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Interactive comment on "Unravelling the enigmatic origin of calcitic nanofibres in soils and caves: purely physicochemical or biogenic processes?" *by* S. Bindschedler et al.

Anonymous Referee #4

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Thank you for giving me the opportunity to review this excellent manuscript. I would recommend publication with minor revisions. The paper presents a very thorough overview of the calcitic nanofibres, a rather ubiquitous continental nanostructure often found in association with needle fibre calcite (NFC). The authors have submitted a very well structured paper that is clearly presenting the key features of calcitic nanofibres, the difference with NFC as well as the various hypotheses of formation. They have obtained very convincing data collected from experiments that produced very similar mineral/organic nanostructures out of sequential enzymatic digestion of cell walls of selected fungal species. These organically produced nanofibres through laboratory fungal degradation represent rather conclusive evidences for a biological origin of such

mineral deposits. The nanofibres, which can be seen as a type of microbialites, are commonly found in continental environments and may play an important role in the continental C and Ca cycle, and by extension, in the global cycle. Bindschedler et al. are proposing a solid and convincing explanations for nanofibre formation. They are discussing the alternative and are making an excellent case for their theory. This paper should be published.

Main Remarks: I would only make one remark concerning the methods and methodology used to image these structures using SEM. The studied objects are often a mixture of both organic and mineral (sometimes difficult to separate or identify). The authors perfectly knows that the SEM methods and the associated methodologies (drying processes) used to prepare the sample for observation may greatly impact the result, especially when the study is mainly based on (nano-) morphological comparison. In order of avoiding comparing artifacts, the sample preparation should be the same for each sample and various SEM techniques should be used and compared.

Remark 1: If I am not mistaken here, the paper gives the impression that the authors are using two different preparation techniques for (1) natural and (2) laboratory samples. Natural samples are dried following a classical alcohol series followed by TMS treatment and the laboratory ones are freeze-dried. Why these differences in treatment. Why are the authors not applying the same methodology for both, especially if the goal is to compare mineral product in close association with organics, in which different sample treatments can produce different artifacts.

Remark 2: In the method section of the paper, the authors are indicating the used of low-temperature SEM (cryo-SEM) on natural samples. First question: Why using this SEM method on natural samples only (an not on the laboratory ones)? Second question: Why are the authors not using any low-temperature SEM pictures in the paper in order to compare with the more classical techniques of chemical drying? I believe that a comparison between high vacuum SEM (normal drying), low-temperature, variable pressure and even wet SEM mode would make a stronger demonstration in order

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to compare such minute structures. This would allows a complete characterization of such nano-structure and reduce the risk of studying artifacts. In addition, the same methodology and methods should be applied on natural and laboratory samples.

I would appreciate if the authors would comment on these points.

Few comments on the manuscript.

Line 66: it would be interesting to quickly summarize on what criteria this organic signature interpretation is based.

Line 88: small side note: this difficulty of recognizing biomineral is exactly the reason why Perry et al 2007 proposed a new use for organomineral. Biomineral is a proof of life; an organomineral needs more investigations. We may never settle this sensitive discussion amount biomineral and organomineral. However, I would argue that Perry's view offer a 'usable' and practical view of the question. Defarge's definition is very elegant, but very theoretical. So far, it remains very difficult to prove the absence of something, especially microbial activity. Therefore, the definition by Defarge has not been widely used. Even in buried sediment exposed to early diagenetic stages, microbial processes can strongly impact the precipitation and dissolution of minerals. Defining clear, usable terms is very important especially for discussion about biotic versus abiotic processes, or biologically-, organically-, inorganically-mediated precipitation, knowing that some organics can be abiotically produced (relevant for the origin of life).

Line 396: (ii) instead of (iii)

Line 398: see general remark. Why freeze-dried samples instead of chemically dried.

Line 421: the use of osmium is very cleaver.

Fig. 1D: This is not really the same scale. If one take a close up of a area from b, one could achieve pretty similar result. Could you have a lower magnification picture showing this orientation?

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