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Interactive comment on "Paleodepth variations on the Eratosthenes Seamount (EasternMediterranean): sea-level changes or subsidence?" *by* S. Spezzaferri and F. Tamburini

Anonymous Referee #2

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First of all my apologies for the delay in submitting a review of this manuscript.

Other than reviewer #1 I do find this an interesting effort to discriminate between tectonics and sea level, and I think the authors have chosen an appropriate site. However, some points need clarification before the manuscript is convincing. My main hesitations concern 1) the dating of Site 967A and the correlation with the LR04 stack of d18O (Lisiecki & Raymo, 2005), and 2) the paleodepth reconstructions and the comparison to global sea level.

1. In comparison with the resolution of the LR04 stack of d18O, the resolution of the biostratigraphy of 967A is fairly low. The authors do not explain how they construct the

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correlation lines between Site 967A and the d18O stack in figures 2 and 3. It is not sufficient to simply draw a line between a data point in 967A and the corresponding data point in the stack. Lisiecki and Raymo (2005) suggest that: any paleoclimate proxy taken from a marine core with reliable d18O data can easily be aligned to the LR04 stack through the use of automated graphic correlation software. In addition, I am not convinced about the accuracy of the datums in 967A, for several reasons already outlined in papers published in the ODP160 volumes (Initial Reports and Scientific Results; including Spezzaferri et al. 1998). The presence of all major zones is no guarantee that the sequence is complete (other than the hiatus that is mentioned). Only part of the datums is tuned, and there are differing opinions about the tuning. In short, I doubt whether all correlation lines in the figures 2 and 3 hold if examined in detail. If not, only a rather general correlation can be made with the LR04 stack. For these reasons I would like to see a more thorough evaluation of the correlation between Site 967A and the LR04 stack.

2. Paleodepth reconstruction follows the method outlined in Hohenegger (2005). The advantage of this method is that it uses depth ranges of species. However, this is at the same time a disadvantage because it introduces two types of bias, one being taxonomic concepts and the other being the nature of sampling, which is never complete even of extant species. Depth distributions of extinct species can only be estimated (e.g. C. italicus), and species depth ranges may have shifted over periods of millions of years (compare no-analog problem in Mekik & Loubere (1999)). Species depth distributions differ between basins (already noticed by Bandy & Chierici, 1966). In his paper Hohenegger (2005) corrects for this effect by using generalized depths, but this concept broadens the depth ranges. In addition the method is, like other paleodepth methods such as P/B ratios, sensitive to paleoecological conditions and to low diversity. For instance, in sapropels low-diversity stress taxa tend to dominate the assemblages and these normally have a rather broad paleodepth range. In short, also to this paleodepth method there are several more potential sources of error than reworking etc. mentioned by the authors, as is evidenced by the error bars in figure 2, and it makes

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no sense to mention depth levels with a precision of 0.5 m. A second point concerns the interpretation of the LR04 stack. The authors need to explain why they interpret the d18O fluctuations fully as sea level. According to Lisiecki & Raymo: While a stack alone cannot address the relative contributions of ice volume and temperature to the benthic d18O signal, a good stack does provide an accurate estimate of how much total change must be explained.

For the moment it appears to me that only a rather general comparison can be made between Site 967A and the LR04 stack, with the conclusion that tectonics play a significant role at Site 967A and that after the MSC probably an additional depth effect is caused by global sea level fluctuations. This is, however, not new and to my feeling not the main conclusion the authors want to stress. Rather than this, to me the main conclusion of their manuscript is the fact that it appears to be possible to discriminate between sea level and tectonics. The abstract should be rephrased in this sense and better formulated. In addition I invite the authors to include a more thorough discussion section about correlations and depth reconstructions in their manuscript.

About Figure1 I agree with reviewer #1. I would suggest however, for people who are not familiar with the area, to present a small map of the Mediterranean as an insert of a more detailed location figure.

Technical comments: Lisiecki instead of Lisieki (several occasions) p.118, line 6: a hiatus 49 kyr or 4.9 kyr (p.123, line 11)? p.120, lines 11-15: this is nonsense. p.120, lines 16-17: are the ages in Emeis et al. (1996) astronomically calibrated? p.120, Discussion, lines 17-24: please rephrase. The present phrasing does not reflect the differing views in an appropriate way. p.122, lines 24-25: only if the correlation is sufficiently detailed p.122, lines 26-27: this asks for a reference p.123, lines 3-5: I do not understand this statement References, p. 124, lines 26-27: incomplete reference References, p. 125, lines 19-20: Lisiecki References, p. 125, line 29: incomplete reference

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Table 1: please mention sources. Depth range of L. lobatula should be extended to deeper. Typo: Uvigerina rutila.

Please check the captions of figures 2 and 3. Caption to figure 3: is full of typos, and what do the numbers represent? 5 = 5.00 Ma? 12 = FO Gephyrocapsa spp. larger than 5.5 micrometer; 15 = Pseudoemiliana lacunosa (probably autocorrection of Word) last line: nannofossil (not nannofossils) bioevents; this last statement is only partly true, if at all.

References (other than in reference list of the manuscript) Bandy, O.L., Chierici, M.A., 1966. Depth-temperature evaluation of selected California and Mediterranean bathyal foraminifera. Marine Geology 4, 259-271. Mekik, F., Loubere, P., 1999. Quantitative paleo-estimation; hypothetical experiments with extrapolation and the no-analog problem. Marine Micropaleontology 36, 225-248.

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