

Anonymous Referee #2
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I was asked to review the paper "Hydrothermal activity lowers trophic diversity in Antarctic sedimented hydrothermal vents" by James B. Bell, William D. K. Reid, David A. Pearce, Adrian G. Glover, Christopher J. Sweeting, Jason Newton, and Clare Woulds.

I find the paper well in the scope and focus of the Journal and the scientific work carried out is surely of high quality. Data are abundant, protocols and procedures of sampling and analysis are adequate and the techniques used are relevant.

This manuscript is the natural continuation of the previous paper written by the same author pool on the same site and it completes the previous findings. Although the results are interesting and well supported, I find the manuscript very long and often difficult to follow and wearisome. In particular, the discussion is not straightforward, lengthy and, in my opinion, it lacks a strong structure. Too often it winds and results tortuous, forcing the reading to go back in order to find the "fil rouge" to follow. I would warmly suggest to shorten the whole manuscript and in particular the discussion. In my opinion, the discussion should follow fewer clear, strong and important points, starting from hypothesis moving through the results and finally offering the conclusions and the answers to the main scientific questions.

This point has been fairly raised by both reviewers. We agree that the discussion could be structured better and will address this point in the revision, through a clearer focus upon the hypotheses presented and reduction in overall length.

I would suggest to insert some more tables and figures that better present the results: for instance, the data reported in the paragraph 3.1 lines 297-304 are not listed in any table nor well represented in a figure and this is a pity. Since the scientific and technical effort behind this work is huge, I would suggest trying to valorize it more by showing all the numbers and cite tables and figures more in the text than in the supplementary material.

We will add more figures and tables where appropriate, keeping in mind limits relating to the journal style.

I have only one strictly scientific comment to make: in lines 686-687 the authors say "Neotanais from the off-axis site had the lowest $\delta^{13}\text{C}$ and $\delta^{15}\text{N}$ values of any non-siboglinid taxon (Fig. 5), suggesting a significant contribution of methane-derived carbon". This sentence may be misleading: while I agree that a lower $\delta^{13}\text{C}$ may suggest the metabolism of methane-derived carbon, I fail to see how a lower $\delta^{15}\text{N}$ signature may support this hypothesis, since methane does not contain N. It would be better to reformulate the sentence.

We will remove reference to Nitrogen isotopic values as suggested by the reviewer, so as to avoid confusion.