because those results were compared to coretop samples representing much longer time periods (see also answers to E. Cortijo's comments on Figs. 1 and 11, below). Regarding the negative correlation of real and modeled fluxes of *G. menardii*, unfortunately, we do not have any explanation for that. However, fluxes show a clear negative correlation in the second half of the sampling period only. We could not detect any global systematic error regarding the predicted seasonality of any species as stated on page 864, lines 4-7.

Response to interactive comment by G. Ganssen (Referee)

- "1. The minimum size of the counted foraminifera should be consistent. What sizes

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were used in which data sets? The relative abundance and flux of *N. quinqueloba*, for instance, will strongly increase when a minimum size of >125 micrometer is used instead of >150 . (page 863, line 9)"

Tables 1 and 2 indicate the respective sieve size used for each sediment trap study. The minimum size of the counted foraminifera is in fact a critical point (e.g. Carstens et al., 1997). However, within the scope of this investigation we had to use every available data set, and most of the data were available in the given size fraction only. This point has been clarified in the original manuscript, chapter 4.2.

- "2. A differentiation of foraminiferal species between phyto- and zooplankton is essential when primary productivity serves as a basic parameter."

The relevant trophic levels are all based on primary productivity. See also answer to R. Schiebel's comment on page 850, lines 27-28, above.

- "3. In the authors' approach the species concept will always limit the output results. Although no differentiation will be possible for genotypes (page 866, lines 21-22), a differentiation of eco-phenotypes may improve the predictivity value of the model. ..."

Agreed. But a differentiation of eco-phenotypes is not feasible, since this would imply re-counting all trap samples, which would furthermore reduce the data set for each "species" significantly.

- "4. The concept of the paper is based on a "very modern analogue" situation. Sediment trap, hydrography and productivity data document the situation of the most recent years, while the data obtained from sediment surfaces reflect information of up to several hundreds of years. While the modern data might reflect already changes due caused by the Global Change, this potential information is at least partly lost by mixing of the most recent particles with pre-industrial material through bioturbation. Clues for possible most recent faunal changes are given by the work of Barker and Elderfield (Science, 297, 833-836, 2002). The enhanced carbon dioxide concentration of the

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atmosphere causes alkalinity changes in the ocean and results in stronger selective dissolution of certain foraminiferal species. (page 866, lines 7-10)"

We are primarily interested in the first-order global distribution patterns and consider Global Change effects as a secondary effect in the framework of our study. However, the difference between recent and pre-industrial foraminiferal fluxes shouldn't be too big, if the main effect is a reduction of shell weight as investigated by Barker and Elderfield (2002).

- "5. In Figure 12 the authors compare modeled and measured foraminiferal fluxes off Somalia. A comparison of all available data (hydrography, productivity, relative abundance (and partly fluxes) from plankton tows, sediment traps and sediment surface) could be a helpful check for the model predictivity. For the four species shown, modeled and measured fluxes match well. However, three of the most dominant species (*G. bulloides*, *G. ruber*, *N. dutertrei*) are not shown. What is the reason for not showing them, do the model results not fit the observations? If so, what might be the reason? A possible reason for the mismatch for *G. bulloides* in upwelling regions is given on page 862, lines 1-9. Off Somalia, the available data of export productivity for a depth of 1000m could directly confirm this explanation."

To keep the paper concise we decided to only show examples of species fluxes at key sites. However, we showed fits as well as misfits. Unfortunately, we do not have data on export productivity off Somalia at our disposal, so we cannot compare it to our model results.

- "6. In Figure 14 (see also page 867, lines 3-9) the seasonal shift of the modeled fluxes for *G. bulloides* are shown for the N-Atlantic. The timing of the shift (January-April) is leading that of the spring bloom and that for the migration of *G. bulloides* as proposed by Ganssen and Kroon by several months. What is the reason for this discrepancy?"

As already mentioned in the original manuscript (page 862, lines 6-9), higher export productivities are badly covered in the calibration data set, which might be a possible

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explanation for the mismatch of *G. bulloides* fluxes in the N-Atlantic.

Response to interactive comment by E. Cortijo (Referee)

- "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?"

A constant density difference criterion of 0.125 sigma units is widely applied in oceanography to define the mixed-layer depth. However, we have further specified our calculations of the MLD (calculation in polar regions and minimum MLD) in the new version of the manuscript (see chapter 2.1). The discussion about the definition for the calculation of the MLD is beyond the scope of this paper. As shown in Table 3, the MLD is of higher relevance only for *G. truncatulinoides*. Moreover, for that species our multiple linear regression approach explains only 33.7% of the variance in the data (adj. $r^2=0.337$).

- "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 from IGOSS and SST for the global model experiment is coming from WOA 2001, why aren't these two data bases the same?"

It is right, that the model calibration and the global model experiment are not forced by the same SST data. For a direct relation to trap samples (calibration) it is more appropriate to use actual SST data from the time of trap deployment (IGOSS data set), because thereby it is possible to account for interannual variations in SST. When model results are compared to trap data not included in the calibration, the model was forced with actual monthly IGOSS data for the same reason (as explained in chapter 2.5). The SST data for the global model experiment are from WOA 2001. As these are long-term monthly mean values, the model results obtained by this forcing can better

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be compared to coretop foraminiferal data, which represent much longer time periods than the trap samples used in the calibration. This issue has been clarified in the figure caption.

