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***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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Received and published: 26 September 2011

Dear Adrian, thanks a lot for your interest in reading our manuscript. We really appreciated your careful review. Your comments will greatly contribute to improving our paper. Please find below a discussion of your comments with an indication of how we intend to accommodate your suggestions in the revised version of the manuscript:

**Abstract:**

Referee Comment (RC) - “Carbonate platforms are, depending on the setting, rid-

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dled by stratigraphic hiatus surfaces.... 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”.

Author Comment (AC) - We perfectly agree with you. Incompleteness of carbonate platform successions is perhaps the ultimate problem hindering correlation with deep-water sections and the chronostratigraphic scale. Obviously, there is no room in our paper to deal exhaustively with this topic, but we are going to add a short hint in the abstract and a paragraph in our discussion. The main point that we will do is that in our attempt to correlate incomplete carbonate platform  $\delta^{13}\text{C}$  curve with complete reference curves of hemipelagic successions, the only gaps that really matter are those that are so long as to completely obliterate a major feature of the curve. Let's take the OAE1a carbon isotope excursion (CIE) as an example. According to Li et al. (2008) its total duration (C3-C6 stages) ranges between  $\sim 1.0\text{-}1.3$  Myr. Obviously, if there is a  $> 1\text{ Myr}$  gap in our section at exactly the “right” point we will completely miss the CIE. With a gap of shorter duration the excursion will probably still show up in the  $\delta^{13}\text{C}$  curve, albeit incomplete, i.e. distorted (different, more asymmetric shape) or with a subdued peak. But what would be the effect of many, “regularly” distributed, short gaps (let's say each a few ky)? Only features of the CIE that last a few kyr have a chance to be completely obliterated but the CIE will still show up in the record. So the problem basically is how to estimate the duration of the gaps. We took a somewhat pragmatic approach: 1) the reference carbon isotope curve is taken as complete; 2) if a section shows a CIE which is similar in “shape” and absolute value and occurs in the “appropriate” position (based on biostratigraphy or other independent methods, i.e. strontium isotope stratigraphy), we conclude that gaps are not a problem for this section; 3) we use lithostratigraphy, biostratigraphy and, again, carbon isotope stratigraphy, to constrain the position and duration of gaps in less complete/more incomplete sections (see pp. 806-808 of our manuscript).

RF - “I am perhaps overly sceptical, but I politely doubt the stratigraphic value of the

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

AC - Thanks a lot for your comment. The points you raise certainly need to be clarified and we will do this in the revised version of the manuscript. Please see the short discussion given below.

### 3. Materials and Methods:

RC - 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  $\delta^{87}/^{86}\text{Sr}$ ”.

AC - Thank a lot for pointing our attention to this potential source of misunderstanding. We will follow your suggestions in the revised text.

### 4. Results:

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

AC - Done! Thanks a lot for your suggestion.

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

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RC - Page 805, 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...”

AC - Thanks a lot for your comments. We will add something to give a less simplistic account of potential diagenetic effects.

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

AC - We agree that many of our statements start with overly cautious words. We will rephrase using more affirmative statements: ...”this suggests that variations in  $\delta^{13}\text{C}$  are not the results of facies changes”.

## 5.2. Platform-to-basin chemostratigraphic correlation

RC - “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”.

AC - We understand that we are perhaps confusing the reader. We will rephrase as follows: “was not shaped by local changes”.

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

AC - Done! We are aware of this nomenclatural rule but apparently we still made some mistakes.

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RC - Page 810, 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”.

AC - You are perfectly right. The text as it is might be confusing. We will try to clarify our opinion on this topic in the revised version. Basically, there are three related issues: 1) The Orbitolina level of the Southern Apennines is a well known lithostratigraphic and biostratigraphic marker (cfr. De Castro, 1963; Cherchi et al., 1978). It marks the first occurrence of Mesorbitolina texana and Mesorbitolina parva in the southern Apenninic carbonate platform. There is no doubt on its stratigraphic value on a regional scale. It occurs in homotaxial position with respect to other biostratigraphic events in all the sections we have seen (all over the southern Apennines, not only the three sections studied here) and accordingly has been used as a marker in geological maps since the last century. Its value as a regional marker is confirmed by our chemostratigraphic correlation between the three studied sections (see pp. 806-808 of our ms). 2) As we state since the abstract, the FO of M. parva and M. texana is a biostratigraphic event that is widely recognized at the Tethyan scale. One of the main conclusions of our paper (based on chemostratigraphy) is that it was incorrectly dated in the southern Apennines. Its isochrony of course needs to be tested with independent methods (chemostratigraphy?) but our chemostratigraphic correlation says that it is Early, not Late Aptian in the southern Apennines. This is a hypothesis that can be tested/falsified either in the southern Apennines or elsewhere. 3) In our paper we propose the hypothesis, supported by carbon isotope stratigraphy (see p. 812, 18 and following) that our Orbitolina level of southern Apennines (the one with M. parva and M. texana), could be correlated with the UOBs of the Helvetic Alps and with the “Couches supérieures à orbitolines” of the French Vercors, whereas our Palorbitolina limestones (with P. lentic-

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ularis) could be correlated with the LOBs. We are aware of the doubts raised on the stratigraphic value of the LOBs by previous authors (Clavel et al., 2002; Huck et al., 2011) but we still think that the hypothesis of Orbitolina beds as a supraregional response to palaeoenvironmental perturbations merits to be evaluated (see also Burla et al., 2008). Following you cautionary remarks, we will add some references and some comments in our discussion on this topic.

Thanks a lot again for your suggestions that will certainly lead at a considerable improvement of the final version of our paper.

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

**SED**

3, C395–C400, 2011

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