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Comment on bg-2021-153

Carlton Brett (Referee)

Referee comment on "Pyrite-lined shells as indicators of inefficient bioirrigation in the Holocene–Anthropocene stratigraphic record" by Adam Tomašových et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-153-RC3, 2021

Review of manuscript by TomašovyÌ□ch et al.

This paper presents an exceptionally well documented case study of a phenomenon rarely seen in modern environments but common in the deep time fossil record: pyrite formation within shells. The authors present a wealth data on the relative frequency and association of pyrite with other taphonomic indicators of residence time and also provide data on the actual ages of the shells based on amino-acid racemization. The results provide a powerful case for the development and preservation of pyrite in closed spaces of dead organisms and shows that pyrite linings are most frequent in areas of higher sedimentation rates and slow mixing or bioirrigation. The authors even present data that indicate an upward increase in pyrite formation within shells of the later 20th century that parallels evidence for increased eutrophication owing to anthropogenic activity. In fact, the presence of pyrite may be a more sensitive indicator of sluggish rates of bioirrigation than ichnofabrics.

The paper is extremely well written, well organized and thoroughly referenced. And supported by a large data set and thorough statistical analysis. I noted only a few minor errors in the text and references, which are marked on the pdf. I also have the following queries, mainly out of interest in the subject; I refer to relevant line numbers. The authors may wish to comment on them.

Line 75. Here and elsewhere, throughout, the term “microniches” is not quite right; perhaps “microenvironments” is preferable.

183, see also 613.: this paragraph well explains the tight closure of *Varicorbula* valves. But is there a reason that *Varicorbula* shells *remain* closed after death instead of splaying open at the hinge ligaments as do most bivalves? Does the ligament groove serve as a

sort of locking devise? This may explain why they are most frequently pyrite lined. Perhaps a brief discussion of comparative taphonomy of these clams would be useful here.

180s and 480s: Alternatively, do these clams frequently perish within their burrows as opposed to many that seem to rise to the surface during mortality. Under such conditions, burial within sediment may not require obrution, but I do not think this applies to most other articulated and closed bivalves let alone completely articulated multi-element skeletons.

319-320: how is it that pyrite formation does not set in until after 10 years in the sediments? If, as assumed the development of sulfides is associated or at least initiated with decay of organic matter contained within the enclosed spaces of shell cavities. But one would guess that such OM should be largely gone after just a year or so. Is the issue that the initial sulfides are monosulfide gels that only later recrystallize to recognizable pyrite?

349: it seems odd that shells with periostracum preservation should be negatively correlated with pyrite linings as one would suspect both might indicate higher rates of burial and reduced decay.

385: this result: increased borings in the HST vs. TST seems paradoxical as sedimentation rates are normally predicted to increase into the later HST. Any explanation?

Is it also possible that anthropogenic activity is related to increased rates of runoff and therefore, faster burial of organisms?

Lines 443-445. This is a very interesting finding in line with observations of iron sulfide blackened shells in ancient assemblages in which there is a strong positive correlation between other taphonomic indicators of long residence time and darkening: Kolbe, S., Zambito, IV, J.J., Brett, C.E., Wise, J.L., Wilson, R.D., 2011. Brachiopod shell discoloration as an indicator of taphonomic alteration in the deep-time fossil record, *Palaios* 26: 682-692.

480-onward. The result that shells of the same age may or may not show pyritization does not seem necessarily to support the contention that the pyritization occurred gradually during normal burial rather than during burial events. It is certainly possible that pulses of burial could entomb not only live or recently dead individuals, but many others that had died and decayed prior to the event. Pyrite would be localized in those that were buried with soft tissues intact whereas the "dead shells" would show little tendency toward sulfate reduction or pyrite formation.

- this observation (of loss of Fe from sediments under persistent anoxia) is very important, as it may help explain the absence of pyritized fossils in truly black, laminated sediments that we have documented repeatedly (see Brett, C.E., Dick, V.B. and Baird, G.C.,1991. Comparative taphonomy and paleoecology of Middle Devonian dark gray and black shale facies from western New York. In Landing, E. and Brett, C.E., eds., Dynamic Stratigraphy and Depositional Environments of the Hamilton Group in New York Pt. II. State Museum Bulletin 469, p. 5â□□36.)

One implication is that, while eutrophication and increased organic matter may enhance pyrite formation up to a point (because of lower benthic O_2 and reduced bioirrigation); too much OM may shut down the process.

570: forgive my ignorance, but I do not understand your use of **hysteresis** here. Perhaps explain a bit.

577: it is not intuitive that large size would be associated with lowered oxygen levels (resulting from eutrophication); in many cases low oxygen has been attributed to stunting and diminutive organisms. Apparently in *Varicorbula* the stunting affect is offset by increased growth. Presumably this is well documented; a reference might be useful.

613: but why do these bivalves not open shortly after death? If they are not buried rapidly (episodically) then how do they come to remain tightly closed?

Line 635: this also contrasts with nanopyrite infillings that seem to increase together with other signs of degradation with residence time.

Illustrations are fine and require no modification.

References I have gone through and highlighted all that appear to be correctly cited; most are fine but there are a number of minor problems:

The following are **slightly out of alphabetical order:**

Briggs et al. 1991., Boyle, Bjereskov, Degobbis. Meysman, Schieber 2012 (should come before Schieber and Baird)

Wrong date

Brush et al. 2020 (2021 intext)

Kralje et al. 2019 (2020 in text)

Slagter et al. 2021 (2020 intext)

No date (in ref list) Hunda et al. [2006]

In reference list but apparently not referenced in text:

Aller, 1982

Arcon et al. 1999

Clark ad Lutz, 1980

Eliott et al. 2007

Faganeli et al 1985

Palinkas et al. 2007

Powell and Stanton 1985

Cited in text but not in reference list:

Alvizi et al. 2016

Stanton and Powell, 1985

Wignall et al., 2010

Need to designate a and b references in text:

TomašovyÌ□ch 2019 a vs. b

Overall: this is an excellent paper, which needs only minor revision for publication. The paper provides much new data and reaches important conclusions regarding the little known phenomenon of early diagenetic pyritization; it has important implications for sedimentation rates and rates of bioirrigation, for the taphonomy of exceptional fossils, and potentially for conservation paleobiology and evidence for anthropogenic effects. The results will be of interest to geochemists, sedimentologists, taphonomists, paleobiologists and perhaps conservation paleobiologists. I strongly recommend publication with slight correction.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-153/bg-2021-153-RC3-supplement.pdf