Biogeosciences Discuss., 9, C188–C190, 2012
www.biogeosciences-discuss.net/9/C188/2012/

Biogeosciences
Discussions

Interactive comment on "Scaled biotic disruption during early Eocene global warming events" *by* S. J. Gibbs et al.

Anonymous Referee #1

Received and published: 28 February 2012

The paper by Gibbs et al deals about changes in calcareous nannofossil assemblages across the lower part of the Early Eocene, when the Hyperthermals ETM1, ETM2 (H1), H2, I1 and I2 occurred. The manuscript is well written and organized and figures are nicely drawn and informative. In this ms, the authors propose a method to quantifying biotic variability in nannoplankton population. This method uses calcareous nannofossil abundance data at genus level; this approach combines the use of a best smoothed fit software (i.e., SiZer) with a quantitative assessment of assemblage variability (ÆĨCV). In principal, this method is quite interesting because it utilizes standard dataset (relative abundance) and eventually obtains an estimate of nannofossil variability trough time by means of very simple calculations. However this approach has, at least, two weak points that are basically related to the dataset used: - The first comes from the choice to use relative abundance data. As evidenced by Gibbs et al.

(2010): "because counts always total 100, it is not possible to say whether changes in relative abundance actually represent a real or absolute increase or decrease in the abundance of an individual taxon". - The second comes from the choice to merge together data collected at species level. The final result is a starting dataset based on calcareous nannofossil relative abundances at genus level. This step results in a loss of information that could bias the final output. In fact, the changes in relative abundances at genus level cannot reflect the changes in abundance at species level, the increase or decrease of a single species as well as the appearance or disappearance are not considered in this method. For instance, the appearance of new species belonging to Sphenolithus and their changes in relative abundance close to ETM2 are not included in the nannoplankton variability evaluation and this could produce a final underestimation of the actual modification. To me these points are major issues and should be resolved or at least highlighted in the text. Some other issues are posted throughout the text: Page 1237, Line 7: "quantify marine biotic variability. . .". I suggest the authors to understate a little, what they actually do is to estimate the variability of calcareous plankton (mainly calcareous phytoplankton) assemblages. Page 1238, Line 20: Your references here do not take in account recent important contributions focused on Early Eocene Hyperthermals such as these (Lunt et al., 2011, Galeotti et al., 2010; Agnini et al., 2009; Stap et al., 2010). Page1238, Line 26: CIE, write in full. It is the first time you use this acronym in the text. Page 1240, Lines 1-4: This point is not clear to me. The authors wrote that CIEs are found in correspondence to clay-enriched dissolution intervals, and then stated that the lysocline has not risen to the paleodepth of the site. Which is the evidence? This is based on $CaCO_3$ content, calcareous nannofossil and/or planktonic foraminiferal assemblages or whatever. . .. Page 1242, Lines 1-3: Again. . ..the analysis at genus level might be not enough detailed. In principal, the relative abundance pattern at genus level can be quite smoothed and do not evidence for any significant modification. By contrast, if we observed the relative abundance at species level, data can tell a very different story. See also discussion about species specific affinity treated in Dunkley-Jones et al. (2008; p.11). Page 1242, Lines 7-8: In the following lines, the authors try to explain why fasciculiths dominate the signal in a specific portion of the record. Why Coronocyclus and Zygrhablithus do the same in different portions of the record? Page 1242, Lines 9-10: Your method points out that the relative abundance pattern of Fasciculithus drops down at the P/E boundary. The loss of species richness is not taken into account, because you use the relative abundance at genus level. This likely results in a possible underestimation of the biotic changes occurred during this perturbed interval. Page 1242, Line 26: What do you mean with obvious? This issue is the focus of much debate in several K-Pg papers. Please explain. Page 1244, Lines4-5: Up to my knowledge (e.g., Bukry, 1973, Edwards and Perch-Nielsen, 1975; Wei and Wise, 1990; Monechi et al., 2000), Z. bijugatus is considered a quite fragile holococcolith, though this issue is far to be solved. Dissolution susceptibility proposed by Gibbs et al. (2010) for this taxon can not be considered conclusive. Page 1245, Line 13: In Figure 6, you plot the abundance pattern of Z. bijugatus. If Z. bijugatus is more prone to dissolution (than you proposed) then the calcareous nannofossil changes exactly coincides with the onset of the dissolution interval. The estimate of dissolution susceptibility is still an open issue, especially for taxa such as the holococcolith Z. bjiugatus. Page 1246, Lines 14-17: If this phrase refers to your data you should use genera instead of species. In addition, what you observe is not surprising because dominant taxa (such as Coccolithus and Toweius) are highly tolerant cosmopolitan genera able to survive in altered environmental conditions. Page 1247, Line 17: The hypothesis of a possible threshold has been already proposed (see for instance Lunt et al., 2011; Stap et al., 2010; Agnini et al., 2009). Page 1247, Line 23: This is very likely. . .during Plio-Pleistocene glacial-interglagial cycles the estimate change in temperature is of about 4 °C and no dramatic changes is observed in calcareous nannofossil assemblages.

Interactive comment on Biogeosciences Discuss., 9, 1237, 2012.