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Discussions

Interactive comment on "Metazooplankton diversity, community structure and spatial distribution across the Mediterranean Sea in summer: evidence of ecoregions" *by* A. Nowaczyk et al.

Anonymous Referee #1

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Comments for the evaluation of the submitted manuscript "Metazooplankton diversity, community structure and spatial distribution across the Mediterranean Sea in summer: evidence of ecoregions" by A. Nowaczyk, F. Carlotti, D. Thibault-Botha, and M. Pagano.

Based on a 3000 km transect across the Mediterranean Sea during June- July 2008 authors described changes in the composition and abundance of zooplankton. The paper contains interesting and new data that mereits to be published. The main interest is that both the eastern and western Basin were sampled (almost) simultaneously, so we can have an instantaneous view of these gradients W-E related with the gradient of

oligotrophy of the Mediterranean to the East. I don't think however that these results are really a revision, and I would recommend to re-writte this paper and resubmit – to this or to another journal – as a work of original research, instead of a review of ecoregions.

More specific comments. Title. This work are only done on epipelagic zooplankton, so deep-sea or high sea, in concrete zooplankton species distributed below 200 m, were not sampled and ignored. This important matization should be in the title.

Methods. I don't think that this transect can be used to discuss about ecoregions. The sampling was not random at different regions of the Mediterraenan but each stations are dependent of the next because there was a pre-established track from E to W. I also think that further (comparable) data for this approach is required. Also the transect was performed in summer and can vary seasonally, at least between homogenized or sratified periods.

Results Clustering is not a convincing method for this objective. In spite the strong gradient introduced (from E to W) groups were not clear (there were a low segregation of groups), high similarity among groups (up to 70%) and any test comparing the significance among groups was used. PRIMER with PERMANOVA is a useful tool to establish this in paralell to the use of MDS analysis (See Cartes, Fanelli, Papiol, Zucca, 2009 Deep Sea Res as example of how this method can be used for zooplankton composition data)

Discussion There was not any metanalises here, only a discussion in where most of the conclusions were in agreement with those found by previous studies, so the current study seems not open any novel hypothesis on the current knowledge of epipelagic zooplankton. In general all results confirmed tendencies already found in previous studies. Particularly, results in the point 4.6 (ecoregions) were not conclusive. It seems that mesoscale structures are more important defining assemblages than possible influence of surface productivity depending of regions. Authors should give a clearer

conclusion here in this way (if I interpreted this correctly) or in another. In any case, discussion on regionalization of zooplankton is not conclusive and the main aim of this revision is rather weak. I insist in the interest of the current data as new, for an original research paper that sure could be published in Journal Plank. Res., as a review is far of a consistent approach.

Interactive comment on Biogeosciences Discuss., 8, 3081, 2011.