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Interactive comment on "Orbital forcing of terrestrial hydrology, weathering and carbon sequestration during the Palaeocene-Eocene Thermal Maximum" *by* Tom Dunkley Jones et al.

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Review comment on "Orbital forcing of terrestrial hydrology, weathering and carbon sequestration during the Palaeocene-Eocene Thermal Maximum" by Tom Dunkley Jones et al.. By: Victoriano Pujalte, Emeritus Professor, Basque Country University. Bilbao, Spain, December 2017

The main objective of the paper by Dunkley Jones et al. seems to be the reconstruction of the mechanisms of the climatic and isotope recovery at the end of the transient warming event known as the Paleocene Thermal Maximum (PETM). To that end they have studied a segment that comprises the PETM interval at the classic Zumaia section

(deep marine Basque Basin, western Pyrenees, Spain). Their reconstruction is based on a new cyclostratigraphic age model derived from the analysis of bulk sediment elemental composition of 248 small samples collected at ~3 to 5 cm resolution, from ~4.3 m below to ~8.2 m above the assumed base of the PETM. The authors claim that the results reveal a prominent cyclicity in SiO2 and Si/Fe ratio across the studied segment that corresponds to precession-paced (or half-precession) lithological cycles. Additionally, 129 small samples were analyzed for the stable isotope composition (δ13Ccarb and δ18Ocarb) of bulk carbonates.

I am not an expert in power spectra and similar statistical analysis and, consequently, I will center my comment on field observations from different sections of the Basque Basin, with which I am well acquainted. First, I summarize the overall features of the upper Maastrichtian−lower Eocene succession of this basin and discuss the cyclostratigraphy proposed by Dunkley Jones et al., illustrating my discussion with three supplementary figures. Finally, after some remarks on other aspects of the paper, I offer my evaluation of the paper.

Overview

Upper Maastrichtian−lower Eocene successions of the Basque Basin are basically composed of hemipelagic marl/limestone alternations and allochthonous deposits, the latter mainly turbidites. The hemipelagic deposits are ubiquitous, while the turbidites are numerous in some sections and few or absent in others. For instance, turbidites occur at the upper Maastrichtian and the Danian of Zumaia, but not in coeval successions of Sopelana and Hendaia. In these three sections Dinarès-Turell et al (2013) were able to correlate bed to bed the upper Maastrichtian hemipelagites, and to demonstrate that their accumulation was controlled by precession cyclicity. Also, Dinarès-Turell et al (2014) correlated bed-to-bed the Danian hemipelagic limestones of Zumaia and Sopelana and successfully compared them with deep-sea records of Legs 198 (Shatsky Rise, North Pacific) and 208 (Walvis Ridge, South Atlantic). The "prominent limestone bed" quoted by Dunkley Jones et al (page 3, line 34) provides another example. This “bed” is a widespread marker horizon of the Basque Basin, usually named the green limestone (e.g., Baceta, 1996; Schmitz et al., 2001; Pujalte et al., 2014). This green limestone contains one single turbidite at Zumaia, but 9 turbidites at Ermua. Yet, when the turbidites are “subtracted” the thickness of the green limestone becomes identical in both sections (see figures 4b and 6 in Schmitz et al. 2001). These, and other examples, prove that, in contrast with the hemipelagites, the resedimented deposits of the Basque Basin are randomly distributed.

Pre-PETM cyclicity at Zumaia.

Supplementary Fig. 1A illustrating this comment shows a field view of the pre-PETM interval studied by Dunkley Jones et al. at the Zumaia section. Supplementary Fig. 1B is a partial and simplified version of fig. 2 of Dunkley Jones et al., which depicts the same segment of the section shown in Fig. 1A.

Seven distinct marl/limestone cycles are recognizable in Fig. 1A just below the green limestone, numbered from -1 to -7 (in yellow boxes). Six “pre-PETM precession cycles” are numbered in black in Fig. 1B, from -9 to-14. Cycles -2 to -7 in Fig. 1A and cycles -9 to -14 in Fig. 1B occur approximately in the same part of the section, a correlation that reinforces their validity. Further, cycle -1 in Fig. 1A seems to have an expression on the SiO2 plot of Fig. 1B (cycle -8?).

However, the cycle numbering of Dunkley Jones et al. conveys the idea that the $\sim$4 m thick segment of the section below the PETM spans 14 precession cycles. If so, what is the record of cycles -1 to -7? Are there some missing cycles? A hiatus? (their filtered plot only displays 10 cycles). Pujalte et al. (2014) concluded that the green limestone is condensed and spans three precession cycles, are some of the "missing cycles" recorded in the green limestone? These uncertainties should be clarified by Dunkley Jones et al.

