Review Sinnesael et al. Cyclostratigraphy of the Middle to Upper Ordovician successions of the Armorican Massif (western France) using portable X-ray fluorescence

The manuscript of Sinnesael et al. present new portable XRF data obtained from an Ordovician succession from western Brittany (France). The sequence stratigraphy was previously published. The succession corresponds to the record of an open marine environment, with deposits covering from the shoreface to the lower offshore. This change in environment trigger changes in type of sediment deposited (sand vs. clay) but also change in sediment rates, with higher sediment rates in more proximal deposits.

I understand from the manuscript that the section is protected and no sampling was performed because of this reason. Measurements were thus performed in situ using spectral gamma ray and portable XRF (pXRF). Spectral analyses were performed from the potassium content approximated from the spectral gamma ray and pXRF. Two members were analyzed: the Morgat Mb., consisting of claystone deposited in a lower offshore environment, and the Kerarmor Mb., representing alternations between claystone and sandstone deposited in an upper offshore to shoreface environment.

We largely agree with the two previous paragraphs that describe some of the main aspects of the presented manuscript, although we don't fully agree with the statement in the third sentence, which does not reflect our text as such. In our interpretation changing rates of sedimentation also depend on changing sea level.

From the spectral analyses done on the K series, the authors conclude that the Morgat Mb. Contains an excellent record of the Milankovitch cycles, unlikely to the Kerarmor Mb. The authors attribute the inability of spectral analyses to identify the sedimentary record of the Milankovitch cycles in the Kerarmor Mb. to the highly unstable sedimentation rate, demonstrating that the sequences, the processes and environments at the origin of the deposits have to be identified before applying Fourier analyses, and their derivative methods, on a sedimentary series. To be franc, this conclusion brings little novelty. This is a useful reminder that cyclostratigraphy, before being a statistical challenge, is a geological challenge. If one wants to suggest to have the record of the Milankovitch cycles, they have to prove first that they have climatic and/or eustatic cycles which average period is within the range of the astronomical cycles. From a Fourier analysis, this is possible to find a frequency content that mimics the astronomical cycles, with however no climatic or eustatic significance. Relying only on a Fourier transform done from a geophysical signal is thus not enough, especially if event deposit, such as storm deposits, are included in the sampling and the frequency analysis. This shall be obvious, and one might consider this conclusion as naive. This is however sometimes forgotten in publications.

'Excellent' is maybe a too strong word for our interpretation of a potential Milankovitch signal in the Morgat Mb. Our conclusion is more carefully formulated, acknowledging that in the absence of precise independent age constraints, a Milankovitch origin is hard to demonstrate conclusively. But indeed, the more homogenous muddier facies of the Morgat Mb. did seem to show less challenges in terms of the interpretation of the spectral analysis results compared to the more sandstone-rich Kerarmor Mb..

We agree that cyclostratigraphy is in the first place a geological challenge before a statistical one, and that this is *an sich* not a new insight. Our paper explores how these theoretical insights are applied to a Paleozoic case study with its specific challenges in terms of for example non-stationarity of the signals and a lack of high-resolution independent age control. And indeed, some of the considerations described here by reviewer 1 are very often not considered or overlooked in publications, which is exactly what forms an important motivation for the presentation of this manuscript in its current form – an effort that was much appreciated by reviewer 2. We want to emphasize that the new pXRF work is also a substantial part of the manuscript and seems to have been accepted without major reservations by both reviewers 1 & 2.

