

We thank the reviewer for his valuable comments and suggestions for our manuscript entitled "*Ecological response to collapse of the biological pump following the mass extinction at the Cretaceous-Paleogene boundary*". Below follows a point-by-point response to the comments by the reviewer. Comments by the reviewer are **in bold**, our reply is in normal font.

[A good citation for this is Hilting et al., 2008, "Variations in the oceanic vertical carbon isotope gradient and their implications for the Paleocene-Eocene biological pump", *Paleoceanography* 23:doi:10.1029/2007PA001458..

We will incorporate this citation in our revised manuscript, although we must admit that the argumentation in this paper is quite difficult to follow.

[The authors don't really have any direct evidence that the biological pump collapsed at their site, because they have no benthic $\delta^{13}\text{C}$ values. If they could generate these data their story would be further substantiated.]

We agree with the reviewer that obtaining a benthic foraminiferal $\delta^{13}\text{C}$ record would further substantiate our otherwise indirect evidence that the efficiency of the biological pump decreased at our study sites. However, given the poor preservation of carbonate in our records, with all foraminifera showing full recrystallization, it not feasible to generate a reliable benthic foraminiferal $\delta^{13}\text{C}$ record for the Okçular section.

Nevertheless, although we not have the direct evidence that a benthic foraminiferal $\delta^{13}\text{C}$ record would have provided, our records can be regarded as indirect evidence. Our records show an increase in nutrient availability in the surface oceans, whilst there is a decrease in food supply at the sea floor. This suggests a causal link, i.e. a reduction of the transport of organic matter from the surface ocean to the sea floor (=a reduced biological pump strength), resulting from a reduction in the efficiency of the biological pump (a smaller fraction of the organic matter produced photic zone is transported down.)

Moreover, it is generally assumed that the collapse of the biological pump at the K-Pg boundary is a consequence of the ecosystem reorganization that resulted from the mass extinction. Since these extinctions occurred on a global scale, it is to be expected that they also occurred at the Okçular section, as at all previously studied sites in the Tethys, and that, as a result, the biological pump also collapsed here.

[The collapse of the biological pump should indeed lead to enhanced nutrient recycling into the photic zone and should also expand and shoal the oxygen

minimum zone. The authors might want to consider this in light of their interpretations of indicators (or lack thereof) of dysoxia at various sites.]

We agree with the reviewer that it is likely that the enhanced nutrient recycling in the photic zone should lead to an expansion of the oxygen minimum zone. In particular, when there is a reduction of transport of organic matter out of the photic zone, and, hence, more remineralization in the photic zone itself, it is to be expected that the oxygen minimum zone will shoal. It is therefore not unimaginable that the low-diversity benthic foraminiferal assemblages in P0 in our study sites might also be influenced by lower oxygen concentrations. However, in contrast to some other sites (e.g. Coccioni and Galeotti, 1994; Kaiho et al., 1999), there is no other evidence from the sites investigated here pointing towards truly hypoxic conditions. Therefore, in our study, we cannot make any firm conclusions on this matter.

It is important to realize that minimal oxygen concentrations in oxygen minimum zones depend on a large variety of factors, and therefore, at different locations, oxygen minimum zones can have very different minimal oxygen concentrations, ranging from 0 ml l⁻¹ at some extreme sites, to up to barely below 5 ml l⁻¹. Although it is very well possible that oxygen concentrations decreased at the seafloor, this decrease would not necessarily have to result in hypoxic conditions at our study sites. It is therefore possible that such a decrease in oxygen availability was not reflected in the benthic foraminiferal assemblages.

[I think the contrasting behavior in the open ocean (deep sea), e.g., page 12, paragraph beginning line 24, can be understood by the relative resistance of the more recalcitrant organic matter that the deep sea usually gets anyway to more intense surface ocean recycling with the collapse of the biological pump. In other words, the deep-sea benthic foraminifera continue to receive recalcitrant organic matter at barely diminished rates despite the collapse of the biological pump.]

We thank the reviewer for this valuable suggestion. This idea might provide a very good alternative hypothesis for explaining the observed differences between the signal at most continental margin sites versus the signal at Shatsky Rise. We will include a brief section discussing this alternative hypothesis.

[Page 1, Line 16: I think the "now unequivocally shown. . ." comment about impact as the cause of the extinction should be removed; the comment is irrelevant to the current manuscript and might be considered by some as a "pot shot" at the volcanic origin idea. The way this same idea is put on line 31 is better "It is now commonly accepted. . ."]

We agree and will adjust this sentence in our manuscript accordingly.

[Page 2, Line 12: My modeling did not suggest that "productivity had to continue nearly unabated . . . (Kump, 1991). Rather it showed that burial had to continue nearly unabated. Burial could have been in shallow water or on land, where the required productivity would not impact the ocean's vertical carbon isotope gradient. Primary productivity certainly COULD have continued unabated, but

export productivity had to have been diminished (unless the whole ocean became destratified and well-mixed).]

We agree and will adjust this paragraph in our manuscript accordingly.

[Line 14: "persistence"]

OK, corrected.

[Line 22: remove "to" after "from the photic zone"]

OK, corrected.

[Page 3, Line 17: "changes in, for example, temperature . . . "]

OK, corrected.

[Page 4, line 14: Might be good to foreshadow the main conclusions at end of this Paragraph.]

Although we understand and appreciate the suggestion by the reviewer, we feel that foreshadowing the main conclusions at the end of the introduction would give the impression that we were biased towards the outcome of this study. We therefore do not consider this the most elegant approach.

[Page 5, line 18: "quantitative" ?]

We thank the reviewer for pointing out this error in our manuscript. The correct term should indeed be "quantitative".

[Line 22: indicative "of"]

OK, corrected.

[Page 6, Line 1: data "were"]

OK, corrected.

[Page 12, line 22: some of the effects of the impact, like the trace-metal poisoning, could have been relatively long-lived. See for example Jiang et al. Nature Geoscience 3, 280 - 285 (2010).]

The reviewer is correct that, in the worst-case scenario of Jiang et al. (2010), trace-metal poisoning could have lasted for up to 10 kyrs. However, this is assuming the worst-case scenario (i.e. assuming 100% metal solubility). When more realistic metal solubilities are assumed (1% solubility), the duration of a possible trace-metal poisoning is considerably reduced, i.e. to 1 kyr (Jiang et al., 2010, supplementary material). This, indeed, is considerably longer than for example the duration of the hypothesized impact winter, but still relatively short-lived compared to the long-term biological and paleoceanographic reorganizations that occurred after the K-Pg boundary mass-extinctions, which occurred over hundreds of thousands of years.
