**Climate
of the Past
Discussions**

Interactive comment on "High resolution cyclostratigraphy of the early Eocene – new insights into the origin of the Cenozoic cooling trend" *by* T. Westerhold and U. Röhl

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This manuscript presents a comprehensive geochemical (X-ray fluorescence) dataset on Eocene sediments from ODP Site 1258 located in the tropical western Atlantic for the early Eocene interval spanning magnetic chrons C20 to C24. By exploiting this high-resolution dataset along overlapping sections from the 3 Holes of this Site, the authors propose a revised splice that is about 6 m shorter than the shipboard splice. A consideration on inferred average sedimentation rate from this Site indicates that the difference is about 400 kyr. In this study the authors target a time interval for which some uncertainties arise in the Geomagnetic Polarity Time Scale (GPTS) chiefly due

to ambiguity in numerical age constraints of certain tie points in the scale construction and related volcanic ashes that are actually used in the schemes. Therefore, this cyclostratigraphic work is much welcomed in addition that extends upwards a still-floating Paleogene astronomically calibrated time scale. The time model is constructed by cycle counting and also by relative tuning to an arbitrary 405-kyr cosine function. The developed time model is used to shed light to an ongoing controversy steaming from radiometric dating of ash layers from the Green River Formation in the USA and their intrabasinal correlation with local magnetostratigraphies and the GPTS. The new astronomically calibrated GPTS differs notably with respect to current scales (CK92 and GPTS04) the authors relaying on previously published magnetostratigraphic interpretation for the studied Site. Moreover, in addition to the cycle pattern recognition and counting the authors make use of several spectral analysis techniques to decipher the nature of cycles along a particular interval of the studied record that are interpreted to reflect obliquity. This unique obliquity component that is extended for about 800 kyr within chron C23r is related to a minimum in the very long eccentricity cycle which is a key element to further constraint accuracy in developing new astronomical computations. This feature of the astronomical forcing in this tropical Site allows gaining insight of the early to middle Eocne climate evolution. The authors propose a increased influence of high-latitude processes on low-latitude deep ocean chemistry at the termination of the Early Eocene Climate Optimum (EECO) right at the onset of the long-term Cenozoic long-term cooling trend. The manuscript is well written and rigorously presents data, analyzes it appropiately and discusses consequences concisely. This is a data-rich paper that makes use of additional suplementary information. Overall, this is and excellent paper that deserves prompt publication. My suggestions next are only intented to streghten the case for a good and reliable time model and to facilitate aspects to the reader.

1- Pivotal to the time model presented in this manuscript (and therefore to its consequences and interpretations) is the construction of the composite section ("splice"). I have no doubt that the authors have done the best in doing that. They provide all nec-

essary information in the form of tables in the supplementary material for others to be able to rebuild and translate scales. The authors mention that the original splice was mainly based on the magnetic susceptibility records which had ambiguous correlations in some cases. I would strongly suggest this part to be more exhaustive. The reader should readily be convinced on the appropiateness of the new composite and appreciate better where the differences are with respect the original splice. Fig. S1 partly addresses this point but I would rather prefer to see the new XRF scanning data both on the original mcd scale and mbsf scale along also with the susceptibility data. Then the authors could enfasize on the details and changes they choose to follow and the reader would better value the usefulness of the high resolution XRF tecnique and be more conviced on the time model.

2- I'm a bit concerned on the magnetostratigraphic data. The authors directly follow the interpretations provided by Suganuma and Ogg (2006) that carried a shore-based discrete sampling as the shipboard magnetometer measurements where generally compromissed by weak magnetizations. Given that the Site location was located around the paleoequator during the time span considered in this study, immediate interpretation of polarity from demagnetization data is severely hampered (which would be normaly based on clustering of positive and negative inclination data as rotary drilling prevents the use of declination). Suganuma and Ogg adequately follow a method that uses relative declination of removed vectors or, in other words, evaluates the relative directions of the low-temperature components (secondary magnetization) vs. the directions of higher-temperature characteristic primary components to infer the actual polarity of the later. Suganuma and Ogg recognize that this can be ambiguous in some cases and also than can lead to erroneous interpretations. However, they develop a ranking scheme from 1 to 4 to classify the reliability and stablish a magnetostratigraphy from 1-3 rated samples. Curiously enough, Suganuma and Ogg also indicate for Site 1258 a reddish brown chalk that spans the Lower Eocene (Ypresian)–Middle Eocene (Lutetian) at ∼40–100 meters composite depth (mcd) that has strong magnetic intensity with a relatively steep downward-directed magnetic inclination. This type of

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component has been interpreted to a drilling overprint in several ODP Legs and I wonder how this would compromise the interpretation of the primary components following the method described above. I'm not critizizing this approach but perhaps the authors could shortly describe this in the manuscript. More importantly is presentation of data in Fig S2. One is actually confused on the units used for the inclination axis and its real meaning (clarify this). Caption of that Fig. refers to Table S1 (should it be Table S6?). It is shoking to observe the presence of a fault in Hole A that seems not to be present in Hole B apparently guarateeing the completeness there. This fault removes a subtantial portion of sediment. I'm really confused here looking Fig S2. Why reporting the magnetic data in the revised rmcd scale, and also the original mcd for that matter, does not bring the incliantion data along the faulted interval compatible among the two Holes? In addition Suganuma and Ogg mention the posibility of another unrecognized fault either in Hole A or B that have truncated Chrons C23n and/or C23r. I thing all this aspects deserve a little more attention and discussion in the manuscript.

3- The discussion of the wavelet spectra presented in Fig. S3 sometimes is not obvious or becomes umbiguous even with the aid of marking some distinct bands across the sepctra. The authors could perhaps emphasize better this issue in some passages. It also appears to me that the meaning of the cross-hatched regions indicating areas with edge effects is flawed. In principle this is used when time series are numeracally padded at the edges. The authors should specify this in the methodology. However, since we are dealing with "truncated" time series in the 4 diagrams presented in Fig. 4 I wonder if the authors performed the wavelet analysis strictly on the lengh of the presetend time-series (and padded the edges), or more aproppiately they used the real data outside the length of the respective diagram (in which case the cross-hatched region would be meaningless).