As suggested by reviewer 2, a short additional paragraph discussing some examples and comparisons should strengthen this point. We suggest adding the following paragraph at the end of the current discussion (L404):

"Dealing with cyclostratigraphic uncertainties in a Paleozoic integrated stratigraphic framework is not an easy task (e.g. Sinnesael et al., 2019; Ghobadi Pour et al., 2020). Studies that, similarly, target less conventional facies in younger stratigraphical intervals might in general have more robust independent age constraints (e.g. Noorbergen et al., 2018) or more reliable astronomical parameters like insolation curves available (e.g. Vaucher et al., 2021), while this much less the case for the Paleozoic (e.g. Laskar, 2020) - often resulting in looser temporal constraints on astronomical interpretations. For example, Sinnesael et al. (2021) reinterpreted the expression of astronomically forced Upper Ordovician sedimentary cycles on Anticosti Island (Long, 2007; Elrick et al., 2013) resulting in a different interpretation of the duration of the cycles by an order of magnitude. The use of correlations and ages that only are loosely constrained, in order to imply astronomical origins of sedimentary sequences, is not uncommon when interpreting lower Paleozoic records (e.g. Sutcliffe et al., 2000; Gambacorta et al., 2018). Other common practice is the application of spectral techniques on stratigraphic records that might not be ideal for such type of analysis because of, e.g., their variable lithologies and associated variable expression of the proxies used (e.g. Zhong et al., 2018). These challenges accentuate the need for further developed cyclostratigraphic methodologies that are not simply a copy of what has been shown to work well for younger stratigraphic intervals; instead we need techniques that are adapted to the reality of the more limited availability of accurate independent age constraints and the lack of well-preserved open marine pelagic sections that characterize the Paleozoic sedimentary record."

Saying that, I find the spectral analyses of the Kerarmor Mb. superficial, in a sense that, knowing that the time of deposit of sandstone beds can be regarded as instantaneous compared to the time needed to deposit the claystone beds, the authors should try spectral analysis of the K content on the section "sandstone-free", i.e. removing data and thickness of the sandstone beds from the series to only keep the decantation deposits, which sedimentation rate and variability of K content was probably much more stable. In complement, the authors could use the study of Dabard et al. (2015) to convert the sequences they attribute to the precession cycles to they expected average period (so they make an orbital tuning from the sequencing of Dabard et al., 2015) to remove the variations in the sedimentation rate obviously depending on the sandstone beds only.

Indeed, the sedimentation rate of sandstone beds is closer to instantaneous compared to the mudstones. The removal of 'event beds' (e.g. turbidites, volcanic ashes, small slumps, …) is not ideal, but a not uncommon practice in cyclostratigraphy. In this case, however, removing the sandstones from the sandstone-dominated Kerarmor Mb. would mean removing most of the stratigraphy… Reviewer 2 actually states *"—, avoiding some fiddling that most often are nonsense (e.g. removing sandstones from the succession, only keeping the clay… and forgetting that each base of a sandstone bed is an erosional surface remobilizing cms to tens of cms of shales).*" We briefly reiterate our conceptual understanding of the depository mechanisms for these storm-dominated deposits:

1) HCS arenites and also intercalated argillites are first transported and then deposited by storms. The type of transport, and consequently the time of the deposition process of each layer, is a function of grain size.

2) The stratigraphy of storm-dominated terrigenous deposits, and thus the representation of the time, consists largely of sedimentary voids. Layers are laid down when sediment is available and not all storms produce layers. Therefore, layers are produced by the successful storm that produces deposition.

3) Clay deposits, which are deposited in the lower offshore of the same terrigenous platform described above, are also produced by storms with the same process of intermittent deposition that is a function of successful storms. These deposits also possess the same time gaps not represented by sediment , as described for the previous.

The variation of K is function of grain size which controls the mineralogy. Removing the sand removes the main indicator of bathymetry variation.

The general aim of the manuscript was to show difficulties of applying and interpreting basic spectral analyses when certain underlying conditions of these statistical techniques are not met (e.g. non-stationarity of the sin waves, for example caused by large changes in sedimentation rates). More advanced forms of spectral analysis that build upon the basic techniques might therefore also not be valid.

Although it is at first sight an attractive suggestion to use the Dabard et al. (2015) astronomical interpretations for a tuning; we fear there is the danger for circular reasoning to then use the tuned series to test for a potential Milankovitch origin of the original signal.

In general in this manuscript, I find the graphic representation of the spectra unclear. The description of the spectra is also extremely superficial.

