Solid Earth Discuss., 3, C199–C202, 2011
www.solid-earth-discuss.net/3/C199/2011/

Solid Earth
Discussions

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

B. van de Schootbrugge (Referee)

van.de.Schootbrugge@em.uni-frankfurt.de

Received and published: 30 May 2011

This study deals primarily with an organic carbon isotope record from open ocean sediments of Toarcian age exposed in Japan that supposedly preserve the well known large negative carbon isotope excursion (CIE), one of the hallmarks of the Toarcian Oceanic Anoxic Event (T-OAE). This excursion has only been shown to occur in Europe, although recently somewhat similar records have been obtained from Argentina and Canada. Hence, this paper presents further evidence for a global occurrence of the excursion. The authors also present biostratigraphic information based on the ranges of radiolaria. Using these two data sets that allow for a global correlation of the Toarcian negative CIE, the authors advocate the "methane-hypothesis", i.e. the negative CIE was the direct result of methane released from the sea floor altering all

exchangeable carbon reservoirs nearly simultaneously.

One major problem with the here presented data set, compromising the value of this contribution, is that much significance is attached to a single data point that has a very negative signature of -57 per mil. Although its significance may be seriously questioned, the authors use it as a tie-point to correlate the Japanese section with sections in Europe. Such an outlier in a data set should either be ignored, or its uniqueness should be explored in great detail. The authors fail at both.

It is essential for this paper that the authors show what kind of organic matter is present in their samples. Is it refractory highly degraded organic matter originating from radiolaria and possibly other zoo- and phytoplankton? The application of bulk sediment organic carbon isotope records is becoming increasingly murky, and I am questioning whether metamorphosed cherts are an ideal substrate to do this type of work. Considering the enormous range of fractionations exerted by various organisms from plants to bacteria, if organic carbon isotopes are used as a tool to constrain the global carbon cycle, like in this paper, it becomes imperative to know the source of the organic matter. The authors should either check the organic matter with palynology or with organic geochemical techniques. Furthermore, most of the chert samples have only between 0.1 and 0.01% TOC. What is the influence of such low TOC values on the isotopic measurements?

On page 388/389, lines 27 to 30, and lines 1 to 4: What is the logic here? Because Suan et al. (2008) found the excursion to be present in brachiopods, the conclusion is that belemnites are either not present during the OAE (belemnite gap) or sampled the wrong habitat. This has to be rephrased. If the excursion is truly global and present in all exchangeable carbon reservoirs it should be present in plants, brachiopods and belemnites. I think one needs to be carefull to dismiss belemnites as unreliable recorders of carbon isotope signals, especially if on the other hand they are being used for all sorts of other (problematic?) analyses, like Mg/Ca, Sr-isotopes, and oxygen isotopes. On long timescales, belemnites do very well at recording carbon cycle changes, and their signals for the T-OAE, although noisy, are consistent between the UK, Germany and Spain (see for example Gomez et al., 2008).

I am not convinced by the biostratigraphic inferences made in this paper. The radiolaria data are correlated with sections in Oregon, which also contain ammonites. But how well are these sections in Oregon constrained themselves, and how were they correlated to the standard zonations in Europe? Studying the paper by Pessagno and Blome (1980) in Micropaleontology (and not Marine Micropaleontology as listed), leaves many open questions. I am sure that the Pessagno and Blome study presented a major advance 30 years ago, but the resolution is incredibly low and does not allow for the detailed correlations proposed here. Furthermore, the marker species mentioned (Trillus elkhornensis and Eucyrtidiellum sp. 2) are not very usefull. The first has a first occurrence somewhere in the Upper Pliensbachian and ranges into at least the Bajocian. The second marker is an undescribed species. The authors have to do a better job at educating the readership how exactly the biostratigraphic framework of the sections is achieved. Concerning the low resolution of the available biostratigraphy, it also seems somewhat optimistic to put absolute ages next to the section that have a precision of up to a thousand years (Fig. 3).

The correlation presented in Figure 4 leaves many questions unanswered. The data set from Littler et al (2010; not 2009 as listed) for the Yorkshire Coast section is not completely represented. There appear to be data missing from the Tenuicostatum Zone? In the figure caption it says that no data were adjusted for sedimentation rates? Please explain? As mentioned above, the -57 per mil can not be used as a tie point. It certainly does not represent the Toarcian negative CIE.

The way forward for this paper is: 1) present a detailed overview of the composition of the measured TOC. 2) Improve the biostratigraphy, or at least make it more understandable. 3) Present higher-resolution C-isotope data to strengthen the correlation with European sections.

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