PETM cyclicity at Zumaia

Dunkley Jones et al. claim in page 3, lines 29-30 of their manuscript that, at Zumaia, "there is, as yet, no robust cyclostratigraphic framework established for the distinct fine-grained siliciclastic unit (FSU), which spans the majority of the PETM CIE". They are right. I have, however, strong reservations about their cyclostratigraphic model, and indeed about the possibility that such a model will ever be established at the FSU, at Zumaia or elsewhere in the Basque Basin.

Part of my scepticism is derived from the abovementioned fact that allochthonous deposits, which account for the bulk of the FSU, are randomly distributed in the Basque Basin. Thus, the FSU is about 4 m thick at Zumaia, but only about 3 m thick at Trabakua pass (Pujalte et al., 1995). Further, the FSU is 20 m thick at the Ermua section and contains numerous thin-bedded calciclastic turbidites (Schmitz et al., 2001), while at the Aixola section, situated just 2 km to the south of Ermua, it is about 10 m thick and contains just a few turbidites (Pujalte et al., 1998). In the face of such variability, what is the guarantee that the FSU at Zumaia has a complete record of the PETM? In addition to that, the regular cyclicity of Si/Fe and filtered cycles in fig. 2 of Dunkley Jones et al. suggests a steady, if cyclical, rate of clastic input to the Basque Basin during the PETM. Observations do not support that possibility. As described by Pujalte et al. (2015), and shown schematically in the inset of the supplementary Fig. 2 to this comment, coarse- and fine-grained PETM siliciclastic units were respectively deposited within and outside a deep-sea channel that flowed along the bottom of the Basque Basin. Successions almost exclusively composed of coarse-grained deposited, typified by the Orio section, were accumulated at the axis of the channel. They are made up of an irregular stack of sandstones and pebbly sandstones, but establishing whether their accumulation was cyclic is prevented by numerous cross-cutting erosional surfaces (fig. 11 and supplementary fig. S2 of Pujalte et al., 2015).

The Barinatxe section, a coastal cliff section situated to the North of Bilbao (Bernaola et al., 2006) provides some clues to resolve the issue. This PETM section, which includes both coarse- and fine grained deposits, is incomplete, having being accumulated, on the shoulder of the deep-sea channel, on top of an important truncation surface carved on upper Maastrichtian marls (inset in Supplementary Fig. 2). The PETM deposits crop out on the wave-cut platform and fine details of its facies arrangement are clearly visible. Coarse-grained deposits are multiepisodic, occasionally having internal erosional surfaces, but do not exhibit any clear cyclicity (Suplementary Fig. 2A). More to the point, the fine-grained deposits consist of numerous stacked 1-2 cm thick fining-upward packages of sand- or silt-sized quartz grain grading up to clays. The base of the packages is erosional. These packages indicate that the accumulation of the FSU at Barinatxe occurred through numerous small-scale depositional events of variable magnitude (Supplementary Fig. 2B, C). No larger scale cyclic arrangement is visible. In several cases, the thickness of the quartz-rich parts varies laterally within short distances (e.g., those arrowed in Supplementary Fig. 2B). Base on that, it seems logical to assume that the accumulation of the FSU at Zumaia took place through multiple depositional events, but of smaller scale than at Barinatxe, that is, less than 1 cm thick. Therefore, sampling at $\sim$3 to 5 cm resolution, as was carried out by Dunkley Jones et al, is insufficient to unravel an inherent cyclicity of the Zumaia FSU, if any. The Barinatxe section provides yet another piece of relevant information, namely that the original color of the FSU clays is dark grey (Supplementary Fig. 2B, C). As demonstrated in Supplementary Fig. 3, the dark grey is the original colour of the FSU, while reddish colour of the FSU at Zumaia is a secondary feature acquired by weathering. It is thus possible that in the weathered Zumaia section the iron is remobilized and, if so, the seemingly vertical cyclical variation in Si/Fe ration within the FSU might just be an artifact.

Final remarks and evaluation

It is clear from the above discussion that I mistrust the cyclicity model of the FSU proposed by Dunkley Jones et al. However, some of their conclusions may still be valid. It is on that base that I suggest a major revision of the paper.

I agree, for instance, that detrital sedimentation rates greatly increased during the

PETM due to changes in the magnitude and frequency of extreme rainfall and runoff events, although these conclusions are hardly new (e.g., Schmith et al., 1997, 2001; Schmitz and Pujalte, 2007; Pujalte et al. 2015).

Also, their carbon isotopic profile, although more detailed than that presented by Schmitz et al (1997), provides essentially similar information. In effect, both concur that the FSU was deposited during the PETM and that the return to background isotopic values (i.e., the δ13C inflection point F in fig. 2 of Dunkley Jones et al.) began approximately at about 4 m above the top of the green limestone. That may serve as a base of a less ambitious, but still workable, age model.

Since the main objective of the paper by Dunkley Jones et al. is the reconstruction of the climatic and isotope recovery at the end of the PETM, I suggest to the authors to buid their model upon that workable age model.

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Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2017-131/cp-2017-131-RC2-supplement.pdf

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