

# ***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 #2**

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My first impression on this manuscript which made me exciting is “carbon isotope stratigraphy” of the title. To me, a researcher on paleoceanography focusing on Neogene, relies on foraminiferal oxygen isotope too much. Not like foraminiferal oxygen isotope in the Neogene period of which the major controlling factor is global ice volume, carbon isotope is hard to explain due to multi-factor influences. It is not easy to distinguish various influences on carbon isotope, and therefore not easy to make global comparison based on carbon isotopes. However, the core of “stratigraphy” should be global comparison. Putting astronomical tuning or the age model aside, discussion on carbon isotope stratigraphy is less than one sixth the length of whole manuscript. The comparison of the bulk  $d_{13}C$  record between Site U1403 and the Zumaia section in

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northern Spain displays not only good correlation on long-term changes in 66-67.5 Ma but also anti-correlation on 405 Kyr cycles in the period older than 67.5 Ma. The great difference in d13C which the authors ascribed to discrepancies of global and regional carbon cycle demonstrates again that the bulk carbon isotope is not a reliable proxy to make a common timescale for the marine or terrestrial sedimentary sequences for global comparison. Thus, the “carbon isotope Stratigraphy” as a tool for global comparison seems to be overestimated and should be reconsidered in the revision particularly its role in the title. Obviously, carbon isotope stratigraphy is not the core of this manuscript. The in depth discussion on it is weak.

Cyclostratigraphy which the authors depended on to construct the timescale of the Maastrichtian interval of Site U1430 is obviously the core of this manuscript. The tuning material is the constant elemental ration (Fe/Ca on natural Logarithm scale) obtained by high resolution XRF core scanning and the Magnetic Susceptibility. The important steps of tuning are the initial identification of the orbital cycles in the elemental and MS records and the subsequent comparison with the 405-kyr components of the eccentricity. The authors did a good job on mathematics for orbital tuning on which I have no doubts, but there are some questions left to be clarified. I believe that the manuscript will become easier for the readers to understand after carefully responds to these questions.

A precondition for the construction of a timescale by orbital tuning is the continuity of the sedimentary sequence. Detailed introduction of this content seems to be missing in the text except for the reminding that the reviewers and readers should refer to other references. Part 2, the geological setting, is too simple and short to be treated as an independent section. Actually, they can be integrated into Part 3, the materials and methods. But I believe that most readers hope to see a detailed introduction on the lithology in this manuscript.

The authors calculated the cycles of the variability in elemental and MS records in depth domain and corresponded the dominant cycles ranging from big to small to 405



kyr, 100 kyr, 41 kyr and 21 kyr orbital cycles. The fundamental of making such a comparison is that the ratio of the cycles in depth is similar to the ratio of the dominant cycles of eccentricity, obliquity and precession in age. However, similarity of the ratios of dominant cycle between in depth and in age is a necessary not a sufficient condition of orbital tuning. The initial age model derived from biostratigraphy and magnetostratigraphy and absolute dating is the sufficient condition of the orbital tuning. The authors didn't mention them in their manuscript. They need to prove that why the dominant cycles of the proxy records in depth domain correspond to orbital cycles based on the initial age model.

The 405-kyr long eccentricity cycle is the basic tuning component which determines that the accuracy of their timescale is less than 405 kyr. Why don't they increase the time resolution of the astronomically tuned timescale to obliquity and precession since they have so high resolution records of elemental ratio and MS?

What is the age of the K/Pg boundary in their timescale?

The authors mentioned that "Carbonates of the studied nannofossil oozes consist mainly of coccoliths and benthic and planktic foraminifera (Expedition 342 Scientists, 2012)", and concluded that "bulk oxygen isotope values therefore predominantly represent a surface-water signal". Since there are benthic foraminifera in the carbonates of the sediments which is obviously a signal of bottom water, why the bulk oxygen isotope mainly represents a surface-water signal"?

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