Biogeosciences Discuss., 8, C1418–C1421, 2011
www.biogeosciences-discuss.net/8/C1418/2011/

Biogeosciences Discussions

Interactive comment on "Evidence for microbial dissolution of pyrite from the Lower Cambrian oolitic limestone, South China" *by* W. Liu and X.-L. Zhang

W. Liu and X.-L. Zhang

xzhang69@nwu.edu.cn

Received and published: 6 June 2011

Referee 1 thoroughly reviewed our manuscript and raised a couple of tough questions on investigating the evolution of microbial iron oxidation. Generally, the critical comments are helpful to improve to manuscript and meaningful for our future studies on tracing the evolution of microbial dissolution of pyrite. It seems that the referee has no doubt about the morphological evidence that we described in our paper, i.e. characteristic pitting patterns with pyrite surfaces and microbial fossils preserved as iron oxides, because the referee believes that our optical results are new and impressive. However, the referee places too great hopes on our research. The major questions raised by the

referee are the biotic mechanisms of pyrite dissolution under pH-buffered conditions and biogeochemistry of interstitial water in the ancient ooidal sediments. Of course, we will enhance the discussion on these issues in the revised manuscript by discussing the possible oxidants and mechanisms that have been worked out in previous experimental studies. But we do not think these questions can be solved in this small paper. The microbial iron oxidation is an ongoing research topic. Most researchers focus on the diversity of iron oxidizing microbes and their distribution in natural environments. Many questions remain unsolved, for instance the biological mechanism, the interaction between microbes and minerals, and the biogeochemistry at the contact between microcolonies of iron oxidizing bacteria and mineral phase.

The purpose of this paper is to report the dissolution pits and fossilized microbial sheaths of probable iron oxidizing bacteria. Such results were previously unknown in ancient sediments. In our opinion, the morphological evidence (optical results) alone can argue that iron oxidizing bacteria exist in ancient sediment. In comparison with using geochemical or molecular data, e.g. isotopic signature or biomarker, our study is a more direct way to trace the evolution of microbial iron oxidation.

Other more specific comments are addressed below:

1. There is no clear research question formulated in the introducing part.

We agree with the referee and develop a clear theme on our research at the end of the introduction section in the revised version of our manuscript.

2. Looking on microbial iron oxidation would require first a brief discussion on the formation mechanism of euhedral pyrite in the dynamic carbonate dominated, ooid-forming sedimentary environment.

A brief discussion on the formation mechanism of euhedral pyrite and relevant literatures are added in the discussion section.

3. Afterwards a biogeochemical discussion should focus not only on a comparison

with laboratory culture studies that were carried out under completely different pH conditions. It should include the question of biogeochemical processes, including, e.g. oxidants, the composition of interstitial waters and the question of what happened to the precipitated iron oxides in the buffered solutions. One sample found away from any corroded pyrite of unknown mineralogical composition is not a clear evidence for the presence of a pyrite oxidation product (the occurrence of Cr in the SEM analysis is unusual).

We enhanced the discussion on the possible oxidants, geochemistry of interstitial water, and the pH conditions during microbial etching in the revised manuscript. However, rebuilding the biogeochemical processes is not an easy task. It requires a long-term, in-depth study to ensure its accuracy by biogeochemical specialists. In this paper, we would like to account our morphological data in palaeontological point of view. As for the presence of Cr in the EDX analysis, we have no good idea to explain its presence, but it does not have much influence on our results.

4. Following the research question on microbial iron oxidation and the related biogeochemistry in a carbonate environment would require the inclusion of geochemical analyses of trace metal (and isotope?) contents of the intergrowing carbonate matrix (to deduce the composition of reacting interstitial fluids) as well as the application of micro-analytical techniques on the direct surroundings of the pyrite crystals.

It sounds like a nice suggestion and will be helpful to interpret the biogeochemical processes of microbial etching pyrite. Again, we are not geochemists and are unfamiliar with the techniques. Additionally, such facilities are currently not available in our lab. We will consider inviting geochemists to join us and do some geochemical analyses in our future studies on this topic.

5. The whole manuscript requires heavy editing of language and style.

Generally, we think, our manuscript is readable and fits the journal style. Of course, English is not our native language, and there might be some grammatical errors and

unclear sentences. Any way, we will consult English specialists to edit the language in revising the manuscript.

Interactive comment on Biogeosciences Discuss., 8, 2035, 2011.