

## ***Interactive comment on “First-order estimate of the planktic foraminifer biomass in the modern global oceans” by R. Schiebel and A. Movellan***

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Review of the manuscript entitled " First-order estimate of the planktic foraminifer biomass in the modern global oceans" By Schiebel and Movellan.

### General comments

This manuscript present different data sets used to provide a estimate of planktic foraminifer biomass at a global scale. This peculiar type of data are of large interest since planktic foraminifer are believed to be one important planktic functional type to be included in global ecosystem models (See LeQuéré et al 2005; Glob. Change Biol., 11, 2016–2040). However, in situ (within the water column) biomass reports are totally missing and only density exists, both in the water column and on cores sam-

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ples. Thus the data set presented here is unique and of a great interest. Moreover, the whole development of the results itself is unlike to be subject to strong bias (such as size estimate biases, or species estimate biases that can happen when the work have been done under different methodologies) since all the data used here are originating from the authors lab, and using a similar methodology. The data have been carefully analyzed in order to identify potential bias of the approach (such as differences in extraction protocol depending on species).

However, few points need to be better constrained or better presented.

First of all, the overall methodology concentrated on protein biomass, and most of the figures and table were afterward presented in carbon biomass. Such a direct transformation should be directly indicated in the material and methods. It is moreover not clear what carbon mass is presented here: On page 253 (line 22-25) you are talking about calcite-carbon mass (about 36%of protein biomass), this one is not a biomass estimate (it is not "living" mass); on page 255 you are more precise and you present that you used an equivalent conversion factor (1 unit of protein= 1 unit of carbon) originating from Zubkov et al 1999. I think this latter was used, but it should appear sooner in the ms. Moreover, this estimate originate from estimates on bacteria, and could be different for foraminifer, knowing that some of them can even include starch (page 246, line 27). This should be recognized, but the fact that estimates originating from this study and the ones originating from Michaels et al 1995 (who measured cytoplasmic carbon) are similar seems to indicate that this potential bias is maybe not important. All this argumentation needs to be presented in the material and methods, or before presenting any data in carbon biomass. There is also a need to present why the chosen method could be better (or worse) than Michaels et al ones, which actually records carbon biomass without any conversion factors (I guess because the present methods allows to works on single individuals and allow to recover the shell afterward, but this needs to be highlighted).

Secondly, on the two dataset presented here (one used to construct the size/protein

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weight relationship, the second is a calculation of stock biomass using previous datasets), only one is effectively available. The fact why the first dataset is not made available should be mentioned (I suspect because it will be more extensively presented in a second paper "Movellan et al 2012, biogeosciences discussion in prep" which is not yet submitted.). It should be stated also whether or not those data will be available in future. I am also not sure of the editorial guideline regarding this reference (page 250): this work is not yet submitted (and thus should be presented as "in prep" and not "2012") and thus I don't know if citing a reference not published is accepted by ESSD. However, the two times this manuscript is cited correspond to methodological points and I'm not sure about the necessity to cite this work then. On the other side, if the complete dataset will be presented in it, maybe it could be cited at this point.

Finally, I have the impression that nearly everywhere in the manuscript there is a confusion about biomass (stock) and production (rate): in the abstract, table4 and discussion (page 257-258) are reported data in TgC yr<sup>-1</sup> which then are rates estimates (production) but are in all cases except one (page 258 line 6) reported to correspond to biomass estimates. This should be corrected in abstract, tables and manuscript.

Additionally, I understand why you chose to only apply your calculation on an homogeneous data set with homogeneous sampling and counting protocol (originating from the author), but could your methodology applied on other published datasets (even crudely) to provide a larger, comprehensible atlas of foraminifer biomass? If possible it would be interesting to provide this, maybe providing caution to readers on the reliability of this second "atlas" because of a lack of consistency. Among others, it seems to me that multinet data from Field, 2004; Kuroyanagi and Kawahata, 2004 and Watkins et al., 1996; 1998 are using almost the same protocol and mesh size, and data are available in database. Maybe it could improve your work.

Detailed comments

Page 249, line 22: some samples were frozen immediately and then analyzed latter

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(Poseidon cruise) , because samples seems not to have been shock frozen (ie. in liquid nitrogen) this could have lead to artificial breakage of lipidic membranes and thus may helped for protein extraction. This is those samples that have been used to assess the efficiency of the procedure on different types of foraminifers. Since the size weight relationship seems not to be different between cruise (fig 4) thus this do not seems to be a problem, but still could be discussed somewhere in the manuscript.

Page 250: the two Movellan references should be "in prep" and not "2012" (until you submit it)

Page 250, line 12: those samples correspond to the Meteor and Tansei-Maru cruises ?

Page 251, Line 26: how many data were rejected?

Page 252, line 23: can you provide similar estimate of the efficiency of extraction for other species

Page 253 PFAB definition should be indicated at it first occurrence and not two lines later

Page 253: you should indicate that starting from fig 6, it is a different dataset that you analyze. Indeed, because fig 6,7 and 8 are abundance+size observations extrapolated to weight using the previous findings. Thus all your figures then shows that there is no size (and thus weight) difference within latitude and seasons but that deeper foraminifers are smaller (and thus lighter) if I am well understanding the data.

Page 253, line 21 11.27 mg of ? protein? Carbon? Calcite?

Page 253, Line 24: this is calcite-carbon of biomass-carbon that you are talking about here?

Page 258: the need for a good inter-comparison of carbon/protein measurements may also be mentioned here

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Table 4: global annual production (not biomass). It is confusing to have the entire table in protein mass and only this estimate in carbon. Could this latter have been provided both in protein and carbon?

Fig 1 & 2 could be combined in one figure with two panels. The same could be done for Fig 6,7 and 8.

Fig 3 : “measured individual planktic foraminifer protein biomass of 21 specimens between 70 and 800  $\mu\text{m}$  test size” it is actually 754 specimens from 21 taxa

Fig 3: The “submission of data to Pangaea” is not at the right place (or this is a confusion and you have not sent the right data). From the data seen on pangaea it should be between the second and third box starting from the bottom.

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