We regret that the power spectra are perceived to be unclear, and are open for more specific suggestions on what is not clear and what can be improved. The EHA figures used in the manuscript look very similar to the evolutive Fourier transform Figure 2 presented by the reviewer.

We add the following sentences to additionally describe and clarify the spectra in more detail in words:

L295: "Here, we use evolutive harmonic analysis (EHA) to evaluate the spectrum of the signal as it evolves throughout the stratigraphy. The frequencies that explain more variation within the moving window will have higher spectral power and are shown by redder colors (Fig. 5B). This approach has the advantage over a single periodogram or multi-taper spectrum that it can also be used to evaluate how stratigraphically consistently present a certain period might be or not. Spectral analyses indicate two main periodicities: a longer one of ~1.5-2.0 m (0.5-1.0 cycles/m) and a shorter one around ~0.5 m (1.8-2.2 cycles/m) cycle thickness (as indicated by the dotted lines on Fig. 5A). The rest of the EHA shows very little elevated spectral power for other frequencies."

L303: "Compared to the NGR EHA, the pXRF EHA suggests additional frequencies with lower spectral power (Fig. 5). These seem, however, less stratigraphically continuous."

I redid the spectral analyses. Below are the spectrum and the power spectrogram of the K content in the Morgat Mb. It appears that the frequency the authors chose (1.5 m, 0.5 m and 0.3 m) are not the highest powers or confidence levels regarding a red noise. Can the authors explain their choice? Is it based on their stratigraphic continuity? What is the origin of the other frequencies?

We appreciate the effort of the reviewer to redo some of the spectral analyses. We do, however, not fully agree that there is a large difference between both analyses. The most significant peak from the reviewer's Fig. 1 is 0.25 m, which is in perfect correspondence with our statement on L303 '*and new dominant cyclicity around 0.25 m (Fig. 5F).*' – and so not 0.3 m. The 1.6 m peak is very close to our quoted ±1.5 m peak. It is correct that the ~0.5 m peak is not very present in the pXRF K spectrum, just like we mention in L302-303: *', a less pronounced 0.5 m cycle*'. We mention the 0.5 m cycle here, because it appeared earlier on in the spectrum of the lower resolution NGR data. A lot of the 'other frequencies', are actually close to 0.5 m. Indeed, an important argument for the choice of the mentioned frequencies is their (partial) stratigraphic continuity as evaluated by an evolutionary type of analysis, where only looking at the total spectrum of a signal might have the risk of highlighting spectral peaks with high significance power, but that actually do not appear in large parts of the section. The issue of statistical testing of spectral peaks in cyclostratigraphy is also subject of debate, as for example debated in the Vaughan et al. (2011) reference suggested by the reviewer. In summary, both ways of spectral analyses seem thus consistent with each other – we also provide all the data and script we used in the supplementary materials.

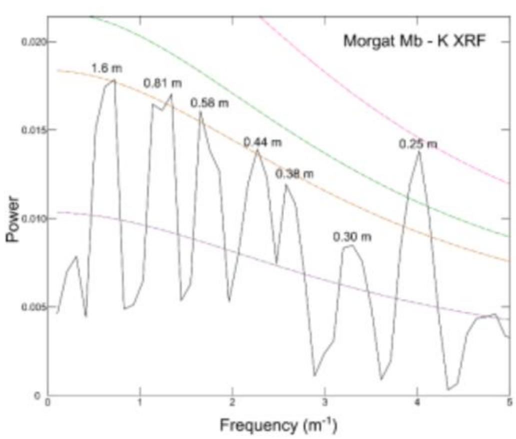

Figure 1: 2π-MTM spectrum of the pXRF K content from the Morgat Mb. Purple line is median smoothing; brown line 90 % confidence level; green line 95 % CL and pink line 99 % CL.

