The manuscript *gchron2021-45* by M. Sinnesael is a (very) well written, well documented and well-presented case study describing efforts in order to decipher astrocycles from a Lower Paleozoic succession, for which a strong sedimentological background is available from a series a previously published papers. Corresponding to an epicontinental archive (no oceanic record for the considered time interval!), relatively shallow marine deposits are here considered with different, yet combined signatures (depositional facies, pXRF, gamma-ray); the results are of course somewhat disappointing, as the case is as expected challenging. It is good —and rare— to read a paper where the authors remain scientifically 'honest', not hiding but highlighting the many problems —even if, in theory, they are well known by the community—, 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).

According to me, the paper could be accepted with only a minor revision required. Below some typos and suggestions for further improving the manuscript and its impact.

We thank reviewer 2 for appreciating one of the major messages of the paper, and acknowledging that discussing the presented issues – even though they should be known problems – is very valuable for the community.

Below we reply to each minor suggestion individually:

Line 42: pre-Cretaceous (don't forget the Precambrian record) Implemented

Line 68: Geol. setting and BIOstratigraphy Implemented

Line 70: a comma is needed after Fig. 1A Implemented

Line 71: on? the cliff Change 'on' into 'at'.

Line 86: a few words about the total related time duration according the chronostratigraphic time scale would here have been welcome (in addition to lines 255-260), to have in mind the temporal significance of the studied interval.

We added the following sentence to better situate the general reader:

"Overall, the Postolonnec Fm. spans roughly fourteen million years starting close to the start of the Darriwilian Stage (~467 Ma) and ending close to the end of Sandbian Stage (~453 Ma)."

Line 185: it means that some of the 'stratigraphic surfaces' with nodules are hiatial surfaces with zero 'averaged' accumulation, indicating also high variations even in shales successions. Need a short discussion?

We agree that there is *stricto sensu* no such thing as 'continuous sedimentation', with also in shale successions possible variations in sedimentation, including no deposition, therefore we would suggest adding: ", indicating that even in the more mudstone-dominated intervals changes in

sedimentation rates can be pronounced." Also see our reply to Reviewer 1 on suggestion to remove sandstone intervals.

Line 193… that can fossilize evoluting sedim. facies (?) Changed to 'preserve'.

Line 198 also mention the usual absence of event beds in the more distal successions

Added: "Usually, more distal successions feature also much less event beds."

Line 231: I would have expected a few word about the mineralogy of the placer deposits, either on the basis of thin sections, or XRF analyses: relationships discussed here need to refer to relative contents in zircon, rutile, or monazite (or others)

We now 1) refer to previous work focusing on the mineralogy of these placers "e.g. zircons, monazite and titaniferous minerals as described by Pistis et al., 2016; 2018" and 2) added following sentence (L222): "Our 'single spot per bed' measurements do not allow for specific mineralogical identifications, but an approach that combines multiple pXRF measurements for the same sample for coarse-grained igneous rocks has shown promising results to extract mineralogical information (Triantafyllou et al., 2021)."

Line 250: … is, at this stage, only suggestive…

We believe that the current formulations are already careful and specific enough in their current form.

I would have like to see clear location of placer deposits (e.g., arrows or stars positioned adjacent to the logs)

Excellent suggestion, we have now added these to Fig. 4.

