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Solid Earth Discussions

Interactive comment on "Shallow water carbonate platforms (Late Aptian, Southern Apennines) in the context of supraregional to global changes" *by* A. Raspini

A. Immenhauser (Referee)

adrian.immenhauser@rub.de

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Referees comments on ms Solid Earth Discuss., 3, 901–942, 2011 Shallow water carbonate platforms (Late Aptian, Southern Apennines) in the context of supraregional to global changes by author A. Raspini

I have read this paper with interest as it deals with complex paleo-ecological and environmental questions in mid-Cretaceous carbonate systems that are still poorly understood.

The author presents his data from the Upper Aptian Monte Tobenna and Monte Faito sections. The data include stratigraphic sections, thin section petrography and conven-

tional carbon and oxygen bulk isotope chemostratigraphy. The focus is on Orbitolina mass occurrences, on "marker beds" characterized by what is referred to as an opportunistic faunal content (microbial carbonates and Salpingoporella dinarica-rich facies). These facies types and their characteristic biota are then discussed in the context of palaeo-bathymetry (sequence stratigraphy), assumptions regarding trophic levels and other ecological parameters.

In summary, I find this paper useful but after having read it several times, I do not walk away with the feeling that I have learnt a great deal of new things here. I am aware of the fact that many similar papers have been (and will be) published and hence there is, in essence, nothing wrong with this type of studies. My problem is that I do not feel that this type of work is really advancing our level of knowledge in a substantial way. I guess we lack good data on palaeo-nutrient levels, alkalinity, sea water pH for the Aptian (and most of the Mesozoic) and so on and so forth. This is why I refer to this paper as "useful" as opposed to for example "novel" or "exciting". Obviously, studies such as this one often form a good fundament for future, more conceptual work.

Below I list some aspects that have caught my attention and that may require renewed attention of the author:

Abstract I find the abstract not really well written. To start with, most people would want to phrase an abstract in short, concise statements. The first sentence goes over six lines and is in fact a paragraph in its own. Please break this one in several short and clear statements.

Further on, I suggest reconsidering the organization of the abstract. Most editors agree that the following structure of an abstract is often helpful:

1. (topical) Background 2. Methods 3. Results 4. Conclusions/Implications

There are clearly reasons, why we sometimes deviate from this structure. I also do not understand why the author refers to his work as preliminary? Should we publish preliminary data? I find this confusing.

1. Introduction I must admit that I am sceptical when it comes to so-called "Orbitolina" levels. This results from our work in Oman, where we were able to actually trace Orbitolina facies in the extensive outcrops of the Aptian in the Haushi-Huqf region over distances of many tens of kilometres. What we find in these outcrops is clear evidence for regionally significant Orbitolina facies that grades into Orbitolina-lean facies over distances of some kilometres laterally (Immenhauser et al., 2004, GeoArabia, 9, 153-194). I am not saying this is the case here, I do not know these outcrops, but I doubt the uncritical notion that Orbitolinids form genuine marker levels. They may, but how would we know? Is the biostratigraphic control in these rocks good enough to say so? How much do we know about the paleo-ecology? Some more discussion and arguments would make the paper much stronger here.

Regarding the functional morphology of Orbitolinids. I guess Vilas et al. were the first to describe this episodes and we published one of the early papers providing quantitative data on the functional morphology of Albian Orbitolinds again from Oman (Immenhauser et al., 1999, JSR, 69, 434-464). I refer to this because I am confused to the term "flat-conical". The terminology, that at least I knew at that time, was that Orbitolinids either have flat morphologies (meaning less illumination due to greater levels of turbidity or bathymetric deepening) or conical morphologies (meaning a lower surface area suggesting more illumination suggesting less turbidity or shallower waters). In my dictionary, I find the definition: conical - relating to or resembling a cone. A cone differs substantially from an object that is flat. Once more, I find this confusing. Please explain.

Similar to the Abstract, I find the organization of the Introduction chapter at least unconventional. On page 905, the authors commences with text on aims and then go back to discussion-style text. I find this less than helpful and would welcome a better structure.

