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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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It was with interest that I read and reviewed the manuscript by A. Raspini. At the same time, I was somewhat astonished by the fact that two different manuscripts on the very same stratigraphic interval have been submitted by different Italian researchers to the same special issue in SE, with very little cross referencing. In fact, the sections studied by A. Raspini are sandwiched by the sections studied by Di Lucia et al. and all sections appear to be representative of the same paleogeographic region.

In reading this manuscript, it strikes me that the age interpretations are very different between the two manuscripts. Whereas for Di Lucia et al., the Orbitolina Bed has a

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middle early Aptian age (coeval with OAE 1a), for Raspini, the same lithological unit dates from the middle late Aptian and correlates with the Fallot episode. Raspini bases his age inference on the presence of *Mesorbitolina texana* and *M. parva*, whereas Di Lucia et al. uses a correlation between $\delta^{13}\text{C}$ records to corroborate their age attribution. I personally tend to favor the solution offered by di Lucia et al., based my own experience with Mesorbitolinid stratigraphy in Aptian sediments of the Alps (Föllmi and Gainon, 2008, *Sed. Geol.*; Föllmi, 2008, *Rev. Paléobiol.*). In an eventually revised version of this manuscript I would therefore welcome more arguments for the middle late Aptian age attribution. I especially would like to see a justification of the biostratigraphic results, despite the problems with this elsewhere. It would also be good to know why the 4‰ positive shift in the $\delta^{13}\text{C}$ record from D'Argenio et al. was correlated with a minor, 1‰ shift near the base of the late Aptian, rather than with the large positive shift in the middle early Aptian in corresponding pelagic sections. The used correlation is quite arguable, and I don't think that the $\delta^{18}\text{O}$ record can be used to justify this correlation given the rather poor resemblance with the Pacific record.

Evidently, most other interpretations depend on a correct age attribution and as long as this issue is not resolved, it does not make much sense to review those in more detail. The only observation that I can add here is that some inferences on the quality of the surface waters and temporal changes therein appear rather qualitative, without direct geochemical evidence (e.g., inferred nutrient levels).

It is probably in the interest of the editors of the special volume to include manuscripts, which are not too incoherent in terms of their interpretations of the same subject and which are also linked by cross references. I would therefore invite the author to correspondingly revise his manuscript and I would be very happy to review a new version in more detail.

Karl Föllmi, Lausanne, 10-01-2012

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