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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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We very much appreciate the comments of Victoriano Pujalte, and the time and effort he has taken to review this manuscript in detail. Within this discussion, we separate out initial responses to these comments, areas that we will refine in any revised manuscript, and requests for further clarification.

We deal with each of the three main sections of this review below.

Overview

In general we agree with comments made in this section. The main points of this

overview appear to be that:

1) the hemipelagic succession of Zumaia is controlled by precession cyclicity (Dinarés-Turell et al. 2013) and is of a continuity that can be correlated to far-field deep-ocean successions from the Pacific (ODP Leg 198) and South Atlantic (ODP Leg 208). As such, they have been demonstrated to accurately record global, orbitally-forced, climatic variability.

2) that there are turbidites within the Basque Basin, including the Zumaia section. However, within the Zumaia section these are typically thinner and less frequent (e.g. comparison between Zumaia and Ermua, Schmitz et al. (2001)). Within the Paleocene succession they have not prevented the global correlations noted above.

Together these indicate that the depositional environment and successions at Zumaia are proven to be suitable for detailed cyclostratigraphy, even in the presence of thin interbedded turbidite deposits.

Does this situation change across the PETM interval? According to multiple authors, it does, but not to an increased turbidite contribution to the sediment thickness, but by a marked reduction in turbidte contribution, such that there is an absence of these deposits during the Siliciclastic Unit (SU) at Zumaia and a greatly reduced contribution of turbidites to the thickness of sediments at Ermua (Clare et al., 2015; Schmitz et al., 2001).

As in our response to Reviewer 1, with Victoriano's expertise in this area, we would greatly appreciate being pointed towards any evidence of identified turbidite horizons, or unconformities, within the SU at Zumaia, which we would then be able to subtract from our current data analysis. To date, we have no evidence of such deposits, and in the absence of evidence to the contrary, we interpret the Zumaia PETM succession, as have previous authors, as representing relatively continuous sedimentation.

"The lithological succession across the interval displayed in Fig. 2 appears to be continuous. Although studied in detail, no unconformities have been found." (Schmitz et al. 1997).

Pre-PETM cyclicity at Zumaia

We very much appreciate Victoriano's excellent figure and discussion of this. I am not yet able to provide an answer to the questions raised, as this needs detailed discussion with co-author Thomas Westerhold, who is the expert in cyclostratigraphy and the Paleocene timescale. I will, however, ensure that this is gone over in detail and any findings incorporated into a revised manuscript and, ideally, provided to Victoriano for further discussion during this process.

PETM cyclicity at Zumaia

Victoriano appears to have two key concerns about the identified Si/Fe cycles at Zumaia:

1) "In the face of such variability [variations in thickness of the siliciclastic unit across the Basque Basin], what is the guarantee that the FSU at Zumaia has a complete record of the PETM?"

Personally, I can't "guarantee" anything. I can only make my best interpretations based on the evidence available. As Victoriano's work has made clear, there is a complexity in sedimentary successions in many of the sections in the Basque Basin, before, during and after the PETM. At Ermua, the section is dominated by turbidite deposition before and after the PETM event (Schmitz et al. 2001). This is why we choose to try to undertake this work at Zumaia, which, from previous studies has been established as having lithologies sensitive to precession scale climate variations that can be correlated globally, and, in all studies to date, has been considered a continuous succession through the PETM.

To me, it seems a little odd to now propose that the Zumaia PETM section is significantly incomplete or truncated. It's not clear on what evidence, from the Zumaia

section, this suggestion is based and where potential gaps in the section might be? I fully agree that other sections show erosive features, cross-cutting relationships and the like, but, for example, these are sections consisting of "irregular stack of sandstones and pebbly sandstones" clearly deposited in higher energy environments. There is no sign of such coarse grained deposits in the Zumaia SU, or for high-energy sedimentary structures.

I'm sure Victoriano is absolutely right that there aren't cyclic patterns of sedimentation in these other successions, as presumably these are dominated by relatively few, high-energy and highly erosive short-lived events. Such events are clearly not the main driver of the very fine-grained (Schmitz et al. 2001) sedimentation within the SU of the Zumaia section, which are likely related to processes of background fine-grained (<28 μm) sediment transport.

2) "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."

I agree with Victoriano that deposition at Zumaia could have occurred through many, many multiple sub-cm depositional events. This is absolutely fine. But in order to generate the observed cycles in bulk element chemistry with a period of 0.45 m, there must have been a consistency in the chemistry of these sediment pulses over many consecutive events. In other words, multiple successive events are carrying similar bulk sediment elemental compositions in order for a coherent signal to persist on the decimeter to meter scale.

