

Interactive comment on “The importance of different spatial scales in determining structure and function of deep-sea infauna communities” by J. Ingels and A. Vanreusel

Anonymous Referee #2

Received and published: 4 March 2013

General comments The paper by J. Ingels and A. Vanreusel is dedicated to analyze the effect of different spatial scales in driving the structural and functional biodiversity in the deep-sea benthic communities, giving a strong contribution in testing ecological hypothesis in the deep sea, one of the most remote environments on Earth. The authors presented a considerable and composite dataset, since samples were collected from two regions of European continental margins in the Atlantic ocean, from different habitats (canyon and open slope) in each region and from different stations at different water depth (from 700 to 4300 m water depth) in each habitat. So that, this paper is very suitable for the publication in Biogeosciences. The manuscript is quite well structured, written and clear. The experimental approach is rigorous and well described.

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However, to my opinion, there are some considerations that authors should take into account, to make the paper more easily readable. Here below the detailed comments.

Title I am wondering if is appropriate to refer to “function of communities”. Indeed, to my opinion, the paper deals with the “functional diversity of the communities” and not to the function that such communities plays as a whole in the ecosystem. So that, maybe it should be better to refer to “. . .in determining the structural and functional diversity of deep-sea infauna communities” in the title.

Abstract Pag. 196, Line 5 and Line 10. Again, maybe it could better to refer to “functional diversity of communities”, instead of “function of benthic communities”. The “ecosystem function of communities” is quite unclear. Again at Line 14, to my opinion the “function” is not a descriptor of the community as a whole (at least in this paper). On the contrary, different nematode species have different functions in the meiobenthic community. Pag. 196, Line 7. Which “other ecosystem” the authors are referring to? Terrestrial? Marine, coastal? Pag. 196, Line 10-12. “Here, we used an extensive integrated dataset of free-living nematodes from deep-sea sediments to unravel which spatial scale is most important in determining benthic infauna communities.” To my opinion, there is not a spatial scale “most important” than another. First of all, it could be strictly dependent upon the analyzed world ecoregion, or habitat, water depth, etc. Then, more appropriately, maybe the authors should refer to different spatial scales in influencing in different way benthic infauna communities, through different drivers which act at different spatial scale.

Introduction Pag. 197, Line 2. Which kind of variability? In abundance, standing stock, diversity? Pag. 197, Line 5. Maybe it could be better to change Levin et al. 2001b as Levin et al. 2001a, since is the first article by Levin et al. cited in the text. Pag. 197, Line 9. “e.g.” could be eliminated. Pag. 197, Lines 10-12. This paragraph could be better explained. “Geographical barriers” and “sediment grain size” are not “phenomena”, but environmental or topographic settings/characteristics. Moreover, they could be considered as “environmental drivers” which are cited in the following sentence.

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Maybe, it could be better to refer to biotic or abiotic factors acting at different spatial scale, from local to regional spatial scale. At Line 12 and 21 the references are missing. The sentence at Lines 13-18 is too long. Pag. 198, Line 2. Since this paper deals with standing stock and diversity, maybe it could be better to explicit which are the effects (on abundances, standing stocks, diversity?) of different drivers on communities. Pag. 198, Line 21. Ingels et al. 2011b has not been cited, so far. Pag. 199, Line 1. To better connect this paragraph with the previous ones, it could be better to refer the submarine canyons as deep-sea habitats. As suggestion: "Among the deep-sea habitats, submarine canyons can perhaps be considered as the most heterogenic topographic systems". Moreover, it sounds too exaggerate the statement "Submarine canyons can perhaps be considered as the most heterogenic habitats in the marine realm", since there are other heterogenic habitats in the marine realm (tropical coral reefs, as just an example). Maybe it could be referred to deep-sea environments. Pag. 199, Line 9. References are missing. Pag. 199, Lines 11-13. To my opinion, the aim of this study was not to address the question "what is the most determinant scale for processes that regulate. . .function of marine meiofauna", but functional diversity of meiofauna. Pag. 199, Line 17. Maybe it could be useful for the authors to refer to different spatial scales as regional, macro-, meso- and micro-scale. Pag. 199, Line 17. "Irish Margin and Western Iberian Margin" is not a spatial scale. Maybe authors should use a more proper term. Again, water depth (ca. 700, 1000, 3400 and 4300 m) is not a spatial scale. Maybe authors should refer only to a m or km distance among stations at different water depth.

Materials and Methods Pag. 201, Line 25. "2.2 Sampling design and sample processing": the title of this paragraph refers to the description of the sampling design. However, the rigorous description of the sampling design is in the following paragraph (Pag. 204, Line 7). To my opinion, the titles of the paragraphs could be changed in something like "2.2 Sampling strategy (or collection) and processing" and "2.3 Sampling design and data treatment". Pag. 203, Line 5. The authors did not explain what was the purpose of nematodes length and width measurements. I guess that it

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was for the estimation of the nematode biovolume then converted into biomass values. Pag. 205, Line 1. Since the authors used the PRIMER v6 and PERMANOVA, I am wondering if it is better to use the output of CAP analysis (comprised in the PRIMER package) instead of the MDS (which has no statistical meaning) to visualize the data in a bidimensional plot.

Results To my opinion, it could be better for the readers to have a table with the raw data used for the analysis (also in the Supplementary material). Overall, the Results section appears sometimes difficult to read. To make the section easier for the readers, the suggestion is to present the data following the description of sampling design, according to the different spatial scales (from the widest to the smallest, for instance), instead of following the investigated variables. This scheme was applied in the Discussion section, which results more clear than the Results one.

Discussion I suggest to avoid acronyms, to make this section easier to be read. The authors should be more precautionary, since they are just hypothesizing the relation between their results and the environmental/trophic variables, without demonstrating them. Pag. 209, Line 13. I cannot understand how the sampling design may affect the analysis of latitudinal pattern. Maybe authors meant the sampling strategy or collection, which are very difficult in particular in deep-sea environments. Pag. 209, Line 13. Which patterns? In abundance, standing stocks, biodiversity?

References Please, double check the References. Gallucci et al. 2009 is cited in the text but is missing in the References, while Kiriakoulakis et al. 2011 is cited in the References but is missing in the text.

Tables Table 1. Please, explicit the Unit for Sediment surface, and the meaning of MUC (multiple corer?) and PC (ISIS) (push core from ROV ISIS?).

Interactive comment on Biogeosciences Discuss., 10, 195, 2013.