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Interactive Comment

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

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In the interactive comments several critical issues are raised with respect to this manuscript. I tend to agree with the most substantial ones and would like to add a few points of concern, in part overlapping with the earlier comments.

The formula on p. 118 contains one division sign too many. This leads to paleodepths of billions of meters. By taking out one division sign the estimates listed in the appendix are obtained.

The authors write on p. 119: paleodepth estimation calculated with this method is reliable, as it depends on a geometric mean of gradient values. This is clearly shown by the minimal difference between the real depth of the Site 967 (2553 m) and the estimated depth of the upper part of the studied sequence (2549.5 m)

A brief explanation of what a geometric mean is needed and why it provides a reliable estimate (this is preferable over just a reference to Hohenegger 2005). Despite the excellent (or perhaps rather fortunate) agreement between the most recent Pleistocene estimate and actual depth, this example reveals potential weaknesses of the method. Why is that? The depth estimate is based on the presence of only one single species, Articulina tubulosa. This species happens to have the deepest range of all taxa with a geometric mean of 2549.5 m! The upper four samples all contain just this single miliolid species, therefore the estimated paleodepths are all identical. There are a couple of problems with this. 1) This means that the even the largest fluctuations in eustatic sea level of the entire Cenozoic, that is those of the late Pleistocene (˜100-200 m), have left no trace in the sea-level record of Eratosthenes Seamount. Yet, lesser fluctuations in the earlier Pleistocene are thought to be reflected in the depth record. That is peculiar. A look at the data matrix shows that every time a species leaves or enters the record there is a change in estimated paleodepth. This is logical from the formula, but it is unlikely in a variable natural system. Is it not more likely that this is an artifact? At paleodepths of 1500-2000 m, a bigger sample would generally reveal more consistent ranges of the rare species which in smaller samples only appear now and then? Sample size thus interferes with the depth estimates. In turn, the individual wiggles are thought to reflect eustasy. This brings me to: 2) Why is Pleistocene diversity so extremely low at this site? Is this true diversity or does it perhaps result from the low numbers of benthic specimens flooded in between planktics (in a small ODP sample)? Is it related to size fraction? Were many taxa not identified and left out? In all 24 Pleistocene samples together only 11 species occur of which Articulina and Gyroidina neosoldanii seem to be the most common species. To me this is unusually low. In order to be able to assess applicability of the method it is necessary to know what the number of observed specimens is. As long as it is not certain that the observed specimens represent the true qualitative composition of the stratigraphic level, any assessment is fundamentally flawed. 3) If for some reason the upper sample would have contained only Pyrgo murrhina instead of Articulina tubulosa, the paleodepth estimate would have been only 1581 m, a difference of 1000 m just by the absence of one species and presence of another one. Where both occur the paleodepth estimate is 2112 m; 1756 m when adding Gyroidina neosoldanii. This shows the vulnerability of the method in low diversity situations and/or when the number of specimens is low. The lower stratigraphic intervals do not seem to suffer much from this problem

In her comment Ellen Thomas was unfortunately confused by the bars in fig. 2a, thinking they represent species depth ranges. In fact the bars in this figure refer to calculated confidence intervals (95%? 99%? calculation should be added or ref to Hohenegger 2005) on the paleodepth development. This figure shows that for the Pleistocene and upper Pliocene it is pointless to make any distinction between paleodepth changes resulting from subsidence or eustasy (the effect of sedimentation is not considered). The confidence intervals are apparently in the order of 1000s of meters. In other words there can be no confidence of any degree in these estimates – which is in fact quite surprising. The situation for the older stratigraphic units is not much better, with confidence intervals still up to a 1000 m. For these stratigraphic intervals the eustatic changes were not more than some tens of meters (fluctuations in oxygen isotopes of less than 0.5 permill, including temperature effects). Yet, even if we completely disregard the confidence limits (as in fig. 3) the estimated depth fluctuations are in the order of 100s of meters. Some wiggles may match with the isotope stacks, but their amplitudes are off by almost one order of magnitude. My conclusion from this exercise would be that the method could perhaps provide a rough estimate of paleodepth development, but there is no way of distinguishing any eustatic signals.

Another problem also addressed from slightly different perspectives by the second anonymous reviewer and Ellen Thomas is that in the larger part of the sequence the resolution of the benthic foram record is much too low to accurately correlate with the isotopic curves. A good start is made in the lowermost Pliocene, where resolution is

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in the order of 1 sample per 10 ky. At that time the basin was not too deep, so the method used might work well in this or similar interval (although the sea-level of the Mediterranean seems to have differed from the global one because of the shallow sill at Gibraltar). In order to make a solid case for the potential of the method used and to distinguish between subsidence and eustasy, I would suggest to focus on a particularly well constrained stratigraphic interval in high-resolution and with a good and rich benthic foram record which would lead to estimates with narrow confidence intervals.

I doubt that there can be any method, employing foraminifera from middle bathyal (600-100 m) or deeper environments, that enables to distinguish between sea-level fluctuations of less than 50 m (i.e. pre-Gelasian). This is because foram assemblages from for example 850 m and 800 m are usually not that much different and when they are depth itself is unlikely to be the controlling factor. Even if they would have been very different in the past, there are so many factors interfering with depth distributions of foraminifera that it would be impossible to unravel this on the basis of general modern distributions. This sounds pessimistic, but in my view that is realistic. Trying to work this out for shallower sequences (neritic to upper bathyal) seems to be a more feasible effort, even more so when comparisons are made with estimates based on P/B ratios (with all its restrictions) and quantitative analyses of benthic assemblages.

Interactive comment on eEarth Discuss., 2, 115, 2007.