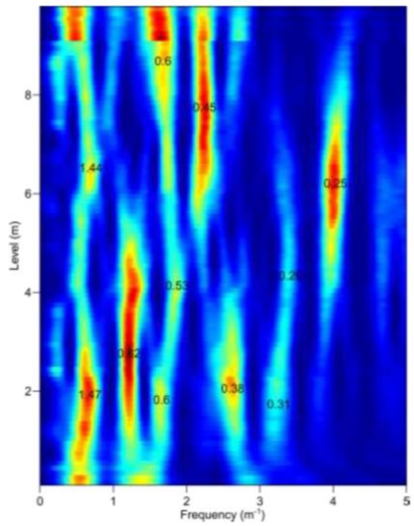

Figure 2: Evolutive Fourier transform of the pXRF K content from the Morgat Mb. The red lines are spectral peaks. The blue color indicates the spectral background. The Fourier transform were done on 5-m intervals.

In the chapter of the spectral analysis of the Kerarmor Mb., the author apparently experienced difficulties in calculating the long-term trend, which surprised me. In short sections, it may indeed not be trivial, however in this case, applying a locally weighted scatterplot smoothing curve with a coefficient of 0.5 allows the lowest frequencies to be decreased to low values while preserving the spectral peaks at higher frequencies. Notice that with this procedure, no spurious peak is produced at low frequencies, following the recommendation from Vaughan et al. (2011).

It is correct that for the short sections of the Kerarmor Mb. and Morgat Mb. we only applied a linear detrending. The topic of the importance of different levels of detrending is actually discussed in detail for the analysis of the whole Postolonnec Fm. (e.g. Fig. 7 and L361-371) and further discussions. One of the goals of this manuscript is to highlight the challenges and pitfalls of using classical spectral analysis tools on records that do not respect the underlying assumptions needed to apply these techniques in the first place.

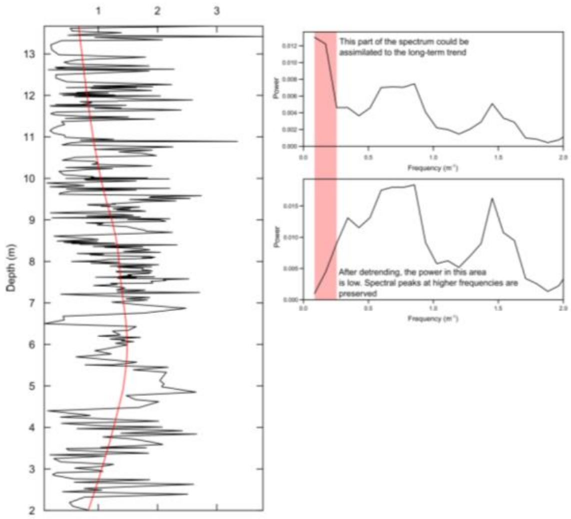

Figure 3: Detrending of the pXRF K content of the Kerarmor Mb. Top right figure is the spectrum before detrending (only the average of the series is set to 0 for clarity). Bottom right is the spectrum after detrending applying a LOWESS with a coefficient of 0.5.

In summary, I find the description and the design of the experiment extremely superficial, and additional work is needed in my opinion. So, at the moment I am not convinced by the design of the study and I think extra work is needed to make this manuscript suitable for a publication at gchron.

We regret this opinion. The first main concern raised by reviewer 1 is that there is no real contribution in discussing challenges and pitfalls of using classical spectral analysis tools on records that do not respect the underlying assumptions – while this is a real problem in the current literature, as appreciated by reviewer 2. To bring this point forward more clearly, we add a short paragraph discussing some relevant examples and the end of the current discussion. We believe that our detailed reply regarding suggested differences in results between our spectral analysis and some presented by reviewer 2 demonstrates that the results are in essence comparable. We also add some additional description of the spectra in the main text to make them clearer.

Below are typographical corrections I found:

Line 138: SiO2: the “2” must be in index Implemented

Line 333: “We now can”: this is actually “We can now” Implemented

Line 393: “more higher”: remove “more” The meaning should have been ‘additional’ instead of ‘more’. This was indeed unclear and has now been changed.

Line 403: “one can”: repeated twice, remove one of the two Implemented