

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-284

Jutta Wollenburg (Referee)

Referee comment on "Deposit feeding of a foraminifera from an Arctic methane seep site and possible association with a methanotroph revealed by transmission electron microscopy" by Christiane Schmidt et al., Biogeosciences Discuss.,
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The manuscript does address relevant scientific questions within the scope of BG. It also presents partly novel data.

Are there substantial conclusions reached or not is questionable as the manuscript intends to show results from feeding experiments that were obviously too short or failed. *N. labradorica* is usually a deep infaunal (limited by the availability of oxygen) dwelling species that covers its test with a sedimentary cyst. In these experiments the sedimentary cyst was removed, which should have led to significant stress for the animals as thereby also a significant amount of ectoplasm is removed. Being placed in petri dishes with artificial seawater and after what is called a starvation period of 2-4 hours (why this range?), that from my experience is too short for foraminifera specimens to recover, either cultured *M. sedimenti* was added or not. The maximum experimental incubation time was 20 h. Of the 17 specimens selected only 2 specimens showed very a total of 3 supposed methanotrophic bacteria close to the reticulopodial network that rests in the final chamber of rotaliids. Obviously, none of the identified bacterial remains could be definitively related to the provided bacterium. Furthermore, obviously all specimens showed clay particles in the final chamber, that was not provided during the experiment. The fact that clay was observed in the last chamber of all specimens and only 2 specimens showed 3 bacteria that eventually from which two were not even provided during the experiment, indicate that clay and bacteria were eventually in place before the start of the experiment. Although I consider it logical that *N. labradorica* may facultatively nourish on bacteria, the experiments are no proof. Rotaliids extend and retreat their pseudopodial network into the final chamber and whatever attaches to these filaments will be found there. If we look at the colorful Fig. 2a, it becomes obvious that we would expect any nutritious material predominantly in the older test parts (see also Wollenburg et al, 2018 Fig. 8b). The final chamber is a place of activity like a garage for pseudopodia or for short storage, but if you don't find traces of the provided bacteria in older chambers, their presence in the last chamber could just be accidental. Support for this suggestion comes from the very few observations of bacteria and the absence of *M. sedimenti*. So, I consider the experiment as too short or rather failed and would focus the manuscript on the great TEM figures and place some speculative assumptions on experimental pictures (by the way I like the pictures), or to redo the experiments for a significantly longer period of time and with labelled bacteria. This would also prove that the specimens even survived the treatment, a 24-h experiment on specimens that immediately died during the treatment would show no

different TEM pictures, and as no pseudopodial activity was reported, the survival rate is unknown.

The scientific methods are sufficiently described and can be reproduced by scientists.

I don't consider the experimental results sufficient to support the interpretations and conclusions, but I would like to see a revised version focusing on the TEM pictures especially from the TEM pictures with the paired clay and bacteria vacuoles. I don't understand why the authors elaborate on geochemistry methods, when neither $\delta^{13}\text{C}$ or other geochemistry aspects have been applied to the investigated specimens. I would recommend to either get rid of this method chapter, except for the basic oceanographic feature or to put them in context to the foraminiferal data. A basic information on the sedimentary composition at the coring site is needed to put the clay particles in context. Furthermore, is there a preferred grain size spectrum in the food vacuoles? If yes, why? What does this tell us.

The description of experiments is sufficiently complete and precise to allow their reproduction by fellow scientists.

I think that the authors should leave their inner circle of referencing. Especially regarding carbon isotopes there is a misinterpretation of published work that essentially relies on the assumption that foraminiferal shells reflect the bottom or pore water signature they are dwelling in not their nutrition. And regarding Wollenburg et al (2007) the statement they place is wrong. This study confirmed that foraminiferal shells reflect the emanation of methane and its signature.

The authors usually give proper credit to related work and clearly indicate their own new/original contribution.

The title clearly reflects the contents of the paper, but should be addressed to *N. labradorica* only

The abstract has to be shortened to be concise.

The overall presentation is not well structured and should be streamlined.

The language is fluent.

The whole text contains a lot of repetitions and should be rewritten in a way that one aspect is addressed predominantly at only one place in the introduction and discussion. There are also a lot of typos and missing blanks.

The supplementary material could be improved by showing successful TEM pictures of all investigated specimens.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2021-284/bg-2021-284-RC1-supplement.pdf>