**Biogeosciences
Discussions**

# Interactive comment on "Unravelling the environmental drivers of deep-sea nematode biodiversity and its relation with carbon remineralisation along a longitudinal primary productivity gradient" *by* E. Pape et al.

**Anonymous Referee #2**

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## General comments

This is a very interesting study dealing with the relationship between marine biodiversity and ecosystem function, a subject which, particularly for the deep sea, is still considered as *"terra incognita"*. For this, the authors used a nice, although not perfect, data set which expands from the eastern Atlantic to the eastern Mediterranean and which covers a depth range from approx. 1000 to 3000 m. They used substantial data analysis methods, the MS is well written and the results and conclusions are concrete

and convincing. The paper fits nicely within the scope of Biogeosciences and in my opinion it will be well received not only by marine ecologists but also by ecologists working in other fields. There are only a few points which, if taken into consideration, would further improve the MS.

## Specific comments

My main concern is the use of nematode biomass as a measure of ecosystem function. Biomass is not a direct measure of a function but rather a measure of the state of an ecosystem. It can be used as a proxy for the transfer of energy between trophic levels or as a proxy indicating the production of a heterotrophic ecosystem, such as the deep-sea, but, by no means, can it be regarded as a real measure of deep-sea functioning. In the present study particularly, the authors used a benthic element (nematodes) to estimate biodiversity and then they used the same element (nematodes again) to calculate biomass which they converted through a model to respiration. In my opinion this may result in a looping effect leading to wrong conclusions. Nevertheless, because measuring processes and functions in the deep-sea is quite difficult, the present approach may be acceptable but I would like to see this problem being discussed in the paper.

The first paragraph of the Introduction (i.e. from the start to line 13 of page 19021) can be omitted completely since it is a bit irrelevant to the title and the subject of the paper. You are not dealing here with how many species there are in the deep-sea or with how diverse nematodes are but with the relationship between biodiversity and ecosystem function. Thus, given also the fact that the Introduction is already large enough, I suggest to leave this part out.

The results are written in a rather dense way which makes it very difficult to read them or to extract the necessary and desired information at a glance. Instead of providing all

these model details and P values in the text flow I suggests to include them all in one or more tables. That way, one could immediately see which index shows significant or non-significant trends with what factors and the text in the results could be more descriptive and human readable.

## Technical comments

1. (Page 19021, Line 12): I believe it is the "Intermediate disturbance hypothesis" and not theory as written.

2. (Page 19024, Lines 9-10): Why do you expected a negative and positive influence of POC flux and grain size? While this is probably true for grain size it is not clear why POC flux should have a negative effect when there are various examples showing a positive relationship between productivity and nematode diversity (e.g. Lambshead et al. (2000) in the North Atlantic, Lampadariou & Tselepides (2006) in the Mediterranean etc.).

3. (Page 19025, Line 23): Use the word approximately instead of the $\pm$ symbol as this later means that possibly zero nematodes have been measured.

4. (Page 19028, Lines 4-9): All this paragraph on the nematode respiration calculations needs further explanation as it appears that the formula given in line 6 is not exactly following the calculations of Bovee & Labat (1993). At least I could not find it in that paper.

5. (Page 19030, Lines 7-10): Why do you give and discuss the results of the LME when you explain that when the random effect was not significant you removed it from the model? This just causes confusion. Please see also my specific commend No. 3 and try to write the results section clearer by concentrating only on the significant and important results.

6. (Page 19030, Line 22): P values for J' are not given as with the other indices.

7. (Page 19030, Lines 20-25): Why are you not discussing here the depth relationship for J', TD and MI? Do they not exists? (Again see specific commend No. 3)

8. (Page 19038, Line 22): It would be nice to suggest which other functional traits, which have not been considered here, might be of importance for future studies.