**Biogeosciences
Discussions**

Interactive
Comment

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 #1

Received and published: 15 January 2013

General comments: Very nice dataset and good analyses. This paper will be well-received by ecologists. I have, however, a few reservations about how some of the analyses were conducted. In particular, I would like to see POC flux included in the analyses of the diversity-function relationship instead of biomass. POC flux is such an important factor linked to function in the deep sea and it really should be controlled for. This should be relatively easy to do, but it may change the conclusions of the paper.

Specific comments:

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Interactive Discussion

Discussion Paper

Introduction

Line 61: more recent paper produced an estimate of 1 million or less marine species. See Appeltans et al. 2012 Current Biology 22: 2189

Line 76: end of sentence should read: ".. competition, and predation."

Line 78: delete "regional"

Line 86: the difference between studies is probably due to the different range of productivities sampled. In this respect, it would be useful if the authors commented on where their study sites may lie along the productivity continuum (i.e., are they oligo-, meso-, or eutrophic). This would help predict what kind of relationship might be expected, in particular if we assume that the overall relationship for the full productivity range is unimodal (see Rex and Etter 2010 book).

Line 97: The authors may want to look a little further in the literature, as it appears that some models are more likely than others, the redundancy model in particular. The results of Danovaro et al. for the deep sea are quite exceptional in that the relationship is exponential. The authors may want to discuss which model they think deep-sea nematodes will fit.. for example they are typically very diverse, and in some cases, with high within-genus diversity (eg Acantholaimus) and therefore with high level of functional redundancy?

Line 134: It would be worthwhile to include something in the Introduction about how nematode diversity may affect ecosystem function. It is still a black box, but some likely processes should be mentioned.

Line 152: So function is based on a proxy of proxy. A little indirect. Perhaps the authors could simply consider respiration rates only. Respiration is of course strongly correlated with biomass, which itself largely reflects POC input. Instead of accounting for the effect of biomass in analyses of the diversity-function relationship, I would instead run the regression for POC flux (to control for the likely impact of this factor on respiration)

followed by diversity. I think the authors will agree that POC input cannot be ignored as a driver of ecosystem function in the deep sea, and that its effect needs to be controlled for. And I would not use biomass in this analysis.

Line 164: Please provide details of core surface areas. Surface area may affect nematode diversity estimates, i.e., larger core will include a larger number of species aggregations and therefore tend to yield larger diversity estimates than smaller cores. Eg Warwick and Clarke 1996 JEMBE

Line 180: replace "data on" with "estimates of"

Line 189: did the diversity estimates for the top 10cm take into account the difference in nematode abundance between layers or are they simply averages of all the layers (the latter would be incorrect)?

Line 190: the rarefied genus richness is based on rather low abundance (18). One core had 18 nematodes only? What was the size of that core?

Line 231: or body size diversity? Seems more directly relevant

Line 258: here I would use POC flux instead of biomass. See previous comment.

Line 273: Please explain. I do not understand why the quadratic term was added.

Line 309: is there a relationship between longitude and POC (linear or otherwise)? The authors need to check for collinearity between predictor variables before running the regression. Some variables may be strongly correlated.

Line 319: Could the authors confirm that all the relationships they are mentioning here are for partial regressions, and not marginal regressions? Need to make this clearer throughout text. Please provide a table of results for these results from your regressions. Same for previous paragraph.

Line 346: which assumption?

Line 347: I am really not sure that the factor region is random. I doubt these areas were in fact chosen at random from a wider selection of potential sampling sites. I believe this should be changed in the analyses (i.e., fixed). Again a table of results should accompany this paragraph (regression results).

Discussion:

Line 358: there are still some fairly major issues with using EG(18), especially since it is based on a small number of individuals in this ms. This index can be strongly dependent on dominance and aggregation (the latter will, in turn, be affected by the size of your cores!), especially at small sample sizes (see Gray 2000 JEMBE p.11-12). So it is difficult to say which index is more representative, or less biased, and one cannot be chosen as more meaningful in this context. Perhaps the authors could include N in their regression analyses, so that the effect of abundance on diversity indices can be controlled for? In any case, it would be good to see if there is a relationship between N and diversity indices. Quantifying diversity is always a problem so it make sense to at least acknowledge potential biases.

Line 376: include reference(s) to back up statement.

Line 382: in my mind these analyses of depth-diversity relationships are getting old. We all know that it is difficult to compare between studies for a wide variety of reasons. What is interesting is to understand the underlying mechanisms. So yes, whatever results one gets will always contrasts with at least some studies. Rex and Etter summarised what we know quite well and I don't think there is a need to make elaborate comparisons.

Line 412: see comment about collinearity.

Line 433: Nice discussion.

Line 442: such as?

Line 452: replace "strongly" with "may". But one would expect such a positive relation-

ship anyway, no? especially when considering only genera, which are more likely to differ than species. I think the authors are taking it a little bit too far when jumping from diversity of life history strategies to resilience and function.

Line 472: so how much overall variability was there in genus richness per core? Could the authors try to explain perhaps how many additional genera might lead to, say, a 50% increase in respiration (or whatever) according to their analyses (even though of course causation cannot be implied)? This would help to put things in context a bit.

Line 490: but the environmental variables were not controlled for in these analyses, were they? I thought only biomass was controlled for. See previous comments.

Line 501: or there might just not be a relationship between functional diversity and function. This should be acknowledged.

Line 507: also species vs genus, sediment depth. . .

Figure 5: what about a graph showing relationship between POC flux and respiration?

Interactive comment on Biogeosciences Discuss., 9, 19019, 2012.