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## Comment on bg-2021-153

Lidya Tarhan (Referee)

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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-RC1>, 2021

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In this manuscript, Tomašových et al. assess the preservation, pyritization and age and depth distribution of valves of the hypoxia-tolerant bivalve *Varicorbula gibba* in Adriatic Sea sediments. The authors compare these *V. gibba* taphonomic data to sedimentary and radiometric proxies for mixed layer depth and sedimentation rate, as well as sedimentary biogeochemical data. They conclude that sedimentation rate likely played a strong role in enhancing long-term pyritization of *V. gibba* valves (i.e., by limiting extents of oxidation in the uppermost sediment pile, and by shuttling valves below the mixed layer on relatively rapid time scales). The authors also observe that *V. gibba* valves with pyrite linings appear to be more prevalent in the portions of cores corresponding to the late 20<sup>th</sup> century, correlative with increases in seasonal hypoxic events and episodes of eutrophication. They therefore conclude that these hypoxic episodes, by deleteriously impacting the local infaunal community (particularly bioirrigators), directly resulted in valve-associated reducing microniches and decreased potential for reoxidation of valve pyrite linings, allowing burial of pyritized valves to outpace bioirrigator-mediated reoxidation in sites characterized by high sedimentation rates. With this work, the authors lend new insights into the role played by bioirrigation (and feedbacks between sedimentation and bioirrigation) and eutrophication in pyritization, with important implications for how pyritized fossils in the stratigraphic record can be used to reconstruct not only the taphonomy of body fossil assemblages but also changes in the extent and timescales of bioturbation.

This manuscript represents an impressive body of work with potentially important implications for not only reconstructing environmental-ecological-taphonomy feedbacks on historic time scales but also in the deep-time stratigraphic record. I am therefore supportive of publication, and below I highlight a few relatively minor aspects that I have confidence the authors should be able to readily address in a revised version of the manuscript:

I would have liked to have seen more extensive discussion, up front (e.g., in the Introduction, the Methods or a new section of its own) of the ecology of *Varicorbula gibba*—for instance, whether it is infaunal, semi-infaunal or epifaunal; feeding ecology;

seasonal variation in abundance; and ecological relationships to other local taxa. We are not told until near the end of the manuscript (l. 580-581) that *V. gibba* is among the assemblage of shallowly burrowing detritivores and deposit-feeders that have been previously documented at Adriatic prodelta sites such as Po and Isonzo. However, the ecology of *V. gibba* seems very relevant to the manuscript's consideration of impacts of hypoxia and eutrophication on local benthic communities. For instance, is it an opportunistic taxon? Does its relative abundance in benthic communities actually increase under conditions of hypoxia—or in spite of being hypoxia-tolerant, are impacts of hypoxia on *V. gibba*, on the whole, deleterious (as they are inferred to be for the bioirrigating community)? This in turn could impact some of the authors' assumptions regarding rates of *V. gibba* input to the sediment pile. The authors explore various models for the distribution of *V. gibba* valves at depth (and from that exercise conclude that a model of uniform loss from the mixed layer for pyritized and non-pyritized valves is most parsimonious), but this appears to be premised upon an assumption of invariant input rates. If *V. gibba* abundance (or relative abundance) varies with bottom-water redox state and degree of nutrient loading this may not be a valid assumption, however. In addition to providing further detail on *V. gibba* ecology earlier in the manuscript, the authors should discuss these assumptions and the extent to which they can be constrained or justified.

Similarly, in the manuscript's discussion of rates of "loss" of valves from the mixed layer (e.g., l. 201-204 and elsewhere), I suggest replacing use of the term "loss" (which is interpretive and connotes a null model that other processes—for instance, additive processes that may variably impact pyritized vs. non-pyritized valves, or those of different age 'cohorts'—do not lead to differences in abundance or distribution) and instead phrasing this in terms of relative abundance or distribution—at least prior to the Discussion section of the manuscript.

