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

### **Anonymous Referee #1**

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Influence of chemosynthetic ecosystems on nematode community structure and biomass in the deep eastern Mediterranean Sea, by N Lampadariou, V Kalogeropoulou, K Sevastou, K Keklikoglou and J Sarrazin

### General comments

The authors present an interesting manuscript dealing with the meiofauna and nematodes from two different mud volcano systems in the eastern Mediterranean. Whilst mud volcanoes have received increasing interest in recent years, the study of meio-

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fauna from these systems is lagging behind in comparison with other faunal disciplines. The proposed manuscript will in my opinion be well received by the scientific community interested in benthic ecosystems, and the research results presented are an important addition to the literature out there on this subject. It will allow comparing the faunal assemblages found at other seep/mud volcano systems, something that will gain attention in the near future as more information on these ecosystems becomes available. The effort made to present such a complex benthic ecosystem with various distinct subhabitats in a coherent and clear way should be applauded. The manuscript is generally well written and provides clear general and specific information on chemosynthetic ecosystems in the deep sea, which should enhance readership, and addresses the research questions posed adequately. I anticipate my overall positive review, but would like to make some comments/issues that should be addressed.

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. Question 2 and 4 for instance, steer in the same direction and can be combined: “Do different mud volcanoes harbour different and specialized meio-fauna/nematode communities and are they distinct from surrounding deep-sea sediments”. Question 3 is integrated in this question as well in the sense that if different volcanoes harbour different communities, than they vary on a regional scale (Indeed, the regional scale is mentioned in both questions). Question 3 could therefore be seen as redundant. 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.

While the presented paper does not present ground-breaking concepts, ideas or tools (it is, in essence, a descriptive study as acknowledged in the abstract, although the use of ROV sampling techniques is state of the art), the study presents new, previously unavailable, data that will be of great interest to scientists of various disciplines in the field of chemosynthetic ecosystems. New data is one of the requirements to be

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suitable for publication in BG and the study fulfils this. The innovative nature hence lies in the conclusions as a result of the descriptions, i.e. the distribution patterns, the gained insights in mud volcano subhabitat complexity and variability, the contribution of individual mudvolcano diversity to total diversity in the Eastern Mediterranean Region. One concept the authors touch upon is the indication of nematodes occurring in deeper sediment layers, and this is ascribed to biological rather than physicochemical factors. Despite the fact that similar observations have been reported 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.

Nevertheless, the general conclusions in response to the scientific questions posed are substantial and well-presented in my opinion, whilst the scientific methods and assumptions come across as valid and clearly outlined. The description of the sampling programme is clearly presented. The paper is well-embedded in the recent literature, with a clear indication of the authors’ new contribution in the relevant scientific fields. The overall presentation is well structured and clear, using fluent and precise language, making it a very readable article.

## Specific comments

Title: The title is accurate and reflects the content of the paper well.

Abstract: well structured, concise and clear, but I would suggest one correction

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). I am not sure whether I can follow the reasoning for accepting that biology is more important than environmental

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characteristics in terms of being more determinative for the subsurface peak in nematode abundance that has been observed. Surely, it must be both, namely the fact that the physicochemical characteristics allow for certain nematodes that can thrive in such environments to attain high subsurface densities. A bacterial relation may exist as well, enabling certain nematodes that have perhaps established a prokaryotic symbiosis to cope with the conditions. I feel that this discussion point needs more attention if the authors want to put it forward as one of their conclusions.

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.

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

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

18134: 22. Syntax error. “. . ., whereby one of the main objectives was to. . .”

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

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

Material and methods: Well-structured and well-written, with concise information for the reader. Few comments.

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.

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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.

18138: “Similarly (to what). . .”

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

Results: few comments

18140:19. With an average of 1127 ind/10cm<sup>2</sup>, this means that the other sample obtained at the mussel beds of A'dam had only 262 ind/10cm<sup>2</sup>. 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.

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.

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

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

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)

18148:1-10. To me, a subsurface peak only seems evident at the summit location (and

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even then, the vertical profiles seem to correspond with general deep-sea sites). At the other mud volcano microhabitats, the vertical profiles of the nematode densities seem to follow the generally accepted pattern, i.e. densities decrease substantially with increasing sediment depth. I feel that there is not enough evidence to support the idea of a subsurface peak occurring at all the study locations. To refer to the other examples given in the manuscript, Hauquier et al (2011) report max densities at 2-4 cm with >1000 ind/10cm<sup>2</sup> (true and clear subsurface peaks compared to the surface layers) and when considering the evidence given by Zeppili et al 2012, the high densities at the subsurface seem only high because sediment layers 1-3 cm were combined and compared against half the volume of sediment of the 0-1cm layer.

The deep penetration of nematodes does not seem to be aberrant to what is found at many other 'normal' deep-sea sites when looking at percentage contributions to total nematode abundances.

I have difficulty, following the reasoning when the authors claim that oxygen penetration of sulphide concentrations are not the main factors governing the depth penetration of meiofauna at seeps, and that other factors such as food availability, sediment mixing and root penetration are equally important. Please don't understand me wrongly, there is a case to be made (particularly for the relation with prokaryotes as food or some kind of symbiosis) but a clear train of thought as to why the authors think this is the case is needed, as well as evidence to support this hypothesis. In my view, the reasoning and evidence as currently presented is insufficient.

In short, the densities given in this manuscript does not seem to give a clear indication of a thiobiotic peak or substantial deep penetration by nematodes (but I would be pleased to see otherwise). Some other studies do, and I think that more argumentation is warranted to support the investigated locations to be part of that group, as well as the reasoning behind the supposition that biological factors are driving the existence of thiobiota.

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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.

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)

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

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

Conclusions: deep penetration of nematodes and the thio-biotic 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.

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

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

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