

Interactive comment on “The distinct roles of two intertidal foraminiferal species in phytodetrital carbon and nitrogen fluxes – results from laboratory feeding experiments” by Julia Wukovits et al.

Anonymous Referee #1

Received and published: 4 June 2018

In this paper, Wukovits et al. report the results of two series of short-term experiments involving *Ammonia tepida* and *Haynesina germanica*, two benthic foraminiferal species commonly found in intertidal environments. In particular, three different temperatures (15°C, 20°C, and 25°C) and two food sources (diatom vs. chlorophyte) were tested in this study. The goal of these experiments was to investigate carbon and nitrogen uptake and turnover by *A. tepida* and *H. germanica* and the impact of these processes on intertidal organic matter fluxes. Overall, the manuscript is well written and the findings are interesting, in particular when discussed in the context of the carbon and nitrogen

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cycles in intertidal environments. However, I do have some issues with this work, as I explain more in detail below. Because of this, I recommend major revisions before the manuscript can be accepted for publication in “Biogeosciences”.

Major comments

Introduction. I think that the manuscript would benefit from a greater overview of: 1) the carbon and nitrogen cycles in coastal environments 2) the role of benthic foraminifera in the carbon and nitrogen cycles. Some information is provided in the Discussion section of the manuscript. However, I think that a general overview of these processes should be included in the Introduction, as well.

Line 49. The authors briefly mention previous studies on feeding preferences/strategy. Considering that these are important points that are discussed later in the manuscript, I suggest providing more information regarding past experimental studies. In doing so, the authors can better emphasize the novelty of their work in the context of earlier investigations.

Line 56. This work might be of interest to readers who might not be familiar with foraminifera. Thus, I recommend to better explain what the authors mean by “release of OM derived carbon and nitrogen in foraminifera” and how this connects with OM remineralization processes in coastal environments.

Material and Methods. I think that the authors should provide more information regarding their experimental design. For example, for Experiment 1, why did they choose to terminate the incubation after 24 hours? Is this enough time to obtain a significant result? Why chlorophyte as a food source was not tested in Experiment 1? Why *H. germanica* was not included in Experiment 2? Why was 20°C (and not 15°C or 25°C) the temperature tested in Experiment 2?

Lines 88-90. How were these atom%’s established?

Line 93. Is 28 PSU the same salinity as at the sampling site?

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Lines 103-109 and 124-128. My suggestion is to explain the statistical treatment of the data in a separate section.

Line 132. “The sediment core data, together with the data from the laboratory experiments, were used to estimate (. . .)” The authors combined sediment core data with data from laboratory experiments to estimate total foraminiferal biomass and foraminiferal C and N processing. My question is why? The data obtained from sediment core (“natural samples”) should be compared (and not combined) with the ones obtained from the laboratory experiments, as experiments are a simplification of the natural environment.

Line 140. After decalcification, the authors kept the foraminiferal at 50°C to dry for three days. Are the authors using a published protocol? If so, please cite the reference. If not, is it possible that such a long drying step could have altered their results?

Table 1, 2nd column. “50 – 55”. Are 50 the number of specimens used in the 24/fed experiment and 55 the number of specimens used in the 24/starved experiment? If so, please specify. [h] should be [hrs] for consistency with the rest of the manuscript.

Results. I invite the authors to consider reporting the data presented in figure 1 as an additional (supplementary?) table.

Figure 1 c and d. Considering that the temperature is specified in the x axis, I do not think that the authors need to color code the data points, also because the “middle” shade of gray and the darker shade of grey cannot be easily distinguished. An alternative might be using different symbols for different temperatures. Also the meaning of “ns” is not included in the caption.

Figure 2a. Can the data be differentiated based on the temperature of the experiments? Maybe different symbols (or colors) can be used for this purpose.

Figure 2b. The figure is a bit confusing. Again, I would recommend using different symbols (or colors) for different trends.

Figure 3. Chlorphyte should be Chlorophyte. Also, not all the symbols of the figure

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legend correspond to the symbols on the plots.

