

Interactive comment on “Phytoplankton community disruption caused by latest Cretaceous global warming” by Johan Vellekoop et al.

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The paper by Vellekoop et al. reports the coincidence of a major global warming recorded in sea-surface waters at the end of the Cretaceous to a dinoflagellate bloom and evaluates the consequences of such a bloom on the marine benthic ecosystem. This manuscript is very well written, the methodology is sound and the figures of excellent quality. I have already reviewed a preliminary version of this paper which was submitted to PNAS and gave (1) a positive review with only minor modifications. One of the main interesting points raised by the other reviewers of the PNAS version were that (2) one was not convinced the paper was not ground-breaking enough to warrant pub-

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lication in a broad-audience journal. (3) It was pointed out that the selling point leans on the idea that the oceans during the LMWE were increasingly thermally stratified, to make the link with model climate change and dinoflagellate response. Connected to this last point, the reviewer did regret that no modelling was part of the study to further support this idea. (4) it was also pointed out that the conclusion that warming caused half a million year of toxic dinoflagellate blooms is not sufficiently supported by the results and that other alternative environmental changes such as sea level change, hydrological cycling (weathering, runoff and sedimentation) could have also influenced the studied sites like across the PETM where the same area appears to respond to such factors. With respect to point (1): the authors took into account a point about a possible response to environmental stress such as the potential ocean acidification that has now been evoked by several groundbreaking papers for the top Maastrichtian and have added these potential aspects in the discussion. Most of the minor points that I raised in my detailed comments to the previous version have been taken into account and corrected. With respect to points (2) and (3), the link to ocean stratification is now better supported and established thanks to the authors ruling out alternatives (see point below) and therefore the link to future scenarios for recent climate change is perfectly valid. I believe that the results brought up by the authors are enough to support their conclusions and do not require modelling for the purpose of this manuscript. The article in its present form fits very well to the audience of Biogeosciences. With respect to points (3) and (4): the authors took these comments into account and now deal with alternative aspects in their discussion. They bring arguments to rule out sea-level change and profound changes in hydrological changes that would modify runoff and salinity so that the warming and associated ocean stratification remain the best explanation for the observed changes in the dinoflagellate community. I have no further comments to this new version. In my opinion, the authors have tackled well in this new version with the 3 previous reviews of the manuscript submitted to PNAS and therefore, the present paper could be accepted to Biogeosciences with no additional corrections.

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