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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

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General comments I have read this manuscript carefully and with interest, as it presents a multi-disciplinary approach (using sedimentology, chemostratigraphy, palaeoecology, micropalaeontology) to address issues that, not being novel, are still under debate, thus unresolved. The author presents data from two Upper Aptian carbonate sections in the Apennines (Southern Italy). The focus of the paper is on the possibility that particular facies types, namely microbial carbonates, and associated palaeobiota, namely mass-occurrences of the foraminifer genus Orbitolina and Salpingoporella dinarica (dasycladacean algae) rich-deposits, reflect palaeoecological responses to supra-regional

sea-level fluctuations. The author uses data from facies analysis (field and petrographical characterization), micropalaeontology (foraminifera and calcareous algae), chemostratigraphy (Carbon and Oxygen stable isotopes) and cyclostratigraphy, makes a separate discussion of each dataset regarding sea-level changes and then tries to make a link among overall data and external controls, taking into account several studies from elsewhere. As stated, in my view trying to apply an integrated approach to address a complex palaeoenvironmental and paleoecological subject, is, in essence, correct. Though some of the main points addressed in this work are not raised for the first time, bringing new material and additional studies in order to help unravelling issues for which interpretation stills lacks stronger evidence is fine and useful. We don′t need only to see very novel things published, in the sense that new ideas need to be tested with further applications and that fundamental knowledge will always be based on, and improved by, systematic and replicable studies. However, as a read the text, I became somewhat uncomfortable because the way the study is presented makes the final integration a bit difficult. I also find that some of what would be, according to the author, the most crucial elements shedding new light on the palaeoecological controls (namely isotopes) are not robust enough, or at least the way they are reported in this manuscript is not convincing enough. I will explain as I go through the structure of the manuscript and I suggest that the author improves these matters and the structure of the text, in order to strengthen potential adding value of his work. After careful revision considering the points addressed below, I would support acceptance of the work for publication on Solid Earth.

Specific comments Title The focus of the paper is on the possible palaeocological expressions in shallow carbonate systems of externally induced changes. Therefore, I think that this could be more clearly indicated in the title, maybe adding something like ".... global changes: re-appraisal of palaeocological events as signs of carbonate factory responses". That's the author's choice, but for me it would help focusing the reader′s attention and highlight the relevance of the "peculiar facies" (which to me are the most interesting subject).

Abstract I find that the abstract is not satisfactory and should be re-written. It doesn't give a clear picture of the type of studies that have been made or used, neither of the intended multidisciplinary approach in order to support the overall interpretation. It starts directly with mention to isotopes curves (a matter to be discussed), as being a typical chemostratigraphy paper (which it isn't, with advantage); then goes into conclusions of sea-level phases versus particular (peculiar according to the author) facies without giving a clear basis to the reader regarding what is the point of the paper.

1. Introduction I am sure the author did a detailed and serious facies study, as reflected in the way he shows to know the units, their stratigraphic pattern and regional distribution, plus his previous publications; thus sedimentary data don't rise me any doubts. The author uses many previous work, of his own or from others; credit is adequately given to the latter, but a clearer mention to the type of studies undertaken by each author would be better.

2. Geological setting OK.

OBS. A new item of Methods must be introduced here and thus material/methods separated from descriptions below.

3. The studied sections Here I felt the need of a brief summary of the successions, which is not presented. Just something like, for instance: "at Monte Tobenna... x metres of green marls are overlain by y metres of fossiliferous limestones..." and so on, and the same for Monte Faito outcrop. There are the logs, of course, but a summary in the text is needed for the reader not familiar with the field, before dealing in more detail with the facies types.

4. Facies analysis Generally OK, but: - separation between description and interpretation could still be improved. - at 4.1 (line 5), instead of saying "...lithofacies B4 and B3...", name the facies (and link them to the name of the item 4.1.2, microbial carbonates). - to give an idea of the relative proportion and distribution of both types of microbial dominated-lithofacies (B3 and B4) would be relevant, as cryptalgal bind-

stones and Thaumatoporella are two rather different things, with the latter closer to algae in terms of palaeocological behaviour, as we know.

5. Cyclic stratigraphy I understand that the author relies on published previous work, which is cited, and that's why he doesn't explain in detail the criteria used for defining the cycles, their limits and order, and so far. But, anyway, the summary of the cyclical patterns reads a little confusing; I suggest that the author tries to make it more clear. I also don't understand the purpose of the last paragraph here (lines 18-21): is it to stress that the author is aware of possible misinterpretations at these intervals?

