

Interactive comment on “Bio-chemostratigraphy of the Barremian–Aptian shallow-water carbonates of the southern Apennines (Italy): pinpointing the OAE1a in a Tethyan carbonate platform” by M. Di Lucia et al.

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Dear Editor, dear authors,

Thank you for giving me the opportunity to review this ms. Personally, I am very interested in the topic of shallow-water OAE1a research and hence a fundamentally positive (but perhaps equally sceptical) reviewer. Di Lucia et al. provide a combined biostratigraphic and chemostratigraphic study of three Barremian–Aptian carbonate sections from the southern Apennines of Italy. The authors propose that the Selli Level of basinal

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organic-rich sediments corresponds to a stratigraphic interval between the “Orbitolina level” and the second acme of *S. dinarica*. I realize that the focus of this paper is mainly on the stratigraphy. On page 809 (line 10–11), the authors state that the paleo-ecological interpretation is the topic of a companion paper that must be based on this stratigraphic scheme.

In essence I find this a well written and mostly concise paper and I support publication pending some revisions that are moderate in nature.

Below I detail my comments:

Abstract:

Carbonate platforms are, depending on the setting, riddled by stratigraphic hiatus surfaces commonly due to subaerial exposure and karstification or subaquatic non-deposition and erosion (elevated hydrodynamic level). Some authors proposed that up to 90% of the time contained in these sections is in fact represented by discontinuities. This is perhaps an extreme end-member scenario but even 40–50% of a given time interval condensed in hiatal surfaces represents a major problem. I guess this should be mentioned as it represents – in my view – one of the main problems related to platform top chronostratigraphy and platform-to-basin correlations. Even with a superior stratigraphic tool (that we still lack), the correlation of hiatal sections (platform) with more or less complete ones (basin) is a less than trivial matter.

I am perhaps overly sceptical, but I politely doubt the stratigraphic value of the so-called “Orbitolina level” except when accepting a significant error bar in time. My experience with middle Cretaceous platforms, notably in the Middle East, has confronted me with regionally limited “Orbitolina levels” that graded laterally into Orbitolina lean facies in nearby sections over distances of some tens of kilometres. A recent paper (Huck et al. 2011) dealing with sections in France, has equally lead us to conclude, that care must be taken when Orbitolina facies is used as stratigraphic marker. In my view, the main obstacle is our limited knowledge of the paleo-ecological factors that control the

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presence and abundance of orbitolinid limestones. I realize that the authors summarize their critical view in this question on page 810.

3. Materials and Methods:

3.2. Stable isotopes: I might be pedantic, but science knows hundreds of “stable” isotopes. Why not telling the reader that you refer to the $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ isotope systems?

3.3. Strontium isotopes: With an increasing number of laboratories exploring the potential of “stable” strontium (i.e., $\delta^{88/86}\text{Sr}$), it might be a good idea to mention that you refer to $\delta^{87/86}\text{Sr}$.

4. Results:

Page 795, ln. 13: A minor point, I guess “Formation” (Calcari con Requenie e Gasteropodi) should be in upper case?

5. Discussion: 5.1 Reliability of the $\delta^{13}\text{C}$ record

Page 805, ln. 14-25: The authors present a rather “classical” view of the Allan and Matthews (1982) model here. Whilst the opinion expressed in the ms under review is true to some degree, the story is perhaps more complicated. Yes, under moderately humid climate, exposure surfaces are characterized by depleted soil-zone CO_2 . But no, this is often not the case in the case of mainly mineralic soils and/or under arid to semi-arid climate. Yes, the vadose zone is characterized by isotopically depleted oxygen values under moderately humid conditions but no, this might not be the case in a near-coastal setting. Rainwater is increasingly depleted in ^{18}O with increasing distance to the marine source and increasing altitude. The situation is therefore more complex in near-coastal and/or evaporitic settings that may lead to invariant or even ^{18}O enriched values. A few statements regarding the paleo- climatic setting of the locations under study might be usefully here.

I find the statement: “This suggests that variations in $\delta^{13}\text{C}$ cannot be related solely to

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facies change (page 806, ln. 2-3)” less than helpful. This as elsewhere, the authors clearly express their view of the superior “chemostratigraphic” approach in shoal water sections. Please consider.

5.2. Platform-to-basin chemostratigraphic correlation

Similarly to my last comment, I find the first statement (page 806, lines 26 etc.) difficult and in contrast to previous statements. Particularly the remark: “. . . was not entirely shaped by local change. . .” is in clear contrast to the overall key points of this ms. I welcome the fact that the authors treat their data critically and consider the complexity of the tools chosen. Nevertheless, “not entirely shaped by local change” in my view translates into something like “80% local versus 20% “global” signal. If this were the case, then the conclusions presented here must be questioned. Please consider.

Page 807, ln. 21: Please use late/early/middle for time (age) and upper/lower/middle (Aptian) for rocks. A Late Aptian (Gargasian) age for. . .

Page 810, ln. 3 and following: The opinion that the Orbitolina level is a solid stratigraphic marker is brought forward here once more. This notion probably represents widely held opinion. As mentioned before, I find that this concept requires a re-evaluation as stratigraphic and paleo-ecological factors intermingle in a complex manner. The authors seem to partly agree with my scepticism as laid out on page 810, line 25 and following. But they do so after they have left the reader with the impression that they basically accept the above standard concept. I find this confusing.

In conclusion: This is an interesting and critical paper. My comments might encourage the authors to become even more critical in some of the key aspects mentioned. I find, but this is my opinion only, that the text is in parts somewhat confusing as classical concepts are first applied and then questioned, re-applied only to become criticized again on the next page. Please consider!

I hope these comments are of use? My compliments to the authors for a nice and

topical paper.

Bochum, 22.09.2011

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Interactive comment on Solid Earth Discuss., 3, 789, 2011.