Section 3.2 I am not sure I understood how the authors were able to distinguish the contribution of one food source over the other in the mixed diet.

Discussion. The authors mention the presence of chloroplasts in *Haynesina germanica*. How about *Ammonia tepida* (cf. Jauffrais et al., 2016)?

Lines 294-296. I think that the authors make a very interesting point here. Can they expand on this?

Minor comments

Line 12. Should “ ^{13}C & ^{15}N ” be “ ^{13}C and ^{15}N ”? This comment applies to the rest of the manuscript.

Lines 14-19. Throughout the manuscript, the results obtained in *A. tepida* are discussed before those obtained in *H. germanica*. I recommend maintaining the same structure in the abstract, as well.

Line 25. “Coastal sediments represent the largest pool of marine particulate organic matter (OM) (...)”. Can the authors add some numbers (maybe a percentage?) regarding how big the OM pool is in coastal sediments? In my opinion, such a number will provide a good context to discuss the data obtained from the experiments and to discuss the importance of remineralization processes mediated by benthic foraminifera in coastal environments.

Line 36. “(...) e.g., temperature or OM quality”. This should be “temperature and/or OM quality”.

Lines 40-41 and 47-48. These sentences are not very clear. Please, rephrase.

Lines 58-59. Considering that the experiment described at lines 58-59 is Experiment #2, I suggest moving this sentence after the sentence at lines 60-61, which refers to Experiment #1.

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Line 72. M2 should be m2.

Line 75. “Individuals were picked from the sediment in sufficient and collected (. . .)”. In sufficient number?

Line 77. “*Dunaliella tertiolecta* and *Phaeodactylum tricornutum*” The scientific name was already defined at line 58, so this should be *D. tertiolecta* and *P. tricornutum*. This comment applies to the rest of the manuscript, with the exception of tables and figures.

Lines 79-80. “The experiments started after accumulation of sufficient foraminiferal material three weeks after the field sampling.” I assume the authors achieved foraminiferal reproduction during the initial incubation. If my assumption is correct, then it would be good to specify so and provide some information about the conditions used to maintain the foraminifera prior the beginning of the experiments. If the authors know, it might be of interest to know how successful the reproduction event was.

Line 84. $\text{NaH}^{13}\text{CO}_3$, $\text{Na}^{15}\text{NO}_3$ should be $\text{NaH}^{13}\text{CO}_3$, $\text{Na}^{15}\text{NO}_3$.

Line 88. C.f. should be cf. This comment applies to the rest of the manuscript.

Line 108. What do the authors mean with “carbon and nitrogen costs of the two species during the period without food”?

Line 114. Cm-2 should be cm-2.

Line 135. A parenthesis is missing.

Line 137. I suggest including the word “cytoplasm” prior “isotope analysis”, for clarity.

Line 153 (formula #2). atomXsample – should this be atom\%Xsample ? Same for background.

Line 155. I recommend writing the liso formula as the other formulas, for clarity.

Line 184. There is an extra period after Table 2.

Line 205. No comma needed.

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Line 212. *Phaedactylum tricornutum* should be in italic.

Section 4.1 revise references – e.g., a comma is missing between the authors' names and the year of publication and a semicolon should be used to separate different references.

Line 232. Missing parenthesis.

Line 250. Almagor et al. – publication year 1981.

Lines 281 and 290. Missing parentheses around the year of publication.

Lines 288 and 295. Comp. should probably be cf.

Reference. Jauffrais, T., Jesus, B., Metzger, E., Mouget, J.-L., Jorissen, F., and Geslin, E.: Effect of light on photosynthetic efficiency of sequestered chloroplasts in intertidal benthic foraminifera (*Haynesina germanica* and *Ammonia tepida*), *Biogeosciences*, 13, 2715-2726, <https://doi.org/10.5194/bg-13-2715-2016>, 2016.

Interactive comment on *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2018-231>, 2018.

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