

REPLY TO REVIEWER #1

1. Page 692, line 26: You use a duration of 12 Myr for the Aptian (Malinverno et al., 2012), whereas Gradstein et al. (2012) indicate a duration of 13.3 Myr for the same stage. Could you comment on this discrepancy?

The discrepancy mainly depends on two factors: 1) The timescales of Malinverno et al. (2012) and Gradstein et al. (2012) are constructed using different approaches. Malinverno et al. (2012) updated the Channell et al. (1995) M-sequence geomagnetic polarity timescale by incorporating marine magnetic anomaly records from several spreading centres worldwide (Tominaga and Sager, 2010), the radiometric age of magnetochron CM0 (He et al., 2008), and astrochronology-based estimates of the duration of the CM0-CM3r interval (Fiet and Gorin, 2000; Malinverno et al., 2010). The Gradstein et al. (2012) timescale is a revision of the GTS2004 (Gradstein et al., 2004), incorporating new methods and data, improved resolution and accuracy of radiometric dating, and stratigraphic standardization of stage and series boundaries.

2) The different ages attributed to the Barremian/Aptian boundary. The two timescales provide ages for the base of the Aptian (equated to the base of magnetochron CM0) that are 4.8 million years apart. This difference mainly derives from cyclostratigraphy-based duration of the Aptian.

We adopt the timescale of Malinverno et al. (2012) because Aptian ages are consistent with the radiometric age of magnetochron CM0 (He et al., 2008) and the Re-Os age of 120.4 ± 3.4 Ma for the base of the Selli Level (Bottini et al., 2012). Further data and discussion are provided by Erba et al. (in press).

2. Page 693, lines 17-19: You mention that the preservation of calcareous nanofossils provides information on the diagenesis of the studied carbonates. Also, Erba et al. (1999) gave some insights on the diagenetic state of one of the location you are including in your survey - Cismon. It may be worth mentioning their arguments.

The possibility that observed nanofossil changes in abundance and species richness derive from diagenetic modification is real. Assessment of dissolution-overgrowth of individual taxa and/or total nannoflora must be carefully pursued. Indeed, previous

studies of the selected sections include a detailed evaluation of type and degree of diagenetic modifications. As summarized by Erba et al. (2011) nannofossil changes across OAE 1a cannot be ascribed to differential diagenesis, as detailed in Erba (1992a, 1994), Erba et al. (1999), Erba and Tremolada (2004).

The Reviewer is correct in requesting a more detailed description of nannofossil preservation to disentangle primary from diagenetic signals in abundance changes. Accordingly, we have introduced the following paragraph at the beginning of chapter 4.1:

*Calcareous nannofossils are generally common to abundant in the studied sections and characterized by moderate preservation, with negligible evidence of recrystallization in limestones and dissolution in black shales. Only a few samples in the lower part of the Selli Level are barren, possibly due to dissolution. The detected abundance fluctuations of dissolution-resistant and dissolution-prone taxa indicate that diagenesis cannot be responsible of changes that are similar and synchronous in different oceans, and occur within the same lithology, yielding identical preservation. Specifically, nannoconids are most resistant to dissolution/diagenesis and their decline, crisis and acme are proved to be independent from preservation. In addition, as pointed out by Erba et al. (2011) the abundance of delicate and dissolution-prone species (e.g. *B. constans*, *Z. erectus*, *D. rotatorius*) is opposite to what diagenesis would produce.*

*In all three sections investigated samples are characterized by relatively high percentages of *Watznaueria barnesiae*. This taxon has been considered a dissolution-resistant species, and assemblages containing more than 40% are thought to be heavily altered (e.g. Thierstein and Roth, 1991). However, percentages higher than 40% of *W. barnesiae* may preserve a primary signal in oceanic settings, and/or in oligotrophic conditions (Roth and Krumbach, 1986; Erba et al., 1992; Williams and Bralower, 1995; Herrle et al., 2003; Mutterlose et al., 2005). We believe that the relatively high percentages of *W. barnesiae* in the studied sections preserve a primary signal.*

3. Page 694, line 9: I suggest rephrasing as 'before, during and after the OAE1a', since your results from Cismon and DSDP Site 463 cover the latest Barremian / earliest Aptian time interval (segments C1 and C2).