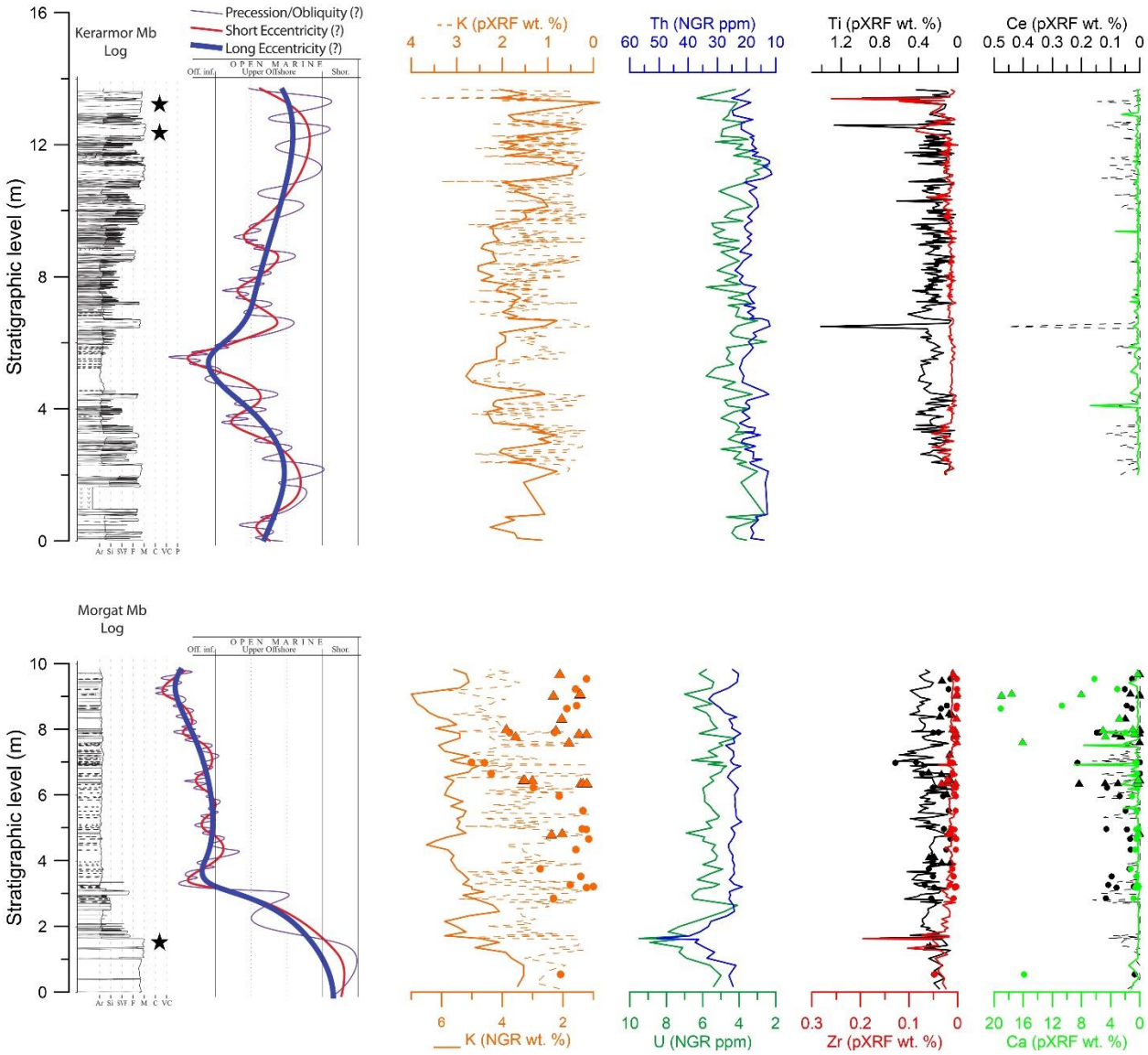

Figure 4: 'Previous caption' + The black stars indicate the stratigraphic occurrence of paleoplacers.

Lines 284-287: if the first element (=i) clearly relates to methodological considerations, the second (=ii) is more a cautionary note or at least a welcome reminder.

We partially reformulated towards "we consider two additional elements to the cyclostratigraphic analysis of the Postolonnec Fm.:".

Line 299: … to have have even more **(e.g.,** Martinez et al., 2016) Implemented

Line 318-319: Even though… to be rephrased?

Indeed, unclear formulation, rephrased now "It is hard to further demonstrate an astronomical origin of these variations in the absence of more precise stratigraphic constraints."

Line 320-320: this short comment would have merited some more explanations and/or hypotheses. It may suggest that even in offshore shale-dominated successions, remobilization may occur, removing/displacing nodules (then behaving as clats); or that condensation may not be systematically expressed by nodules.

