

***Interactive comment on “Lack of  $^{13}\text{C}$ -label incorporation suggests low turnover rates of thaumarchaeal intact polar tetraether lipids in sediments from the Iceland Shelf” by S. K. Lengger et al.***

**Anonymous Referee #2**

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This is a well-written paper describing a straightforward experiment: SIP of IPL-GDGTs and PLFAs in marine sediments. This is currently a topic of interest because the distribution, activity, and carbon substrates of Thaumarchaea in the environment are still poorly understood. The experiments performed were appropriate and were interpreted intelligently. Previous work on related topics was sufficiently referenced and discussed. The lack of substantial  $^{13}\text{C}$ -uptake in the archaea is interesting, especially in light of the  $^{13}\text{C}$ -uptake in PLFAs in the same incubations.

Specific comments:

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Page 12809 Line 2-3: The references cited (Spang et al. 2013 and Brochier-Armanet et al. 2008) discuss the assignment of the Thaumarchaea as a separate phylum, not kingdom. Since “kingdom” is so rarely used now, I recommend avoiding it so as not to lead readers to mistakenly believe it is a new domain.

Line 6: Fuhrman et al. 1992 Nature should also be cited here.

Line 11-12: Reword so as not to suggest there are no enrichment cultures.

Page 12811 Lines 16-20. The authors mention in the abstract and suggest again here that the reason they have chosen these substrates is because they have been shown to be taken up by Thaumarchaea previously. I would caution the authors from assuming that all Thaumarchaea are metabolically identical, i.e. that if one takes up pyruvate they all do. In the discussion, the authors discuss the possibility that the archaea may not be able to consume the substrates provided, but it might be mentioned in the introduction, as well. It was unknown if the Thaumarchaea in the particular communities tested would be able to assimilate all (or any) of these substrates.

Page 12824 Lines 9-21: This is an excellent paragraph. Although the following section is interesting, these three options all remain a possible explanation for these results.

Section 4.3 I am concerned about the calculation for growth rate. It assumes that all of the GDGTs are derived from living Thaumarchaea, which is not known. This assumption should be stated if this calculation is included. A small fraction of the total GDGTs may be highly enriched (derived from a community of fast growing Thaumarchaea) but not detected as such because of a large background of fossil GDGTs. To truly calculate the growth rate of the living community, the authors would need to know or determine the fraction of GDGT that is fossil versus within living cells. If all GDGT detected are indeed derived from living cells, then this growth rate is accurate, but otherwise it is an underestimate.

Page 12825 Lines 20-25: Providing the incubation times in these experiments would

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be useful.

Page 12826 Lines 23-26: I don't believe cells from these depths would rupture at sea level pressure. Can the authors provide a reference?

Page 12827 Line 1: "as well as" should be replaced by "and/or", as in the abstract, because as described above, Thaumarchaea growth may be fast, but simply appear slow due to the dampening of the isotopic signal by fossil GDGTs. Whether or not a small fraction of the community is fast growing or not, the overall turnover time of the bulk GDGTs remains very long, so the overall conclusion does not need to change.

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Interactive comment on Biogeosciences Discuss., 10, 12807, 2013.

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