2. Geological setting No comment, I am sure the author knows his rocks well.

3. Studied sections No comments other than I do not like the admixture of data presentation and data interpretation (for example exposure related features). But then, it is not always easy to really separate the two.

4. Facies analysis It is obvious that the author knows these rocks well and that he has worked on these outcrops extensively. I find it a bit difficult to separate between review-type of text here (i.e. data presented in previous papers but presented again for clarity) and new data that are shown here for the first time. It would be helpful if the author would indicate this aspect more clearly, please.

5. Cyclic stratigraphy Similar to my comments above, this chapter reads like a summary of previous work merged with some new data. I find this difficult because the paper is not sold as a review paper but as an original contribution if I understand this properly. Please comment. Obviously, data presentation and data separation is again not separated.

6. Chemostratigraphy I find it, sorry for repeating myself, at least unconventional that the author has chosen to merge a method chapter (here isotope analysis) with a data chapter. This is possible but not the way a "normal" paper is structured. I suggest commencing with a Methods chapter followed by a Data Presentation chapter. Further to this, I find it inacceptable that the paper merges methods, data and interpretation in one chapter. I think most colleagues agree that separating data presentation and data interpretation is a fundamental rule of scientific writing. Finally, please do not use more than one decimal when it comes to conventional light isotope data. The significance of 2.85‰ is close to nil. Particularly when referring to bulk data. Drilling the subsample only millimetres apart from the sampling point might have resulted in an isotope value of 2.4‰ or whatever. This is simply due to the inherent variability of bulk rock data in these ancient carbonates.

Another problem that strikes me is the quality of the isotope curves. This is a low-resolution data set with something like 40 data points over 80 meters of section, i.e. less than one data point per section meter. Shoalwater carbonate sections, potentially riddled with exposure surfaces, are dangerous ground for low-resolution chemostratigraphy. Genuine isotope shifts due to changes in seawater chemistry merge with isotope shifts due to subaerial exposure surfaces etc. More discussion is given in Immenhauser, A., Holmden, C. and Patterson, W.P. 2008. Interpreting the Carbon-Isotope Record .... In: Dynamics of Epeiric Seas (Eds B.R. Pratt and C. Holmden), 48, pp. 135-174. Geological Association of Canada Special Publication. I do not request the author to cite my work, this is entirely his decision, but the patterns described in this paper and the references cited therein might be helpful here.

My main criticism is the lack of an in-dept discussion of the significance of the isotope sections with respect to diagenesis.

7. Discussion Obviously, much of the previous text qualifies as discussion making this paper difficult to read. The discussion chapter uses previous work and places the findings from the two sections in the context of this work. This is fine. Following my introductory text, I do not see where the author goes significantly beyond the point previous authors have reached before. As such, the paper is still useful because it ads new data from new sections but not more than that. At least this is my view.

I suggest that the author might wish to read Steuber, T. 2002. Plate tectonic control on the evolution of Cretaceous platform-carbonate production. Geology, 30: 259-262. This paper provides a much refined seawater chemistry for the Cretaceous when compared to very low resolution work of Stanley (see figure 8).

8. Final remarks Once more, I am confused. Is this the Conclusions chapter? I did check in the guidelines of Solid Earth. I might be mistaken, but I see no reason why the present paper is structured and written so unconventionally.

In summary, I found this paper difficult to read and difficult to review. This is due to the - in my view - poor structure and the constant admixture of methods, data,

interpretation and even discussion. I find it difficult to separate between new data and previously published work. Due to these problems I find it difficult to judge on the scientific merits of this ms. I trust that this paper has merits as it adds some good quality sedimentological and petrographical data to the some of these longstanding discussions. I am less impressed by the geochemical data. I suggest major revisions and perhaps re-review but this is the editor's decision. I regret that I cannot support the paper in its present version. I tried to work out if some of these problems are due to specific house rules of the Journal but could not find evidence that this is the case. I might be mistaken.

I hope these comments are, given the fact that they are not overly positive, at least constructive.

Good luck with your paper!

Adrian Immenhauser

Interactive comment on Solid Earth Discuss., 3, 901, 2011.