We have changed the sentence accordingly.

4. Page 699, line 10: Although you described in the text how you refined the isotopic segments of Menegatti et al. (1998) and Herrle et al. (2004), and although you reported these schemes alongside with yours on your figures, I am wondering if an additional figure showing the stratigraphic extent and relationship of these three chemostratigraphic schemes with biostratigraphy would not be beneficial to the reader.

We have revised figure 1.

5. Page 699, line 20: Capital letter in 'Kilian Level Equivalent'.

We have corrected the text accordingly.

6. Page 700, line 11: Ap4/C4, the second '4' is missing.

We have corrected the text accordingly.

7. Page 705, line 4: How were these affinities defined? With respect to chemical, sedimentological proxies? It would be worth mentioning this in one introducing sentence, for readers who are not familiar with this type of approach.

In the last decades several Cretaceous marine sequences, located at different latitudes and in different paleogeographic settings, have been investigated in order to reach a better understating of the palaeoecology of calcareous nannoplankton (e.g. Roth and Krumbach, 1986; Premoli Silva et al., 1989b; Thierstein and Roth, 1991; Erba 1992; Street and Bown, 2000; Herrle et al., 2003; Mutterlose et al., 2005; Tiraboschi et al., 2009). Based on quantitative and statistical analyses (Factor Analyses, Pearson's Correlation Coefficients) as well as the paleogeographical characterization, and independent geochemical and sedimentological proxies, the paleoecological affinity of some nannofossili taxa has been reconstructed.

We changed the sentence at page 705 line 4 as follows: *Mutterlose et al. (2005) reviewed the paleoecological affinities of some mid-Cretaceous taxa synthesizing major studies that allowed the identification of species related to paleofertility and*

paleotemperatures of surface waters. More recently, Herrle et al. (2003) have proposed two indices: the Temperature Index (TI) and the Nutrient Index (NI), successfully applied to Albian interval of the Piobbico core (Tiraboschi et al., 2009).

8. Page 707, line 17: Usually, d18O can be interpreted in terms of temperature or salinity fluctuations. Why do you think your record only reflect paleotemperatures? I would be more cautious and slightly rephrase this statement.

To clarify the interpretation of the $\delta^{18}\text{O}$ curves, we would revise the statement as follows:

Although $\delta^{18}\text{O}$ is also controlled by salinity fluctuations, there is no independent evidence of significant salinity changes during the Aptian. As discussed by Weissert and Erba (2004), Early Cretaceous oxygen-isotope records show similar changes at various settings and in different oceanic basins. All data collected so far suggest a positive correlation between $\delta^{18}\text{O}$ trends and paleotemperature reconstructions based on paleontological and/or geochemical (e.g. TEX86) data. Conversely, there is no reliable information of significant paleosalinity changes.

Moreover, the studied sections are pelagic in nature and salinity changes may be an issue in more restricted basins but not in open oceanic environments.

9. Page707, line 23: How did you come up with the duration of 35kyr? Is it from Malinverno et al. (2012)? Please specify.

The duration of this interval is based on the work of Malinverno et al. (2010). This interval was characterized by a marked increase in weathering rates (Bottini et al., 2012) and by a temporary decrease in temperature (this work). We have now added the citation.

10. Page 710, line 22: I find it hard to understand which part of the stratigraphy you are dealing with here. After using the isotopic segments Ap3/C3, you are now referring to intervals of temperature. Would it be worth reminding the isotopic segment between brackets, or having a synthetic figure somewhere with biostratigraphy, the different chemostratigraphic schemes and

these paleotemperature intervals (see my previous comment 4)? It could be more reader-friendly.

