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Interactive comment on "The role of microorganisms on the formation of a stalactite in Botovskaya Cave, Siberia – palaeoenvironmental implications" *by* M. Pacton et al.

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In this study of an early Quaternary stalactite from a cave in Siberia near Lake Baikal, multiple lines of evidence suggest that major shifts in cave dripwater redox conditions occurred repeatedly during previous interglacial stages; furthermore, microbial biofilm structures on the surface of the stalactite may have facilitated subsequent pulses of speleothem mineral deposition. This paper goes beyond many studies in this field, by combining geochemical characterization with careful descriptive analyses of crystal morphology and textures in a complex speleothem, as well as experimental validation of the origin of features observed in the natural material. I agree with several comments

by the two previous reviewers, and find that the authors have adequately improved the paper in response. However, the article can still be improved by addressing the following comments:

The authors' interpretation of a microbial origin for thin calcite layers characterized by 13C depletions observed in association with hiatuses seems reasonable and supported by the evidence in general. I agreed with reviewer Verheyden that stable isotope results from multiple transects across multiple hiatuses should be compared in order to verify the initial results. In response to S. Verheyden (p. C1933), the authors claim that they have analyzed "several 13C proïňĄles using SIMS, and subsequently analyzed them using SEM. Additionally, we analyzed the section using the incremental milling of powder for IRMS", and then go on to briefly describe the results. However, these data are not included anywhere in tables, graphs, or images. These supporting results should be included, at least as part of the supplementary materials, if they are to support the main conclusions. In particular, it would be interesting to compare stable isotope transects across the porous cracks in the continuously milled vs. the original discretely drilled IRMS sequences alongside the SIMS data. This is an essential technical addition to the paper.

As it stands, the rationale for the microbial experiments lack completeness. Like the other reviewers, it seemed initially surprising that bright incubation conditions were used to mimic microbial textures found in aphotic caves, and I was likewise unsure about the origin of the biofilm and why that one was selected for this study. Future readers of this paper would benefit from additional descriptive context when introducing the microbial experiments. For example, a few sentences describing the biofilm origin (as in C1926-27), and your rationale for using the existing EPS mat (active or dead?) from Brazil for comparison to a Siberian stalagmite would be most helpful. Furthermore, the link between this experiment and the passive mineralization hypothesis could be more clearly articulated: is the Brazilian biofilm dead? If not, why are you so certain that the structure is facilitating mineralization, rather than live microbial

mediation?

In the authors response to S. Verheyden (p. C1932), they mention the dearth of global data regarding inorganic vs. biomediation of carbonate speleothem mineralization. Although the authors hint at a framework for understanding the circumstances in which "organic support can be the driving factor", nothing is mentioned in the conclusions of article regarding how this study fits in with an emerging larger picture of biotic vs. abiotic speleothem growth. While I agree with the other reviewers that it is important to interpret results cautiously, in my opinion this conservatism must be balanced when appropriate with the audacity to envision the broad implications of new directions and discoveries in research. Science proceeds not only by sharing observations and the results of experimental tests, but also by suggesting new ideas for further testing. In this paper, the latter could be strengthened. Such a statement about the scope of biomediation's importance would of course be somewhat speculative; yet, I would like to see a mention in the conclusions about how this study fits in with this larger picture. For example, I wonder whether microbial or biofilm calcite deposition mediation might be an important factor in calcite initiation for cave systems near the extremes of speleothem deposition, such as near the permafrost line as in this cave site? If so, such information would be essential to any future high-latitude/high altitude speleothem studies of glacial-interglacial cycles tied to dating the timing of periods of calcite growth initiation and cessation. For example: could the frequency of colonization of speleothem surfaces be an essential control on how quickly calcite deposition resumes following a climatic return to moist, speleothem-friendly conditions inside the cave? Could radiometric dates of the timing of such calcite growth periods have different sensitivity for abiotic vs. microbially-mediated speleothems? Would EPS structures induce calcite deposition more quickly or more slowly? Obvious, most of these questions are beyond the scope of this study, but this article could do with a few more lines pointing the way forward. Again, although any such broad interpretation or hypothesis would be speculative, I would encourage the authors to include a brief discussion about the potential meaning of this line of research, which in my opinion would make this a more influential

paper.

The inference of a former perched peat bog above the cave site from the observed intervals of Mg and Fe oxide deposition on this stalagmite seems plausible, particularly given the lack of recent instances in the cave which could point to a bedrock source. However, one can imagine random pockets of sulfide-rich material in the bedrock which would have altered the stalactite geochemistry only during the period in which it was weathering. Is there a reason why the peat bog model is less speculative than this sort of bedrock heterogeneity? Could the peat bog hypothesis be tested in future studies using biomarkers or fluorescence characteristics (if present in such ancient material)?

Minor corrections On p. 6572 line 12: Use of the term "relativized" is unclear; rephrase. On p 6579, line 14: insert "not" after the phrase "The mere presence of microbes in a speleothem does" Fig. 6d: Does this white arrow indicate a filament? If so, the arrow should be black.

Interactive comment on Biogeosciences Discuss., 10, 6563, 2013.