

Interactive comment on “Influence of chemosynthetic ecosystems on nematode community structure and biomass in the deep eastern Mediterranean Sea” by N. Lampadariou et al.

N. Lampadariou et al.

nlamp@hcmr.gr

Received and published: 21 March 2013

Reply to Anonymous Referee #1

Reply

We would like to thank Anonymous Referee #1 for the constructive comments. We have addressed all the remarks and have amended the manuscript accordingly to include all the suggestions made. Our response to individual comments can be seen

C9132

below.

General comments

Reply

The reviewer made two general comments which we addressed as follows:

1. Although the research questions are clearly mentioned at the end of the introduction, some of them seem redundant in several ways, and could be combined, making them stand out more.

...

Perhaps the authors should reformulate the questions or argument their logic why these questions stand apart from each other. I do think, however, that the questions posed fit within the scope of BG.

Reply

We accepted the suggestion of the reviewer to combine questions 2 and 4 and we also eliminated question 3 as it seemed redundant. The questions are now formulated as follows:

- (a) Are meiofaunal standing stocks as well as local (alpha) diversity enhanced in mud volcanoes compared to nearby deep-sea sediments?
 - (b) Do different mud volcanoes harbour distinct and specialized meiofauna/nematode communities and are they different from adjacent deep-sea sediments?
2. One concept the authors touch upon is the indication of nematodes occurring in deeper sediment layers, and this is ascribed to biological rather than physico-chemical factors. Despite the fact that similar observations have been reported

C9133

in previous literature, I find the explanation rather 'fragile' and in need of more evidence or more convincing argumentation. Perhaps a very useful addition could be to work out this phenomenon in more detail and present a conceptual scheme by which the deeper occurrence of nematodes can be explained, backed up with data and hypothesis, instead of presenting the observation rather anecdotally. As it stands, it comes across as rather conjectural.

Reply

We agree with the reviewer that the evidence for suggesting a deeper penetration due to biological rather than physicochemical reasons is rather weak. Reviewer 2 also commented on the vertical distribution of nematodes (see "Reply to Anonymous Reviewer 2"). Since both reviewers thought that our data were not strong enough to support such a statement we plan to correct this in the new version of the manuscript by taking the following actions:

- (a) Following the suggestion of Reviewer 1, we will further analyze our data in order to assess which biological factors could be related to this phenomenon. More specifically, we will explore: (i) if the distribution of other benthic communities (e.g. macrobenthic, microbial) that have been sampled during the same cruise at the same stations can explain such a phenomenon and (ii) if the observed pattern can be explained by looking further into the different functional groups or the functional diversity of nematodes
- (b) We will use physicochemical data obtained from this cruise (published by Ritt et al., 2012), to further explore and discuss the relationship between the vertical distribution of nematodes and the physicochemical environment
- (c) Irrespective of the above two actions, we will also temper our statements regarding the deeper penetration of nematodes. More specifically, and since this was also a suggestion made by Reviewer 2 (see "Reply to Anonymous Reviewer 2"), we will remove those sentences making strong statements on this matter from both the Abstract and the Discussion.

C9134

Related to this and after having considered all the comments with regard to the vertical distribution, we have decided that it would be more appropriate and straightforward to split the vertical distribution graph (Fig. 4) into two separate graphs, one for each mud volcano, as we did with all other figures.

Specific comments

The reviewer made also a substantial number of more specific and technical comments which we addressed them all as follows:

1. 18132: 16-17. Not sure whether this 'conclusion' should be mentioned in the abstract considering this fact is not very well evidenced in the discussion. The authors should consider leaving it out of the abstract, or ameliorate the argumentation in the discussion (cf general comments, and comments for the discussion)
...

Reply

This comment is also related to comment No. 2 of the General Comments section (see above). Although we have tried, as the reviewer suggested, to look further into this subsurface peak, we feel that we do not have enough data to fully explain this observation and thus support a very strong statement. Therefore, we prefer to take this sentence out from the abstract as suggested by the reviewer.

2. Introduction: Good overview of what mud volcanoes are about, providing sufficient insight for the reader, but research questions need attention. I would also suggest including some information on the identity of the microhabitats that are investigated within the mud volcano ecosystems. This would inform the reader what they actually are and give insights into the complexity of the system. The authors could do this based on the sediment chemistry and visual appearance.

