

# ***Interactive comment on “Maastrichtian carbon isotope stratigraphy and cyclostratigraphy of the Newfoundland Margin (Site U1403, IODP Leg 342)” by Oliver Friedrich et al.***

## **Anonymous Referee #3**

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## REVIEW

Review of “Maastrichtian carbon isotope stratigraphy and cyclostratigraphy of the Newfoundland margin (Site U1403, IODP Leg 342)” by Friedrich et al. for Climates of the Past. The ms describes the bulk carbonate carbon isotope stratigraphy of IODP Site U1403 for the late Maastrichtian using cyclostratigraphy based on Fe/Ca and magnetic susceptibility to establish an astronomically calibrated age model. The record is subsequently compared with that of the Zumaia section in northern Spain and claim is made that the new record reflects global climate change more reliably. In that case this would be an important contribution as the latest Cretaceous is a fascinating interval with both short warming and cooling periods, and potentially leading to a delicate state

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of the climate system that is highly sensitive to the consequences of the impact at the K/Pg boundary. In that respect the ms is suitable for publication in *Climates of the Past*. However, the ms is in its current state not acceptable for publication. Its needs to undergo at least major revision and a second review.

Main issues:

1) Lack of discussion on carbon isotope stratigraphy. The ms focuses mainly on the astronomical age model for the Site U1403 records, but does not go into the details of the consequences of the age model for the interpretation of the resultant carbon isotope stratigraphy and astrochronology rather than stating that the record provides much more a global signal than the regional signal that is preserved in the Zumaia section to which it is compared. As such it is more suitable to serve as a standard for future carbon isotopic correlations. An astrochronology for this interval by itself is not interesting, as it has already been astronomically calibrated on the basis of both deep-sea cores and land-based marine sections such as Zumaia. Hence, the astrochronology should rather be considered as a tool to discuss and interpret the astronomically dated carbon isotope stratigraphy in detail. However, rather surprisingly, the ms does not provide a detailed discussion and interpretation of the record, while at the same time differences with the regional Zumaia record are very obvious, and should be discussed in detail. Also the potential link to the brief warming and cooling episodes accompanied by perturbations in the global carbon cycle should be discussed if the claim is made that this record represents a global signal (see also next point). In my opinion, the ms is now to meagre to be published in *Climates of the Past*.

2) Inconsistency between introduction and discussion. This point is partly linked to the previous one. The introduction goes into the details of the carbon isotope events in the Maastrichtian stating that these events have been extensively studied but that causal mechanisms are poorly understood due to poor age constraints. However, their record only seems to cover part of the MME of the events mentioned in the introduction, which as such is thus inconsistent with the remainder of the ms. It is further not made clear

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at all why this particular excursion should be the MME (see also next point)?

3) Comparison with other records. The astronomically dated carbon isotope record is only compared with the independently bulk carbon isotope record of the Zumaia section. However, no attempt is made to correlate the record to other carbon isotope records that are available, in which for instance the MME has been identified (see Thibault et al., 2012; Voigt et al., 2015 as mentioned in the introduction). It would be very logical to make and discuss these correlations, although astronomically calibrated age models may not have been established for all these records.

Other issues.

4) Astronomical calibration. The astronomical age model is based on the calibration of the inferred 405-kyr cycle in the Fe/Ca ratio and magnetic susceptibility to the 405-kyr eccentricity cycle in the eccentricity time series starting from the K/Pg boundary. However, also the Ba record is shown which seems to contain a distinct cyclicity as well, but why has this record not been included in the cyclostratigraphic analysis and tuning as an extra check on the correctness of the cyclostratigraphic interpretation

5) Ratio. The authors use the ratio of 1:5:20 of spectral peaks as an argument for their cyclostratigraphic interpretation. However, by doing so, they are selective and f.i. a rather strong 200-kyr cycle is observed in many of the spectra, but no attention is paid to this peak and no explanation is provided.

6) Why are there so many co-authors for such a short paper?

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