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Interactive comment on "Resource utilization and trophic position of nematodes and harpacticoid copepods in and adjacent to *Zostera noltii* beds" *by* A.-M. Vafeiadou et al.

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General comments:

This study attempts to analyze the contribution of various food sources in the nutrition of estuarine sediment-dwelling meiofauna (nematodes and harpacticoid copepods) using stable isotope (13C, 15N) signals from potential food items and selected consumer organisms. The major merit of the manuscript is the fact that most of the consumers have been identified to genus- and all to family-level. This makes a comparison between assumptions on the diet and the results from the present isotope study possible and leads to a critical appraisal of so far published assignments to trophic levels and

resource use. Several of the results contradict (or at least seem to contradict) these assignments and the authors plead for caution in using e.g. the buccal morphology classification used in nematode ecology since Wieser's extensively cited 1953 paper. The scarcity of data on meiofauna nutrition makes this study valuable and publication should be envisioned. Unfortunately the authors try to do too much at the same time with the effect that many of the conclusions remain only weakly supported. Instead of focussing on a single habitat and season (where enough material could have been collected for all the planned analyses) they attempt to include the role of different food sources comparing vegetated and non vegetated habitats and two seasons. Sentences such as ". . .suggesting they either utilize a mix of more 13C -depleted (e.g. SPOM) and more 13C -enriched (e.g. seagrass detritus) food sources or, more likely feed predominantly on MPB and/or epiphytes" leave me puzzled. As a consequence, a number of simplifications had to be made especially in using the mixing models and interpreting the results. I share all the reservations expressed in the detailed review of Michel Loic and do not want to add to these.

Specific comments:

I would have appreciated a more clear description of the sampling site (position with regard to tide level, tidal amplitude during the sampling dates). Sample collection is said to be "random" – how has this been achieved? Or was it just "haphazard"? Pooling the meiofauna samples before sorting and identification lead to a loss of information on within-station variability, which could have been considerable. One could have pooled the meiofauna later to obtain enough biomass for the analyses. The fact that the nematode Terschellingia and the Cletodidae have stable carbon isotope signatures that suggest feeding preferentially on chemosynthetic bacteria is very interesting. In my pet group of nematodes, the Stilbonematinae, this seems to be the case (see Ott et al. 1991. PSZN Marine Ecology 12) and is plausible considering their symbiosis with chemosynthetic bacteria. In the case of Terschellingia, where no such symbiosis has so far been described, it would imply a strong selectivity in its bacteriovorous habit.

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How can one explain a δ13C of -41.7 and a contribution of 0.81 in the worm assuming a δ13C of -35 for chemosynthetic bacteria - all other possible food items being less depleted? Furthermore, SOM, SPOM and SLD represent a mixture of the original photosynthetic organic matter (in part mainly the nutrient poor and indigestible structural matter) and of microorganisms that have utilized the more valuable compounds and converted them into their biomass. Therefore the isotopic signature of bulk SOM, SPOM and SLD will be determined by discrete fractions, which may differ significantly in δ13C and δ15N. Due to their small size meiofauna could selectively graze the microbial component that is more depleted in the heavier isotopes. Even when ingested, the fraction representing the original photosynthetic tissue might not be easily digestible. This would make the results of the mixing model ambiguous and could be the cause of some of the surprising discrepancies between buccal morphology and isotopic composition. This should at least be discussed.

Technical corrections:

Line 74: Kharlmamenko vs. Kharlamenko in references Line 237: this is the first time that the acronym SOM is used. Therefore it should be explained what it stands for.

In conclusion, I think the Discussion section should be streamlined, emphasizing those results that are well substantiated by the data before the manuscript is published.

Best regards

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