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

Regarding the ACE algorithm, we would like to refer back to the Journal of the American Statistical Association, Vol. 80, pages 580-619, which include the original publication by Breiman and Friedman (explaining the algorithm in great detail) as well as 21 pages of discussions.

- "Page 855, lines 14-15: How are the cutoffs mentioned in these lines defined, on which bases?"

The cutoffs were defined "according to the distributional SST ranges of the individual species" (page 855, line 14; see also Zaric et al., 2005). In other words, species fluxes were plotted against sea-surface temperature. The SST range in which the respective species has been found (fluxes higher than zero) is its distributional SST range. For clarification, the citation has been added to the text and a table showing the SST ranges for each species has been added to the appendix (Table A1).

- "Page 858, lines 2-6 and figure 2: The modeled annual foraminiferal flux is not discussed in comparison with observed fluxes, why?"

As explained in the original manuscript (page 863, chapter 4.1.3), the lack of absolute shell fluxes of foraminiferal species on a global scale makes it difficult to assess the corresponding model data (see also answer to R. Schiebel's comment on page 863, line 1, above).

- "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.

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This is true for almost all the species. Please add a few words on the differences seen in these oceans."

One possible explanation might be enhanced selective dissolution in surface sediments (as explained on page 861, lines 10-14), especially in the equatorial Pacific Ocean, where the rather dissolution-resistant species *P. obliquiloculata* and *N. dutertrei* can account for more than 80% of the coretop foraminiferal assemblage in the western and eastern equatorial Pacific, respectively (considering only 18 species included in the model). Another point is, that modeled species fluxes and hence distributions are primarily governed by gradients in environmental parameters (especially temperature). As these are relatively small across the tropics/subtropics (between 30°N and 30°S) the signal-to-noise ratio might be comparably poor in these regions (e. g. across vast parts of the Indian Ocean). Furthermore, it has to be reminded that major parts of the Indian and Pacific Oceans have not been sampled with sediment traps, so large areas of these oceans might not be covered well enough in the calibration data set.

- "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 Oceans: the dissolution hypothesis is perhaps true for Pacific but will not be for Indian Ocean. Is it 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)?"

As was shown by Conan et al. (2002, Mar. Geol. 182, 325-349), carbonate dissolution can be significant in the Indian Ocean as well, and the planktic foraminiferal assemblage preserved in the surface sediment can be strongly modified as compared to the settling assemblage. This citation has been added to the manuscript. For other possible explanations on the relative differences between data and model in the Indian Ocean, please refer to the comment above.

- "Table 3: Are beta1, beta 2 and beta3 supposed to be the a1, a2 and a3 coefficients

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of equation 1? Please define the shaded areas (values above 0.390?)"

The beta-values are not the same as the regression coefficients a_1 to a_3 of equation 1. The beta-values are standardized regression coefficients enabling a comparison independently of the respective units. The shaded area marks the highest beta-value for each species. Unfortunately, that footnote got lost in the published version of Table 3.

- "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?"

There might indeed be a bias on account of the geographical distribution of the calibration data set, e. g. if considering the presence of various genotypes with potentially different ecological preferences (see also comment on G. Ganssen's point no. 3, above). However, the amount of available trap data is already very limited, so we rely on every bit of information we can get. We are unable to estimate the potential bias due to the geographical distribution of trap samples.

- "Figure 7: Please use the same scaling."

Agreed. The scaling has been changed. Now Figs. 7a and 7b have the same scaling.

- "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 do the three runs of the model (one each year) 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*)."

The applied time-lag between sea surface temperatures and foraminiferal fluxes as mentioned on page 853 (lines 18-19) is described in Zaric et al. (2005) and shall briefly be explained here: To account for foraminiferal life cycles, a two-week adjustment was used for all sediment traps. Additionally, a further one-week correction (three

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weeks total) was applied for traps deeper than 1750 m water depth due to foraminiferal settling time through the water column, which translates into assumed average sinking velocities of 500 m per day. This issue has been clarified in chapter 2.1. Since the sediment traps in the Cariaco Basin (Fig. 11) and in the Somalia Upwelling (Fig. 12) were both located in water depths shallower than 1750 m (Table 2), the time-lag between trap data and model data was set to two weeks. The three traps shown in Fig. 13 were all moored at deeper water depths (Table 1), so the applied time-lag includes the additional one-week correction for the settling time and amounts to three weeks in total. In Fig. 11 the model shows differing fluxes for the three consecutive years, because it was not forced with long-term mean temperatures from the World Ocean Atlas 2001 (same data for every year), but with actual monthly sea-surface temperature data (IGOSS database) covering the time of sediment-trap deployment. This was done in order to account at least in part for interannual differences in environmental parameters as explained in the methods section 2.5, page 857, lines 9-13 (see also answer to R. Schiebel's comment on Fig. 11, above). A corresponding comment has been added to the figure caption.

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

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