I would also have liked to have seen additional information on sedimentation rates and mixed layer depths, specifically how these were constrained—given the importance of each of these to the authors' conclusions. For instance, it would be good to include the  $^{210}\text{Pb}$  data, which do not appear to currently be part of any of the figures or tables. The authors state that sedimentation rates, although variable between the different sites and cores, appear to have been largely invariant throughout the deposition of individual cores (e.g., l. 266). It would be good to see that data upon which that assessment is based, as well as further discussion by the authors of whether this is surprising or expected for the prodeltaic sediments of their study sites over the hundred- to thousand-year time scales recorded by these cores.

There is also some ambiguity in the authors' discussion of the role of organic matter in fostering precipitation of pyrite linings on *V. gibba* valves. Pyritization is, as the authors acknowledge, typically limited by the supply of organic matter (as well as requiring a redox interface between iron and sulfate reduction at the localized supply of organic matter). However, something the authors do not directly discuss (though they perhaps allude to this in l. 529-532) but which is, in contrast, discussed by some of the studies they cite (e.g., Raiswell et al., 1993, *Marine Geology*; Farrell et al., 2009, *Geology*; as well as Raiswell et al., 2008, *AJS*) is that the presence of abundant disseminated organic matter in the sedimentary matrix tends to be detrimental to extensive pyritization of macroorganism carcasses. So although the hypoxic conditions fostered by eutrophication may, in the case of their Adriatic sediment samples, have played an important role in the development of a shallow redoxcline and thus pyrite precipitation on *V. gibba*, high rates

of organic matter delivery to the seafloor are unlikely to foster extensive and exceptional fossilization of macroorganism carcasses via, for instance, pyrite templating or replacement in geologic analogues. In other words, early diagenetic precipitation of pyrite framboids on *V. gibba* valves under these conditions does not necessarily equate to exceptional pyritization—particularly given the abundance of sedimentary organic matter noted by the authors. The authors should therefore temper their discussion of how their findings bear upon understanding of pathways of exceptional fossilization via pyritization, and incorporate discussion of these caveats. Similarly, obrution in the typical sense need not involve deep burial, but rather rapid burial (and the associated ‘smothering’ of benthic communities). Particularly if the redoxcline (due to hypoxic conditions) is located in the uppermost centimeters of the sediment pile, an “obrution scenario” and a “hypoxia-mediated reduced bioirrigation scenario” are entirely compatible, and should not be discussed as diametrically opposed alternative models (e.g., as in l. 475-484, l. 622-627). On a more minor note, previous studies of pyritization have suggested that relatively more recalcitrant organics may preferentially undergo pyritization (or more rapid pyritization) (e.g., Briggs et al., 1991, *Geology*; Raiswell et al., 1993, *Marine Geology*); the authors should therefore take care to not oversimplify fossil pyritization as targeting solely the most labile tissues.

l. 196: Although D/L amino acid ratios are of course broadly used, given that this paper may attract a broad audience (including those who do not commonly employ organic geochemical methods), I suggest providing further detail here.

l. 351-352: Please state here how sites were partitioned into “high-” and “low-” sedimentation rates (e.g., what range of values were used for this categorization).

Figure 9: the burial rates calculated for A) and D) (without and with pyrite linings, respectively for Po 3) seem substantially different (by a factor of 2). It would be good to see additional interrogation of the grounds on which it was determined that these are essentially indistinguishable.

### **Technical Corrections:**

l. 97: “...may not be surprising...”

l. 171-172: This sentence contains two separate notations of the mixed layer depth at Brijuni—perhaps a typo?

I. 388: increments

I. 469: reach

For Figure 11 in particular (and, to a lesser extent, some of the other figures), the plots are so closely packed together that it is a little challenging to read the axis labels and attribute these to the appropriate plots. Could the panel components be spaced slightly further apart?

Figure 15: For the A) label, is this supposed to be  $>$  (not  $<$ )?

Sincerely,

Lidya Tarhan