Clearly we are not able to resolve any cyclicity on the sub-cm scale with 3 to 5 cm sampling, but this is not what we are attempting to resolve or observing. We're observing cycles with one to two orders of magnitude greater periodicity ($\sim$0.5m cycles compared to <1cm events) that are sufficiently resolved by the sampling undertaken. Given the length of the SU (∼4 m) and duration of the body of the PETM (∼100 ka), the sub-cm scale Victoriano is talking about would represent sub-millenial (∼250 year) events whilst we are concerned with precession-scale variability.

We still propose that the most parsimonious explanation for the statistically significant, ∼0.45 m cycles in Si/Fe through the SU are orbital-scale variations in the bulk sediment chemistry of this very fine grained material driven by the nature of catchment erosion and transport processes.

3) finally that surface weathering may have impacted the measured bulk sediment elemental compositions.

During sampling the section was significantly cleaned back - as much as possible given the value and sensitivity of the site – to avoid surficial weathering. Further to this, all bulk sediment and isotope analyses were undertaken on coherent blocks of clay, that were again carefully surface cleaned before being powdered and homogenized for elemental and isotopic analysis.

Finally, and this also relates to a comment of Reviewer 1 – that if we can show independent correlation between Si/Fe cycles and other, established cycles, they would recommend publication – in Figure 1 attached to this comment, we show the correlation between calcium carbonate contents and Si/Fe cycles in the interval prior to any of the PETM perturbations (∼ -1 to -4 m). This shows that variations in Si/Fe closely match the established, precession paced, high carbonate ("limestone") and lower carbonate (marl) couplets through this interval. If our approach for the analysis of Si/Fe ratios are accurately tracing bulk element sediment chemistry in this interval, and the precession forcing of limestone/marl couplets, and are not being reset by weathering-related mobilization of iron, I would argue that this provides confidence that the analyses through the SU are similarly unaffected.

In summary, I would appreciate any evidence from either Reviewer of significant

coarser-grained turbidite deposition within the SU at Zumaia, that might bias the Si/Fe record we present. We have not seen any such evidence during our logging of the section, or from the existing studies of turbidite deposition through this whole late Paleocene to early Eocene interval (Schmitz et al. 2001; Clare et al., 2015). With any such evidence, we could factor this into our interpretations and analysis, without this evidence, we have to base our interpretations on the existing evidence of the studied record, that the SU is dominated by the deposition of relatively continuous fine-grained (<28 μm) hemipelagic muds (Schmitz et al. 2001; Clare et al. 2015; our study).

Figure 1. Fine scale detail of bulk rock calcium carbonate contents, showing characteristic limestone / marl couplets (note that the CaCO3 scale is reversed), and Si/Fe ratios. Grey blocks are added as a guide for correlation between the two records - all blocks are of equal stratigraphic thickness and are precisely centered on the peak of the filtered Si/Fe record (red dotted curve).

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Schmitz, B., Pujalte, V., and Nunez-Betelu, K., 2001, Climate and sea-level perturbations during the Initial Eocene Thermal Maximum: evidence from siliciclastic units in the Basque Basin (Ermua, Zumaia and Trabakua Pass), northern Spain: Palaeogeography Palaeoclimatology Palaeoecology, v. 165, no. 3-4, p. 299-320.

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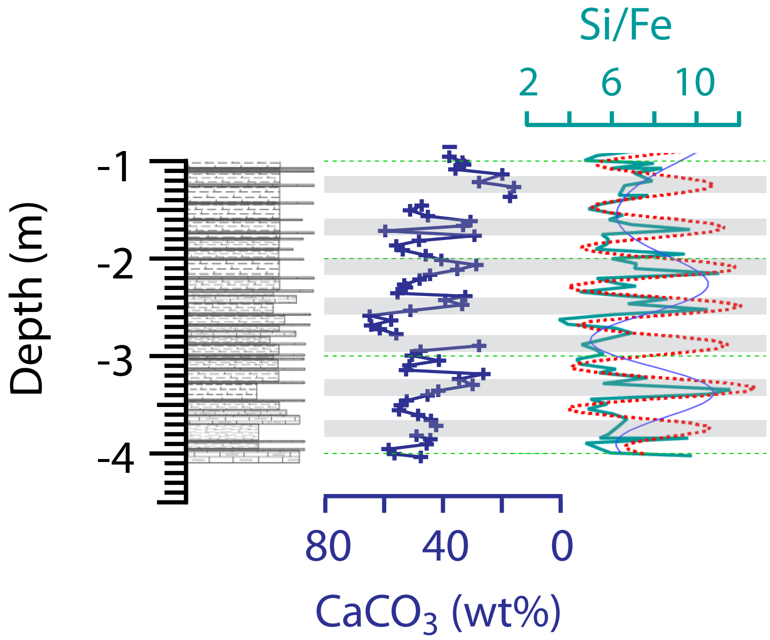

Fig. 1.