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Interactive comment on "Massive and permanent decline of symbiont bearing morozovellids and δ^{13}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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Key issue

1) The reliability of the stable isotope record.

We are aware that oxygen data from onland sections can be, at least in part, unreliable due to diagenesis and we already discuss about that in the ms. On the contrary, car-

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Interactive Discussion

Discussion Paper

bon data produced in on-land sections are overall considered more reliable because the carbon isotope system has proved to be less prone to diagenetic processes with respect to oxygen. The Possagno section is in the same geological setting - a relatively UNDEFORMED sector of the MODERATELY tectonized Southern Alps- of other published Eocene sections yielding a good isotope and calcareous microfossil record that well correlates with ODP sites (e.g. Forada, Farra, Alano, Giusberti et al., 2007GSAB; Agnini et al., 2009, Paleoceanogr.; Agnini et al., 2011, GSAB; Spofforth et al., 2010 Paleoceanogr.). Stable isotope records of on-land sections have been published in high–ranking journals (included papers in which the referee is coauthor), if what the reviewer wrote is correct then we should put in the trash most of the data produced in, at least, the last two decades. That's a choice but we instead consider that this archive is crucial to capture what occurred in paleoenviroments other than the open ocean areas. The integration of all the available data is the only way to have a global view of the paleoclimatic, paleooceanographic and paleoenviromental changes happened in the Earth's history. For these reasons, it is irrational either to believe that only the isotope records from ocean sites (which are not free from problems such as microfossils recrystallization, condensation/hiatus...) are useful or to think to throw away all data from onland sections. As for the meteoric influence we can exclude its effect since samples were taken from fresh quarry cuts and we adopted all possible cautions both during sampling and laboratory preparations. However, we agree that the absolute values of the recorded shifts could be, at least in part, influenced by diagenetic processes but, as documented in many available isotopic profiles (see reference above and many others) the general trends recognized the reference oceanic sites are perfectly consistent with on-land data, that is the shifts are real. This is why we decide to correlate our data with the Demerara Rise isotopic record. In the matter of duplication of isotopic analyses we did not systematically replicate isotopic analyses and we are not going to do it now. To reassure the reviewer about the quality of the data we would like to underline the good reputation of the Stockholm isotope laboratory and, again, the special attention we deserved to prepare and analyze the samples. The moving average is a

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commonly used practice to fix irregular fluctuations in a series; we however show also the unsmoothed data.

Regarding the magnitude of the observed isotopic excursions, we admit that some of the recorded shifts are very small. Nevertheless, we have tentatively named them as 'events' only when changes in isotopic composition are associated with sharp variations in planktic foraminferal assemblages and/or fragmentation index. This is because the increase in fragmentation index as well as the increase in abundance of some taxa have been observed during early Eocene hyperthermals (e.g., PETM, ETM2) at a regional scale. We agree that the identification of some minor shifts such as C21-CIE3 are tentative. We thus decide to add a question mark in the figures. The carbon isotopic shifts sometimes show amplitudes smaller than expected. This could be explained with relatively low sample spacing not close enough to catch the maximum magnitude for each event. This view is also supported by the fact that, in the lower part of the section, where resolution is even lower, some 0.3 per mil shifts nicely correlate with the globally known hyperthermals. In principal we thus cannot exclude that some of minor carbon-isotope shifts observed for the first time at Possagno above the EECO are in fact global and/or that they are true events. Concluding, in our opinion, a further solid motivation for suspecting some of the minor shifts as events is our foraminiferal record. These data mime the variations as recorded from the unquestionable pre-EECO hypethermals in very close published sections belonging to the western Tethyan domain (e.g., Luciani et al., 2007 MarMic; Agnini et al., 2009 Paleoceanogr.). It would be more difficult to explain the recorded biotic changes at Possagno as occurring in correspondence of pure diagenetic, accidental shifts. All these lines of evidences support the decision of not taking into account the suggestions of points 3) and 4). The achievement of new highly-resolved isotope data is planned from an incoming, more detailed, analysis of Site 1051.

As far as the second main issue is concerned (reliability of fragmentation index and coarse fraction), we have discussed in other papers about the negligible influence of