Assuming reviewer 2 refers to Lines 321-322; We tend to think more of this as the latter suggestion. Nodules are interpreted to be an expression of condensation, but there might be various boundary conditions at play, so that there is indeed not always a one-to-one relationship. The conditions of early diagenesis vary with the sedimentation rate, which moves the sediment-water interface more or less rapidly, and all this strongly conditions the stratigraphic result of the nodule concretion. References Loi and Dabard (2002) and Dabard and Loi (2012) studied these aspects in more detail. We suggest adding the following sentence: "That nodules occur less systematically might be related to the interplay of various boundary conditions (as discussed in Loi and Dabard (2002) and Dabard and Loi (2012)) that can lead to a varied sedimentological expression of a condensation interval."

Line 335: wouldn't be better to write something as (?): "the sequences of similar dominant frequencies are…"

We prefer to keep the current formulation as L334-336 is more descriptive, while indeed in our next sentence (L336-337) we mention that these might be related to the same sequences, but here there is already a more interpretative component.

Lines 355-356: would it need a verb?

We added 'they might be'.

Line 364: The expected changes in sedimentation rates are not counted, in this context, in a few percent, but more likely spread over 2 or 3 orders of magnitude (give references?): this should be better underlined here.

Good comment. This is also extremely hard to quantify, especially here where age constraints are virtually absent. Therefore, we would suggest adding 'rates (up to an order of magnitude)' L364.

Lines 380-383: a little too much optimism? Such Zr grains are rare, with a time interval of several million years between crystallization and deposition...

We suggested to add the following complementary discussion: "A conceptually related approach concerns the dating of prismatic zircons in a Darriwilian limestone bed in Sweden (Lindskog et al., 2017; Liao et al., 2020). For both the detrital zircon and non-bentonite associated prismatic zircon dating approaches it is crucial to keep in mind that the numerical age from the dating comes with an additional (larger) uncertainty on its depositionary age. Even when such uncertainties would be in the order of millions of years they can still be valuable in such cases where there are tens of millions of years worth of little chronometrically constrained stratigraphy."

One should also not undervalue a 'maximal depositional age'. Besides its dating uncertainty, the specific dated horizon can really not be older. When looking at zircon ages from, for example, a bentonite, one should indeed also consider aspects such as lead loss (often well dealt with nowadays) or if the zircon crystallization ages do correspond with the actual eruption age. When using biostratigraphy one also has to consider that first or last occurrences of a species/assemblage may not be well identified, may not be true FADS or LADS, may not really be globally synchronous etc…

Lines 395-400: more frequent occurrences of event beds in proximal, sand-dominated interval, which are not or poorly tied to astrochronological controls, might here be emphasized

Thank you for this insight. We suggest adding the following sentence to L393: "In general, there is also a higher occurrence of event beds in more proximal and sandstone-dominated intervals, which are most probably not astronomically influenced."

The conclusion is relatively 'flat'. The utility and superiority of pXRF appeared clearly demonstrated. It would have been also welcome a statement about potential durations of the Dabard et al.'s (2015) cycles: are they only confirmed? Better characterized? With new proposed durations/controls? Is a revision necessary?

We suggest adding the following more explicit sentences to the conclusion (in agreement with the ending of the abstract): "In the absence of precise independent age constraints, it is not possible to fully confirm the astrochronological framework suggested in Dabard et al. (2015). A potential astronomical signal is suggested in the more homogenous mudstone facies, while reliable cycle identification in the more proximal sandstone-dominated intervals proofs to be challenging."

Finally, a short paragraph including a comparison with conclusions of other published Lower Paleozoic case studies (e.g., Long, 2007, Can. J. Earth Sci. **44**: 413–431 or Elrick et al., 2014: *Geology* 2013;41;775-778 among other) or, alternatively, with more recent successions displaying records featured by similar lithologies (e.g., Vaucher et al. doi.org/10.1038/s41598-021-96372-x) would have strengthen the paper.

We will follow this suggestion, also as it relates to some of the comments raised by reviewer 1. 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."

We also add "which is sometimes also applied in similar studies (e.g. Loi et al., 2010)." to L401.