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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.

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We would like to thank the two anonymous referees for the helpful suggestions, which will definitely lead to an improvement of the manuscript. The questions and suggestions of the referees are repeated below.

My main concern is the use of nematode biomass as admeasure 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.

Author's response: Following the definition of ecosystem function given in Paterson et al. (2012), nematode biomass can be considered a measure of ecosystem function since it represents the stock of nematode "material". However, we fully agree that using respiration estimated from biomass as a function is not ideal, since it may not reflect in situ nematode respiration (to be measured as the actual oxygen consumption or carbon dioxide production by the nematodes). We may add to our discussion → "A fourth point is that total nematode respiration was here estimated on the basis of total nematode biomass and temperature (de Bovée and Labat 1993, Soetaert et al. 1997) and may not truthfully reflect in situ respiration by the nematode community. Environmental factors other than temperature (Braeckman et al. in press) and biotic interactions (De Mesel et al. 2006) may influence nematode carbon processing rates. Sounder results would have been obtained from the direct measurement of oxygen consumption or carbon dioxide production by nematode communities in deep-sea sediments."

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.

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Interactive Comment

Author's response: This suggestion will be followed.

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 suggest 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.

Author's response: The results of the regression analyses will be displayed in (four) Tables. This will make reading (a lot) more pleasant.

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

Author's response: this particular paragraph will be deleted from the introduction, as suggested earlier.

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.).

Author's response: We "expected" a negative influence of the temporal variability in POC flux (so not the magnitude of POC flux, which is what was studied in the references that are provided here) on diversity, since this was observed before for deep-sea foraminiferal species (Corliss et al. 2009, Gooday et al. 2012). We suggest to remove these lines here (..that we expected a negative relation..), but add earlier in the introduction the following sentences (after line 1 on page 19022): "The magnitude of productivity is assumed to have a positive effect on diversity (through the stimulation of

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population growth), whilst temporal variability in productivity may depress diversity (by limiting feeding to certain periods of the year) (Chown and Gaston 1999, in Rex and Etter 2010). Seasonality in surface productivity had an adverse effect on foraminiferal species diversity in abyssal sediments (Corliss et al. 2009, Gooday et al. 2012)."

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

Author's response: the "±" will be substituted with "around".

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.

Author's response: Indeed, the formula as it is written here is not identical to the formula in de Bovée and Labat (1993). The formula we used was the same as the one in the paper of Soetaert et al. (1997) which used the data that were given by de Bovée and Labat (1993). This should be acknowledged here: "...were calculated on the basis of individual biomass using the formula of Soetaert et al. (1997), which was based on values provided by de Bovée and Labat (1993)..."

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.

Author's response: That must indeed be confusing. We will now only discuss the significant models and provide all results in tables.

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

Author's response: All regression results (including the P values) will be displayed in

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tables.

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

Author's response: indeed, no significant regressions were found for these indices. This will all be much clearer when all results, whether significant or not significant, are presented in tables. We will also clearly mention in the text those indices for which no trends were observed.

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.

Author's response: We will speculate in the discussion of the improved manuscript on which functional traits may be of importance for nematode respiration rates: "A more relevant measure of functional diversity to nematode carbon mineralisation may be the diversity in digestive systems, mirrored in taxon diversity." Also, although it is briefly mentioned in the discussion (page 19036, lines 13-17; page 19037, lines 12-13), we would like to stress that trophic diversity as it is calculated here may not truthfully reflect the entire spectrum of nematode feeding strategies. → "The diversity in diet composition amongst nematode genera may not be captured by the trophic diversity index."

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