6. Chemostratigraphy The use of this discipline, particularly isotopes, in shallow-water settings is far less common than in hemipelagic series, and improving documentation on particular cases is not bad; however, the reasons why application of isotopic stratigraphy to shallow water carbonates is more restricted must not be forgotten. The author invokes an accurate link between the facies analysis and chemostratigraphy; however, there's a problem here for me because of the low-resolution of the chemostratigraphical analysis, a fact that the author himself recognizes. This low resolution data could be acceptable if such chemostratigraphical (isotopic) data was considered a minor, supplementary topic in the paper, but it isn't; the author assigns it a major role here. Applying isotopes to shallow-water carbonate deposits is always problematic due to the strong diagenetic complexities, more so when using bulk samples; if one adds to this a low-resolution sampling, the obtained data shouldn't be used as an major ground for jumping into conclusions. The author states that, in spite of low-resolution and possible diagenetic overprints, the trends obtained are consistent with other, more robust isotope data and curves from elsewhere, thus are reliable. But this seems to me a weak argument: when deviations exist, one would invoke local, sampling or diagenetic causes; when gross trends are "good", the importance of sampling and analytical results being representative is diminished. By the way, though several works within this scope are considered by the author, I would suggest one more, Huck et al. (2010; see below), as it is also focused on chemostratigraphy of shoal-water carbonate sys-

tems from the Tethyan Aptian, and relates this with OAE1a. So, I think this aspect should be reconsidered: either the author wishes to keep isotopes as a strong ground for his reasoning, and then a better explanation or dataset should be obtained; or he further re-focus on the other elements (the peculiar facies) giving less importance to chemostratigraphy in the present case.

7. Discussion The author makes a separate discussion of each type of elements considered crucial for his interpretation, 7.1 The Orbitolina level, 7.2. Microbial carbonates, 7.3. Salpingoporella dinarica-rich deposits, comparing his material and data with many other works from different sources. Although the causes of the huge Orbitolinid concentrations to produce coeval Orbitolinid levels at many locations in the Tethys realm are not completely understood, previous works (several cited by the author, but see below) and the present arguments for a plausible (not a definite) relationship with allogenic controls are valid. I know the equivalent levels from the Lusitanian Basin in Portugal, where they are a regional marker also, and the overall environmental evolution, transgressive-regressive scenario, so on, doesn't go against the ideas here argued. However, one study I see as lacking in the author's comparisons is Immenhauser et al. (2005; see below), where the authors document microbial-foraminiferal episodes (different taxa than current work) in the Aptian of southern Tethys (Oman), arguing for these episodes being the shallow marine expression of an OAE1a. Thus, an obvious affinity of the present approach with the previous one exists, for equivalent times in the Tehyan realm, so that study must be mentioned and cited. The separate discussion for each topic is suitable and a hard work analysing a broad range of literature on the different topics involved, to frame his own ideas, is a merit to be acknowledged to the author. But as the text is somewhat heavy to read, the final integration turns a bit difficult without a summary of main points. So, an additional item called 7.4.Summary, preparing the reader for the more synthetic conclusions, would largely benefit the outcome of the work, in my opinion. Moreover, accordingly to my comment on 4.1.2., I would like to see a separate discussion on microbial facies B3 and microbial facies B4.

8. Final remarks I would prefer "Conclusions" and preceded by a brief summary of the discussion (see previous comment).

References The overall number, nature and quality of the cited works is appropriate, but I recommended above 2 additional works, whose full references are: Huck, S., Rameil, N., Korbar, T., Heimhofer, U., Wieczorek, T.D., Immenhauser, A. (2010) - Latitudinally different responses of Tethyan shoal-water carbonate systems to the Early Aptian oceanic anoxic event (OAE1a). Sedimentology, 57, 1585-1614. Immenhauser, A., Hillgärtner, H., Bentum, E.V. (2005) - Microbial-foraminiferal episodes in the Early Aptian of the Southern Tethyan margin: ecological significance and possible relation to oceanic anoxic event 1a. Sedimentology, 52, 77-99.

I have carefully checked citations in the text and list of references and found only minor mistakes (see "Technical corrections").

Technical corrections Spelling errors at: - Abstract, line 5: regulating, not regolating. - Abstract, line 11: I don't appreciate the term dictator in this sense (it is also used further in the text), but that's just a terminology opinion. Why not forcing factor, more in tune with common geological terminology? - Introduction, p. 905, 8: Mutterlose, 1998: missing in the reference list. - The studied sections, p. 906, line 15: outcrop, not outcropping. - Facies analysis, p. 908, line 1: Kosir et al., 2004: in the references just Kosir, A. References: - Buonocunto et al., 1994. Is in the list, I didn't find it neither in the text nor in the figures. - Gattacec et al., 2002. out of alphabetical order. - Kosir, A., 2004: in the references just Kosir, A. in the text is Kosir et al. -Bonardi et al., 1992 (caption of Fig.1): not in the reference list. - Piobbico and Cismon, in Weisser et al. 1998, I presume (caption of Fig. 6). - Clarke and Jenkins (1999 not 1995, caption of Fig. 7).

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