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Interactive comment on "Record of Early Toarcian carbon cycle perturbations in a nearshore environment: the Bascharage section (easternmost Paris Basin)" *by* M. Hermoso et al.

Anonymous Referee #2

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The present paper, Record of early Toarcian carbon cycle perturbations in a nearshore environment: the Bascharage section (easternmost Paris Basin), by M. Hermoso and colleagues, present a new dataset (d13C, d18O, Rock-Eval data) for a section spanning the late Pliensbachian –Early Toarcian. The authors explore whether the events recorded in the section are of local and global nature. To do so, they use carbon and oxygen isotopes, together with Rock-Eval data, and compare them to other early Toarcian sections from the similar area. They conclude that the section records the early Toarican anoxic event expressed as a positive increase of d13C values and also local events expressed by a negative CIE within the serpentinum zone. The authors discard a diagenetical issue and interpret this second event as a potential upwelling of cold

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deep waters 12C rich. This is one of the weak point of the current paper. The authors state, and they're correct, that further data could help understanding what's happening, and they name d13C and d15N of organic matter. However, they state in the Methods section that they tried to measure OM carbon isotopic composition and failed. Why do they think they could do better in the future, and in this case why don't they wait until they have a whole dataset to publish one complete paper on this section? This is of importance in 5.3.2 where the authors use the possible changes of d13C in organic matter to support their interpretation (p. 1087 L. 12).

In the discussion, the authors state that the first (and global) CIE can be due to increased pCO2 that would be responsible for the dilution of the carbonate phase by increase detrital imputs. But increased pCO2 could increase weathering, but not necessarily erosion and detritic supply. Is the nature of the clays compatible with this suggestion? The authors additionally mention that increased pCO2 could generate lower preservation of the carbonate. They however mention that the preservation of coccolith (during the second CIE) is as good as in the rest of the section. Is this compatible with bad preservation during the first CIE? It also feels as if increased weathering and acidicifation are not necessarily compatible.

The paper reads fairly well but could be re-written in a much sharper fashion that would greatly help the reader (see detailed comments for some suggestions). The authors need to find a clear and straightforward terminology for the various CIEs/positive trends they mention, and stick to it. Figure 4 and 5 mention CIE 1 and 2, and this terminology is not used in the main text before 5.3.2. They should use "CIE 1" and "CIE 2" throughout the text. The figures are fine (axis and captions could be bigger depending on the final size of the figures), a bit repetitive however. They could add some of the environmental change on Fig. 4 and change Fig. 6 (and maybe Fig. 5) to a crossplot instead of a stratigraphic section. That would be more convincing and easier to read. A (modified) van Krevelen diagram could help instead of Fig. 5. In this regard, where does the %TOC-free come from (Methods) and is there an associated reference? Does is make

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sense only if the sedimentation is carbonate dominated or does it work also when the sedimention is siliciclastic or OM dominated? I would assume the sedimention here is siliciclastic dominated, wouldn't it make more sense to calculate a %TOCsiliclastic-free? Actually, it would be interesting to plot %TOC vs. %carb (Ricken 1993). In 5.3.2, a figure to show the attempted correlation with the Hermoso and Pellenard would be useful. Same for the reference to the Lézin study. These claims need better support in the present paper, otherwise, these two references are not informative. The authors should add the data as (supplementary) tables – or make them available to the reader.

Detailed comments p.1074 Abstract L. 2" of the worldwide" L.9: state that Bascharage is in the Lux. Sed. Area. I'm not sure you need to mention the "so-called Gutland. L.15: define T-CIE L.19 "expressed as four negative steps" p. 1075 the end of the abstract (l.1-4) is not useful, and not supported by the paper. I don't think integrated approach is a new idea. Introduction l. 14 untangle, not detangle l.20 minor portion, instead of tiny portion? l.21 "are recognized based on. . ." l.24-25: "with substantial organic carbon content" instead of "accumulation of OC" would make more sense, except it sedimentation rates are known.

p. 1076 l.10 "in addition" instead of "If this was not complicated enough" l.11 Remove "more broadly speaking" l.18 Remove somehow l.20 eastern instead of oriental l.27 "we characterize" or "we describe" instead of "we attempt"

The Bascharage composite section

p.1077 l.13: composed of l.14: what does "temporally exposed" mean? l.17: I'm not familiar with the notion of "conservative non-observational hiatus". Maybe this could be explained further. Methods

p.1079 l.9 How is reproducibility defined?

Results

p.1080 l.20 "deposits are characterized by low to extremely low carbonate content"

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p.1081 l.1 remove “comprised” l.3 remove “that is” l.20 rocks instead of rock L.21 Revert both sentences: :During the Pl., ratios are stable... The d13C values are strikingly negative...” Otherwise, this would mean that the first CIE is not reliable.

p. 1082 l.2 “When a carb. fraction reappears” is a strange sentence. l.20 are not as intense as l.26 performed instead of attempted. l.26: your statement is confusing. In the methods, you say that TOC were determined with a Rock-Eval machine. So, the Rock-Eval data are collected for all samples. Maybe you should write instead “We used the Rock-Eval characterization of the OM only for TOC content above ∼1%” l.27 Below instead of in advance

Discussion

p.1083 Here, you state that the OM is continental. Rock-Eval alone is not enough to be fully sure. Later (p.1085), you mention that you recovered wood fragments. Why not use this piece of information here instead of in paragraph 5.2? Did you observe these fragments, are they documented, or is it an observation from the literature?

p.1084 l.1 to 5: You can remove this part. There is no point in saying what the signal is not (or add references. Even if they are well-known interpretations, they still can use a reference). l.9 orange clays instead of orange clayed? L.20 the d13C values suddenly drop instead of “this composition...dropping” L.22 remove “in this stratigraphic level” L.22 “with the well-documented early Toarcian negative CIE (CIE 1 in Fig. 4) ” add references. I’m sure there are other studies than Rohl and Hermoso.

p.1085 l.23: the second CIE is within the serpentinum zone. l.25: add ref. about this positive trend. l.26: add ref. about the Med. sections.

p.1086 paragraph 5.3.1 is a bit misleading, suggesting that the present study contains new data about the preservation state of the calcareous nannofossils. Fig. 3 comes from Minoletti et al., 2009 (as clearly stated in the caption), this should be made clear in the text as well.

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p.1087 L.7: "during the second negative CIE" instead of "during the negative CIE" L. 9: remove firstly L.11: remove (organic)

Interactive comment on Solid Earth Discuss., 6, 1073, 2014.