C9135

Reply

The reviewer here makes in fact two suggestions, (a) to pay attention to the research questions and (b) to include some information on the microhabitats. Both have been addressed as follows:

- (a) The research questions have been reformulated (see also General comment No. 1 above). In the new version, instead of four we have two research questions, which we hope reflect better the work that has been carried out
- (b) We feel that providing information on the identity of the microhabitats in the Introduction will be redundant since these are given in details in the M&M section. Nevertheless, in order to satisfy the reviewer's request, we will include in the new version of the manuscript a few sentences briefly describing the number and types of the different microhabitats.

3. 18138:17-21. It would be good to provide a reference here to support the statement

Reply

Accepted: If we understood well this comment, the Reviewer probably means lines 17-21 from page 18133 and 18138 as stated above. This comment was also made from Reviewer 2 and we will add relevant references to this part.

4. 18134: 12. "With regards to. . ."; syntax error: Nematode diversity (subject) is not the dominant taxon (object), please rephrase

Reply

Accepted: We will rephrase the sentence and remove the following part "which is generally the dominant taxon" in order to correct the sentence grammatically.

5. 18134: 22. Syntax error. ". . . , whereby one of the main objectives was to. . ."

C9136

Reply

Accepted: We will correct the syntax as suggested.

6. 18134: 28. Please mention the meiofauna size restriction as has been done for the macrofauna in this section

Reply

Accepted: We will add the size range of the meiofauna (i.e. between 32 and 1000 μm)

7. 18135: 6- 13. Research questions need attention. Point 2 and 4 seem very similar and could be combined, but see general comments, paragraph 2.

Reply

Accepted: This comment has been addressed and the research questions have been rephrased (see detailed answer in General comment No. 1)

8. 18135: 18-19. 10 different microhabitats, 5 at A'dam and 4 at Napoli. Please connect next sentence to include the control mud field sample as part of the 10 microhabitats.

Reply

Accepted: The two sentences will be combined into one to include all 10 microhabitats.

9. 18136: 25-26. Please mention whether the reduced sediment is the gas emission area, so to use the terminology consistently so that the reader can follow the switch to another microhabitat easily.

Reply

Accepted: We will use through out the manuscript the exact same terminology (i.e. reduced sediment).

10. 18138: "Similarly (to what). . ."

C9137

Reply

Corrected: The word "Similarly" will be removed from the sentence.

11. 18138:10. Video-guided push cores using the ROV Victor-6000. . .

Reply

Accepted: The sentence will be corrected according to the suggestion.

12. 18140:19. With an average of 1127 ind/10cm², this means that the other sample obtained at the mussel beds of A'dam had only 262 ind/10cm². This is nearly an order of magnitude difference between samples from the same site. It would be very interesting if in the discussion, the authors could allude as to the reasons why such discrepancy is observed between samples from the same site.

Reply

Accepted: This is a misunderstanding, because the highest density of 1992 ind/10cm² refers to total meiofauna and the average density of 1152 ind/10cm² refers to the average nematode density. However, the difference between the two samples from the same habitat is indeed quite large (e.g. 1992 and 576 ind/10cm² for total meiofauna and 1835 and 418 ind/10cm² for nematodes). Therefore, following the reviewer's suggestion, we will add a few lines in the Discussion in an attempt to elaborate on this large difference.

13. 18141:19. A couple of sentences ago, the authors mention that there are 3 species in the Sabatieria complex, yet here the authors refer to species 4? Is this a mistake? It would make more sense to limit the numbering of species to the number of species that were actually found.

Reply

When identifying nematodes to species level, people tend to use what we call working species, i.e. Sabatieria sp.1 Sabatieria sp.2 etc. It is also common practice, a group of scientists working within the same laboratory to use a consistent

C9138

naming scheme for all projects. For example, if we find 4 Sabatieria species in one project, they will be named with Sabatieria sp.1 through sp.4. If we carry out another project and find only one Sabatieria species, which however is the one we named Sabatieria sp.4 in the previous project, we keep naming it sp.4 in order to avoid confusion as well as to be able to analyze the data together. Renaming the working species constantly, so as to have always an ascending species numbering, would make it after a while impossible to recall which species is which. There are also many practical difficulties involved in such a renaming. For example, during the identification process, one would make a drawing of the species so as to use these drawings as an identification guide. Renaming would mean to go back and correct all the drawings, as well as all the data sheets and then the computer files etc. For all the above reasons we would prefer to avoid renaming our species. This way, by just reading the publication we can go back and find out quickly which species we are referring to. Nevertheless, we understand the confusion this might create to the reader, and therefore, in the new version of the manuscript we will provide the names of the three Sabatieria species encountered in our study in parentheses the first time we refer to them.

