

## ***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 #3**

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Benthic foraminifers feed on organic matter raining down to the sediment-water interface and have differentiated to occupy different niches in the uppermost sediment layers: Some live on the very surface (epifaunal), others delve in deeper sediment layers (infaunal) and occupy specific habitats with successively lower contents of edible organic carbon and dissolved oxygen. All excrete calcite tests, and the  $\text{CaCO}_3$  they produce is in isotopic equilibrium (with some fractionation caused by ontogenetic and vital effects) with the dissolved inorganic carbon (DIC) in their ambience. An epigenetic lifestyle means that the  $\delta^{13}\text{C}$  of the DIC and the tests is set by the bottom water carbon pool; tests of infaunal species have a lower  $\delta^{13}\text{C}$ , because increasing amounts of respired particulate organic carbon add DIC with low  $\delta^{13}\text{C}$  at increas-

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ing depth in the sediment to the porewater DIC pool in equilibrium with the foraminifer calcite. That means that  $\delta^{13}\text{C}$  of infaunal tests should always be lower than that of epifaunal tests, and the difference ( $\Delta\delta^{13}\text{C}$ ) should be related to the fraction of DIC in the infaunal habitat contributed by respiration. That is to a first degree dictated by the organic matter reaching the sea floor (essentially a function of surface primary production – PP – and water depth) and being buried (essentially a function of sedimentation rate), its quality (fresh or recycled), and the prevailing metabolic mode of mineralization (aerobic or anaerobic).

In this manuscript, Theodor et al. explore the differences in the  $\delta^{13}\text{C}$  of epi- and infaunal benthic foraminifer calcite as a proxy for surface water productivity and organic carbon fluxes in the Mediterranean Sea. They analysed a large set of samples from 19 sediment cores situated in a defined water-depth interval spanning (relatively subtle) gradients of productivity and differences in depositional settings, including some where lateral transport of organic matter is likely. The spread of analyses includes differentiation of the  $\delta^{13}\text{C}$  of living and dead individuals, analysis of size-differentiated (ontogenetic) effects on the  $\delta^{13}\text{C}$  in individual species, preferred habitat depths of infaunal species, the depth of the redox boundary in the sediment (color change), and the differences in  $\delta^{13}\text{C}$  of calcite produced by the infaunal species *Uvigerina mediterranea* and by three epifaunal species. Together with satellite-derived annual PP estimates and fluxes of OM at the depths of the sampling sites calculated from empirical formulas, the extensive data set is the basis to explore the hypothesis that the  $\Delta\delta^{13}\text{C}$  of epi- and infaunal calcite of living benthic foraminifers is a proxy for organic matter flux to the seafloor. The authors argue that this is indeed the case in a number of environmental settings of the present-day Mediterranean Sea, except in the Aegean sub-basins, where small-scale variability obscures the relationship. In the course of the manuscript it also becomes obvious that “non-living” tests complicate the issue considerably. This strikes me as being in itself an argument against using this novel proxy in older sediment sequences of environments where sediment reworking is a problem.

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The results of the study are somewhat sobering, because a clear-cut relationship between the isotope gradient and productivity/OM burial was not evident to me. This may reflect the low range of productivity characteristic for the Mediterranean Sea, and an intense microbial loop that affects the fluxes out of the mixed layer. Its ranges of productivity and concomitant OM rain rates to the sea floor are at the lower end of the global ocean (Fig. 5 lower panels show that), and admixture of recalcitrant TOC near rivers and canyons is a known problem. Also, the small-scale hydrodynamic setting and multiple OM sources in the data-rich Aegean sub-basin may obscure a possibly robust and promising relationship. This is indicated in Figure 2, where  $\delta^{13}\text{C}$  of DIC in all Aegean sites is consistently higher than epifaunal  $\delta^{13}\text{C}$ .

Furthermore, the authors had to piece together PP and OM flux estimates from a variety of methods that each have their own error margins, as acknowledged by the authors after comparing theoretical and observed (sediment trap) rain rates.

In my assessment the manuscript should be published, because it is to my knowledge the first and systematic attempt to examine the epifaunal/infaunal  $\delta^{13}\text{C}$  gradient and to develop it as a proxy for organic carbon fluxes in an oligotrophic sea. And it describes results of a massive analytical effort and is in most parts very well balanced in terms of results versus expectations. But the manuscripts should be revised, mainly in terms of writing style. I will send my notes on the printout directly to the lead author. Below are some additional suggestions and comments.

Title: "Deviations" from what? I suggest that you use "gradients"

The way chosen here to calculate OM rain rates for specific sites is somewhat convoluted (2 satellite derived PP estimates and the Betzer, 1984 estimate for OM flux at sample water depth, acknowledged to possibly be unsuitable in the Med). I would have used depth-specific rain-rate output of an NPZD model instead, which should be internally consistent and besides would resolve seasonal variations that may have some influence. If I am not mistaken, the authors may have access to such a model data set.

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(In the future, the authors might consider modeling expected  $\delta^{13}\text{C}$  gradients at given flux, sedimentation, and respiration rates to test their observed gradients against theory. This would also mark sites with significant lateral input of recalcitrant OM).

Did the authors test whether there is a relationship between %TOC in the sediment and calculated fluxes of OM? Figure 4 C looks as if there might be a relationship between the  $\delta^{13}\text{C}$  gradient and %TOC.

354 ff: I was puzzled by the 15 cm up to 30 cm of oxygen penetration in 5 cores from the Aegean Sea. To me that suggests that sedimentation rates at these sites must be very low, which I would not have expected. What would very low sedimentation rates do to explain the anomalous  $\delta^{13}\text{C}$  gradients found at these sites? What is the expected relationship of the redox boundary depth in comparison to the Median Living Depth of *U. med.*, which is relatively shallow at these sites?

366ff: When deep water is replaced the  $\delta^{13}\text{C}$  of DIC should become lower due to the Suess Effect imported from surface water? If it is lowered, how would that steepen the gradient?

408 ff: Elsewhere you state that lateral OM input (because it is recalcitrant) has little effect on the  $\delta^{13}\text{C}$  gradient.

DIC  $\delta^{13}\text{C}$  of bottom waters shown in Figure 2 appear to have been estimated from the values of  $\delta^{13}\text{C}$  analysed here on epifaunal species. Why is there a shift in the Aegean samples, and how do the estimates compare to the values of Pierre (1999)? Have there been more recent analyses of  $\delta^{13}\text{C}$  of DIC to pinpoint the Suess effect on deep-water DIC?

372: I wouldn't call it a close relationship

Figure 3: Re-arrange "stained tests" etc as figure title – they are not axis labels

Figure 5 and 6: symbols don't match legend for Gulf of Lyons samples?

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