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# *Interactive comment on* "Stable carbon isotope deviations in benthic foraminifera as proxy for organic carbon fluxes in the Mediterranean Sea" *by* Marc Theodor et al.

**Marc Theodor et al.**

marc.theodor@uni-hamburg.de

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Response to interactive comment by anonymous referee #1

M. Theodor, G. Schmiedl, F. Jorissen, and A. Mackensen marc.theodor@uni-hamburg.de

We acknowledge the comments by the reviewer. The specific comments helped to improve and to complete our manuscript. Below we respond to the comments raised by the reviewer.

General comment: The paper in review aims to develop a transfer function for determining organic carbon flux to the Mediterranean Sea based on the d13C composition



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of a pair of epibenthic and endobenthic foraminifera species. For that, the authors studied a large number of sites in the western and eastern Mediterranean (Aegean Sea) from intermediate water depths covering a wide trophic range (from eutrophic to oligotropchic). The study was based on the analysis of living as well as dead specimens (separately). For calibration and understanding the isotopic and environmental setting, the authors used different sizes of the analyzed foraminifera, median living depth of the endobenthic species, re- dox boundary depth of the analyzed sediment, TOC of top sediment layer and primary production flux estimates in order to establish the proxy. The authors discuss their results in a very methodological and systematic way. Dis- cussing first what contributes to the wide d13C range of the epibenthic species in the different locations (mainly Aegean vs western Mediterranean and within each part of the Sea) and for the species used (mainly two), being aware of the different water masses, the habitat that they occupy and their isotopic signal. Next they discuss the endobenthic species Uvigerina mediterranea and what controls its d13C values in the different parts of the sea. And finally they discuss the basis for establishing a transfer function for organic carbon flux based on d13C difference between the isotopic composition of the above mentioned epi- & endobenthic foraminifera species. The knowledge about the factors that control the isotopic composition of d13C of the analyzed species exist for more than two decades. In this study the authors went a step further and tried to develop a transfer function for organic carbon, based on the “rules of the game” something that was not done so far and something that the paleoceanographic community is looking for eagerly. However, this seems to be a complicate task and it works only for certain places in the Mediterranean while in others the picture is still unclear. Still the enormous work that was invested in this study is worthwhile because it shows the potential that exist in this direction. It also shows that some parts of the puzzle are still missing but the authors are on the right way. Right now the final result, the transfer function that was developed is applicable only for certain conditions in the Mediterranean Sea. This was clearly stated by the authors and should be clear also to potential users in the future. The paper should be considered as an important step in the attempt to progress in producing a transfer function however more work and understanding is still needed. Finally the paper is warmly recommended to be published in Biogeosciences Discussion as it is. I had very minor suggestions, see below.

Response: Thank you for the generally very positive vote, especially for appreciation of our attempt for establishing a transfer function of organic carbon fluxes in the Mediterranean Sea.

Specific comments: Comment: Please indicate how many specimens were used for the stable isotope analysis Response: In total 417 tests were measured. The differentiation between epi- and infauna as well as stained and unstained tests was added in the "material and methods" chapter. In addition, the range of measured specimens was added for each species.

Comment: Line188 – should be site 602 Response: corrected

Comment: Line 216 fig. 4 – the redox boundary depth appears in 4b and not in 4a while the MLD (line 217) appears in 4a – just replace Response: corrected

Comment: Line 218 – in these figs there is no difference between stained and unstained thus it is not clear to what do the authors refer in their statement in line 218/9 Response: we agree, the statement was removed

Comment: line 221 – this statement is true only for a few cases – in many cases this relation do not exist (see sites 592, 595 596 an 599) Response: we agree, the text was specified accordingly and the existing mismatches have been explained.

Comment: line 252 – were the suspicious relocated specimens removed from the database? Response: Yes, relocated specimens were removed; they were also marked in Fig. 2. For the estimation of the $\delta$13CEpi see response to comment on line 272

Comment: line 265 – the 2nd on is extra: on surface on Response: corrected

Comment: line 272 – I can understand the logic of choosing the highest d13Cepi value in table 1 but what about values that were used and their origin is not mentioned at all at that table - for example for sites 601, 394, 395, Canyon and Slope? – please add explanation what is the basis for choosing these values Response: This is an important issue. We have chosen the $\delta$13C values of P. ariminensis as the best bottom water reference. If no specimens of P. ariminensis were available, we had to substitute the bottom water signal by the other measured epifaunal species or interpolate the value from nearby sites. We have specified the description for the estimation of $\delta$13CEpi in the revised manuscript and discussed the possible uncertainties for the sites where no data of P. ariminensis were available.

Comment: line 889 – difficult to see in fig. 2 different symbol sizes for different test sizes. Response: We removed the sentence because it referred to a previous version of figure 2.

Comment: line 890 - In the same fig. it is difficult to understand how the authors determined which value to use for d13CDIC – they should be more specific in their explanation. Response: The explanation was extended. In order to account for the different ways of $\delta$13CDIC estimation, we also referred to the discussion chapter 4.1 (see also comment and response to line 272)

Comment: An example of how the picture is still partial is looking at the database of the dead foraminifera. The transfer function was developed on the database of the living (stained) foraminifera. At the same time also the dead (unstained) foraminifera were studied. Unfortunately, the dead assemblage failed in showing the same trend as the living ones (as shown clearly in fig. 5) – something that need to be addressed by the authors. Response: Although the values of dead specimens do not seem to fit the transfer function, this mismatch can be explained by the presence of relocated tests. Especially sites 537 and 396 revealed much lighter $\delta$13CUmed values for unstained tests compared to heavier values for stained tests. Including these values strongly alters the correlation and also illustrates a potential bias in the application of the transfer function on fossil data. We have extended the discussion of this problem in chapter 4.3.

Comment: Another thing that should be taken into account is that the authors based the use of several proxies such as primary productivity flux, TOC etc on external sources, something that should be taken into consideration. Moreover – the authors should comment on that describing how much this should affect their final results. Response: Although the TOC values were partly used from published data (Möbius et al., 2010), the measurements were carried out on samples from the identical locations, thus minimizing incompatibilities. External data were used for primary production based on satellite data, which allowed generation of a homogenous data set for all sites. In chapter 4.3, we have addressed the reasons, sources and possible errors of external data in order to accommodate this issue appropriately.

Comment: And another problem is using the complicate region of the Aegean – for understanding general processes in the Mediterranean. It might be that this region should be kept for more advanced studies and not for those that want to establish the rules of the game. Response: The Aegean Sea was initially chosen as an ideal test bed because it is characterized by strong trophic N-S-gradients, i.e. with oligotrophic conditions in the South and meso- to eutrophic conditions in the North. In the course of our study it turned out that the regional benthic foraminiferal $\delta$13C values include the signal of significant lateral organic matter fluxes resulting in a decoupling of the $\delta$13C signal from the vertical organic matter fluxes. Nevertheless, we have decided to include these data in our study because they clearly highlight the potential uncertainties and pitfalls of our transfer function, which could also occur in other environmental settings such as continental margins or offshore river mouths etc. We consider the proper illustration of this bias crucial for further development and application of our approach.

Cited references: Möbius, J., Lahajnar, N., and Emeis, K.-C. 2010. Diagenetic control of nitrogen isotope ratios in Holocene sapropels and recent sediments from the Eastern Mediterranean Sea, Biogeosciences, 7, 3901-3914.





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