14. 18141:24. I'm guessing the authors mean "i.e. (that is to say) Aponema" instead of "e.g. (by example) Aponema" since there is only one Aponema species that has been observed in the study

Reply

Accepted: The reviewer is right and we will change e.g. to i.e.

15. Discussion: few comments. Validity of the arguments put forward in support of biological over physicochemical importance in driving the observed thiobiotic abundance peak.

Reply

Accepted: This comment refers to the General comment No.2 and will be ad-

C9139

dressed according to the suggestions from both reviewers (see our detailed reply to General comment No.2).

16. 18145: 15-18. There are several more recent articles that describe this phenomenon, which is a common feature for seeps. E.g. general statement about the widespread occurrence of this phenomenon in Vanreusel et al. 2010 (PLoS ONE)

Reply

Accepted: We will update the citations including more recent ones and we will also include the paper by Vanreusel et al. 2010 as suggested by the reviewer.

17. 18148:1-10. To me, a subsurface peak only seems evident at the summit location (and even then, the vertical profiles seem to correspond with general deep-sea sites) ...

Reply

Accepted: This comment is similar to General comment No.2. As has been mentioned already, we will deal with this problem by taking a number of actions in order to satisfy both reviewers (see our detailed reply to General comment No.2).

18. 18150: 15 and further. Does this make it an issue of sampling effort? The more area is covered by sampling, the higher diversity will be. . . corresponding with the well-known species-area relationship.

Reply

Accepted: This is actually not a comment but rather a question. The reviewer asks whether the patterns observed in our study as well as in the studies by Van Gaever et al. (2010) and Vanreusel et al. (2010) could be an artifact due to the different sampling effort applied to different spatial scales. We will address this question by making a note in our discussion about this possibility.

C9140

19. 18152:1-7. 18152:1-7. Is it not possible that the sparse occurrence and lack of taxonomic effort has prevented description of this species? The fact that the deep sea remains severely undersampled, definitely for nematodes, means that there is a very high possibility that new species are encountered with every extra sample, but that doesn't mean endemism is high. Moreover, the high occurrence of singleton species suggests that there must be more individuals of the same species to validate their presence in light of population ecology principles.

I would smoothen the statement that it is highly likely that the recovered Aponema species is endemic.

I fully agree with the last statement in this paragraph, however, namely that it is very difficult to draw conclusions on endemism, without molecular techniques (But also more exhaustive deep-sea sampling so that true communities may be identified)

Reply

Accepted: Of course the reviewer is right here since this is indeed very likely. This is why we stressed that it is difficult to draw conclusions on endemism without molecular evidence. Nevertheless, as the reviewer suggested, we will smoothen our statement and we will also add a sentence making clear that all this is very speculative also because of the deep-sea being understudied.

20. 18152:19-20. I would specify: “. . . nematode communities in the eastern Mediterranean and for seeps in general”

Reply

Accepted: We will change the sentence making it more specific as suggested.

21. 18152:24. Adverb instead of adjective: “patchily”

Reply

Accepted: We will make the grammar correction.

C9141

22. Conclusions: deep penetration of nematodes and the thiobiotic observation are not mentioned in the conclusion, so I would remove it from the abstract as well + smoothen the conjectural statements on this topic in the discussion or elaborate in order to present a more convincing case.

Reply

Accepted: [Again a comment on the vertical distribution which has been addressed before \(see our reply to General Comments No.2\)](#)

23. 18152:26. “. . .on specific environmental characteristics and the availability of recruits nearby of species that are able to thrive in such conditions”

Reply

Accepted: [We will make the addition to the sentence.](#)

24. 18153: 1-6. “. . . but these ecosystems remain severely undersampled, supporting the need for more evidence to substantiate these hypotheses”

Reply

Accepted: [We will make the addition to the sentence.](#)