

Interactive comment on “Massive and permanent decline of symbiont bearing morozovellids and $\delta^{13}\text{C}$ perturbations across the Early Eocene Climatic Optimum at the Possagno section (Southern Alps of northeastern Italy)” by V. Luciani et al.

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General comments

The manuscript by Luciani et al. ‘Massive and permanent decline of symbiont bearing morozovellids and $\delta^{13}\text{C}$ perturbations across the Early Eocene Climatic Optimum at the Possagno section (Southern Alps of northeastern Italy)’ provides detailed records (PF genera, C, O isotopes) of a 65 m thick deep water sequence spanning the lower to

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middle Eocene, including the EECO. Some additional data based on a similar but low-resolution dataset from Blake Nose Site 1051 are provided for comparison of the main observed patterns. The paper emphasizes the role of EECO on the evolution of mid latitude planktic foram communities, notably on the demise of the *Morozovella* which is observed both in Possagno and at Blake Nose, suggesting a general cause and effect relationship. This is somewhat reminiscent of the replacement of *Praemurica* by *Morozovella* some 10 million years earlier (Quilley and Norris 2003 – GSA SP 369), suggested to be related to climate change close to the Danian/Selandian boundary (Guasti et al. 2006 – Marmic; Jehle et al. 2014 – Ferrara volume)

Key issues

Although these records are valuable in the light of climatically linked evolutionary developments, there are a couple of fundamental issues to be resolved on some parts of the data of the Possagno sequence. These are mainly: 1) the reliability of the stable isotope record in identifying early to middle Eocene climate-related $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ excursions and 2) the reliability of the fragmentation index (%F).

The Possagno section largely consists of indurated pelagic limestones (red to grey Scaglia facies) with intercalated marls. The rocks were folded during Alpine compression phases and may have been deeply buried prior to uplift (no information is given on this). The key point to stress here is that these rocks are very different from age-equivalent sediments drilled by DSDP/ODP/IODP and that are commonly not lithified. This has several consequences: 1) the rocks analyzed here have gone through an extended diagenetic pathway during compaction, subsidence, compression and uplift. In sections exposed on land, meteoric influences may also have contributed to modifying the original geochemical signatures. Yet, despite of all these factors, the average obtained O and C values are roughly in the expected range for an early Eocene mid-latitude open marine record. This, however, does not mean that the observed rapid isotopic fluctuations are reliable recorders of paleoclimatic and paleoceanographic change (e.g. Speijer 2014 – Ferrara volume).

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Obviously, the authors are aware of the fact that diagenetic may have influenced the record. They note for instance that 'The oxygen isotope amplitude range shows up to 1.5‰ differences between adjacent samples, which possibly may reflect potential diagenetic overprint', but they fail to provide evidence of having scrutinized their isotopic data in any way. Shifts of 1.5‰ are much larger than any of the known Eocene hyperthermals (except for the PETM), so I can only conclude that there is a serious problem with at least part of the data. The authors chose to retain all data and employed a 3-point moving average prior to interpreting the data (although strangely the unsmoothed data are compared with the smoothed benthic record from Vandenberghe et al. in GTS 2012). Obviously this procedure leads to subdued fluctuations in the range of those known from the deep-sea (around 0.5 ‰). Smoothing noisy data based on variability of a natural system (e.g. seasonality) is fully justifiable in order to obtain the overall patterns of one or more sequences. However, smoothing data in order to reduce the influence of secondary artifacts in order to obtain a reliable primary signal that remains strongly dominated by rapid and large shifts is not appropriate. It's a sort of white-washing of unreliable data: in this way, the resulting subdued fluctuations in $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ are made acceptable and are key to the further interpretation.

There are various ways, directly or indirectly to evaluate potential diagenetic/meteoric overprint of the $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ data: 1) generating replicate data. Perhaps the authors did this already, but this is not clear from the text (the supplementary data file could not be retrieved). 2) through using cross plots for both systems. This indirect method is widely used. E.g. Corfield et al. (1991- Terra Nova) carried out a similar, but low-resolution, study on the Cretaceous-Eocene scaglia limestones of Central Italy and concluded that some intervals with strong co-variation of $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ are suggestive of a strong diagenetic and/or meteoric overprint. In contrast, the present authors choose the opposite approach: strong correlation between the most negative $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ values are indicative of true climatically related excursions. Note that there is clearly a caveat with this approach, since also in unaltered deep-sea deposits negative CIEs often correspond to negative OIEs (i.e. hyperthermals, like PETM, ETM2, C260

3, etc.), so primary and secondary signals may be difficult to distinguish. However, as long as diagenesis cannot be ruled out, it is safe not to infer a primary signal 3) using cathode luminescence on a subset of the samples provides an additional direct approach to discern secondary alteration. 4) In case there are levels with well preserved, non-infilled, planktic foraminifera these could be isotopically analyzed in order to provide a potentially more reliable surface isotope record to which the whole rock-record (mainly micrite derived from nanofossils?) could be compared.

In conclusion, the authors should provide solid arguments to exclude diagenesis or meteoric influences potentially causing the numerous isotopic excursions prior to relating the data to environmental and/or paleoclimatic shifts.

