

Interactive comment on “Microbial colonization and alteration of basaltic glass” by J. Einen et al.

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Microbial colonization and alteration of basaltic glass

by J. Einen, C. Kruber, L. Øvreås, I.H. Thorseth, and T. Torsvik

General comments: This manuscript addresses a topic of great interest: the composition of the microflora colonizing glass rims of basaltic glass. As basalt covers a great deal of the ocean floor, its endolithic biosphere may have a significant impact on alteration processes and cycling of elements between the hydro- and geosphere. The manuscript by Einen et al. is from a group of authors that has been spearheading the research on bioalteration of basaltic glass. Its particular value lies in a well-designed experimental approach that helps to constrain the basic parameters and limitations of bioalteration. Though the authors did not set off endolithic activity in their microcosm experiments, the general understanding of the ecology of basalt-associated prokary-

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otes will benefit from this contribution. I recommend the publication of this manuscript in Biogeosciences after some revision.

I find it hard to understand why the potential role of archaea was not considered in the experiments. Two articles, one even from this group of authors, report archaea to be associated with submarine basalt. In this light it is surprising that no archaea specific primers have been used for 16S rDNA analyses (page 279). The authors should clarify why they focused on bacteria only.

Parts of this paper read like a technical report. This particularly applies to the chapters 4.3 to 4.7 of the discussion. The manuscript is generally pretty short on the discussion side. The different numbers of bands in the DGGE profiles have important implications on the ecology of the glass-associated microorganisms. Though some aspects are discussed and good points are made, I see more potential for a comprehensive reflection of the results. Microcosm experiments are mostly discussed individually leaving room for more comparison of the results from different microcosm experiments.

Having said this, the first paragraph of the ‘conclusions’ should be moved to a ‘discussion’ chapter; in the ‘conclusions’ only consequences should be drawn. In this paragraph the results of this study are compared to the results of an earlier study and the implications of the differences are discussed.

I feel that the referring to previous work is a bit biased toward publications by members of this group of authors. Additional useful references include the following: Banerjee et al., 2006, EPSL 241, 707-722. Fisk et al., 2003, G-cubed, GC000387. Furnes and Staudigel, 1999, EPSL 166, 97-103. Rogers and Bennett, 2004, Chem Geol 203, 91-108. Schumann et al., 2004, Geomicrobiol J 21, 241-246.

The style of the written English is pretty simple, but acceptable. However, once in a while the text gets a bit too monotonous when several sentences in a row have the same verb. I spotted quite a few mistakes, particularly with regard to grammar (see technical corrections below) and suggest that the text should be checked by a

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native speaker. Finally, I need to stress that this reviewer is a geologist and unable to evaluate parts of the text on microbiological techniques and phylogenetic relationships. I suppose that these aspects will be covered by another reviewer.

Specific comments on the manuscript: (1) Page 273, title: I am afraid that the first part of the title ‘Microbial colonization and alteration...’ can be misleading. I feel that most readers will think that ‘microbial’ also refers to ‘alteration’. However, you conclude that all the alteration observed in your experiments is abiotic. I suggest to modify the title to avoid this confusion. (2) Page 274, lines 2-3: It is a bit unusual to refer to and highlight previous work in an abstract; even more so as you refer to your own work. I suggest to avoid this citation here. (3) Page 274, lines 3-6: I would agree with this statement, but it is highly speculative as the metabolisms of endolithic microorganisms are still unknown. An iron-based metabolism seems to be the most likely metabolism of endolithic microbes, but the uncertainties involved make your statement problematic. (4) Page 274, line 11: ‘PCR’ should be written out. I realize that this is probably common knowledge, but then it is inconsistent to explain ‘SEM’ and ‘DGGE’ but not ‘PCR’. (5) Page 274, line 16: ‘Parallel ... habits’ is too unspecific. Judging from your Fig. 5D, ‘bladed aggregates’ or ‘bladed rinds(?)’ might be appropriate. Please check throughout the manuscript. (6) Page 274, line 16: Crystals should not be referred to as being ‘filamentous’. I suggest to use ‘acicular’ or ‘fibrous’ instead. Please check throughout! The precipitates shown in Fig. 5C are very small. I get the idea that they are not just straight acicular crystals. Is something attached to them or do the crystals have dendritic(?) extensions? (7) Page 274, lines 22-28: Rather than using ‘oligotrophic’ to describe the organisms, I would suggest to describe them as ‘chemoheteroorganotrophic’ (I think that is what they are, correct?). Considering the scientific question you are dealing with, the information provided in this way would be more clear-cut (see page 290, line 22). (8) Page 275, lines 22-24: Both studies cited, concluded that the microflora is dominated by bacteria. Other studies, however, concluded that archaea (Fisk et al., 2003) or even eukarya (Schumann et al., 2004) are significant components. Though you may not necessarily agree with the interpretation expressed in these pub-

