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Interactive comment on "Towards an unbiased estimate of fluctuations in reef abundance and volume during the Phanerozoic" *by* W. Kiessling

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1. General comments

I greatly appreciate the insightful reviews of the anonymous referees. What I take from the reviews is (1) the approach, methods and results are welcomed but (2) some more details on the methods and a more comprehensive discussion of the results are wanted. This is no problem and will be done in my upcoming revisions. The specific comments can be addressed as follows (referees' comments in italics):

2. Specific comments

2.1. Referee #1

Provide specific examples for justifying assumptions. Some specific examples are already provided in the text (p. 1498-1499). I will add a few more. One should note, however, that the assumptions in the paper are either straightforward (oceanic reefs, carbonate cycling) or better justified by statistics on the full dataset (subsurface reefs, GDP-effect), rather than by selected examples.

Provide additional tests to bolster the new curves, i.e. are they better correlated with environmental changes? Independent tests are currently difficult to perform. I have already cited (Copper, 1994) who has indicated similar results based on a more qualitative approach. Performing new cross-correlations with inferred earth system parameters is beyond the scope of this paper. Since the detrended time series of the adjusted values (first differences) are strongly correlated with the raw curves (Table 1), a better correlation with environmental change is not to be expected. In any case, even if the new curves showed a better fit with environmental parameters than the raw curves, this could not be used to prove the validity of the new curves. Perhaps the new curves are still biased to some extend (as discussed in the paper), but the bias is far more homogeneously distributed than in the raw data.

Removing certain reefs introduces a new bias. The referee is correct that the results would be more trustworthy if it could be shown that (1) the proportion oceanic reefs has remained constant through time and (2) the chances of reefs to becoming buried has not changed. Assumption (1) has already been discussed in the paper (p. 1491), where I state that because the proportion of oceanic reefs in the pre-Jurassic is simply unknown, my adjustments should only be taken for reefs resting on continental crust. Assumption (2) cannot be proven either, but the justification of removing subsurface reefs is demonstrated by the strong cross-correlation with reservoir potential suggest-

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ing that 60

GDP adjustment needs further explanation and justification. I would prefer not to repeat published results at length, but I follow the referee's suggestion and shall add some more details upon revision. The 5 reef limit per country was arbitrarily chosen to reduce statistical noise. However, all analyses were also conducted using the full dataset and yielded the same basic results.

Why are reef overestimated when they occur in less-developed countries? Judging from a number of papers that I have analyzed for the PaleoReefs database, studies in less developed countries tend to be less detailed and include extra-reefal carbonates into estimates of reef size. One specific example comes from the Jurassic Esfandiar Limestone in Iran, which has been qualified as a several hundred meters thick reefal limestone (algal reef) in older surveys (Stöcklin et al., 1965), when there is nothing but small microbial biostromes and bioherms in the region (Franz Fürsich, pers. comm. 2003). Sometimes the overestimation of reef sizes may also be intentional (to attract funding for exploration), but because this statement is politically problematic, I do not want to add this to the text.

Can the GDP effect be demonstrated beyond the correlation? The strong correlations are best argument for demonstrating the bias of GDP on the reef record. Qualitative comparisons, however, underline these results. For example, the widespread Late Jurassic (mostly Oxfordian) sponge-microbial reefs of Europe are apparently much more common in Germany (GDP/area=$ 7.755 10^6 km^{-2}; 15 detailed records of this reef type) than in Poland (GDP/area=$ 0.550 10^6 km^{-2}; 4 detailed records of this reef type), although summary maps (e.g., Leinfelder et al., 1996) indicate a very similar extend of the so-called spongiolithic facies in both countries. Other examples are the apparent truncation of a Middle Miocene reef tract along the Red Sea Coast at the Egypt/Sudan border and the distribution of Early Cretaceous reefs in the Mural Limestone of the southern United States and Mexico.

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Interactive Comment

The technical corrections are all fine and will be considered in the revised paper.

2.2. Referee #2

The difference between the curves before and after adjustment needs to be shown. This difference is actually shown at every stage of adjustment (Figs. 2-6) and the summary figure (Fig. 7) compares the original curves with the ones after final adjustment. The data listed in Tables 2 and 3 also summarize how important measures (standard deviations, volatility, overall similarity) are affected by the adjustments. I can expand on the data listed in Table 3 (e.g., showing cross-correlations after every step of adjustment), but apart from this I think the current curves speak for themselves.

The meaning of the final curves should be discussed in more detail and compared with Mackenzie and Morse (1992). As stated above (comment two of referee #1), the cross-correlations of the new curves with environmental changes are beyond the scope of this paper. Also Mackenzie and Morse (1992) only provided analyses of bulk carbonates without separating reefs. In any case, the revised version will have some more discussion of the implications of the new curves.

Provide examples for GDP calibration. See comment 6 of referee #1. More details will be provided in the revised paper.

References

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