The second major issue concerns the amount of fragmentation of the foraminifera. In principle %F can be a very useful tool indicative of partial dissolution during or after sedimentation, commonly used in Quaternary research, where it concerns soft sediments. As non-filled foraminifera are also susceptible to mechanical breakage, the use of %F assumes requires gentle and uniform processing methods. In the current study, the foraminifera are derived from lithified marls and limestones and the various lithologies have been treated in different ways, limestones with cold-acetolyse, the marls with 30% H_2O_2 and gentle ultrasound. The authors indicate that these methods result in fairly well preserved assemblages. Yet, all three methods are known to potentially contribute to fragmentation, unless the foraminifera are consistently infilled with cement. Luciani et al. 2007 indicate that cold-acetolyse basically leaves the assemblage unaltered and this was tested against 'standard methods' (= using H_2O_2 ?). However, the 2007 paper does not reveal whether tests were run on the same samples nor whether H_2O_2 was used. So based on the limited information given, the outcome of that study seems to suggest that cold-acetolyse is less destructive than H_2O_2 , which in my view merely confirms that H_2O_2 should not be used in sample processing when quantitative data are to be generated (see also the excellent experimental comparative study by Kennedy & Coe, 2014 – J. Micropal). Since different methods have been used for

different lithologies and also the state of infilling is not discussed, the interpretation of the %F is not straightforward. Highest fragmentation is observed in the 14-22 m interval. This is the interval containing the marly beds. Is this fragmentation primary and does the marly deposit and the fragmentation relate to a rising lysocline, or are these samples predominantly processed with a potentially highly destructive H₂O₂ solution? This needs clarification.

In order to obtain a better grip on the value of the fragmentation index (%F), absolute numbers are needed (Nguyen & Speijer, 2014 – MarMic). Where %F covaries with reduced foraminiferal numbers per gram, fragmentation due to dissolution is very likely. Note that the weight of the sand fraction (or coarse fraction, e.g. Hancock & Dickens, 2005 – ODP 198) is in this respect less useful as this strongly depends on the cleanliness of the washed residues and whether the foraminifera are filled in or not. Planktics with infillings contain much more mass than an empty shells – multifold for planktics with highly inflated chambers. If virtually all foraminifera are infilled, especially if this is calcite cement, then this would be an advantage, because 1) the weight data of the sand fraction would probably strongly correlate with the numbers (unless shell sizes vary strongly) and thus provide a more accurate tool and 2) the foraminifera are much more robust against the various processing methods.

So the authors first need to clarify the state of infilling of the foraminifera (non-filled, to partially filled (%) to completely filled) in order to strengthen the interpretation of their carefully collected and large quantitative data set.

Further comments

p. 675: Note that the P/E boundary is now at about 56 Ma (Vandenberghe et al. 2012 – GTS2012; Hilgen et al. 2015 - Newsl. Strat.)

p. 682: Based on only these two presented dataset the authors conclude that morozovellids prefer open ocean settings more than areas close to land. This is poorly argued. There may be many other differences between Blake Nose and Possagno that

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could have caused this difference (e.g. current systems, seasonality, upwelling?). The abundance of radiolarians at Blake Nose may rather suggest that the pelagic ecosystem of these two areas is quite different, perhaps because of upwelling? In addition, Tethyan shelf sequences also often contain large numbers of Morozovella. At any rate, the authors must provide more convincing data (to be found in the literature) showing that in general morozovellids are more common in open ocean settings. They could or perhaps should include available information based on subtle differences in isotopic compositions of Acarinina and Morozovella.

p. 683: It might be useful to also plot radiolarian abundance (on the total CF?) for Blake Nose in order to observe any relationship with the planktic foram genera.

p. 686: Experimental dissolution studies suggest that subbotinids are generally more susceptible than morozovellids. However, (experimental) dissolution is in the first place strongly dependent on size and weight, favoring the preservation of large taxa, such as some Morozovellids. If subbotinids dominate assemblages with high %F, this could be a result from subbotinids being the larger taxa (can be easily verified) and/or that they are the most abundant taxa in the primary assemblages (e.g. at the top of the sequence).

p. 686: “When assuming that dissolution has affected assemblages, it follows that the dominance of acarininids during the EECO and hyperthermal events may represent a taphonomic artifact. This assumption appears yet to conflict with the results from the upper part of Possagno in the Chron C21n interval, where significant decreases of subbotinids, associated with distinct acarininid increases, correspond to negative shifts in $\delta^{13}\text{C}$ values in the absence of carbonate dissolution, as expressed in low F index values” This is a false argument. Similar to the previous comment: High relative acarininid abundance can result from partial dissolution of smaller taxa or result from a genuine ecological factor (e.g. oligotrophy). One reason does not exclude the other. Also here it boils down to failing information on foraminiferal numbers. These issues become much more transparent when relative abundances and %F are accompanied

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by absolute foraminiferal numbers (or even better PFAR – planktic foram accumulation rates - if age control is sufficient). This can reveal whether relative increases are true increases or just resulting from decreases of other taxa (closed sum effect)

p. 688: “The hypothesis of increased nutrient availability in the lower part of the EECO interval at Possagno is supported by the entry of relatively high concentration of radiolarians, considered as eutrophic indices (Hallock, 1987).” This might be correct, but this begs for an explanation for the fact that Blake Nose yields high numbers of radiolarians throughout the sequence, whereas the planktic foram record is overall similar to the one of Possagno.

p. 689: explain what is meant by ‘muricate crisis’.

p. 690: replace ‘bleaching’ for ‘symbiont loss’. Bleaching relates to corals losing their colorful photosymbionts. As far as I know this term is not used for symbiont loss in modern planktic foraminifera.

p. 691: ‘It would be interesting to compare flux data of calcareous nannofossils before and after the major evolutionary change recorded across the EECO (Agnini et al., 2006; Schneider et al., 2011) to test a potential reduction in their overall productivity’. As indicated above, this could and in my view should have been done for the planktic foraminifera too (as secondary producers) and could resolve some of the problems addressed.

p. 712: It should be stressed in the caption that the long-term record is a benthic record and not a pelagic/whole rock record like Possagno’s. Note also that the figure is from the chapter of Vandenberghe et al. 2012 in Gradstein et al. 2012.

All in all, this paper still needs quite a bit of work to make it an important contribution to the field.

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