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acetolyse methods on foraminiferal fragmentation. Of course Luciani et al. (2007, MarMic) tested the influence on fragmentation by using the H2O2 and acetolyse methods on the same samples: otherwise it would not have been a reliable test. Not clear to us how the referee deduces that the H2O2 method is even more destructive for foraminiferal tests. If that was true, most of the published foraminiferal data from onland sections (not only from the Italian Scaglia facies of Southern Alps) would be unreliable. Since the two different preparation techniques we used are not regularly distributed throughout the Possagno section (with most of the sample prepared with acetolyse), the greater foraminiferal fragmentation should casually distributed, on the contrary the fragmentation index increases in correspondence to the isotope shifts. In particular, at Possagno, the interval between 14 and 22 meters does not consist just of marly lithologies as the referee claimed. Indurated limestones are present and we prepared the investigated samples both using cold acetolyse and H2O2 methods. High F-index values characterize both marly and carbonate samples, and this, in turn, implies that the preparation technique seems not to affect the preservation of the fossil microfauna. More likely high fragmentation-index values result from genuine paleoceanographic mechanism. A similar record (great fragmentation index coinciding with carbon isotope shifts) occurs also for other cited sections from the Belluno Basin during warming events, e.g. from the MECO at Alano (Luciani et al., 2010P3), where the standard method was adopted (hydrogen peroxide) and from the PETM record of the Forada section (acetolyse method; Luciani et al., 2007 MarMic). Specifically, at Alano, samples were washed including the finest fraction (between 38 and 63 micron): if the H2O2 method is so much corrosive as the referee believes, the smaller forms should not have been preserved. On the contrary, they are well preserved and abundant so that their fluctuations in abundance give significant paleoecological information (Luciani et al., 2010, P3). However, we recognize and briefly discuss in the text that the interpretation of a huge rise of the lysocline is problematic considering the middle-lower bathyal setting where the succession was presumably deposited. Regarding other preparation techniques: the freeze-thaw method cited for foraminiferal extraction (Kennedy and

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Coe, 2014) refers to very different lithologies, organic-rich indurated Jurassic mudrock from the Toarcian OAE. In addition, the method implies to boiling the samples thus preventing any possibility of isotope analysis. Freeze-thaw and boiling were very old methods abandoned for a long time; it is possible, however, that they might give good results as in the case presented in the cited paper.

As for the absolute counts of foraminifera (number per gram), it would be obviously indicative of absolute and not relative changes among the populations. This method is generally adopted for benthic forms, which are rare and large-sized with respect to planktic tests in deep-water successions. Such a count for planktic foraminifera would consume a huge amount of time due to the huge number of tests, even adopting a quartering as small as possible, and also considering the high total number of samples analyzed in this paper (over 150). The preservation of foraminifera is similar throughout the section "varying from moderate to fairly good", thus assuring a reliable taxonomic identification even though test are recrystallized and essentially totally filled. This refers to the endless disputation on the preservation vs dissolution/overgrowth issue. We have added this information at p.678, line 2. This should therefore further strengthen on the reliability of our data with respect to the F-index. Moreover, the cleanliness of washed residues cannot have influenced the coarse fraction count because it was very carefully executed. Finally, we define in the text the ultrasonic treatment as 'gentle' (e.g., low-frequency at 40kHz for 30–60 seconds, see Luciani and Giusberti, 2014 JFR) precisely because it was aimed to clean and not to induce mechanical breakage (samples were always checked before and after the treatment).

p.675: OK, the text has been corrected accordingly.

p.682. OK, thanks for the observation, we have added some references here (see below) specifically referred to the Atlantic Ocean. However, our sentence was simply intended as an observation and not as a general conclusion. The large number of morozovellids in some Tethyan shelf sequences could be explained with the reduced water column in shelf environments that favor the surface-water dwelling with respect

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to the deeper-dwelling forms. Line 27: . . . “indicate a morozovellid preference for open ocean settings of low-latitudes, as suggested also by other authors (Berggren, 1978 Micropaleont.; Boersma et al., 1987, Paleoceanogr.; Premoli Silva and Boersma, 1988 P3).

We have added some references supporting the small differences in isotopic composition between morozovellids and acarininids but we think that the best position is paragraph 5.3, p. 687, Line 27: “Slight differences in paleobiology between morozovellids and acarininids are documented in several cases by minor variations in stable isotopes that commonly indicate a more surface habitat for the former group (Boersma et al., 1987a,b, Paleoceanogr., P3; Pearson et al., 1993JFR; Pearson et al., 2001 Nature).

p. 683: Radiolarians abundance was not shown because it has been estimated with a lower resolution with respect to Possagno.

p. 686: Not clear this part: subbotinids are generally more abundant at Possagno where the F-index is low, with some exceptions. If dissolution favors the preservation of the larger sized forms we cannot explain the decrease of large morozovellids as a merely consequence of dissolution.

p. 686 second part: see long discussion above about the second general issue.

p.689 not clear this request about the explanation of ‘muricate crisis’: it is clearly described that specific steps in the evolution of the two main groups among the muricate forms, acarininids and morozovellids, are characterized by their significant reduction in abundance and size.

p.690: ‘bleaching’ is commonly used also for foraminifera (e.g., Hallock and Talge, 1993 Proc. colloquium on global aspects of coral reefs: Health, hazards and history,Miami, Florida; Hallock et al., 2006 Proc. 10thInt. Coral Reef Symposium, Okinawa, Japan; Wade et al. 2008 Evol Ecol; Schmidt et al., 2011 Limnol oceanogr; Edgar et al. 2013 Geology).

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p. 691: OK, good suggestion for a possible future study but with reference to a limited number of samples. . ...

p. 712: Thanks for the suggested corrections.

Interactive comment on Clim. Past Discuss., 11, 671, 2015.