

## ***Interactive comment on “An open marine record of the Toarcian oceanic anoxic event” by D. R. Gröcke et al.***

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Received and published: 8 August 2011

Dear Bas,

Thank you for reviewing our manuscript, ‘An open marine record of the Toarcian oceanic anoxic event’, and for your comments.

It is not our intention with this manuscript to advocate the ‘methane hypothesis’. We document the presence of the, as you put it, well known carbon isotope excursion that is used to define the Early Toarcian oceanic anoxic event and the origin of which is still a matter of discussion. The negative carbon isotope excursion recorded in the studied sediments represents the first documented Toarcian excursion in a truly pelagic environment away from land and outside the Neotethyan realm from which most records

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of this excursion are known – the exceptions being, as you point out, Argentina and Canada, which are nonetheless proximal, shallow records.

We think that in your review you place great emphasis in the very negative (-57‰ value that occurs embedded in the negative excursion. More emphasis than we thought we had placed in this data point ourselves. It is not our intention to focus on that very negative value. However, it is not correct to say that this value is used as a tie point to correlate the excursion with the excursion as recorded in epicontinental sections in Europe, which are lateral equivalents of the studied section. Removing the very negative value – which we reproduced and are confident it is not the product of analytical error – would still yield a negative excursion that we are able to correlate to other localities. Exploring the very negative value is beyond the point of the present manuscript. We will, however, rewrite and clarify the manuscript where needed in order not to convey the idea that this point is a fundamental part of the study.

In what concerns the resolution of the carbon isotope data, we think that one sample every 10 cm is not low resolution and is typically sufficient to extract the signal from the sediments. Higher resolution does not (in many cases) result in a different carbon isotope record and because our current resolution is finer than that of biostratigraphy, this would not result in an improved correlation. The ‘belief’ in the correlation rather than the correlation itself is strengthened by higher resolution data.

We do not see how correlating bulk organic carbon isotope records is obscure or, as you phrase it, ‘murky’; particularly when the data are supported by biostratigraphic data, such as in this study. And there are many papers from Palaeozoic successions where organic carbon-isotope stratigraphy has been successfully completed on highly metamorphosed sediments (greenschist facies). The record of other Phanerozoic events are correlated used carbon isotope stratigraphy. After correlation, the successions can be compared and we learn a great deal from the comparisons. If it works for those events, why should we question the method for the Toarcian in particular?

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There are always issues with biostratigraphy as it has been shown over and over again that the range of species is not always synchronous as demonstrated for the Cenomanian/Turonian oceanic anoxic event (see Tsikos et al. 2004). In fact we are certain that carbon-isotope stratigraphy is the best method on which to correlate and constrain global successions, especially across different oceanic basins that are constrained by different biostratigraphies.

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Interactive comment on Solid Earth Discuss., 3, 385, 2011.