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lications, they should be mentioned. See also general comment above. (9) Page 277, line 1: Please indicate the size of the pieces the glass was crushed to. (10) Page 278, lines 24-26: Though this is a well-established technique, I think it would be desirable for future studies to also use field-emission environmental scanning microscopy in order to better control the relation of attached microorganisms to alteration features. (11) Page 279, lines 22-23: Does this mean that you did not consider the presence of archaea? If so, I wonder why you excluded archaea from your analyses. See general comment above. (12) Page 281, line 19: I suggest to add 'secondary' between 'and' and 'mineral'. (13) Page 282, line 6: It is five microcosms, isn't it? (14) Page 283, line 26: Misleading. Which band? In the preceding sentence you mentioned four bands. Even if they represent the same organism, they are still four bands. (15) Page 284, line 1: Is this sentence still referring to microcosm 3B? Please clarify. (16) Page 284, lines 18-21: First you state that no cells were observed by SEM. Then, in the next sentence, you say that 'rods and prosthecate organisms were observed'. This is a contradiction. Please clarify. (17) Page 287, lines 5-6: No. The shape of crystals is not diagnostic in this respect. Microbially-induced carbonates come in all sorts of crystal habits. To test whether or not microbial metabolism drove or contributed to secondary mineral formation requires geochemical analyses. (18) Page 287, lines 8 and 9: It seems to me that this statement is too far-fetched considering your observations. (19) Page 287, line 15: Did you consider that anoxic microenvironments may have developed in your microcosm? Maybe methane was oxidized by anaerobic methanotrophs. This hypothesis is obviously highly speculative, but hard to exclude as you have not screened for archaea. (20) Page 290, line 20-21: Though this is pretty much straightforward, I suggest to add 'chemo' to 'autolithotrophic ...'. To be precise 'chemoorganotrophic' then should be replaced by 'chemoheteroorganotrophic'. The comparison of 'autolithotrophic' (lines 20-21) with 'chemoorganotrophic' is inconsistent, because two different categories out of three categories (sources of energy, carbon, and electrons) are used to characterize the respective organisms. (21) Page 290, line 27: 'abiotic origin'; see comment 17 (22) Caption of Fig. 8: 'Aerobe' and 'Anaerobe' refer to organisms. I am pretty sure that

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'anaerobe conditions' is not appropriate and should be replaced by 'anaerobic'.

Technical corrections: (1) Page 273, line 5: The first letter of 'Earth's' should be a capital. (2) Page 274, line 16: 'Crystal habits' or 'crystal aggregate habits' instead of 'crystallization habits'. Crystallization is a process. (3) Page 275, line 10: 'result' instead of 'results'. (4) Page 275, line 16: D'hondt et al., 2004. Reference missing in the reference list (5) Page 276, line 18: Check written English for correctness. (6) Page 276, line 25 (displayed): Check written English for correctness. (7) Page 278, line 6 (... microcosm replica... .. terminated ...): Check written English for correctness. (8) Page 278, line 8: ... bottles ... were ... (9) Page 279, line 20: ... steps ... were ... performed (10) Page 280, line 23: ... matrix was created ... (11) Page 283, line 7: The spelling of 'rod shaped' is inconsistent in the manuscript (e.g., rodshaped, page 284, line 8). I suggest: rod-shaped; please check throughout. (12) Page 283, line 10: mentioned (13) Page 283, line 26: sequences (14) Page 283, line 29: add comma after 'sp.' (15) Page 284, lines 13 to 16: Please rephrase this sentence. (16) Page 284, line 18: Delete comma after 'Rhodobacteraceae' (17) Page 284, line 27: *Methylobacter* needs to be in italics and the first letter has to be a capital. (18) Page 285, lines 5-6: I am confused here. This statement reads as if only sequences from an aerobic inoculum are marked with an asterisk, but below (line 14) you state that sequences from an anaerobic inoculum are marked as well. Rephrase. (19) Page 285, line 16: Omit hyphen after the alpha (20) Page 285, line 23: delete 'in 5B' (21) Page 287, line 3: 'precipitates' instead of 'precipitations'; check throughout (e.g., caption of Fig. 5) (22) Page 287, line 7: Whereas (23) Page 288, line 5: I think the plural of 'hypha' is 'hyphae', not 'hyphas'. (24) Page 288, lines 11-17: The wording is very monotonous in this paragraph with 'showed' being the main verb of four sentences. (25) Page 288, lines 13-14: 'a sp.'. Do you want to highlight that only one species is dominant? If so, 'a' should be changed into 'only one'. (26) Page 289, line 16: Check written English for correctness. ... affiliation the order ... (27) Page 289, line 17: ... bands were found ... (28) Page 289, line 19: Add a point after 'sp.' at the end of this sentence. (29) Page 289, line 25: ... bioalteration ... was ... (30) Page 290, line 5: Replace comma

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after ‘°C’ with point (31) Page 290, line 7: ... activity than oligotrophic conditions (32) Page 290, line 19: This statement is too colloquial. Maybe just replace ‘microbiology’ with ‘microbiological processes’ and ‘in geology’ with ‘on geological time scales’. (33) Page 290, line 24: ‘potential’ instead of ‘possibility’ (34) Page 290, line 27: hyphae (35) Page 290, line 28: I do not understand: ... ‘bacteria formed pit marks’. Do you mean ‘bacteria forming pit marks’ or ‘bacterially formed pit marks?’ (36) References: In quite a few references the first letters of each word from the title are capitals. Please correct. (37) References: Names of genera and species should be in italics. This applies to at least two references. (38) Table 1: The ‘4’ in ‘methane’ and the ‘2’ in ‘hydrogen’ should be in subscript. (39) Table 2: Be consistent. Two elements are given as ions with electric charge. (40) Table 3: ‘Xd’ is tautologous and should be replaced by ‘d’. If a morphotype is dominant, it is self-evident that it needs to be present. (41) Fig. 9 (note for the publisher): The figure should be enlarged in the final publication

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