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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.

Anonymous Referee #1

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The paper in review aims to develop a transfer function for determining organic carbon flux to the Mediterranean Sea based on the d13C composition 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, redox 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. Discussing 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:

1. Does the paper address relevant scientific questions within the scope of BG? Yes, - as explained above

2. Does the paper present novel concepts, ideas, tools, or data? Yes- see description

3. Are substantial conclusions reached? yes

4. Are the scientific methods and assumptions valid and clearly outlined? yes

5. Are the results sufficient to support the interpretations and conclusions? yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes

8. Does the title clearly reflect the contents of the paper? yes

9. Does the abstract provide a concise and complete summary? yes

10. Is the overall presentation well structured and clear? - yes

11. Is the language fluent and precise? - yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? - yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? - no

14. Are the number and quality of references appropriate? yes

More specific comments:

- Please indicate how many specimens were used for the the stable isotope analysis

- line188 – should be site 602

-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

-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

-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)

-line 252 – were the suspicious relocated specimens removed from the database?

-line 265 – the 2nd on is extra: on surface on

- 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

-line 373

- line 889 – difficult to see in fig. 2 different symbol sizes for different test sizes.

- 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.

-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.

-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. Morevoer – the authors should comment on that describing how much this should affect their final results.

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.

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