Accordingly, to make the text more reader-friendly, we added the isotopic segments between brackets as follows:

The lowermost data point, which corresponds to Interval B (core of segment Ap3/C3), indicates an SST of ~22°C which is the coolest value for the studied interval and well matches with cooler conditions reconstructed from other data. The SST values for the following three data points are rather puzzling: two indicate temperatures of ~23–25 °C and fall in Interval C (end of segment Ap3/C3) - the warmest of OAE 1a - while the third data point shows almost 27°C although it falls in Interval D (segment Ap4/C4), interpreted to correspond to a time of relative cooling. The rest of the samples, encompassing Intervals E to H (segment Ap5/C5), and representing minor temperature fluctuations, fall between 25°C and 27°C. We identify one more discrepancy in the relatively low estimated SST (22.5 °C) for one sample falling in Interval H (onset of segment Ap6/C6), suggested by TI and oxygen isotopes to be a relatively warm interlude.

The requested figure (comment 4) has been already provided.

11. Page 711, line 27: Would there be any other analysis that could be performed to assess this thermal maturation? Rock Eval pyrolysis? Abnormal d18O values?

In our opinion, other measurements for assessing thermal maturity are not needed, since the biomarker ratios are convincing. More importantly, they change over a short interval and thus do not indicate that the sediments themselves are mature but that the organic matter is allochthonous.

12. Page 712, line 10-15: I like the fact that you propose three reasons for these unusual TEX86 values. However, is it possible to decipher which one had the strongest impact? I would like to see, even in one short sentence, why some of these mechanisms are unlikely to have impacted the reliability of your TEX86

signal.

Most probably the Reviewer meant page 711, line 20-30.

We have improved this part of the discussion and added a comment regarding the factor which more realistically affected the TEX86 signal, the part has been revised as follows:

For Cismon, also the highest (coolest temperature) $\delta^{18}O$ values are $\sim 1\%$ greater than those registered at DSDP Site 463 and $\sim 0.5\%$ greater than those at Piobbico. Generally cooler temperatures for Cismon could be explained by different latitudinal settings, the Cismon site being at $\sim 30^\circ N$, the Shatsky Rise at an almost equatorial position and the DSDP Site 463 at $\sim 20^\circ S$. However, this seems not to apply to the Boreal section ($39^\circ N$) characterized by the highest ($\sim 35^\circ C$) SST. It has been shown in several modern settings that TEX86, although calibrated against sea-surface temperature, may sometimes reflect changes in subsurface water temperatures as well (e.g. Huguet et al., 2007; Lopes dos Santos et al., 2010), possibly because the source organisms, Thaumarchaeota, also reside in the deeper thermocline where nutrients such as ammonia might be available. Perhaps the most likely explanation for this discrepancy may be that the TEX86 values from the Cismon core are already affected by the higher level of thermal maturity (i.e. hopane 22S/(22S+22R) ratios of 0.1–0.2). It has been documented that destruction of GDGTs during thermal maturation processes results in lower TEX86 values due to the fact that GDGTs with cyclopentane moieties are thermally less stable (Schouten et al., 2004).

13. Page 717, line 3: Are there any periods of the geological record where such a correlation between zooplankton evolution and volcanism can be observed? If so, I think it would make your argument even stronger.

Yes: there are other Cretaceous cases of zooplankton changes (abundance, diversity, and evolution) and major volcanic-tectonic events. We have added the following sentence:

Similar responses of calcareous zooplankton to inferred ocean acidification were documented for OAE 1a and OAE 2 with major decrease in planktonic foraminiferal

abundance and diversity as well as evolutionary turnover (Premoli Silva et al., 1989b, 1999; Coccioni et al., 1992; Leckie et al., 2002).

Figures:

- **Printed as they are, the figures are too small. Make sure they will be reproduced as full-page figures, if this is not your intention, then increase font size. Especially your figure 6 is not readable as it is.**

We have increased the font size accordingly and we checked that the figures are readable on a “double column page” of CP.

- **Caption for Figure 8: your may want to add an ‘h’ to ‘cyclochronology’.**

We have corrected the text accordingly.

- **Figure 8: you report a methane release just before isotopic segment C3. Since you have cyclostratigraphy, can you estimate the offset between these two phenomena?**

There was a mistake in the figure because the methane release correlates with C3 and specifically shortly precedes the lowermost values of the $\delta^{13}\text{C}$ coeval with the Os spike. We have revised the figure accordingly.

As far as estimates of offsets are concerned the discussion is provided by Bottini et al. (2012).

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