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Interactive comment on “Explosive demographic expansion by dreissenid bivalves as a possible result of astronomical forcing” by M. Harzhauser et al.

Anonymous Referee #1

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This study addresses the role of solar forcing for fluctuations in the abundance of a bivalve species in Lake Pannon, ca 10 myr before present. The authors make a strong case (but see below) that solar cycles had a strong effect and such a direct assessment is potentially valuable and important. Abundance data are recovered from a 6 m drilling core, covering about 8,000 years of Late Miocene time. Previous analyses by the authors of biotic and abiotic proxies recorded in this core suggested that sedimentation and faunal dynamics were influenced by the presence of several distinct solar cycles. This led to a temporal resolution of the sedimentary record down to a decadal scale. Such a high temporal resolution in deep time is fascinating and allows to bridge the gap between ecological and geological time scales that often hampers the mutual

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transferability of paleontological and neontological studies.

That said, I have several substantive concerns about publication of the paper in Biogeosciences:

(1) The idea and the approach are not new. The present authors applied the same methods to the same core (or parts of it) in previous publications (e.g. Kern et al. 2012; Kern et al. 2013) and detected small-scale temporal variations in biotic, geochemical and geophysical data consistent with the periodicity of various solar cycles. The previously investigated taxa included ostracods and molluscs, and it was concluded that fluctuating bottom water oxygenation was the cause of fluctuating abundance patterns. What is new to the present study is the extension of this approach to an abundant bivalve species and the finding that its abundance through time is also synchronous with solar cycles to a certain degree. I feel this warrants publication in a specialist journal though not necessarily in Biogeosciences which aims at publication of substantial new concepts, ideas, methods, or data.

(2) The overall framework in which this study is placed does not convince me. In the abstract and introduction, the authors refer to human induced range expansions of invasive bivalves. However, the waxing and / or waning of geographic range of species is not at all addressed in this study. Rather, data are derived from a single drilling core and therefore are of very local extent and from a geographically static position. Also, in the last paragraph of the conclusions the authors refer to "improvement of existing invasion models, particularly in respect to niche opportunities and invasion dynamics" but again, there is no connection with the actual result of this study. How is a boom-and-bust pattern in time relevant to expansion (and contraction) of a species in space. I doubt it is. What do we learn about "the underlying biological processes" related to range expansion and invasion as referred to in the abstract?

(3) Some of the results are not sufficiently well documented. The abstract states that "our data indicate that the settlement by bivalves in the offshore environment was lim-

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10, C4977–C4979, 2013

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Discussion Paper



ited mainly by bottom water oxygenation". No such data are presented that directly relate to fine-scale fluctuations of bottom water oxygenation. These could be fluctuations in bioturbation intensity (bioturbation seems to vary according to line 10 of section 2.2), pyrite petrology, degree of lamination, geochemical proxies, and others. The shell pavements are claimed to represent autochthonous in-situ census assemblages with no indication of transport or winnowing (section 3, line 4). What is the evidence for this (e.g. degree of fragmentation; right valve / left valve ratio; shell orientation; size distribution of shells etc.)? An infaunal mode of life is mentioned for the Late Miocene to Pliocene dreissenids (section 3, lines 16-17), but later on (line 22) an epifaunal mode of life is inferred for the studied species. On which basis? The presence of a slightly sinupalliate mantle scar is in my opinion a good indicator for an infaunal life habit. Lack of a sinus would be equivocal as to inferred mode of life, but presence of a sinus, albeit shallow, is good evidence for infaunality. What is the evidence to categorise *S. primiformis* as an r-strategist (line 22)?

(4) Some of the methods and the results are not sufficiently well explained. How was the detrending of data carried out? With respect to observed fluctuations and solar cycles it is stated that "The fit is excellent especially in the upper half of the core, whilst in the lower part the filter coincides with the 'signal-bundles' observed in the wavelets. The low-frequency filter at 145–150 cm, in contrast, resolves especially the large scale pattern below sample 1050 but has a poor fit with the uppermost record. The filter spanning the triplet of peaks in the power spectra from 57 to 73 cm has lowest fit with the record below sample 1050 and only a moderately good fit above." Is this evaluation of excellent fit, moderate fit, or poor fit based on visual inspection only or does it have any statistical foundation? Figure 4 needs a bit more explanation how to interpret the wavelet power spectrum.

Interactive comment on Biogeosciences Discuss., 10, 12009, 2013.

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